Polar Fields, Large-Scale Fields, 'Magnetic Memory', and Solar Cycle Prediction

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We consider ‘precursor’ methods using the following features:

A1: “Latitudinal poloidal fields at high latitudes” [Dikpati *et al.*]
A2: Geomagnetic activity [Hathaway & Wilson]
B1: H-alpha patterns [Tlatov *et al.*]
B2: Polar fields, [Schatten *et al.*, Svalgaard *et al.*]

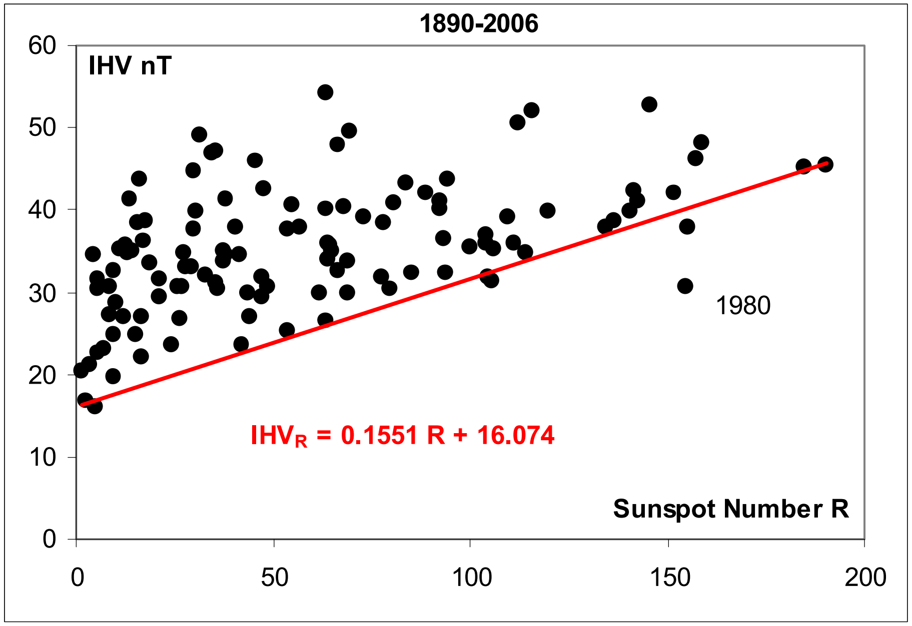
An important defining difference between the methods is the length of the ‘magnetic memory’ of the Sun, that is, the time between the occurrence of the features involved and of the subsequent appearance of the sunspots being predicted. Authors of methods in the ‘A’ group have quoted memory times of 17-40 years (2-4 cycles), while authors of the ‘B’ group advocate a much shorter memory time of only one half solar cycle.

A1: Dikpati has just given us an excellent review of their model, relieving me of the task, except for drawing attention to a recent correspondence in *Nature* [vol. 442, 26(6 July 2006]) by Tobias *et al.*, where they caution:

> The model proposed by Mausumi Dikpati and her team relies on parametrization of many poorly understood effects. […T]he dynamo equations are extremely nonlinear; the solar dynamo is believed to exist in a state of deterministic chaos, making prediction intrinsically yet more difficult. Any predictions made with such models should be treated with extreme caution (or perhaps disregarded), as they lack solid physical underpinnings.

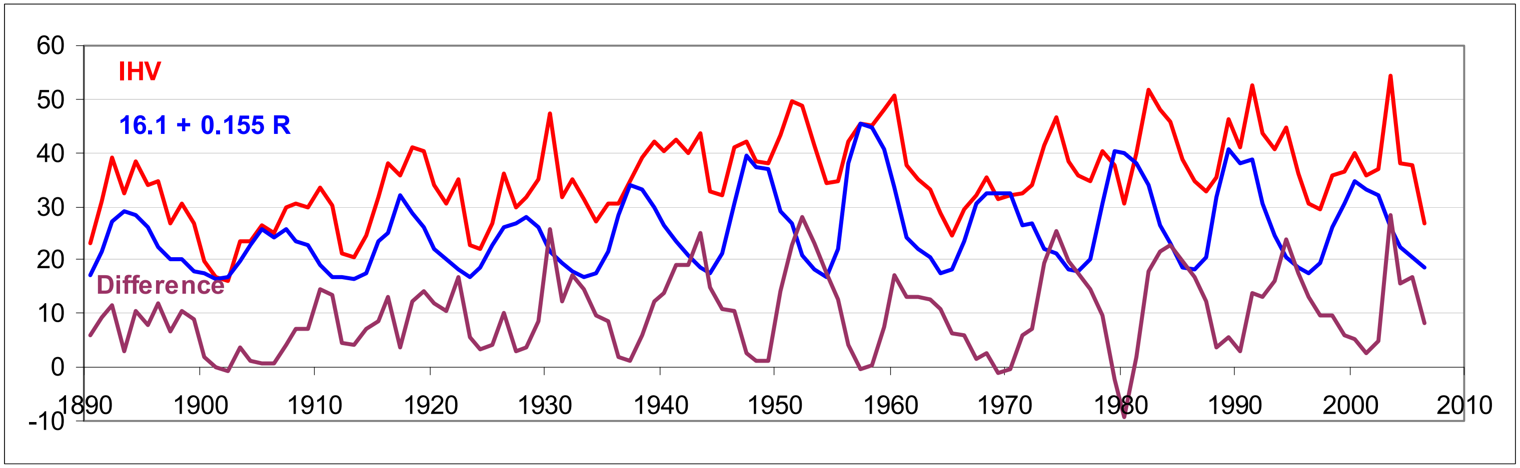
The Sun itself will soon tell us if such extreme caution is warranted. The Dikpati *et al.* model correctly (!?) predicted the erroneous (by up to 40%) sunspot areas of cycle 20 given by Hathaway when matching the (non-overlapping) RGO and USAF sunspot area measurements. It is a weakness of their approach that their model it is not open to experimentation by other researchers (as far as I know). One might want to see what happens if the correct input is used…

A2: Joan Feynman (1982) noted long ago that if you plotted the yearly average of a geomagnetic index (doesn't really matter which one) against the yearly average of the sunspot number, you get a curious pattern that suggests that geomagnetic activity is composed of two parts, one that increases with the sunspot number and one that does not:

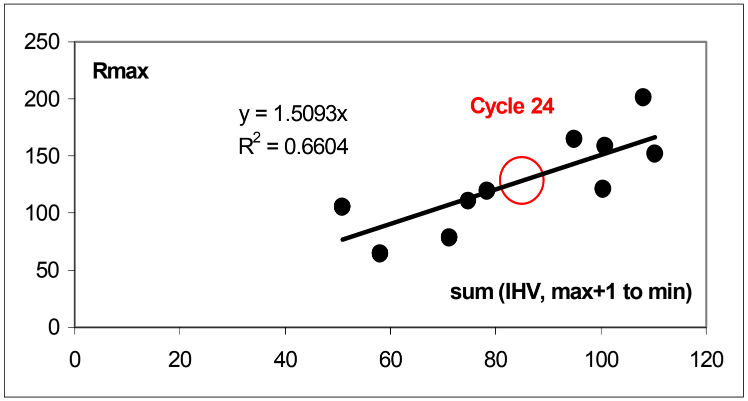


Her analysis has the nice property of being repeatable by others. I show here the plot we get if we use a new index (IHV) that Ed Cliver and myself have devised (so we know exactly what goes into it). Omiting the outlier for the year 1980, one can fit a nice straight line to the bottom envelope of the points to get a measure for the part that depends on the sunspot number.

Hathaway and Wilson now subtract this component from the geomagnetic index values for each year using the sunspot number for each year, and arrive at this:

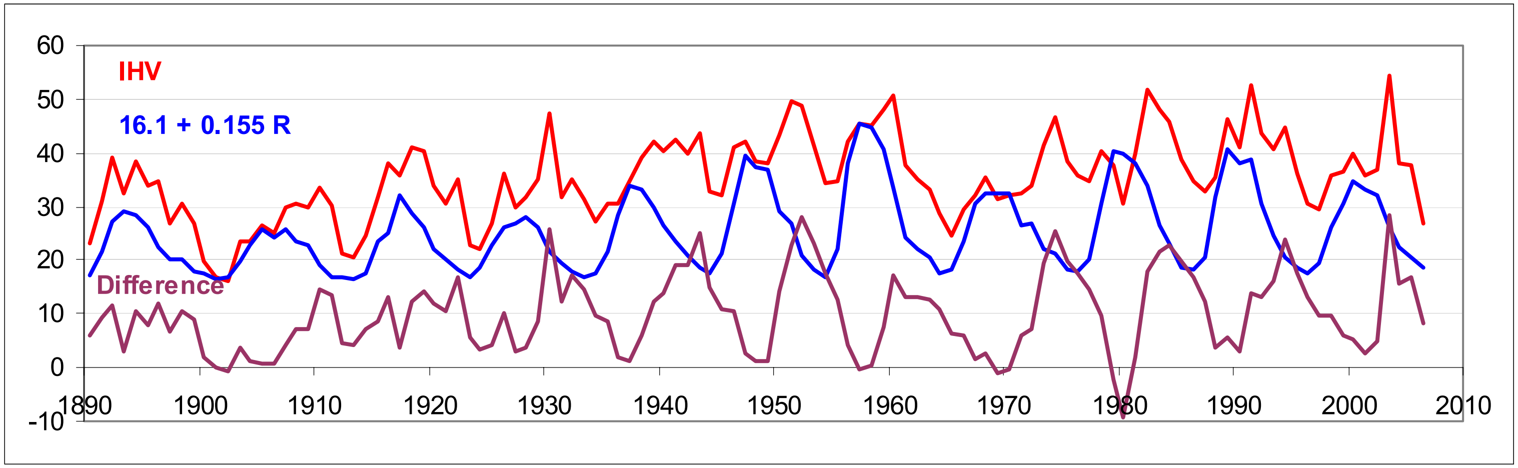


The (purple) difference curve shows a clear maximum just before minimum for every cycle. The size of that maximum is used by Hathaway *et al.* as a precursor for the size of the ***next*** sunspot cycle. Here is what we get by running the correlation:



Hathaway & Wilson advocate that smoothing should be applied to the point where both the sunspot curve and the difference curve have only one peak in each cycle to avoid questions about which of several peaks, if such, to select. Here I used the sum from one year after maximum to the minimum. The predicted Rmax for cycle 24 is 129.

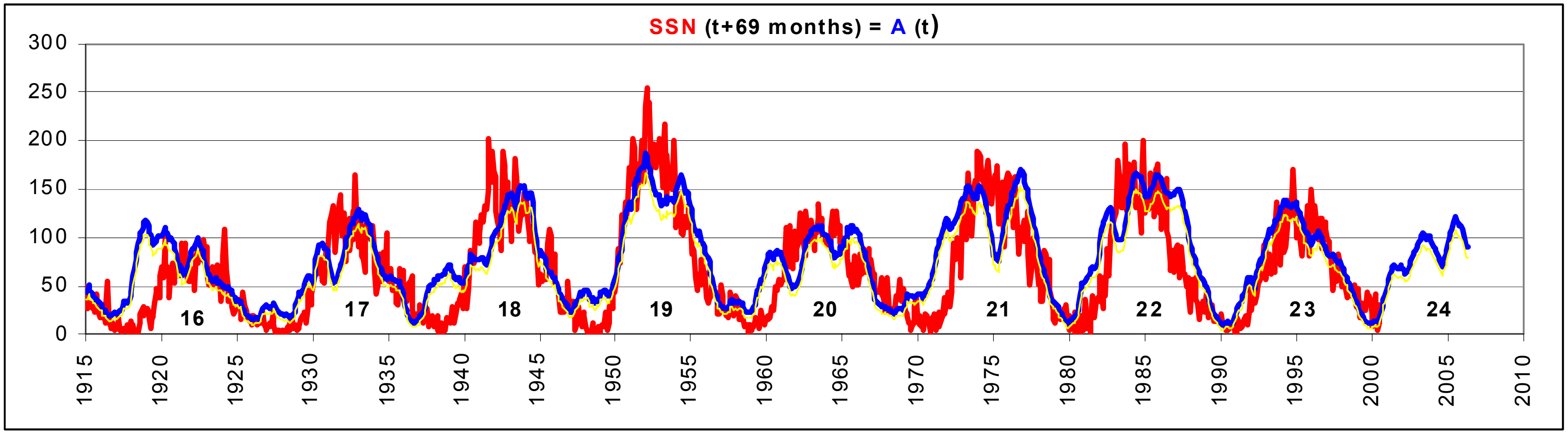
Other ways of smoothing give different results. Hathaway and Wilson use a Gaussian filter with FWHV of 24 months. With this smoothing, the predicted value is Rmax = 160± 25, significantly larger than cycle 23 and on par with cycles 21 and 22. Hathaway & Wilson claim that this prediction is "consistent" with the Dikpati *et al.* predication of 30-50% higher than cycle 23. It looks to me that the "lead" time is less than one cycle:



rather than the 2 to 4 cycles required by the Dikpati *et al.* model. By the ingenious argument that the geomagnetic peak ***really*** belongs to the ***next*** cycle rather than to the cycle where it is, Hathaway & Wilson then claim consistency by asserting that it took 2-4 cycles to create the solar feature that causes the peak ***and*** the next cycle. Such is the state-of-the-art using geomagnetic precursors.

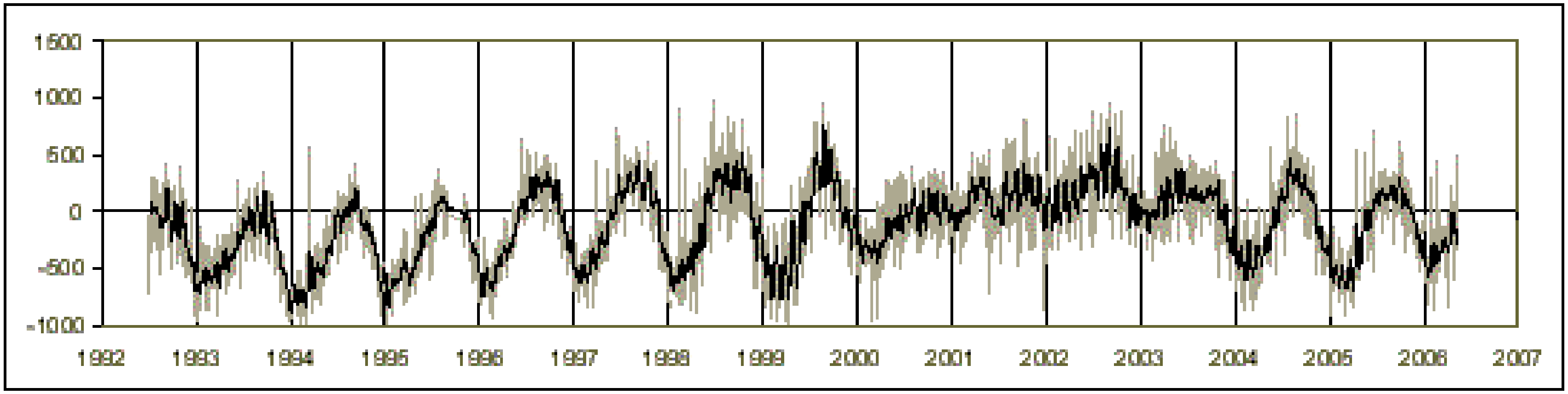
B1: Tlatov *et al.* (2005) use Hα-maps going back a hundred years. They assign fiducial values of -1G and +1G to areas with the two polarities inferred from filaments, filament channels, and bipolar active regions to calculate the spherical harmonic coefficients of the "large-scale" solar magnetic field. They define an index A(t) as a function of t (rotations) using the dipole and octupole coefficients for each rotation:

$$A(t) = k\,(\mu_1^2 + \mu_3^2/3)$$

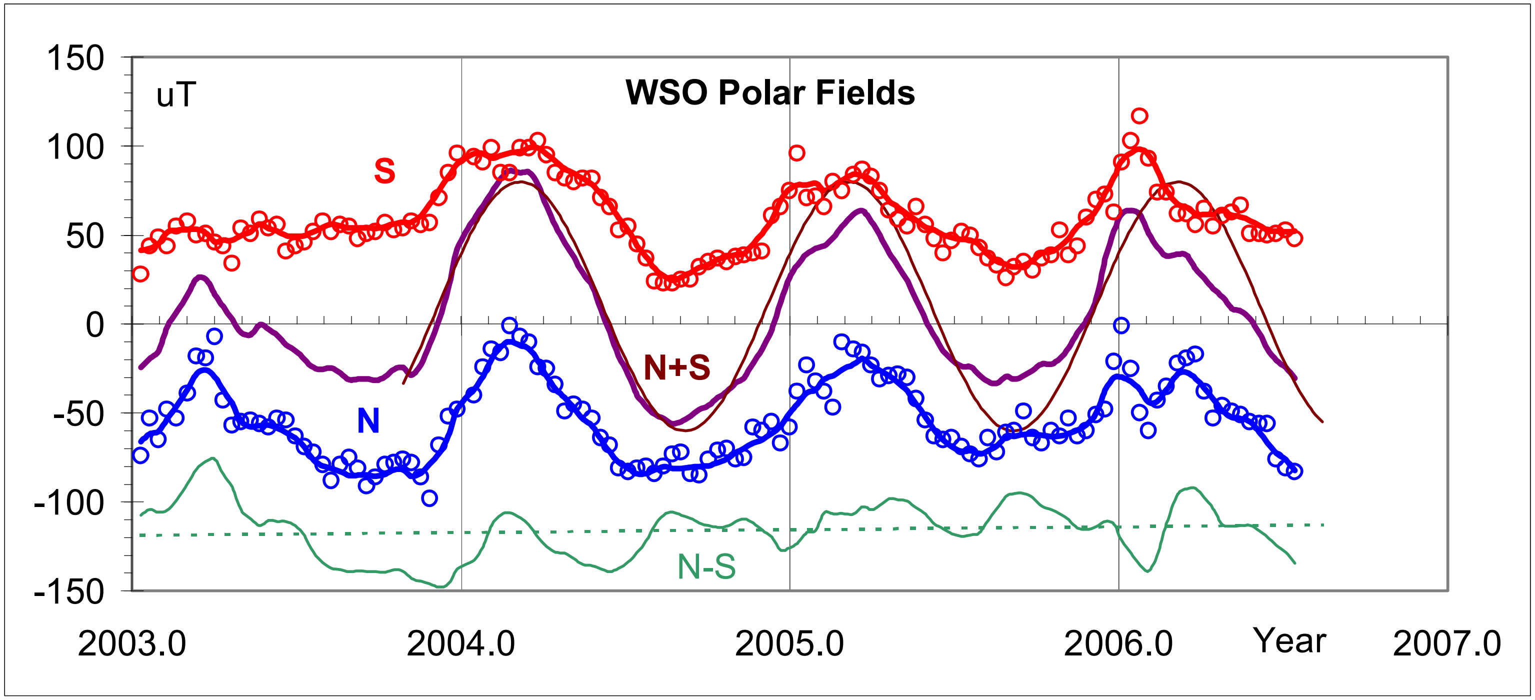


Their A(t) index has a good correlation with the sunspot number 69 months later. In the Figure, I have shifted the (red) sunspot curve to the left 69 months (5.75 years). From the A(t) index it would seem that cycle 24 would be smaller than cycle 23, maybe like cycle 16 or 17. Tlatov *et al.* by conviction belong to the short memory time camp.

B2: The method by Svalgaard *et al.* uses the polar fields as a precursor. The physical basis for the method is the Babcock-Leighton idea that the polar fields serve as a seed for the dynamo processes that create the following sunspot cycle, as Ken Schatten has just described. Our method differs from his in the details. As the polar fields are changing with time, one question is *when* to measure or over which interval to sum the polar fields to arrive at a value to be used for prediction. Away from times of reversal, the polar fields show a strong annual variation as the solar axis tips to and fro by 7.155 degrees. We can derive the polar fields from 17GHz radio maps of the Sun (from Nobeyama). Here you can see the sum of the North and South polar fields and how the B-angle modulation disappears when the polar fields are weak around their reversal. We use the presence of the B-angle modulation to tell us when the polar fields are well-established:

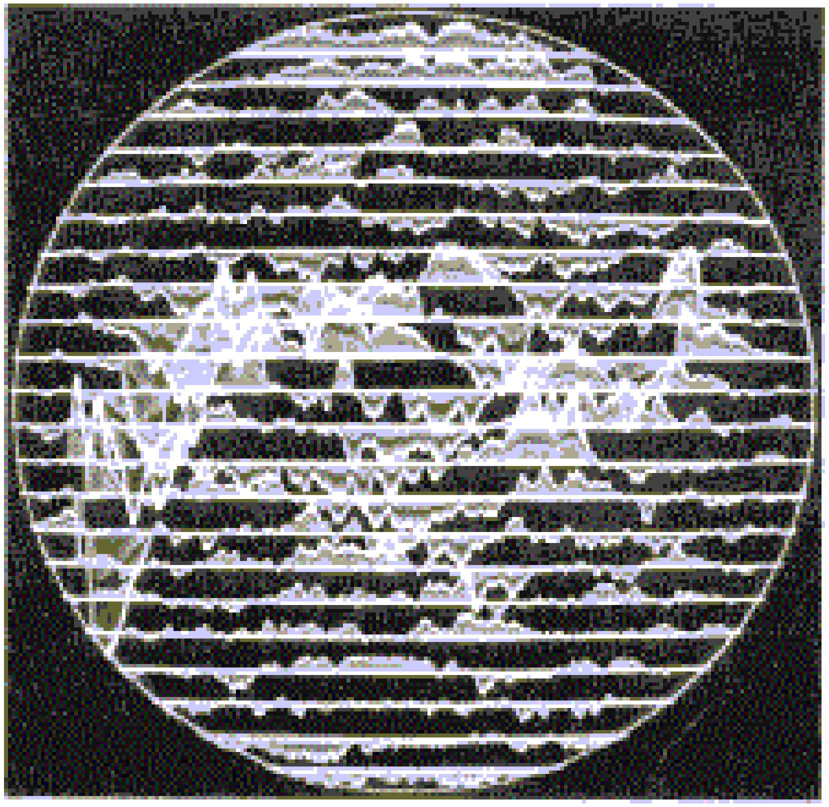


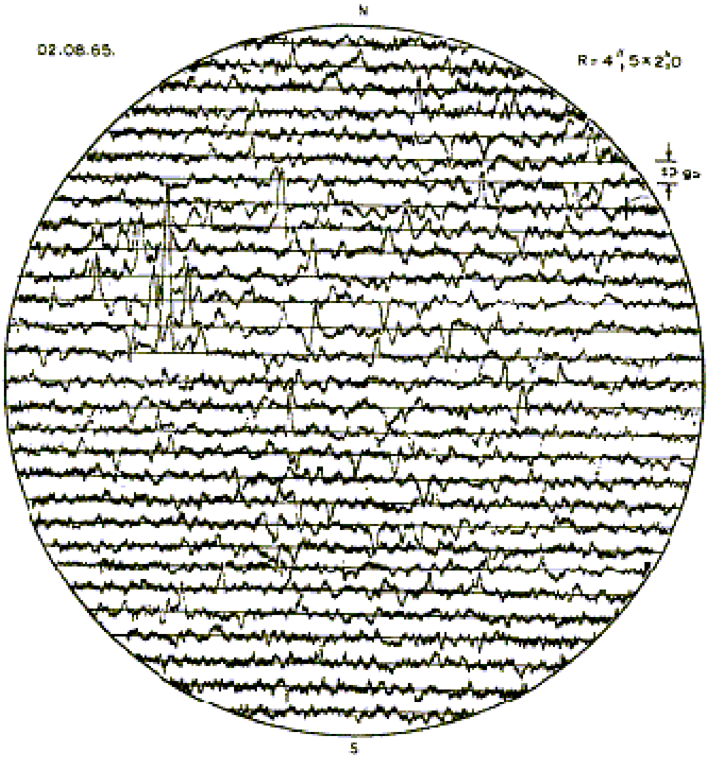
Here are the polar fields measured at WSO since 2003.0. The difference N-S is a measure of the dipole moment. It has been decreasing slightly. The annual modulation N+S is getting weaker as well (maybe more so); possibly indicating a weaker meridional flow compacting the field.



The strange peak near 2006.0 is caused by poor data due to bad weather at the time.

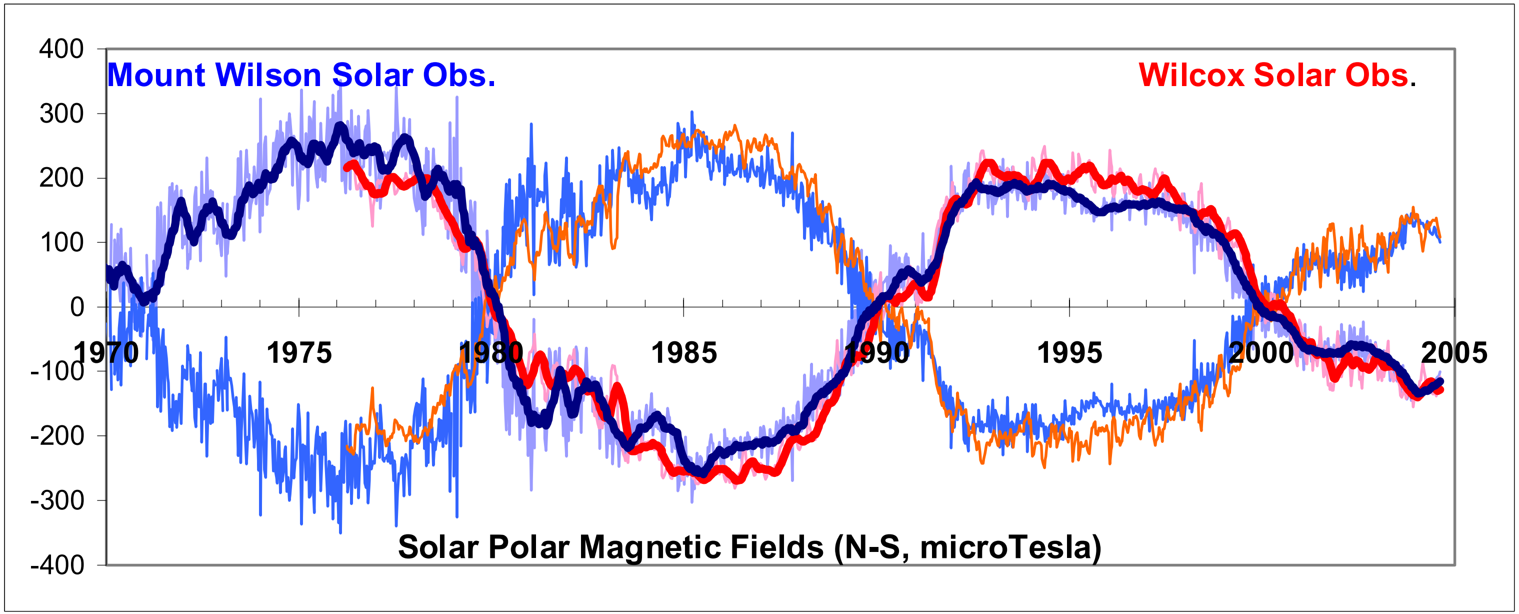
What do we have of direct measurements of the polar fields back in time? They were first measured by the Babcocks in 1952-53. Here is one of their magnetograms (left):



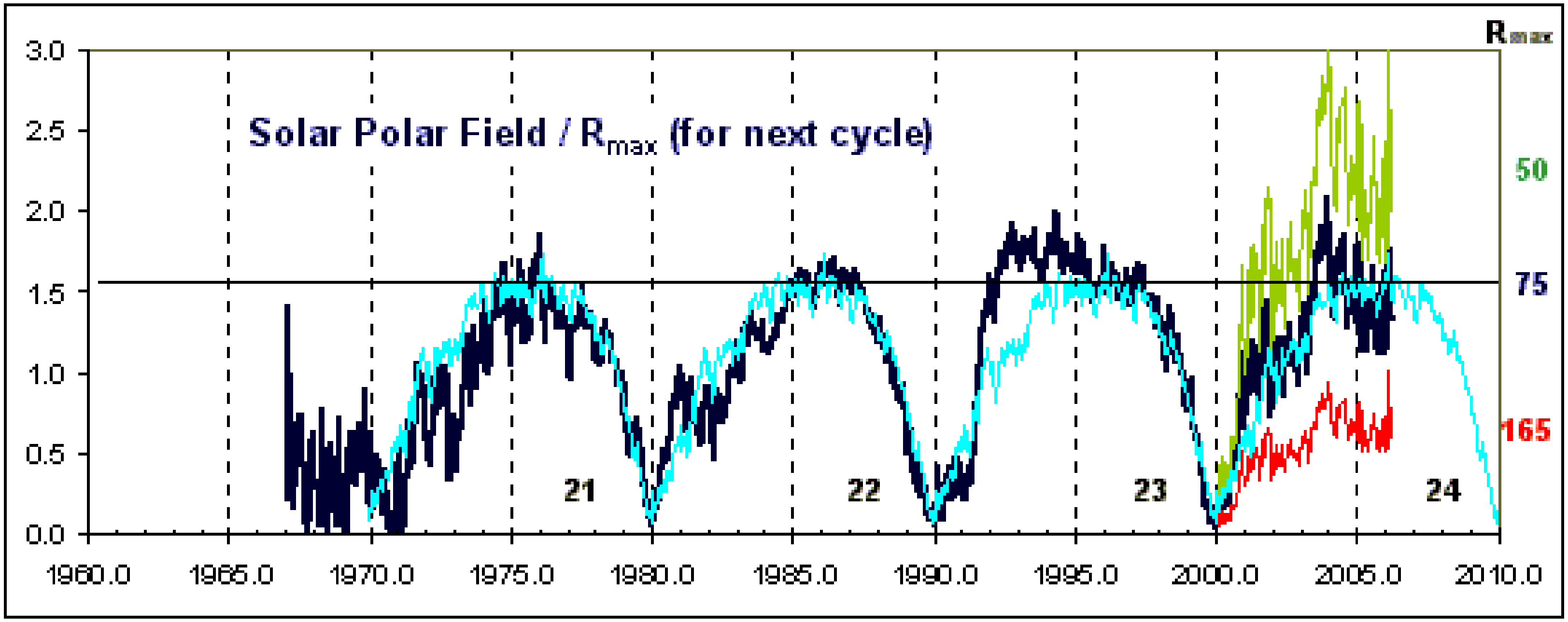


When Serveny at the CrAO tried to measure the polar fields in 1965, he couldn’t measure any coherent fields (right), leading to some doubt at the time whether the Babcock picture was correct. The polar fields were weak prior to cycle 20.

Later on at MWO and WSO the polar fields were again successfully measured. Here is a summary of the measurements of the 'dipole moment' (N-S).



The MWO data has been scaled to WSO for comparison. Note how the polar fields have decreased with time and how small they are now. Dividing the polar field strengths (from reversal to reversal) by the size, Rmax, of the following cycle gives a very similar 'profile', suggesting to us that the polar fields have predictive power one cycle ahead:



Since for cycle 24 we do not know what Rmax is, we have divided by three different choices: 50 (green), 75 (dark blue), and a Dikpati/Hathaway type value 165 (red). The value that scales the current polar fields to match the previous cycles seems to be near 75, which then becomes our prediction based on similarity with previous cycles. The light blue curve is the average profile repeated four times.

Summarizing we get predictions for cycle 24:

Group A:	A1, A2:	**160-185**
Group B:	B1, B2:	**75-100**

Both groups can claim data coverage going back ~100 years, so it is hard to fault a group because of paucity of data. They both predict ~10 cycles. Three of the four show a clear half-cycle ‘lead’ time, although by claiming that these 5-6 year precursors really belong to the **next** cycle, any memory time becomes consistent with the observations and we descend into a near Omphalos-type argument. Luckily the Sun will give us an answer in due time. If the answer is ~130, I’m sure the forecasters can tweak their methods to accommodate, making us no wiser, so let us hope that the Sun makes a clear choice.

A problem with a long memory time is how the Sun gets rid of the magnetic flux, *e.g.* how do we get a small cycle after a series of large ones, which has happened several times. In other words: what drives to Sun to oscillate about a mean rather than drifting off to a state where the cycle is lost. There is good evidence from cosmic ray radionuclide data that even during the Maunder and Spörer minima, the solar cycle was still operating. We are nowhere near answers to such questions, the reported sophistication of currently fashionable dynamo theories notwithstanding.

Summary and Comments

Precursor methods for predicting the size of coming solar cycles generally assume that the flux and distribution of magnetic fields produced in one or more cycles form the ‘seed’ for the coming cycle(s) and that some physics-based or numerological relations exist between the (observed) fields (or their proxies) and measures (such as sunspot numbers or areas and f10.7 flux) of the magnitude of future cycle(s) allow prediction of said magnitude. We review several popular precursor methods and the questions and problems associated with them and with the data on which they are based as outlined below. Large-scale, predominantly unipolar, magnetic regions exist on the solar surface. Such regions are conducive to the formation (often near the region boundaries) of complexes of activity (with sunspots, plages, etc) which themselves help maintain and modify the large-scale fields. Various diffusive processes and a general meridional circulation move the fields around tending to concentrate the flux at higher latitudes (‘polar fields’). Various (loose?) terms and concepts involving these fields and their interactions and derivative effects and proxies have been used, somewhat overlapping and not strictly equivalent, such as ‘latitudinal poloidal fields at high latitudes’ (Dikpati et al.), ‘line-of-sight magnetic flux in the polemost aperture [of the magnetograph]’ (Svalgaard et al.), ‘Area of polar regions’ (Song and Wang), ‘SODA-index’ (Schatten et al.), ‘Combined dipole and octupole index’ (Tlatov et al.), ‘Interplanetary component of geomagnetic activity’ (Hathaway et al.), ‘Drift rates of active latitudes’ (Hathaway et al.), and others. Such variety is not necessarily bad, but does make it more difficult to compare the merits of the various methods, especially since the predictions vary by a factor of two or more. The observational data that support the predictions are often of low accuracy (because the measurements are difficult, e.g. suffering from projection effects) and for direct measurements cover shorter timespans than desirable, forcing the use of proxies, which themselves vary in quality with time. Measurements of the polar fields, either directly with solar magnetographs or derived from 17Gz radio flux data, show at times a strong B-angle dependence as the sun's rotation axis tips to and fro by 7.155 degrees through the year. This effect has been used to determine the time intervals when the polar fields are well-defined. Measurements of the meridional circulation by helioseismology also show a strong (and presumably artificial) B-angle dependence in the presence of counterflows at high latitudes, making interpretation and use of the data difficult. The physics behind the dynamo models suffers from the use of approximations and parameterizations (e.g. of mean-field concepts while the real magnetic field is likely to be strongly concentrated near their generating current sheets breaking into very narrow filamentary structures) and of prescribed kinematics extrapolated from observationally poorly constrained flows. The radial structure and volume diffusion (and decay) of the magnetic field have to be considered as their omission can lead to an unrealistic long-term memory of the system. In particular, we review the observational evidence for making a choice between a short ‘magnetic’ memory time (half a solar cycle) and the longer time (three solar cycles) that is a properties of recent dynamo models.