Comparison HMI (LOS) with Livingston NSO

Leif Svalgaard, 18 April 2011

From 2001 to 2011 Livingston and Penn have measured field strength and brightness at the darkest position in umbrae of 1750 spots using the Zeeman splitting of the Fe 1564.8 nm line [Figure 1]. Only one observation per spot per day is carried out during their monthly telescope time of 3-4 days average. Most observations are made in the morning [7h MST] when seeing is best.

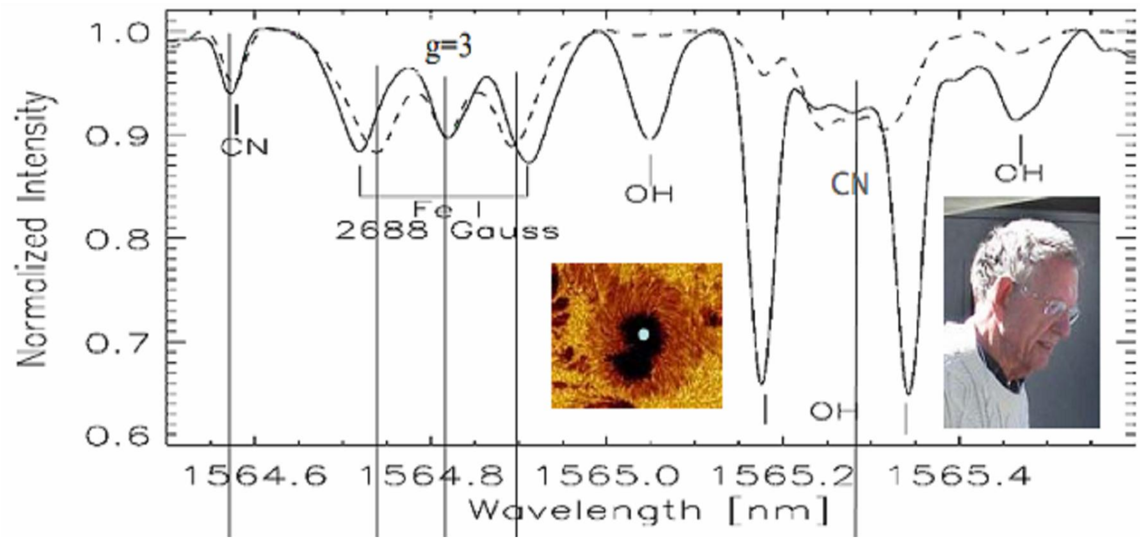

Figure 1. Measuring the magnetic field strength at the darkest area of a sunspot. The distance between the two σ components is proportional to the field strength. The molecular line from OH is very temperature sensitive and almost disappears with decreasing field strength [dashed intensity trace for a different spot].

The line splitting [energy level difference due to the magnetic field] does not depend on the angle towards the observer of the magnetic field. The relative intensities and polarizations of the components do, of course, but that is not an issue with the almost fully separated components at 1564.8 nm observed here. So, Livingston measures the absolute [true] field strength averaged over his [small: 2.5″x2.5″] spectrograph aperture, and not the Line-of-Sight [LOS] field. The effect of scattered light is still an issue, but so it is for everybody.

Over the interval 2001-2011, the temporal average magnetic field has declined about 500 Gauss [~20%] and average spot intensity has risen about 20% [Livingston, Penn, & Svalgaard, *Science*, to be submitted, 2011]. This is seen in Figure 2 that also includes a few scattered measurements before 2001. It is of great interest to know if the changes are a solar cycle effect [and will reverse as we get further into solar cycle 24] or part of a secular trend, which would be unprecedented in modern times. This is addressed in a later section.

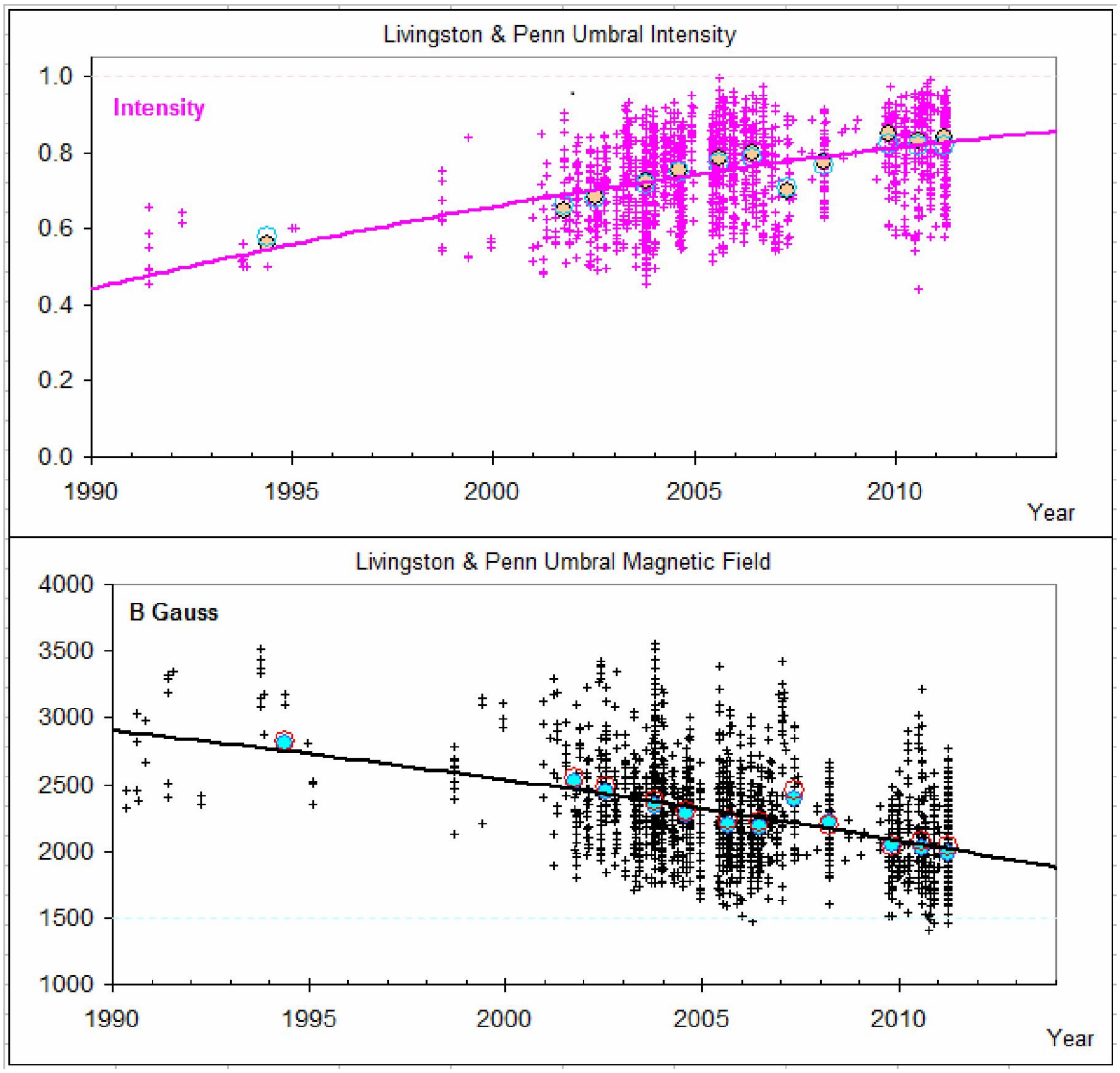

Figure 2. Upper: intensity of the line as fraction of nearby continuum [Bill: which component?] as a function of time. Yearly averages are shown with open, blue circles, yearly median values with filled, rose circles. Lower: field strength as a function of time. Yearly averages are shown with open, red circles, yearly median values with filled, blue circles. All data before 2001 are treated as belonging to one interval only, ~1994.

As the line intensity approaches unity, the contrast with the surrounding photosphere gets weaker and the spots become harder to see [at least in the infrared]. Reduced visibility of sunspots leads to a sunspot number [and area] that is too low. We speculate on the effect of this in a later section.

For each observing day, Livingston makes a drawing of the active region and its spots using the projected [82 cm] image. Each measured spot is given a designation [u*n*, u for umbra and *n* a serial number] and the field strength and line intensity are recorded for each, Figure 3. The drawings allow us to identify the spots on HMI intensity and magnetic field images as shown in Figure 3:

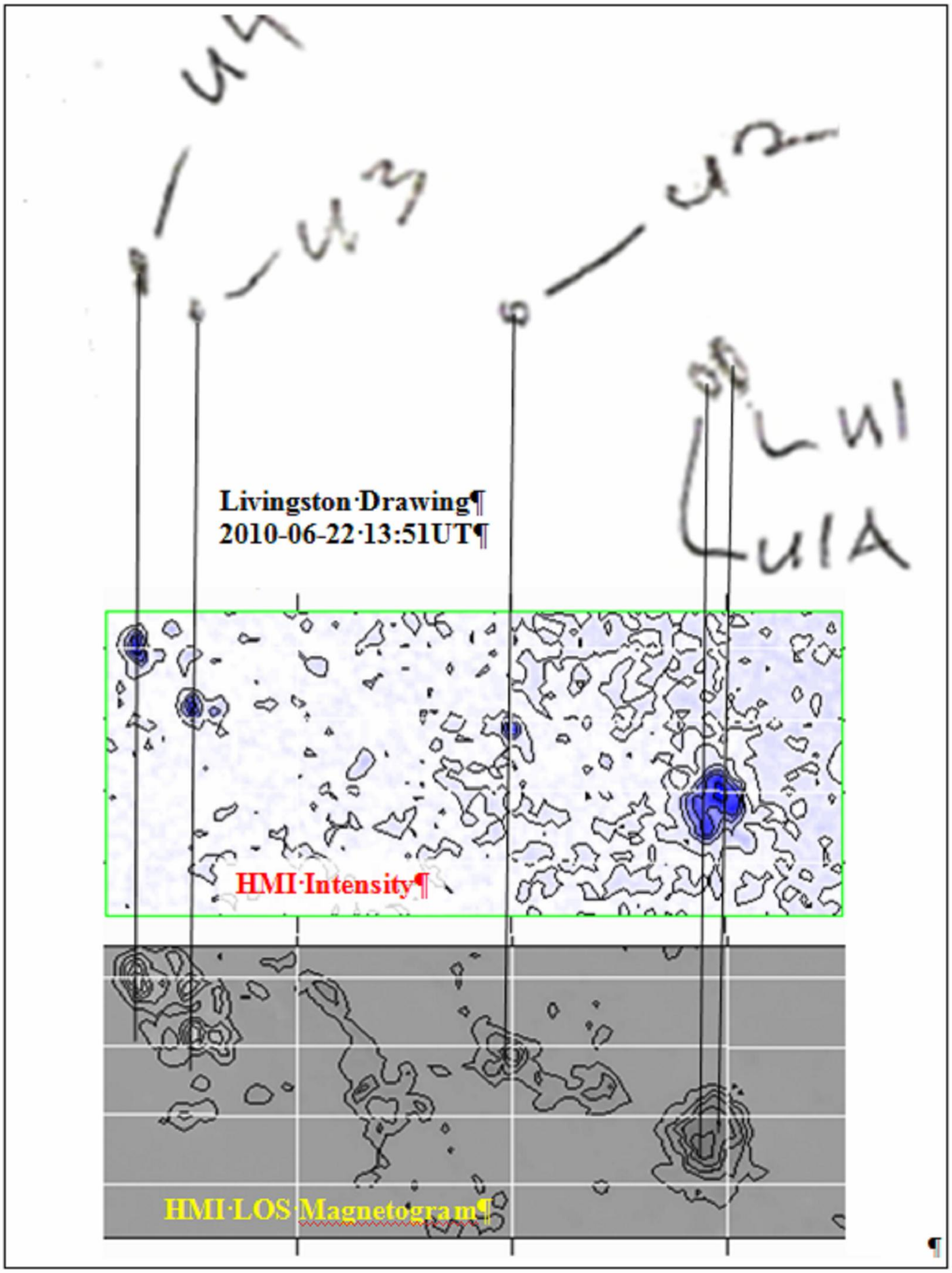

Figure 3. Identification of points of measurements on HMI intensity and HMI LOS magnetogram images. Vertical lines show the corresponding locations.

We have compared the magnetic field strength measured by Livingston for all spots since June, 2010 through January, 2011 [a total of 105 spots where identification could be safely made] with the LOS field reported on HMI [FITS] magnetograms [blue crosses on Figure 4] and with HMI LOS fields divided by cosine of the heliocentric angle, *h*, [red squares], assuming radial fields [which would not be far off for the darkest location – and strongest field – in sunspots]. As the spots are well-distributed in Central Meridian distance [average *h* is 44.6º], we would expect the blue crosses to scatter wildly [as they do]. For the abs(*HMI* los)/cos(*h*) values, we would expect a clear linear relationship with offset zero [if zero-levels are correct] and a slope of unity [if calibration is correct, or we measure what we think we measure]. We do find a respectable linear relationship with near zero offset, but with a slope, 0.58±0.04; much less than unity [by a factor of 1.72].

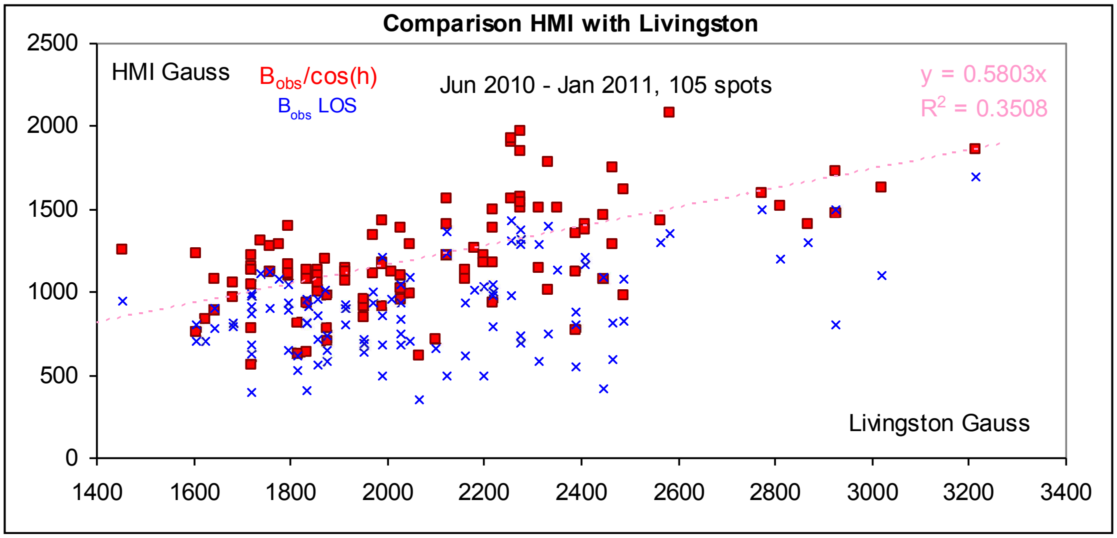

Figure 4. Observed HMI Line-of-Sight magnetic fields in sunspot umbrae compared to what Livingston has observed at the same locations (red squares: corrected for foreshortening assuming radial fields; blue crosses: not corrected).

In email from Jack Harvey [2011-03-21] he writes

"I was comparing some HMI LOS (617.3 line) magnetograms with simultaneous SOLIS VSM LOS (630.2 line) observations soon after HMI data became available. I found that over most of the full disk both instruments gave the same values within ±5%. In a dark sunspot umbra the LOS values were also about the same, namely peaking around 2000 G. Bill had measured the same spot and Hinode observations of it were also available. Bill's measurement was about 3200 G using the 1564.8 line [2000/3200 = 0.63]. The Hinode 630.2 spectra of the spot showed splitting consistent with about 3100 G. So I looked at the raw 630.2 SOLIS spectra to see what splitting they showed. Again, about 3100 G.

My conclusion was that the LOS reduction algorithms give low values for both spectrograph (SOLIS) and filter (HMI) measurements even if the umbral field is nominally pointed toward the observer. Also, when looking at the Zeeman splitting directly in spectra, all the instruments (Bill, Hinode, SOLIS) give nearly the same value which is substantially higher than the LOS results."

Here we confirm this single-spot result and lean towards his conclusion using 100 times as much data.

Why is this important? First, we need to find out why the LOS values are so much lower as much modeling is dependent on these values [and MHD models need the correct field strength]. This is a goal all in itself, but first the observers must be made aware of and accept the potential problem that the reduction algorithms may need revision. Second, the Livingston and Penn findings, if substantiated, may be one of the most significant discoveries in solar physics in a long time. MDI [for the past], SOLIS and HMI [for the present and the future] offer the possibility to extend the Livingston & Penn finding to vastly more data, if we can understand the relationships between the various instruments their and measurements.

We shall end with some comments on the Livingston & Penn finding. In Figure 2 many points fall on top of each other and the number of data points at a given field strength is obscured. In fact, outliers get a better representation. To avoid this, we can construct distribution functions. Figure 5 shows the [normalized] distribution function of the field strengths for each year since 2001, color coded according to which of three groups [early, middle, and latest years] the curves belong.

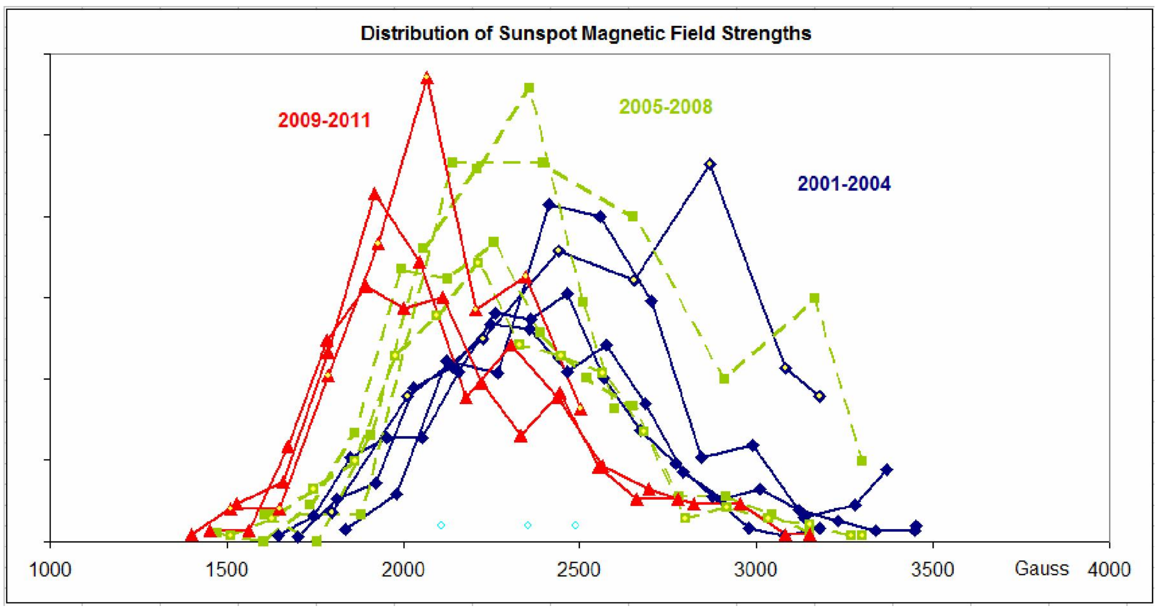

Figure 5. Distribution of sunspot magnetic field strengths for each year 2001-2011, normalized to the number of observation in each year. The color coding is as follows: blue [diamonds] 2001-2004; green [squares] 2005-2008, and red [triangles] 2009-2011. The earliest year in each group has symbols marked with a small yellow dot.

There is a clear shift with time of the distribution function towards lower values of field strength. Lower field strength would result in lower visibility of sunspots. There seems to be a rather abrupt cutoff at or just below 1500 Gauss, at least in the infrared. In Figure 6 we plot the intensity as a function of field strength for all of Livingston's measurements.

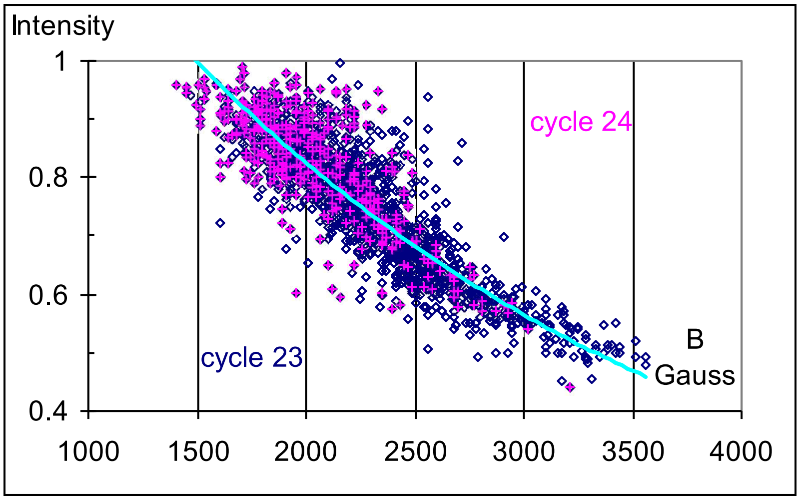

Figure 6. Intensity as a function of magnetic field strength as measured by Livingston 2001-2011. As the field decreases to ~1500 Gauss, the intensities approach 1, at which point the sunspot would be invisible.

If low-field sunspots are less visible we would not be able to see the lower end of the population, but it would still be there and its impact on indices like F10.7 would still be felt [albeit slightly less]. It is well-known that there is a tight relationship between the F10.7 microwave flux and the sunspot number. Perhaps it is less well-known that that relationship has changed during solar cycle 23 [and 24]; in the sense that we now count considerable fewer sunspots for a given F10.7 flux than before, Figure 7.

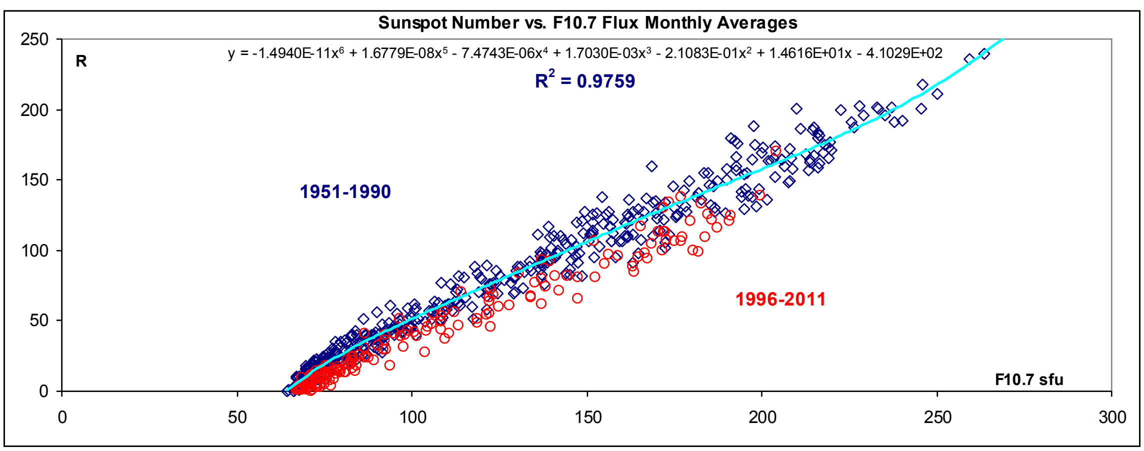

Figure 7. Relationship between monthly means of the F10.7 microwave flux and the sunspot number. Data from 1996 on are shown by red circles. The pre-1991 flux in blue can be fitted with a 6^{th} order polynomial, allowing calculation of a synthetic sunspot number from F10.7 fitting the relation before cycle 23.

Calculating an 'effective' synthetic sunspot number corresponding to a given F10.7 flux using the least-square relation from Figure 7 we can form the ratio between the observed sunspot number and the synthetic one. The result is shown in Figure 8, where we have omitted months when the sunspot number was very low [still, the scatter is high for low solar activity near minima, where we divide small numbers by other small numbers].

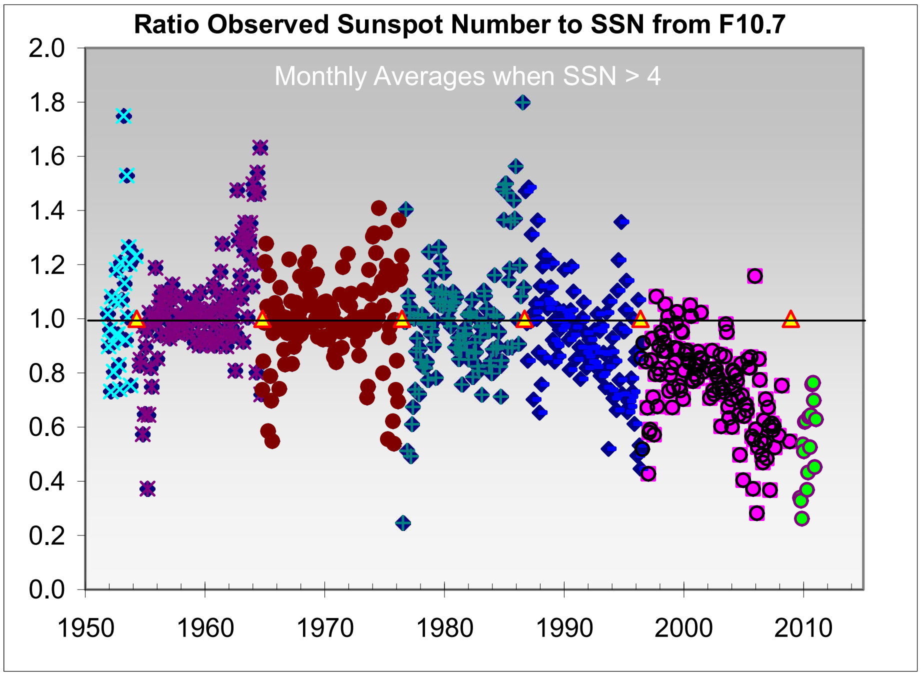

Figure 8. Ratio of observed [Zürich and International] sunspot numbers and synthetic sunspot numbers [calculated from the polynomial in Figure 7]. The yellow triangles mark sunspot minima. Solar cycles are distinguished with different colors. Cycle 24 being bright green.

The expected decrease in the ratio of the number of sunspots observed and that corresponding to the observed F10.7 flux is clearly evident, lending some support for the Livingston and Penn result. Should this trend continue, the Sun may well end up with no visible spots in spite of the magnetic cycle still being operative. One might speculate that this last happened during the Maunder Minimum, 350 years ago.

If so, it is important to follow the Sun's descent into conditions not observed for centuries. HMI and SOLIS observations [backed up by the stable calibration of WSO] and their correct interpretation [based on understanding of the measurements] could be of the highest value in providing a firm foundation for our understanding of this exciting and developing story.

M. J. Penn and W. Livingston

Temporal Changes In Sunspot Umbral Magnetic Fields And Temperatures

The Astrophysical Journal, 649: L45–L48, 2006 September 20

Discussion:

from	Leif Svalgaard <lsvalgaard@gmail.com>
to	Phil Scherrer <pscherrer@solar.stanford.edu>, Todd Hoeksema <todd@sun.stanford.edu>, William Livingston <wcl@email.noao.edu>, Jack Harvey NSO <jharvey@email.noao.edu>, Hugh Hudson hhudson@ssl.berkeley.edu Sebastien Couvidat <couvidat@stanford.edu>
date	Mon, Apr 18, 2011 at 9:47 PM
subject	My comparison of HMI and Bill Livingston's measurements

All,

The last few weeks I have made a [painstaking] comparison of the LOS magnetic field measurements made with HMI for the sunspots measured by Bill Livingston. The main conclusions are that such comparison is feasible and should be continued and that HMI [corrected for projection] measures only 58% of the field strength measured by Bill. This holds for spots of all sizes, from the largest [in the sample] to the smallest pores.

from	Phil Scherrer <pscherrer@solar.stanford.edu>
date	Mon, Apr 18, 2011 at 10:53 PM

As I understand it, Bill measures max field while we measure max flux for 1 arc resolution. The ratio simply implies that the filling factor in the darkest part of the umbra is about 58%. Perhaps the next step is to use Hinode or LaPalma data to make an estimate of the rms flux at higher resolution, and with that model an average "filling factor". Not to mention making the comparison with vector field measurements.

I think the important thing is to be able to model one measurement from the other so we can extend the study over the next decade.

from	Leif Svalgaard <lsvalgaard@gmail.com>
date	Mon, Apr 18, 2011 at 11:01 PM

Harvey compared with Hinode [one spot only though] and found the same discrepancy. I would think that the 'filling factor' would not be the same for large spots and small pores, which the comparison [so far] indicates. Clearly, I'm not done with this. There are also issues with how many pixels to average over. I have used one pixel only. Also I use the 720 second average magnetograms. This was exploratory only. Many improvements can be made, and the process automated to a higher degree. But would not a filling factor of 0.58 be a result in itself?

Many questions [which make it interesting].

from Leif Svalgaard <lsvalgaard@gmail.com>
date Mon, Apr 18, 2011 at 11:04 PM
On Mon, Apr 18, 2011 at 10:53 PM, Phil Scherrer <pscherrer@solar.stanford.edu> wrote:

> *I think the important thing is to be able to model one measurement from the other so we can extend the study over the next decade.*

I agree with this, which I expressed by saying we should try to find out what it is we are measuring.

from Leif Svalgaard <lsvalgaard@gmail.com>
date Mon, Apr 18, 2011 at 11:30 PM

http://www.n.ethz.ch/~wenzeri/download/Wenzel_et_al_SPW6_poster.pdf

models a filling factor in the umbra of 94%

from Phil Scherrer <pscherrer@solar.stanford.edu>
date Tue, Apr 19, 2011 at 7:59 AM

all that is needed for continuity is the ratio. If it is constant then for the continuity question we are done.

So long as we remember to correct for any future calibration changes which are likely to happen since we have not yet addressed the question of absolute field calibration yet. Still other more important things to worry about, such as variations across the field, variations with time of day, etc.

from Leif Svalgaard <lsvalgaard@gmail.com>
date Tue, Apr 19, 2011 at 8:05 AM

Yes, there are many other things. As I said, I'm not done. I'm not so sure that it is a filling factor issue [could be scattered light, too, perhaps]. Most people assume that the filling factor in the darkest umbra is near unity.

Luckily with solar activity picking up recently, there should soon be many more spots to analyze.

from Hugh S. Hudson <hhudson@ssl.berkeley.edu>
date Tue, Apr 19, 2011 at 8:19 AM

Isn't "filling factor" a poor shorthand for "seeing"? I would expect that Bill's observations would have some of that, on the average, and would that not systematically bias his results? Even more a concern, since you find that Bill's Bs are bigger than MDI's Bs? Or is seeing truly negligible at 1.5 microns, which I doubt.

from Leif Svalgaard <lsvalgaard@gmail.com>
date Tue, Apr 19, 2011 at 8:22 AM

If the field is concentrated into kG flux tubes much smaller than the aperture, then we measure the average of a strong field in a few places and no [or very weak] field everywhere else. In the quiet sun filling factors of 0.01 are common. So, not 'seeing'

from Jack Harvey <jharvey@noao.edu>
date Tue, Apr 19, 2011 at 8:43 AM

Hi Gents,
As Leif mentioned, I compared LOS measurements from SOLIS VSM and HMI for a fairly good sized sunspot observed 1-2 August 2010. Both instruments gave about 2kG despite the resolution advantage of HMI. The same spot was measured by Bill at about 3.1 kG (splitting of the resolved sigma components). I got 6302 A spectra of the spot from Hinode and the SOLIS archive and also found about 3.1 kG from the sigma component splitting. The mystery to me is why the LOS signal is so much less than the splitting signal. One would think that somewhere in the umbra the field would be along the LOS and be the same as the splitting measurement. Perhaps the answer is that there is (for this spot anyway) always a pi component that dilutes the LOS signal analysis. Whether it is all due to scattered light and/or modulator inefficiency, or is partly solar in origin is an open question. In olden times (1970s) I took spectra of spots that showed no pi component, so I'm leaning toward instrumental effects as the likely culprit.

One nice thing from the quick look that I did is that it appears to be very practical to use the SOLIS VSM spectra to measure the sigma component sunspot splitting on a daily basis. All we need to do is write some code to analyze the spectra.

FYI, thanks to Bill, I decomposed one of his 15648 spectra of a dark sunspot umbra into its component lines and it is complicated. It takes at least 22 lines in the 12 A around the target line to come close to fitting the observed spectrum. There is no continuum and the region is cluttered with blended OH, CO and CN lines. The 15647 and 15648 Fe line sigma components overlap with each other when the field is large which is another challenge. I hasten to point out that this is an issue only for dark umbras. Brighter ones are far less blended. In my opinion, what needs to be done is to decompose a series of Bill's spot spectra going from brighter to darker umbras to understand the origins of all the blended lines and find ways to model their effects on the strength measurements.

from Phil Scherrer <pscherrer@solar.stanford.edu>
date Tue, Apr 19, 2011 at 12:33 PM

If Bill is really measuring field strength and not flux, then he is not sensitive to filling factor. Just average non-zero field in the effective pixel. And that can be partly compensated by how the I+V and I-V is fit. guess I should read the papers again.... Stenflo's work supports a separation in field strength distribution such that such weak field that might be there can be considered part of the zero field part of the pixel perhaps.

from Hugh S. Hudson hhudson@ssl.berkeley.edu date Tue, Apr 19, 2011 at 12:47 PM

Yes, my understanding is that Bill's approach really does estimate B directly. The other techniques are described as measuring "flux" and so must be sensitive to filling factor and resolution. HMI Gauss being less than Livingston Gauss (Leif's Figure 4) would be consistent with the idea that even HMI is missing structure, i.e. small filling factor and/or something in the absolute reference of what "HMI Gauss" means. I should go back and not re-read, but read the classics for the first time!

from Phil Scherrer pscherrer@solar.stanford.edu
date Tue, Apr 19, 2011 at 12:49 PM

And remember that the HMI LOS is simply the difference between two Doppler calculations. We rely on the pi components modifying the observed centroid difference in proportion to the cosine of the LOS angle. And the unsplit contribution from zero or weak fields to do the same so the net flux is correct. With the junk in the 6768 umbra it is a wonder the MDI does as well as it does (did). And of course we do not measure the centroid really, but some weighted average that has bias from line bisector shape from unresolved velocity etc. One should always take HMI and MDI and WSO style measurements as some sort of index that is pretty well proportional to net flux and not try to do physics with field strength without some rather large uncertainty. It is always amazing to me that the different methods average to the same quantities with an almost unchanging scale factor that depends only on the device, and not on the Sun. To me this says that whatever weak fields there are simply do not matter to the net flux. Independent of how much they may matter to the physical conditions in the photosphere. (But that topic strays from the issue at hand).

from Sebastien Couvidat <couvidat@stanford.edu>
date Tue, Apr 19, 2011 at 1:14 PM

Another problem is that if the field strength is indeed 3.1 kG in this sunspot's umbra, then the LCP and RCP components are at the limit of the dynamical range of HMI. 3 kG means a wavelength separation between LCP and RCP components of 275.7 mA, while the HMI filters are centered on +/-172 mA, +/-103.2 mA, and +/-34.4 mA. So even if OBS_VR=0 (velocity Sun-SDO), if the sunspot is at disk center (and the big sunspot of August 1-2 was not), and if you have no flows in the sunspot, the MDI-like algorithm used to calculate the Doppler velocity and the l.o.s. magnetic field is likely to do a poor job. Then, if OBS_VR is not zero, if the sunspot is not at disk center, and if there are flows in this sunspot, you need to add the resulting wavelength shifts which may send LCP or RCP outside the HMI dynamical range.

from Phil Scherrer <pscherrer@solar.stanford.edu>
date Tue, Apr 19, 2011 at 1:44 PM

So such comparisons, or long term studies, should take spots near disk center for various reasons, and for HMI reasons near noon or midnight MST (UT+7) (actually around 6:45 or 18:45) to get the best repeatable dynamic range

from Leif Svalgaard <lsvalgaard@gmail.com>
date Tue, Apr 19, 2011 at 1:54 PM

Luckily, there have not been many large spots [only two above 3000 G so far], so the problem may not be severe, but certainly something to be aware of. Just got a new bunch of drawings from Bill, so a second installment is coming soon.

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21 April 2011

from Leif Svalgaard <lsvalgaard@gmail.com>
date Tue, Apr 19, 2011 at 8:06 PM

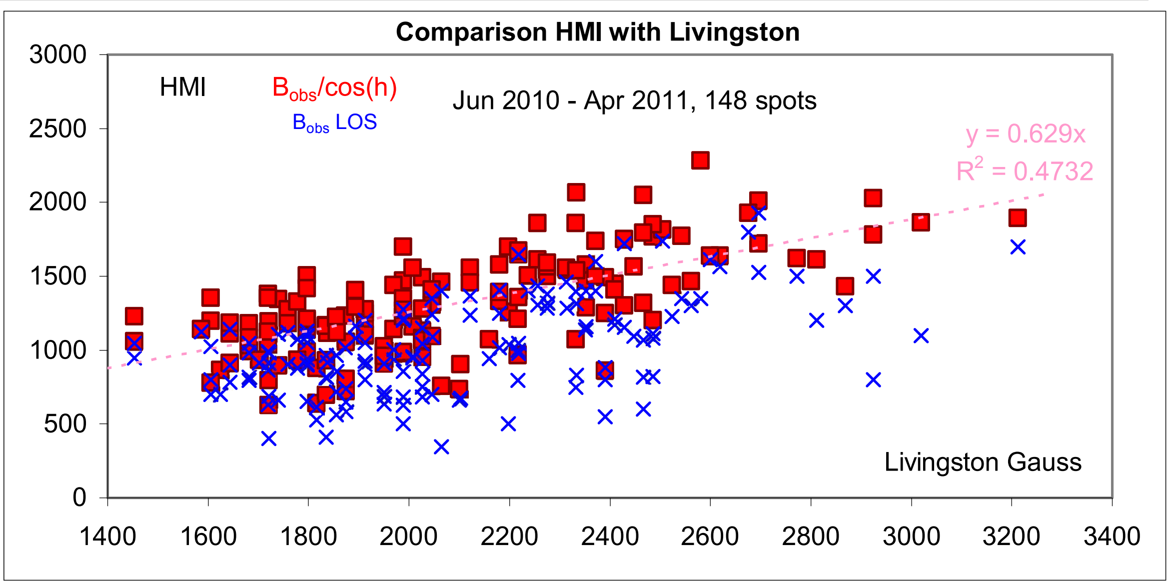
Just to verify:

To calculate the heliographic angle, h, should I do this

ARCSIN(h) = SQRT((x pix - CRPIX1)^2 + (y pix - CRPIX2)^2) / (average(CRPIX1,CRPIX2))

from Phil Scherrer <pscherrer@solar.stanford.edu>
date Tue, Apr 19, 2011 at 8:39 PM
no. pix - CRPIX gives distance from center in pixels. So you need to scale with radius in pixels. CDELT1 gives arc-sec per pixels and RSUN_OBS gives radius in arc-sec.

from Leif Svalgaard <lsvalgaard@gmail.com>
date Wed, Apr 20, 2011 at 6:30 PM
With a correction from Phil [how to calculate the radius], a source of noise was eliminated and the fit is much better now [Figure attached, including new data from Bill]. The ratio is now 0.63 [R^2 = 0.47] rather than 0.58 [old R^2 = 0.35].



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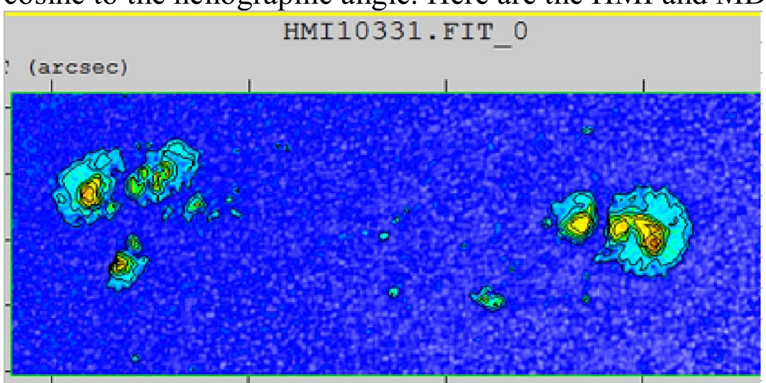
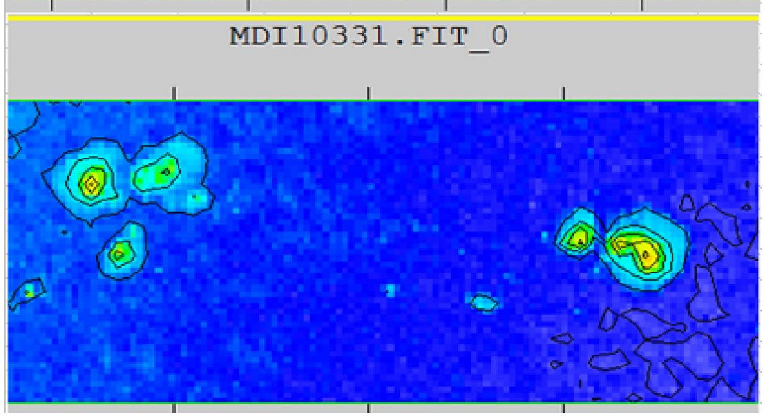
24 April 2011

from Phil Scherrer <pscherrer@solar.stanford.edu>
date Fri, Apr 22, 2011 at 4:41 PM
MDI mag calibration was adjusted to match Mt Wilson in what now looks like an incorrect way.

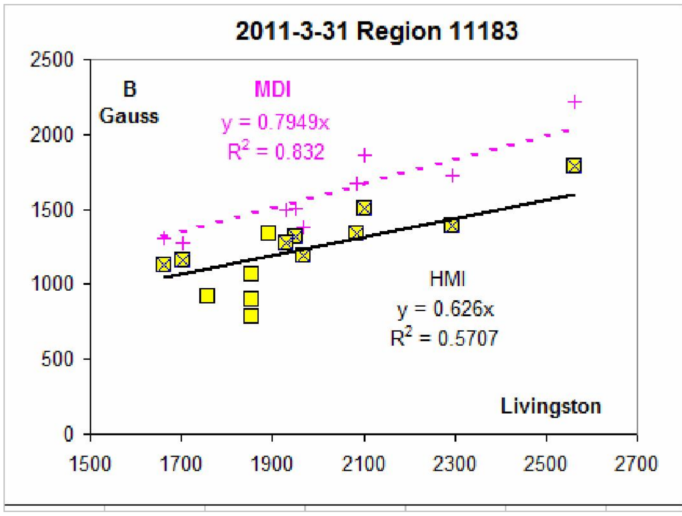
from Leif Svalgaard <lsvalgaard@gmail.com>
date Fri, Apr 22, 2011 at 10:58 PM
Does not keep people from publishing papers based on MDI's calibration, e.g. Schrijver & Title's new paper of magnetic connections [where they quote us MDI = 1.322 HMI]. Perhaps we should advertise the faulty MDI match a bit more broadly...

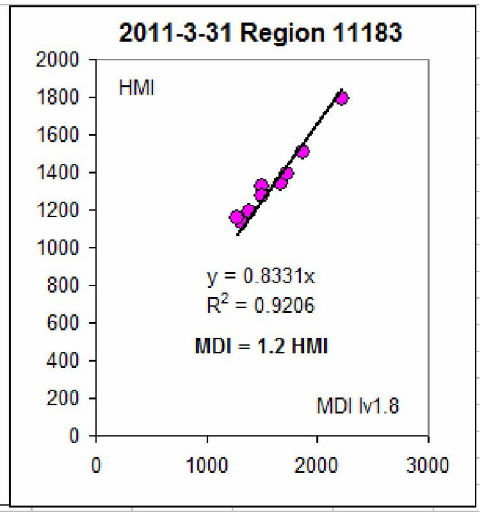
So, I looked at MDI for region 11183 [2011-3-31]. I could find MDI data for 15:00 UT, which may be close enough to the 14:30 UT that is the time on Livingston's drawing and for the HMI data that I matched with. For all these comparisons, I assume that the field is vertical in the darkest part of the umbra, so what is compared is the LOS field divided by cosine to the heliographic angle. Here are the HMI and MDI images of the region:





For the spots where I could unambiguously match up Livingston, MDI, and HMI, I find:





So, HMI is 0.833 MDI, or MDI = 1.2 HMI.

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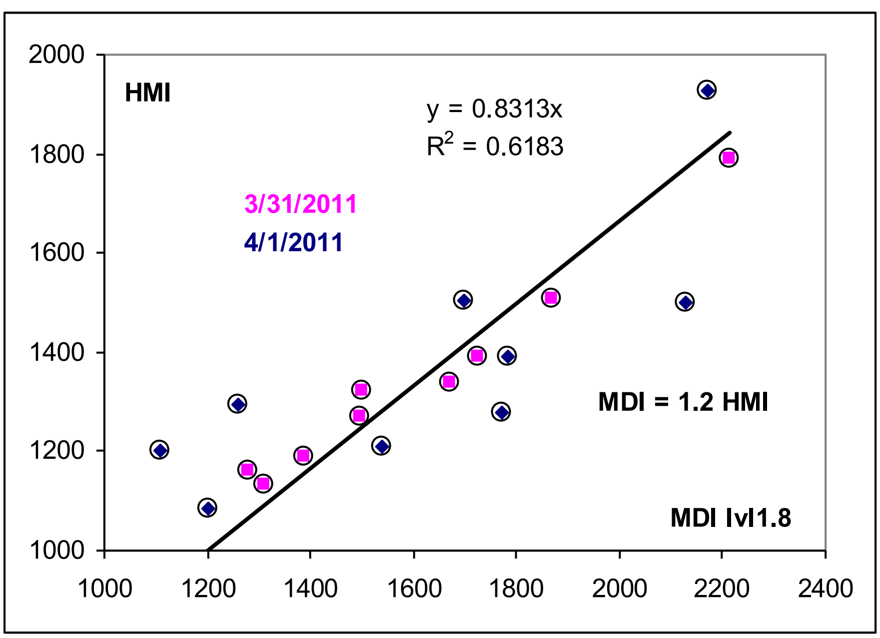
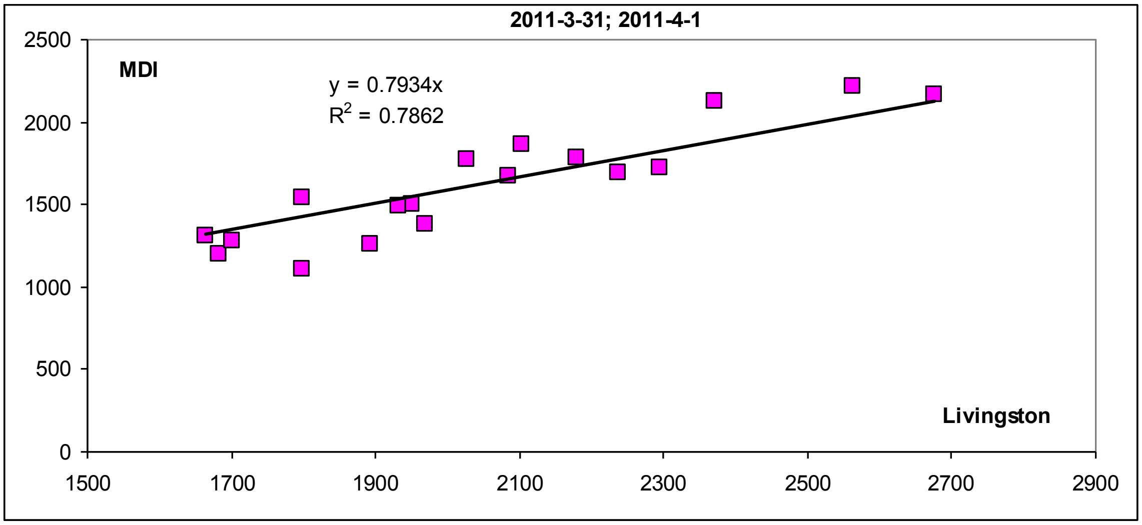
25 April 2011

Adding in 2011-04-01, gives essentially the same result (same spots a day later):





Or B(Liv) = 1.26 B(MDI)