From fschwab@nrao.edu Mon Oct 28 14:37:40 2002
Date: Wed, 23 Oct 2002 11:43:00 -0400
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Subject: Re: strange rms

Frank,

I don't see that the K2 factor is correct. Applying a lag-domain Hanning taper is equivalent to smoothing in the spectral domain by a three-point kernel with weights 1/4, 1/2, 1/4. Thus, the variance should be reduced by a factor (1/4)^2+(1/2)^2+(1/4)^2, which is equal to 3/8. So the rms should be reduced by the square root of 3/8, **or** approximately 0.612372. I.e., I think K2 should be equal to 8/3. This result, though, which amounts to an ~15% difference, of course applies in both the 3-level **and** 9-level cases, so it doesn't solve your anomaly.

Another factor is the effective bandwidth. Such a formula as you're using is valid only **for** a rectangular bandpass. If the filter skirts are **not** sharp, then the effective bandwidth may be significantly less than the nominal bandwidth, **and** the apparent correlator sensitivity will be increased. (And the fractional increase in sensitivity will be greater in the 3-level **case** than in the 9-level **case**.) Suppose, **for** example, that the receiver bandpass is rectangular, say 25 MHz in width, **and** that the correlator is running in 50 MHz mode. Then, effectively, you're sampling at **double** the Nyquist rate, **and** so the 3-level correlator efficiency will be ~88% rather than ~81%, about a 9% fractional increase. The fractional increase in the 9-level efficiency, which is already at 96.9%, will be less (because it can't exceed 100%).

If the bandpass is sloped, then the rms will vary across the band.

When were the data taken? Before Aug. 28, **or** so, there was some correlator misbalancing (see earlier traffic in the commisioning exploder). There would have been a bit larger fractional decrease in the 9-level sensitivity (94.99% vs. 96.93%) than in the 3-level efficiency (80.15% vs. 80.98%) - assuming that I interpreted Mark Clark's e-mails correctly. (I may well have misunderstood something, in which **case** the effect might have been greater.)

- Fred

Frank Ghigo wrote:
> There seems to be some descrepancy between the RMS measured
> in spectra from the GBT spectrometer versus the theoretical
> values. The problem has been noted in the 3-level modes **for**
> 50MHz **and** 12.5MHz bandwidths. Dave Hogg has pointed **this** out
> **for** astronomical checkout data in modes 4N2-6A-12-3 **and**
> 4N2-6A-50-3. These are the only narrow-band 3-level modes
> examined so far.
>
> The rms decreases with integration time as expected, but
> measured values are significantly lower than theoretical.
> On the other hand, **for** 9-level modes, the observed rms
> agrees well with the theoretical.
>
> I will summarize the details below, **and** the main questions
> are:
>
> How is 3-level data treated differently from 9-level in
> the Spectrometer software, **and** in the filler?
>
> Are we using the right theoretical formula?
>
> -- frank
>
>
> We are using the formula (as given in the GBT quick guide):
>
> RMS = K1 * Tsys / sqrt(K2 * Teff * Npol * BW/Nchan)
>
> K1 is the backend sampling factor, **for** which we are using:
> K1 = 1.032 **for** 9-level
> K1 = 1.235 **for** 3-level
>

> **and** K2 is the autocorrelator channel weighting function, **for**
> which we use 2.0 because the spectra were Hanning smoothed.
>
> Here is some data from the checkout of 4N2-6A-12-3:
>
> | Effective Int. Time | Theoretical RMS in mK | Observed RMS, in mK |
> |---|---|---|
> | 150 | 153. | 123. |
> | 300 | 108. | 87.6 |
> | 750 | 68.6 | 54.7 |
> | 1500 | 48.5 | 39.1 |
>
>
> More details in web page:
> http://wwwlocal.gb.nrao.edu/gbt/gbtstatus/checkouts/4N2-6A-12-3_lband/Report.html
> (note that in the report, Dave is using K1=1.36)
>