

# Calibration of Mg II Index

Leif Svalgaard March 2007

This exercise was triggered by an email from Doug Biesecker:

“If you follow the solar irradiance community at all, you'll know that the floor at solar minimum is a source of controversy. Should solar minima all have the same floor or not? Anyway, if one assumes the floor for the coming minimum will reach the same level, we're still a good long way out. I'll leave extrapolations up to the user. Here's a plot of the MgII Index (a chromospheric index, derived from the Mg II line core to wing ratio).”

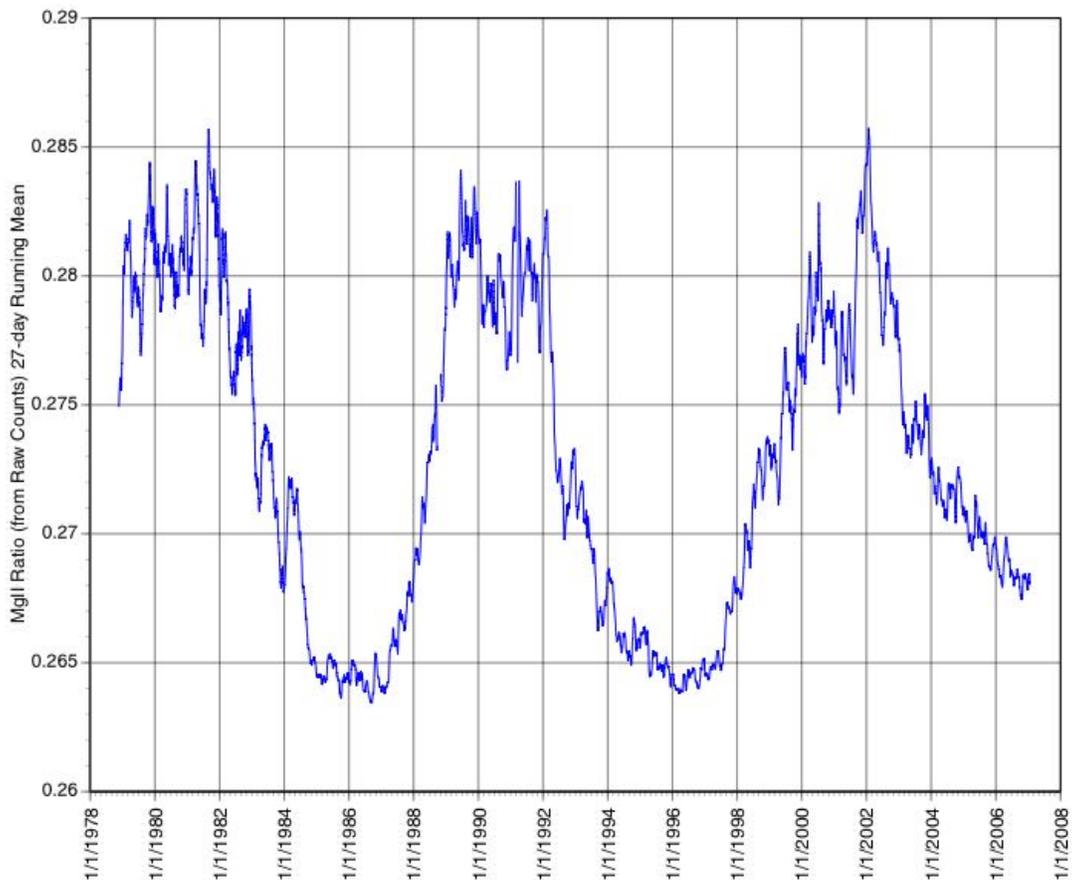


Figure 0.

Larry Puga added:

The data I used is available on our website, at:  
<http://www.sec.noaa.gov/ftpdir/sbuV/NOAAMgII.dat>

This data is produced on a daily, near-real-time basis, and so it has not been adjusted for any instrumental trends for the last year. We usually analyze the data to look for instrumental/satellite degradation effects once a year. As a result it is possible the last year of values may be off by a percent or so, but certainly not enough to change the basic trend of the data.

I took the Mg II data that Larry Puga maintains and compared the data with the f10.7 (adjusted) flux. To compare the two datasets I first scaled the f10.7 flux data to match the Mg II data for 1978-2001 (see below why). There is a very strong correlation:  $MgII = 0.00012114 f10.7 + 0.25614$ , with  $R^2 = 0.9661$ , using monthly means. Figure 1 shows the result. Mg II is shown with a blue curve, scaled f10.7 with a red curve. There is a very good agreement, except after January 1<sup>st</sup>, 2002 where MgII (green curve) is higher than the scaled f10.7:

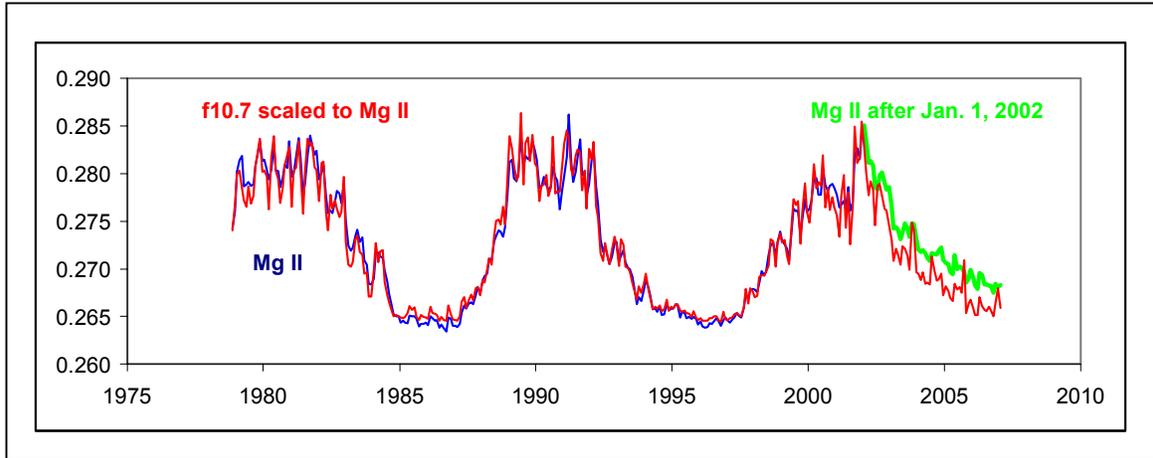
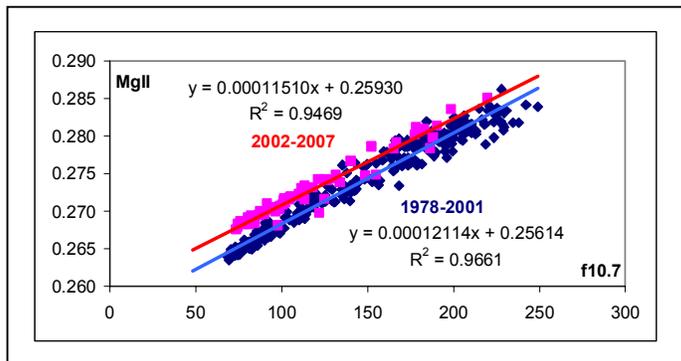


Figure 1.

Figure 2 shows the scaling correlations. It is clear that there are two distinct populations with different scaling: before (blue) and after (pink) Jan. 1<sup>st</sup> 2002:



I take this to mean that there is a **calibration problem with the MgII data** (the f10.7 data matches geomagnetic data very well, so I don't think the radio flux is the problem). Reducing the MgII data by 1.1% brings it into agreement with the scaled f10.7 flux as shown in Figure 3. With this reduction the variation of MgII during the declining phase of cycle 23 is not unusual.

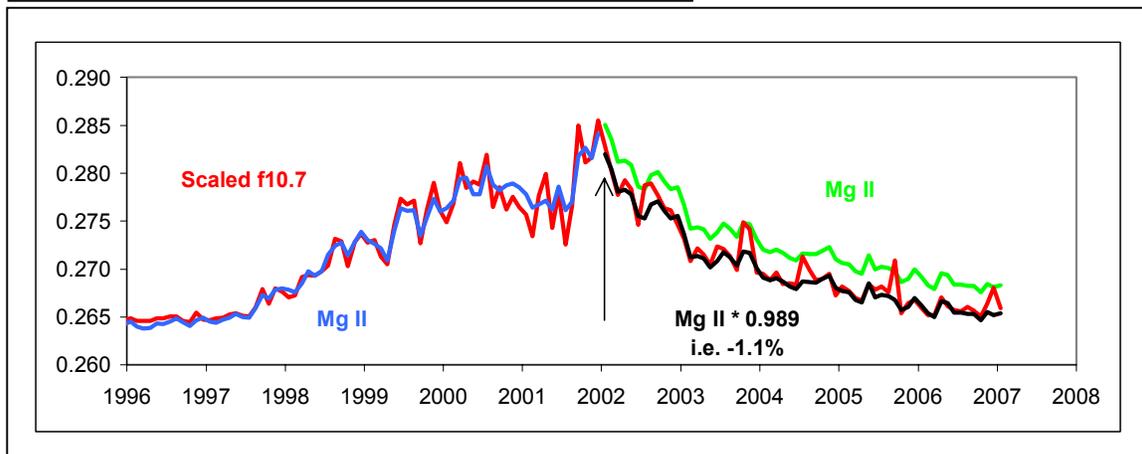


Figure 3

My response to Larry was:

The variation of Mg II during the declining phase of cycle 23 (see the very Figures that you sent us) is abnormal. The drop towards minimum is significantly slower than for the previous cycles. This could suggest that we were a long way from minimum because the curve still has a way to go. I have looked carefully at your data and believe that the calibration is off, not just for the last year or so, but for the whole time since January 1st, 2002. My analysis is described in the attached pdf file [shown above]. The bottom line is that reducing MgII by 1.1% since 2002.0 will bring its variation in line with f10.7 (and geomagnetic data), so there does not seem to be anything special about the declining phase of cycle 23.

David Hathaway had this comment:  
“Nicely done Leif.”

I replied:

“Thank you for the kind words. What is a bit disturbing is that presumably these data are being (have been) used by some unsuspecting souls. It seems to be my destiny to find these calibration errors :-). So far, my list include: aa-index, sunspot number, sunspot areas (your data :-), Mg II, heliospheric open flux, POES hemispheric power input...”

Larry responded:

“Based on your analysis and comments from Rodney Viereck, I would have to say that your analysis is correct, and that the slow drop in the data I provided is not real. The data on the web will be revised, based on Rodney's analysis, as soon as possible. I apologize for the inaccuracies in the plot I provided.”

My response:

“I'm glad to make my small contribution to clear up the behavior of the Mg II index on the declining branch of cycle 23, reducing the confusion (hopefully) about the level of activity at sunspot minima or how far out we still have to go to get down to the same minimum values as for previous cycles. I would be interested in what Rodney had to say.

From Rodney Viereck

“Indeed, our automated processes that updates the file on the web simply applies the standard algorithm to the data. We have known that NOAA 16 SBUV has been drifting a bit but until now, we did not realize how bad it had gotten. The problem is that the satellite orbit has processed so far that the incident angle into the SBUV is too large and it is affecting the values.

So typically, on an annual basis, we do a more careful update of the time series. We have not updated the web with this improved time series. Unfortunately, we also have not kept up with our annual update cycle.

I have done a quick and dirty update to the last good data set. I had to add about two years of data. I have not done a careful analysis so there may still be issues but at least it is an improvement over the web data set.”

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***As of Feb. 14<sup>th</sup>, 2008 the dataset on the website has still not been corrected.***