How Good (or Bad) Are the Inner Boundary Conditions for Heliospheric Solar Wind Modeling?

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A map of the Sun's magnetic field is the starting point (the inner boundary condition) for 'realistic' models of the solar wind and *its* magnetic field through the heliosphere. No matter how sophisticated (and 'physics-based') the model is, it is no better than the quality of that map. There are two main (and in my humble opinion, partly unresolved) issues:

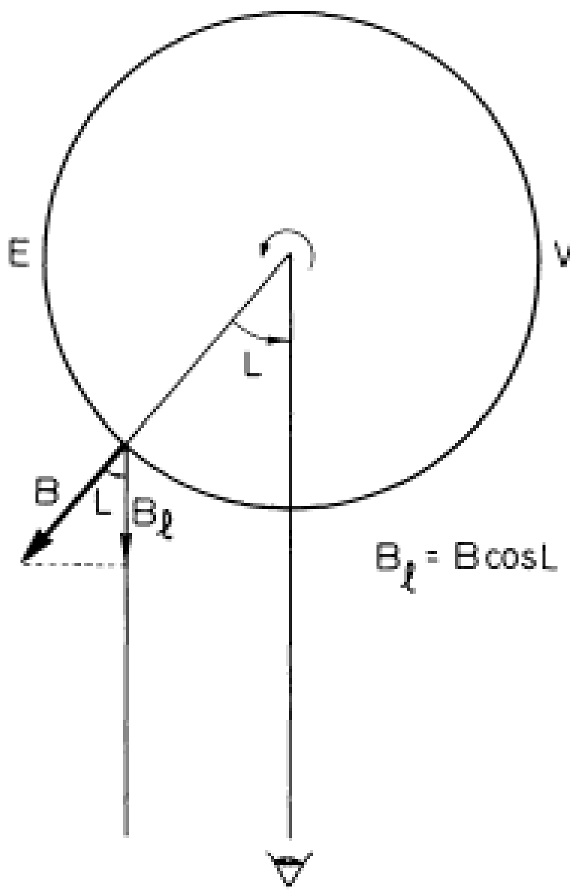
1: The "true" magnetic flux

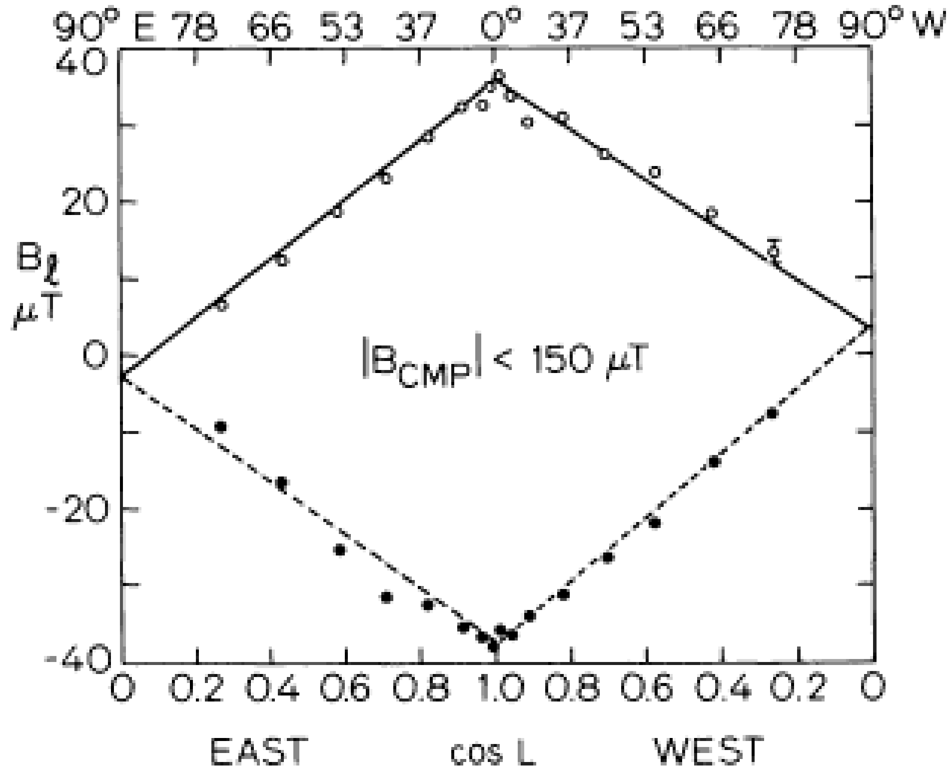
2: The Polar Fields

Our ignorance is often plastered over by suitable parameterizations ("fudging") that, in turn, become limiting factors in applying Physics to the problem. Today, I'll consider the first of these two issues from my perspective. (The other one on Xxxday).

Longitudinal (line-of-sight, LOS) synoptic maps are the basic input to the models. I don't know of anybody producing or using whole surface maps of the full magnetic vector (or how to do that).

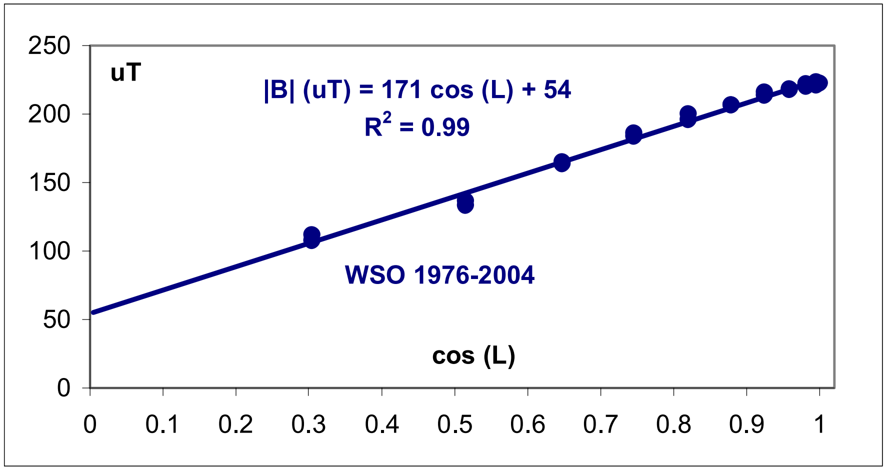
Here is the geometry. The Zeeman effect gives the LOS component which for a radial field, ***B***, and heliocentric angle, L, is $B_l = B \cos(L)$.

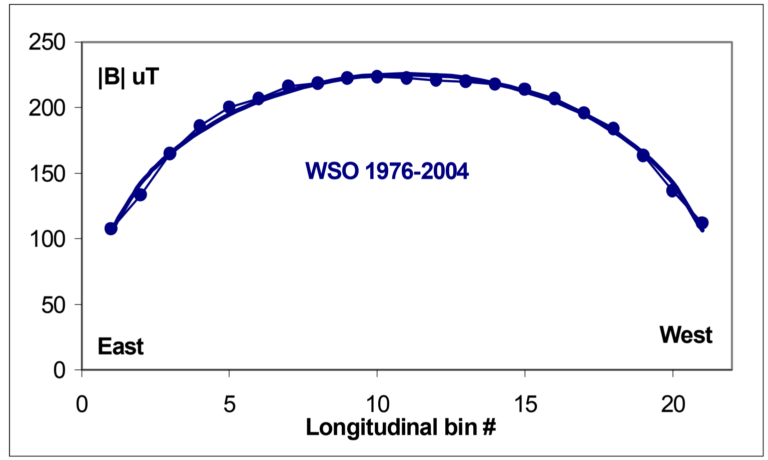




Observations at WSO show that the observed flux is consistent with a cos(L) law.

The earlier analysis was made by carefully following low-latitude regions across the disk. A simpler procedure is just computing the average absolute *B* as measured. This induces a noise component, showing up as an offset. Here is the result of 29 years of WSO data:

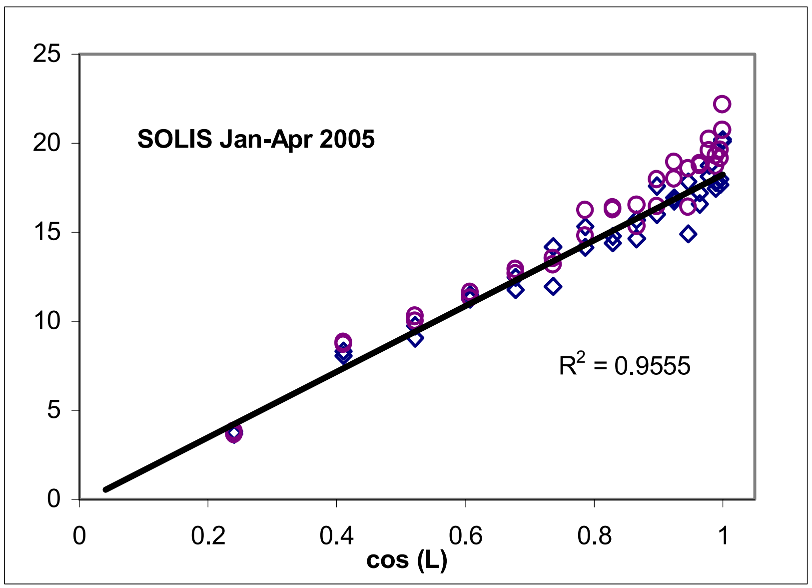


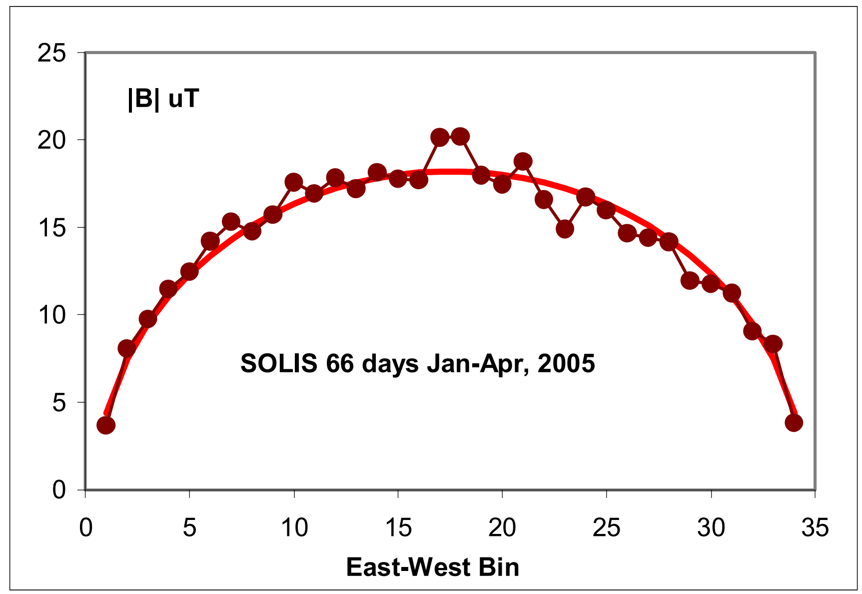


Basically confirming the cos(*L*) dependence. If we plot against distance on the disk from central meridian (*i.e.* sin(*L*)) we get the graph on the right. In the limit of no noise that would trace out a semicircle. The result of the simple analysis is pretty close to that.

The conclusion is that all the **WSO data show that the observed field behaves as a simple projection of a radial field**.

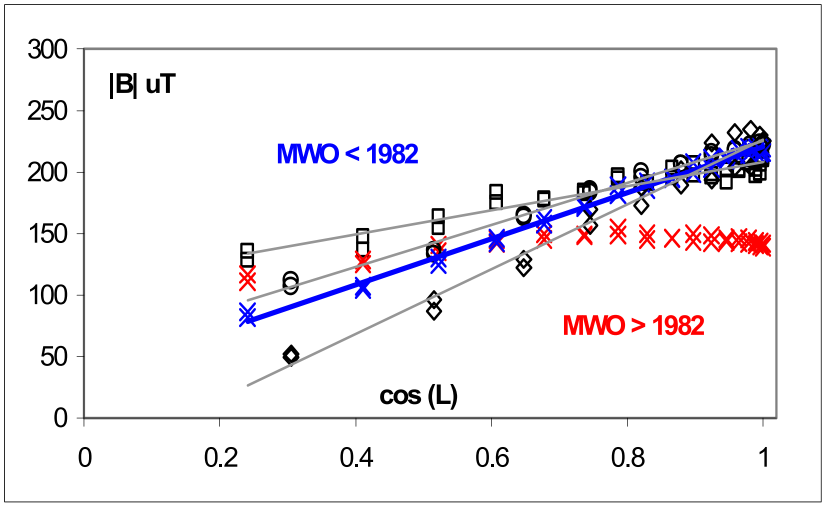
WSO uses the 525.0 nm FeI Line. Maybe this result is specific to that line or the particular instrumentation (double-slit Babcock-type magnetograph) or observation procedure at WSO? The new SOLIS facility at Kitt Peak uses the 630.2 nm FeI lines and records the full Stokes vector. Here is the result of the same type of analysis using SOLIS data:

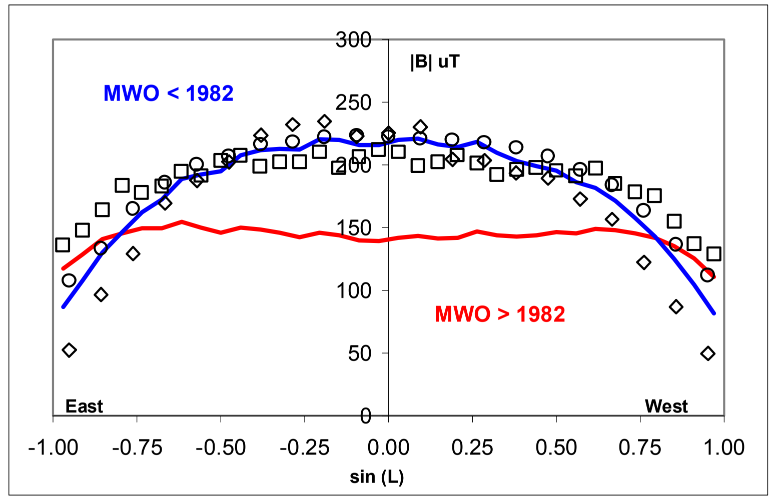




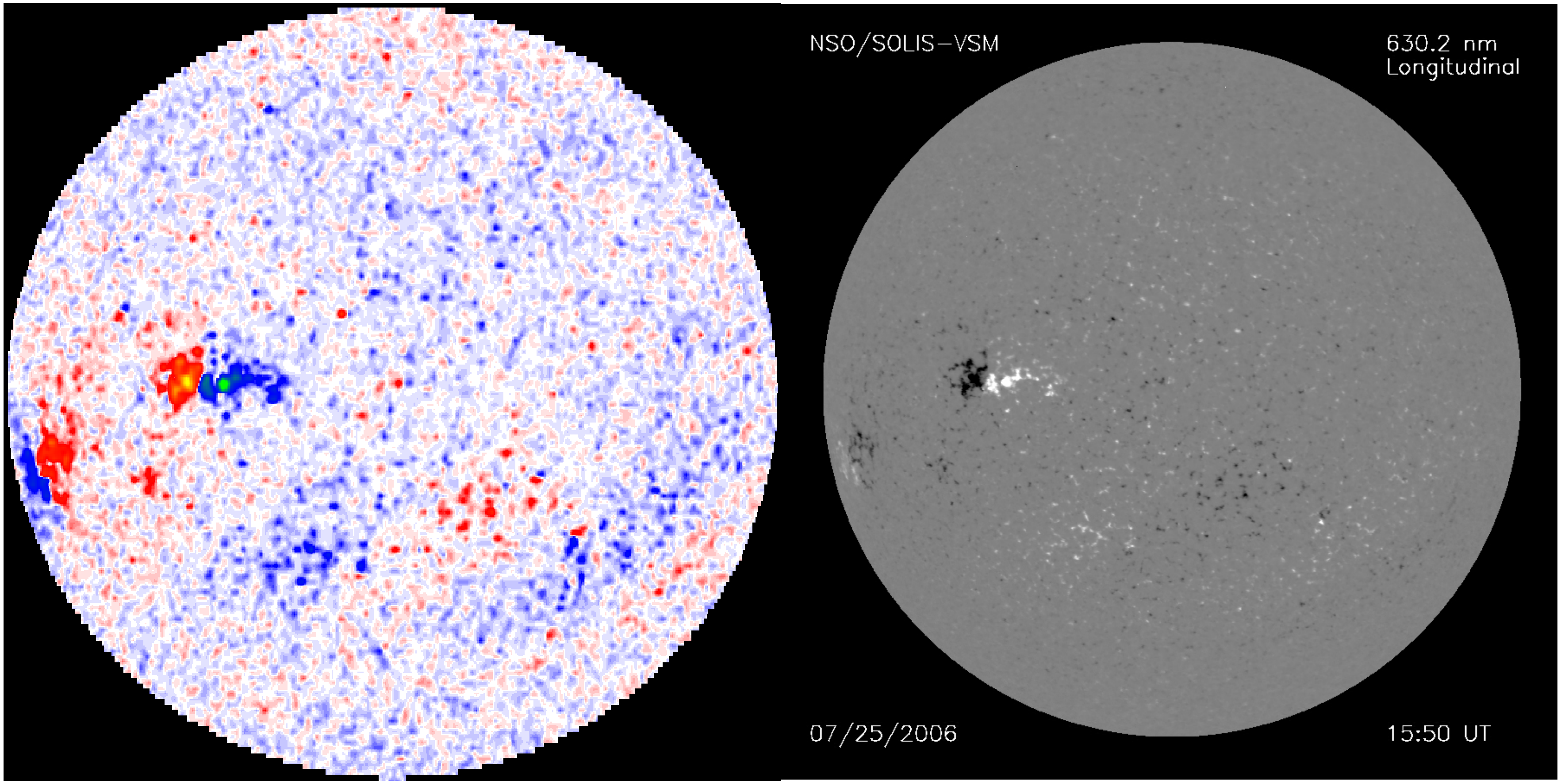
The result is very much the same as for WSO, namely that **SOLIS data show that the observed field behaves as a simple projection of a radial field**.

The MWO (using 525.0 nm FeI) underwent a major upgrade in April 1982, so we do the analysis separately before and after 1982. The noise level was considerably higher for the older data (indeed the offset is large). The blue symbols correspond to various low-latitude bins, pre-1982. The full red line is for the mean of the several scan lines after 1982.



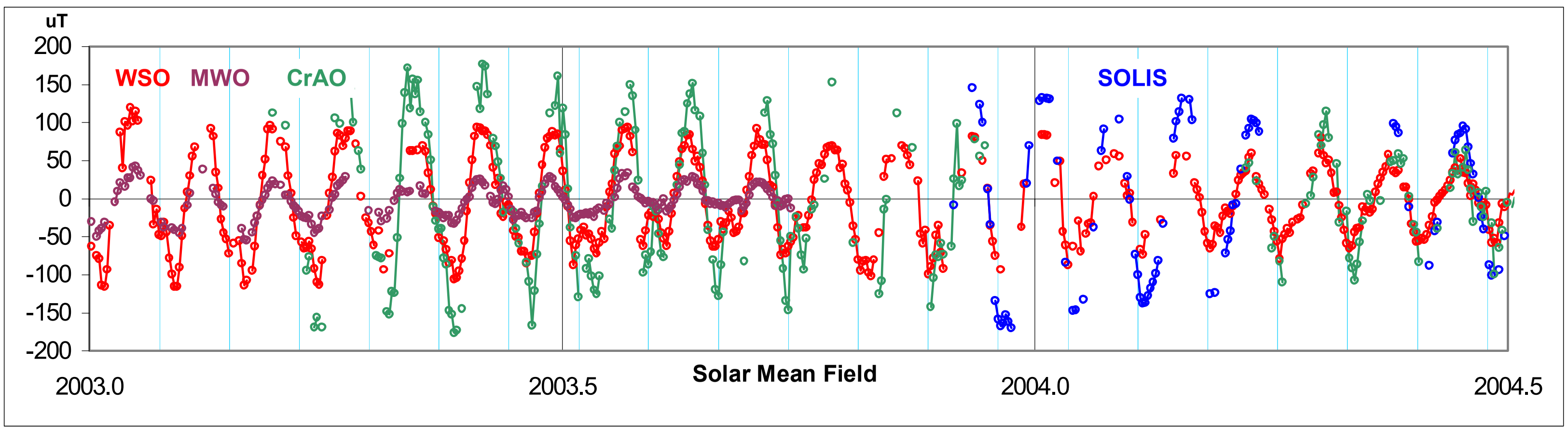


Before the upgrade in 1982, the field measured at MWO behaves just as at WSO and at SOLIS, namely: **MWO pre-upgrade data show that the observed field behaves as a simple projection of a radial field.** This is NOT the case after the upgrade in 1982, and that is the issue I have with MWO: the flux is almost constant across the disk with no weakening due to projection (except at the very limb)



The lack of center-to-limb weakening by projection at MWO can be easily discerning by eye. MWO left and SOLIS right.

The Sun's 'mean field" is the magnetic signal of integrated unfocused sunlight. Most of the signal comes from the central half ($0.5R_o$) of the disk. Most solar observatories have measured the mean field. For others, a good approximation (to a few per cent) can be made by simply calculating the average flux over all pixels. This works because the fluxes at opposite sides of the disk often cancel (e.g. for axial and equatorial dipoles). The mean field is weak (~1 Gauss [100 microTesla] or less). Different observatories measure a mean field of the same sign (and "pattern") but of different magnitude:

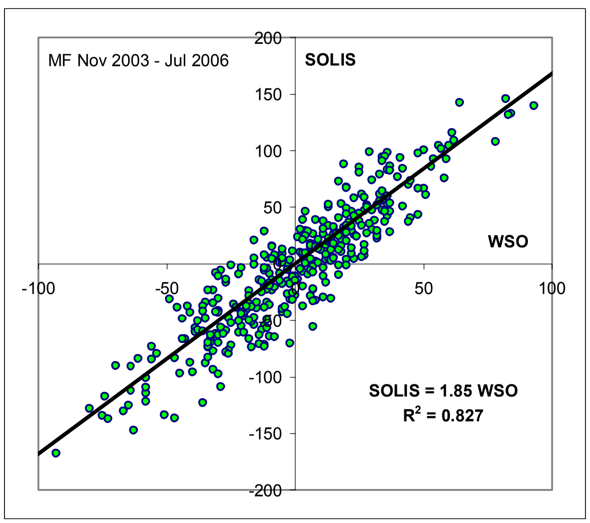


Note the clear 27-day recurrence pattern, seen at all observatories. For each observatory there exists a number such that the mean field measured at that observatory will match that measured at SOLIS. We assume for the sake of the argument that SOLIS measures the "true" magnitude of the LOS of the field.

These “magic” numbers are:

Obs.	Location	Line (nm)	Factor
SOLIS	Kitt Peak	630.2	**1.0**
WSO	Wilcox Solar Observatory	525.0	**1.85**
MSO	Mount Wilson Observatory (after 1982)	525.0	**4.2**
CrAO	Crimean Astrophysical Obs.	525.0	**1.15**
BiSON	Birmingham Solar-Oscillation Network	769.9	**1.2**

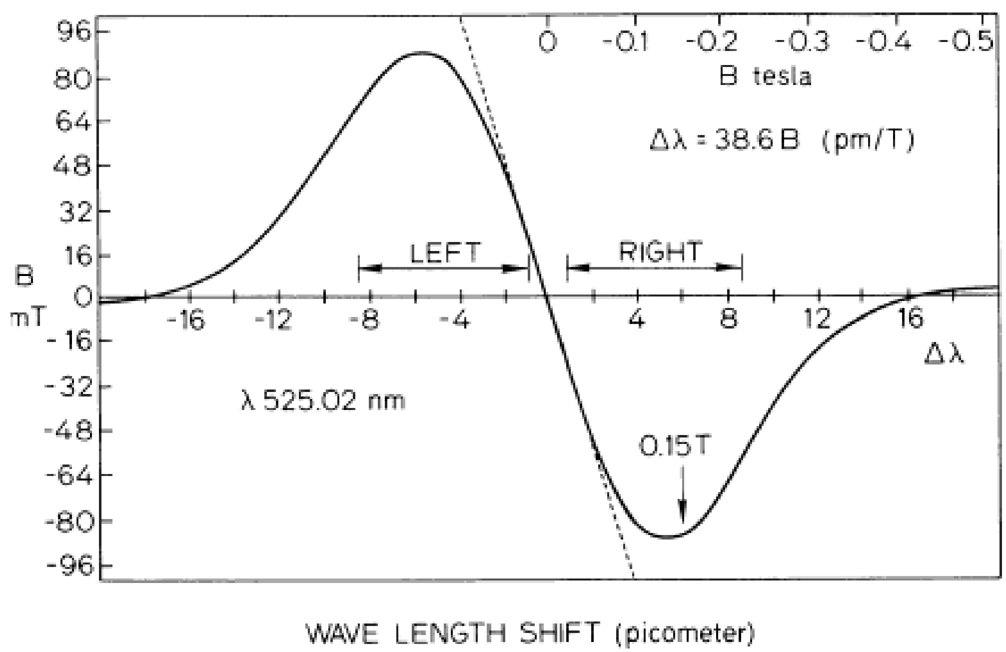
The numbers for SOLIS, CrAO, and BiSON are close to 1 and might indicate some kind of “consensus” or convergence towards to the “truth” although the exact details of their calibration are not known to this author. That leaves WSO and MWO.



Here is the correlation between SOLIS and WSO as an example of how the “magic” number is arrived at.

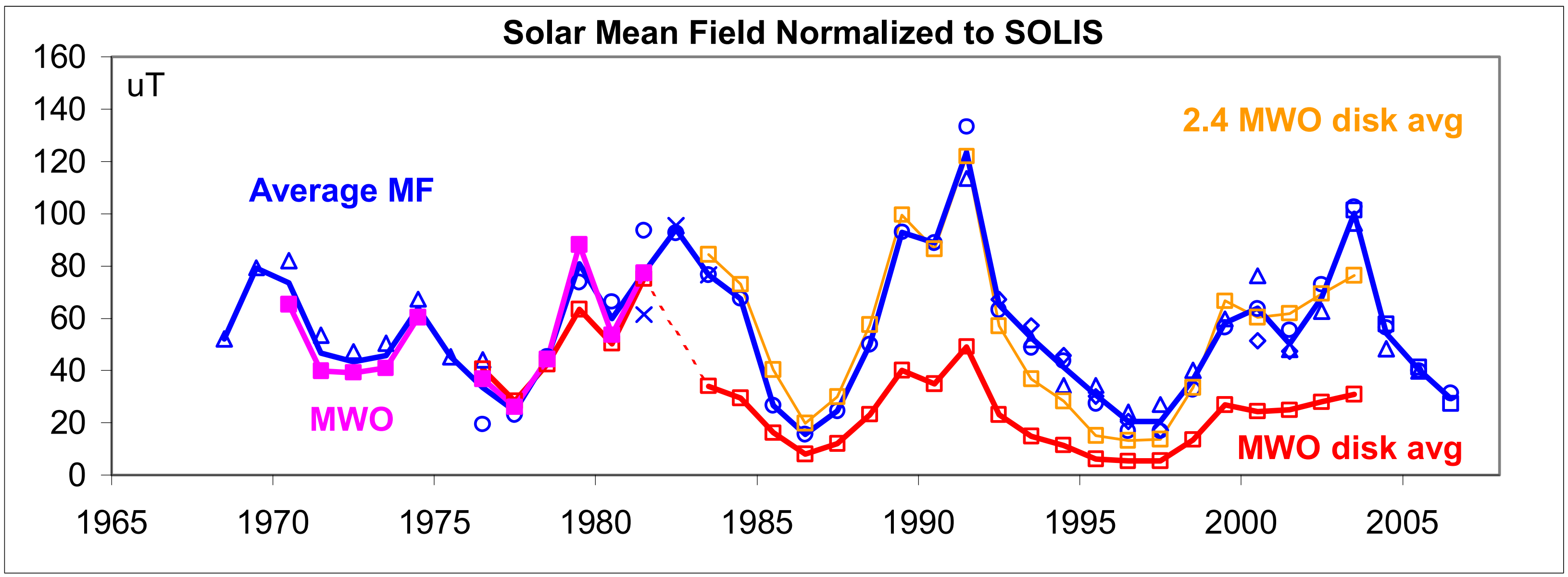
Similar plots are found for the other observatories. In deriving the “scale factors”, the slope of the line was computed by minimizing the sum of the squared differences perpendicular to the line, rather than the ordinary least squares (vertical) fit.

For WSO, I think we know why its values are lower than SOLIS's (the "true" values) by a factor of 1.85. I placed a right-hand circular polarizer in front of the KDP crystal and recorded the magnetic signal while scanning across the spectral line. This was the result:



For weak fields, the signal is proportional to the field strength (dotted line). As the field strength increases, the magnetograph signal weakens and at 143 mT the instrument is saturated and any further increase actually decreases the magnetic signal. If the field strength of the magnetic elements is 150 mT (1500 Gauss) the reading on the magnetograph would be only 83 mT, so the effect of the saturation would be to decrease the flux by a factor of 150/83 = 1.8. A factor of 1.85 as deduced from the comparison with SOLIS would be caused by elements of strength 160 mT (assuming we are on the outside of the peak).

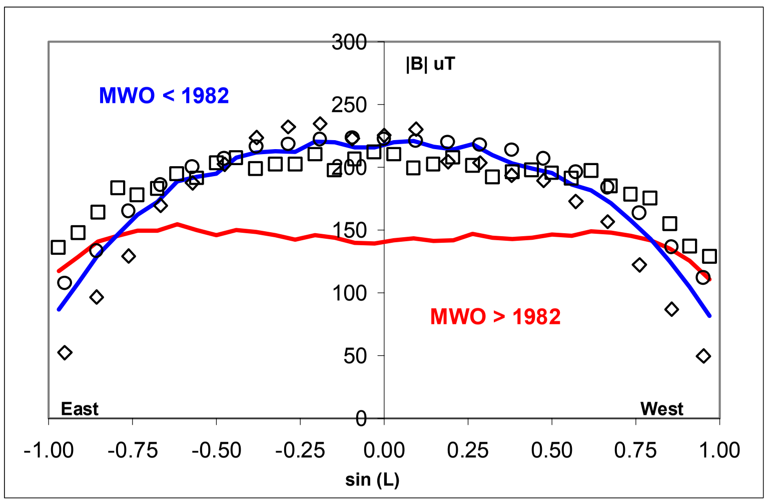
WSO has precisely the same slit-assembly sizes as MWO (it was carefully designed that way), therefore both WSO and MWO should suffer the same magnetograph saturation. In fact, they ***did*** before the upgrade in 1982, because the mean field measured at the two observatories were almost identical, $MF_{WSO} = 1.1\ MF_{MWO}$, before 1982. Combining *MF* data from all observatories (WSO and MWO multiplied by 1.85) we get:



There is good agreement except for MWO after the upgrade where we need to multiply by a ***further*** factor of ~2.4 for a total of 1.85*2.4 = 4.4.

Indeed, the MWO observers have shown that to get the “true” flux their data should be multiplied by $\boldsymbol{M = 4.5 - 2.5 \sin^2(D)}$, where D is the heliographic angular distance from disk center. For the mean field, D is small enough (most of the contribution comes from the central part of the disk) that $M \sim 4$, in agreement with our analysis above.

If you recall the plot of the flux magnitude across the disk for MWO:



The full blue line was actually not the average of the blue symbols, but is the red line multiplied by M (and scaled to match the blue symbols at central meridian, compensating for the different time intervals). The MWO correction factor thus restores the projection effect for radial fields seen before the upgrade and observed at all other observatories too.

The conclusion must be that the ***MWO correction is not a solar property to be applied to the 525.0 line as such, but corrects an instrumental problem with the upgraded MWO***.

The correction must therefore not be applied to data from other observatories

In The Astrophysical Journal, *644*:638–645, 2006 June 10 one finds “Role Of The Sun's Nonaxisymmetric Open Flux In Cosmic-Ray Modulation” by reputable authors Y.-M. Wang, N. R. Sheeley, Jr., and A. P. Rouillard. I quote (my emphasis):

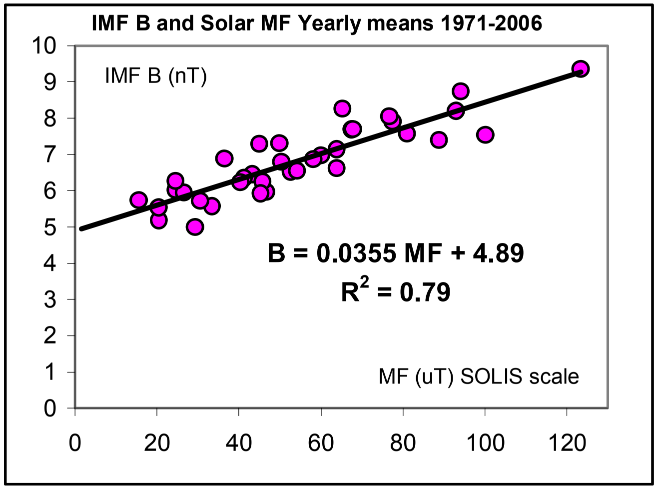
> “***Both*** the MWO and the WSO measurements were corrected for the saturation of the Fe I 5250 profile by multiplying the magnetic fluxes by the factor 4:5 - 2:5 $\sin^2(L)$; as discussed in Wang & Sheeley (1995, 2002) and Arge et al. (2002). This correction, which depends on the center-to-limb angle, was derived by Ulrich (1992) by performing simultaneous measurements in 5250 and the nonsaturating 5234 line”.

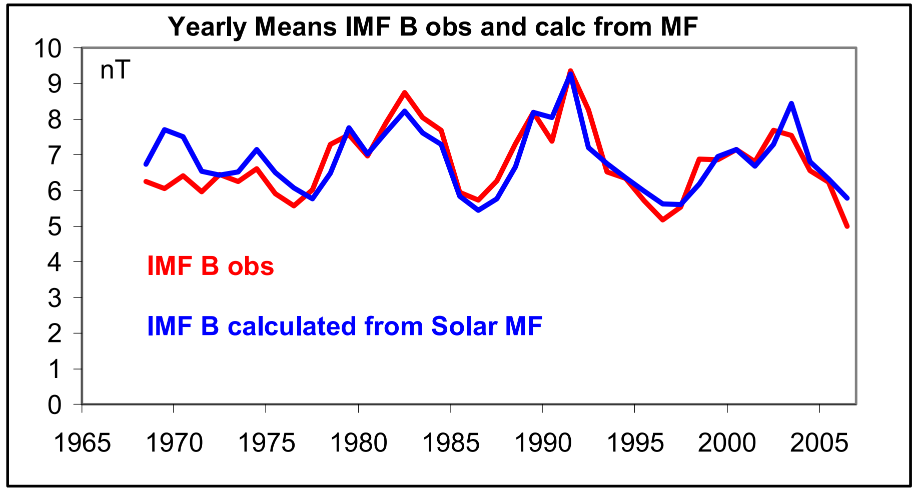
Numerous other papers by numerous other authors do the same. This seems to have become common practice. Why? Because otherwise the calculated IMF field strength comes out too small by a factor of two. A case of two wrongs making a right? It seems to me that this problem weakens the scientific value of the models and the conclusions.

The cause of the MWO correction is unknown. My own (wild) guess is that the cause is an error (correcting for projection?) in the lowest level data-reduction programs.

I have brought the above analysis to the attention of the MWO observers and have been met with a stony silence. Maybe this Workshop might spur somebody to action so that the issue can be resolved. In any event, stop using the MWO correction on anything else.

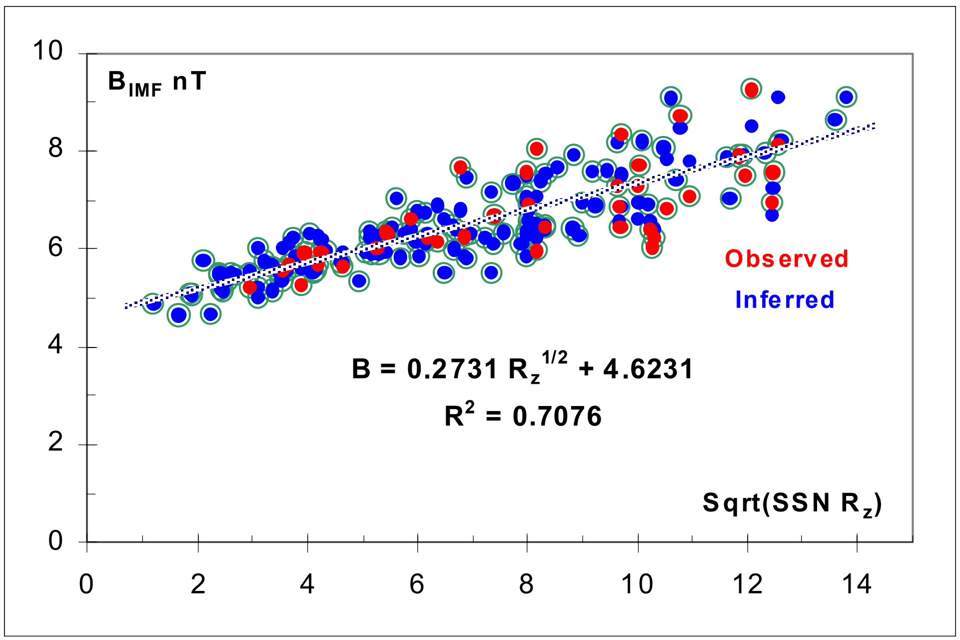
It was shown almost forty years ago that the solar mean field was a good predictor of the polarity and strength, *B*, of interplanetary magnetic field (IMF) 4.5 days later. The field strength on short scale is influenced by dynamic effects in the solar wind (sweeping up as higher speed wind overtakes slower speed wind), but averages over a rotation or a year show a strong correlation:





Most (79%) of the solar cycle variation of the IMF strength stems from the solar cycle variation of the solar mean field (coming from the central 25% of the disk). The IMF sits on a "floor" of ~4.9 nT that does not seem to have varied much. We have been able to reconstruct IMF *B* back to 1872 and find the same floor to be present at all times. It is not clear where the floor comes from. The polar fields have varied more than the floor.

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 (R_Z). Thus their contribution to the average IMF strength will tend to increase as $R_Z^{1/2}$ [for a detailed discussion, see Wang and Sheeley, 2003 and Wang et al., 2005]. We find, indeed, that there is a linear relation between B (sitting on its floor) and the square root of the R_Z:



The best-fit ($R^2 = 0.71$) regression equation is

$$B\ (\text{nT}) = (4.62 \pm 0.16) + (0.273 \pm 0.015)\ R_Z^{1/2}$$

For $R_Z \Rightarrow 0$, $B \Rightarrow 4.62$ nT. It seems clear that the polar fields somehow must be involved in providing the floor. It is not clear how.

The importance of the polar fields is discussed further in my other talk on XXXday.

Summary and Comments

Modeling of the solar wind has achieved a certain degree of sophistication, described by the practitioners as 'realistic, self-consistent, fully 3D, time-dependent, MHD, data-driven, etc'. All that sophistication hinges ultimately on the accuracy and timeliness of the magnetic field data that form the inner boundary condition for the calculations. Some of the deficiencies of the data can be covered up with empirical fudge-factors ('calibrations'). Useful forecasts (even nowcasts) ensue when actual conditions are reliably observed to deviate from 'climatology'. Unfortunately, the magnetic field in the photosphere is perhaps poorly known. The large-scale fields that determine the field higher up in the corona have weak fluxes close to the noise-level of the magnetographs and are thus sensitive to zero-level errors. When strong indications appeared 40 years ago (Stenflo) that much of the flux is concentrated in sub-arcsecond 'flux tubes' it became clear that fixed-width, double-slit, Babcock-type magnetographs would suffer from saturation as the Zeeman splitting becomes comparable to the separation of the slits. For true field strengths of 1500 Gauss (0.15 mT) the saturation factor at the Wilcox Solar Observatory (WSO) is 1.8, meaning that you have to multiply the observed values by 1.8 to get the 'true' field strength. A convenient way of condensing the enormous amount of data in spatially resolved magnetograms is to calculate the 'mean field' of the disk. At most observatories the mean field (MF) is also observed directly by measuring the Zeeman effect using unfocused sunlight. MF observations (one or more values per day) exist from 1968 to the present. We compare all available data from the Crimean Astrophysical Observatory CrAO, Mount Wilson Observatory MWO, Wilcox Solar Observatory WSO, Birmingham Solar-Oscillations Network BiSON, Kitt Peak now SOLIS), Michelson Doppler Imager MDI, and Sayan Solar Observatory SSO. All of these measurements can be made to agree quantitatively by multiplying data from each observatory by a certain factor (or in case of MWO two, one before and one after an upgrade of the instrument in 1982). Each observatory has its own unique factor and they are all different, ranging from one to four. Some of these are understood, e.g. for WSO, others are not, *e.g.* for MWO. Modelers sometimes use the factor for one observatory on data from another observatory, apparently believing that the 'saturation' factor is of solar origin rather than instrumental. The main justification being that it makes the Potential Field Source Surface (PFSS) model better fit the observed interplanetary flux and polarity. Better fit or not, this is nevertheless wrong. If we believe that SOLIS and BiSON are close to the truth ('saturation' factor ~1), the IMF field strength calculated from the PFSS is too low by a factor of two. For WSO and SOLIS and probably the other observatories too, except MWO after 1982, the observed field strength falls off with distance from the center of the disk as if the field was radial and was a simple projection unto the line-of-sight (as we would expect for the Zeeman effect). For MWO after 1982, the observed average field is constant along the near equator across the disk (except very near the limb). By postulating a 'correction' factor that varies with distance from disk center, the projection effect can be restored. This is extremely puzzling, as the 'correction' function varying over the disk is the result of a very careful analysis by the MWO observers. All this is completely mysterious. Maybe the sophisticated models are colossi on clay feet. We need to determine what is really going on.