Review of 'Predicting the Strength of Upcoming cycle 24 Using a Flux-Transport Dynamo-based Tool' by Mausumi Dikpati, Guiliana de Toma, and Peter A. Gilman.

The authors adapt an earlier model to be externally forced to reproduce the strength of the last 12 solar cycles. They then attempt to carry the 'prediction' over to the coming cycle 24. They predict a very strong cycle, possibly the second strongest cycle in the last 400 years. As a definite prediction, the paper is potentially important. Especially since the prediction is discordant from several other recent predications that point to a very small cycle. The measure of understanding is always a successful prediction, so their model would be put to a stringent test. This is of interest to the broad audience of GRL. Unfortunately, the paper is marred by imprecise language and a jargon-laden description of the model and the procedure. With some well-chosen, clear, and simple clarifications the paper can be improved to the point where its publication would be justified as marking an early (and definite) prediction which should be able to either vindicate or refute the theory or the approach.

Below follows a set of points that should be carefully addressed.

Abstract:
"**in contrast to recent predictions by Schatten, who used a statistical precursor method**".

Comment:
1) The Schatten prediction should be properly referenced as should an even earlier, and similar, prediction by Svalgaard et al. (GRL, 2005) using the same method. To call the precursor method "statistical" is not quite correct. The method is clearly physics based. Statistics only comes in when calibrating the polar fields in terms of sunspot numbers or F10.7 flux. The Dikpati et al. method is then also statistical, because it is similarly calibrated.

Page 1, Introduction:
"**many features in cycle 23 are peculiar**".

Comment:
2) Cycle 23 was not particularly peculiar. Cycle 20 was even more so. The polar fields regularly do not reverse at the same time. Sometimes there are multiple reversals (cycle 20 again comes to mind). Claiming special attention should be given to the model's treatment of the (not so peculiar) cycle 23 weakens the paper.

Page 2, last paragraph:
"**Recently Hathaway et al. (2003) obtained similar results, namely that the polar fields from cycle n-2 correlate maximally with the present cycle's sunspot fields**".

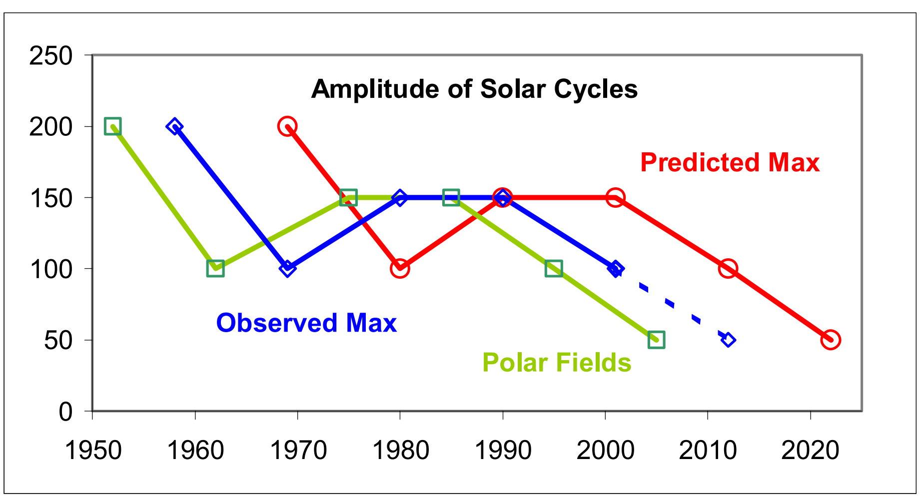
Comment:

3) The Hathaway paper said nothing of the kind. It did not claim that there was any correlation between the "polar fields" and anything. The 2003 paper advocated [Figure 6] some weak correlation (r=0.6) between N+1 cycle amplitude and "drift velocity" at Cycle maximum. An Erratum (2004) says that Figure 6 was in error and gives a new Figure 6 that now represents the N+2 cycle amplitude and that the correlation now is 0.7. Close examination of the two Figures show that the difference is that ONE data point was moved from (2.40,720) to (1.55, 800) and the text now says the previously claimed correlation (r=0.6) at N+1 was not significant. In both Figures, the largest cycle (#19) is a conspicuous "outlier", but should have been most sensitive, thus casting doubt on the whole analysis.

4) Direct comparisons of the polar fields since 1952 with the cycle amplitude show a clear N+1 relationship as is illustrated by the following Table and Figure. The Table shows the Polar Fields (PF) directly measured by solar magnetographs codified into categories (VS=Very Strong, S=Strong, W=Weak, VW=Very Weak) compared with the strength of relevant solar cycles codified the same way. If we assign the polar fields to the cycle in which they were generated, we can then compare PFs for cycle n with observed cycle strength for cycles n+1, n+2, and n+3; and with the n+2 prediction:

n	PF	obs n+1	Obs n+2	obs n+3	pred n+2
1952	**VS**	VS 1958	**W 1969**	S 1980	**VS 1969**
1962	**W**	W 1969	**S 1980**	S 1990	**W 1980**
1975	**S**	S 1980	**S 1990**	W 2001	**S 1990**
1985	**S**	S 1990	**W 2001**		**S 2001**
1995	**W**	W 2001			**W 2012**
2005	**VW**				**VW 2022**

It is clear that there is a good correspondence between PF and Rmax for cycles n and n+1, but none with n+2. The Figure below shows this graphically. For definiteness the categories have been assigned numbers as follows: VS=200, S=150, W=100, VW=50. These values are approximate sunspot numbers and polar fields (in microTesla).



Since the data only encompasses a handful of cycles every additional cycle (like #24) is important in assessing the validity of the various claims. The paper would be better off (and shorter) without any misrepresented reference to the dubious Hathaway et al. analysis.

5) Talking about the ‘polar fields of cycle n’ is somewhat ambiguous, as there are TWO polar fields in a cycle: the old fields before the reversal and the new fields after the reversal. Perhaps the "new" fields are meant. It would be helpful to have this clarified and spelled out.

In section (ii) on page 3, it is said:
"**The externally imposed surface POLOIDAL source is derived from one of the long-term observables, namely the observed spot area. Ideally a Babcock-Leighton type surface poloidal source should be more closely related to the AVERAGE photospheric magnetic flux coming from active regions' decay. But this observable is available only since 1977. Since we FOUND** (where? Reference?) **that the spot area from SOON and NOAA and the photospheric magnetic flux from NSO/Kitt Peak, averaged over a solar rotation, are well correlated (r=0.87) during 1977-present, we derive the surface POLOIDAL source from the long-term spot area data for cycles 12 through 23. In agreement with observations (Wang et al. 2000) we assume that only 10-20% of the flux that emerges survives while transported beyond its original neighborhood.**"

Comments:
6) Presumably the active regions' flux (90% or more of the average photospheric magnetic flux) is TOROIDAL and not POLOIDAL. How the POLOIDAL flux is derived from TOROIDAL flux is not described or hinted at? Maybe the authors' simply mean that radial fields are poloidal and non-radial fields are toroidal (or at least have a toroidal component)? Most people would reserve "poloidal" for large-scale fields that have a dipolar (or at most a quadrupolar) nature.

7) The language used is too loose. What is observed is the magnetic flux DENSITY (gauss = maxwell/cm^2). Integrating over the surface for a rotation gives the FLUX for that rotation. Adding up all rotations for a cycle gives the total flux for the cycle? Not at all, as much of the flux has a lifetime exceeding one rotation and is thus counted several times over. This needs to be clarified. The readers of GRL are not all specialists, so clear language can only improve the readability of the paper for the larger audience that is GRL's.

8) The SOON/NOAA spot areas are only available since 1976, so where is the spot area data for cycles 12-20 coming from? Hathaway's list? What assurance is there that spot areas coming from different observers have the same calibration? Comparing spot areas reported from different observatories shows that they are not the same. How is this handled? If at all? The "ramping up" procedure used by Hathaway does not make sense. It artificially inflates cycle 20 (apparently with no ill effect on the "predictions"...).

9) Only 10-20% of the flux survives. What is the definite percentage actually used (15% perhaps?) and how was it decided? And is it constant? Tell us.

In section (iii) on page 4 it is said:
"**Since the surface poloidal source is not fed from the tachocline toroidal FIELD, we stretch or compress the surface poloidal SOURCE of each cycle to fit with the mean duration of about 10.75 year (the average cycle period during these 12 cycles' span). Thus we maintain the phase coherence between the externally imposed cyclic surface source and the cycle induction of the toroidal FIELD at the tachocline. For the entire span of the aforementioned 12 cycles, we incorporate a steady meridional circulation with a flow speed of about 14 m/s at the surface, which produces a 10.75 year mean cycle period. We perform another simulation incorporating a steady, average flow from cycles 12 through 22 and then continue the simulations with the observed time variations in the meridional flow since 1996.**"

Comments:
10) Above it was FLUX, now it is FIELD (=flux density). The source is presumably from a certain region, so it should be FLUX (=field*area).

11) Stretch/compress the "source". What is being stretched? The flux counted multiple times? [see comment 7].

12) We now learn that the simulation time is constant for each cycle. This seems to be a weakness of the model. Consider the situation of a constant amount of flux (say 500 in arbitrary units) over a cycle, but distributed (in a triangular way) over two cycles that are 9 and 13 years long; the cycle strengths Rmax would then be 111 and 76, respectively, a ~45% change. It would seem that the input flux to the simulation should be adjusted up or down in relation to the cycle length. Maybe this was done. If so, it should be stated precisely.

13) About 14 m/s. What is it? Precisely 14 m/s or what?

14) A 14 m/s flow takes 2.47 years to cover the distance from equator to the pole. What stretches that to 10.75 years? Because there is a much slower return flow at depth and the total circulation time must be considered? Please give a hint for the non-specialist. The 2004 Dikpati paper says 17-21 years, the present paper has “about” 17-23 years. The paper is full of such rather sloppy use of “about” or ranges (“10-20%”). I count 13 “about”s” and 7 “ranges”. That is too many.

15) Steady flow until 1996, observed flow thereafter. The observed "**flow slowed down by 5-8% during 1996-2005**" (Basu & Antia). Consulting the papers cited shows a decline from ~19 m/s to ~10 m/s, a 50% total decline. Maybe the quoted rate (5-8% = ~1 m/s) is per year? This should be clearly stated.

16) Since apparently the cycle strength depends rather strongly on the flow speed (a change of 50% to 30% for cycle 24), keeping it constant for cycle 16-22 should produce

rather large differences between observed and "predicted" cycles, since there presumably were such changes too in the past of the flow speed (if we subscribe to the Copernican Principle that we do not live in a special time - at least as far as the sun is concerned). We would not expect a correlation as strong as r=0.92 between "predicted" and observed values. This indicates to me that the external forcing is too strong. Perhaps making the agreements fortuitous (externally forced).

Figure 2b: "**We also ran our model for about 450 years** (how many exactly?) **with an artificially constructed cyclic surface poloidal source which is random in peak amplitudes**".

Comment:
17) But the real cycles are not random; there are longer-term variations: Ahluwalia (33-year), Gleissberg (88-year), Suess (208-year) and Maunder (400-year?) cycles, some of which are flukes and some that may be real, but the data has them. Put another way: the data has strong positive "conservation" (see: Chapman and Bartels, Geomagnetism, section 16.28, p. 585). A better test would be to run the real data backwards. That would preserve some of those cycles. Random cycles are not good enough for this purpose. Either redo the analysis or remove Figure 2b (would also conserve space).

Second paragraph, page 6:
"**it may also be possible to extend the simulation of past cycles all the way back to cycle 1, which began around 1750. Although we do not have spot area data prior to about 1880**",

Comment:
18) In view of the uncertain calibration of the sunspot areas, one can forcefully argue that the sunspot number should have been used in the first place. Already Waldmeier found that spot area = 16.7 * sunspot number. Within the accuracy one can expect of the model, the better long-term availability, stability, and calibration of the sunspot number strongly suggest to use the sunspot number directly. The longer baseline would strengthen the paper enormously. Everybody would be able to accept that there may be discrepancies in the earlier cycles. Or to be suspicious if there are none. An important test case is solar cycle #4 where some people (going all the way back to Faye in the 19th century) believe that a cycle was lost due to lack of observations. We urge the authors to do the simulation using the sunspot numbers for the present paper. A comparison of the results (area versus numbers) would be important if not for other reason to see how sensitive the model is to its input values.

19) A very convincing, even essential, test would also be to stop the external forcing in cycle #19 and see if the low cycle #20 is forecast correctly. I strongly urge the authors to consider this.