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To: DUMANDERS
From: John Learned
Subject: Calibrating the SPS in the ocean with the laser

There have been ongoing discussions here at HDC about the use of the laser calibrations in the ocean. I would like to set down some thoughts for consideration, particularly upon the question of whether we should use filters in the laser ball that vary the light output with angle inorder to make the response of the various pmts more nearly the same. It is my feeling that the fact that we really dont know the scattering function in the ocean makes the use of the filters angle filters dangerous.

First, what is the in-ocean calibration for? There are various calibrations we need. There is timing calibration, there is amplitude, the combination of the two (slewing), there is the measurement of the absolute pmt sensitivity (including angular dependance), and there is the needed measurement of the effective optical attenuation function (we shall take the function to be exponential for this discussion, thus we need one number, the attenuation coefficient). We also need end-to-end system integrity checks, but these come along free with the above.

The timing calibrations will probably change little between lab and ocean, since they depend upon propogation velocities that are not sensitive to the environment (eg. the transit time in the pmt). The pulse height calibration is also not likely to be sensitive to the environment (particularly since Shige has performed calibrations in the refrigerator), though pulse height is likely to be more variable than the timing calibration. The same goes for the slewing function. I thus look upon the laser calibrator as largely a check upon these quantities.

Much more important for the physics is the derivation of the pmt absolute sensitivity and the water attenuation length, without which we cannot translate a measured muon counting rate to a flux. We can probably count upon the angular sensitivity remaining as it is in the laboratory (since it primarily depends upon geometry and electron optics), so the in-ocean measurement of angular response is again in the nature of a check. The pmt sensitivity, however, must be measured because we may have to adjust the discriminator settings or the HV while at sea which changes the sensitivity, and we also know that the pmt sensitivity does change somewhat with time after turn-on and depends somewhat upon the light exposure history.

In fact at analysis time we can calculate either pmt sensitivity or attenuation length, if we are willing to normalize our rate to other muon flux measurements at some particular depth (about 2km would be the depth of choice since the rock to water conversion is least sensitive there). This is certainly not desirable though. Actually, as Lester Glenn and I showed several years ago, one can distinguish between the effects of attenuation and sensitivity upon muon counting rate (attenuation changes the shape of the pulse height versus rate curve differently than sensitivity), but that is a tricky business and I would rather not count on it.

It seems to me that we can best get the sensitivity by eemploying the absolute laboratory calibration and relying upon the diodes in the calibration sphere as our transfer standard. The only environmental parameter that they should be sensitive to is the temperature, and that has been calibrated by John Clem in the laboratory. The use of two calibrators gives us redundancy, and we have further checks due to the various combinations of pmts and calibrators.

The attached figure shows the results of a trivial calculation of the number of photoelectrons that we may see at each pmt for a complete set of attenuator runs with each laser. The assumed parameters are: a quantum efficiency of 10%, including all optical losses; an effective area of 1/8 m^2; water attenuation length of 40 m; 5×10^6 detectable photons out of an isotropic laser sphere; the standard string geometry (5 m spacing, tubes facing up); lasers between pmt #2 and #3, and between #5 and #6; laser sphere offsets from the string axis of 2.0 and 2.2 m (top and bottom); that the filter choices are attenuation factors of 1, 2, 4, and 8; and that the pmt angular response function is $0.55 + 0.45*\cos(\theta)$, where θ is photon arrival angle relative to the head-on direction.

There are several things one can conclude from the figure. First, as Med Webster and John Clem have long pointed out the range of amplitudes between the maximum and minimum is large, almost 1000 for the lower laser at one setting, and almost 10^4 for all filter settings considered together. This is larger than the dynamic range of our tubes (and electronics). However, it is not really so bad because of two things. First our dynamic range extends from one to several hundred PEs, and is logarithmic in nature. Second there is no limit to how low a light level we can measure, in principle, if we are triggering data recording upon the laser monitor pulses and counting whether a given tube fired or not. Practically that range is restricted by background noise (since one may get fooled by random noise counts) and by the time needed to acquire enough statistics. If the tube rate is 100kcs and we can restrict acceptable counts to a window of 100 ns, then we will get random coincidences between tube and laser 1 % of the time. If the laser runs at 10pps, then we will only get 1 real coincidence every 10 sec, and getting 1000 counts (for 3% statistics) would require many hours. John Clem tells me that the laser battery/power supply will now sustain one cycle every two minutes on each laser, which cycle consists of 8 pulses at each of the four filter settings. This means that we can have about 240 pulses per hour at all (8) filter/laser combinations. This should be fine for the tubes seeing higher light levels, but is going to be a little marginal for tube #1 from the lower laser (only about 17 hits expected). The time taken for calibration is no problem if we are able to simultaneously accumulate muon data. Comments from software folks?

Second, again referring to the figure, one sees that we get a nice range of PE values for all pmts, under the stated assumptions, ranging from about 1 PE up to a hundred or so. One exception is pmt #1 which never gets much light (8 PE max). There does not seem to be much to be done about that unless John Clem can get more light out of the top laser. (Turning the top 2 pmts over would certainly help, as I think John and Med suggested long ago, but would not be good for muon detection.) The other exception is pmt #5 which really does not ever get lower than about 3 PE, but I do not think that is very serious. It seems to me that the most desirable change, if it were possible, would be to increase the output of the laser ball by about an order of magnitude, and to change to filter steps of 1, 4.6, 21.5, and 100.

In all of the above we have assumed that the laser ball is isotropic in its emmission. The plan of the Vanderbilt gang has been to utilize filters that vary the light intensity with angle by as much as a factor of 200, and to mask the laser ball to illuminate only the general direction of the pmts. My concerns are twofold. First, directional light sources must not move (no rotation of the laser ball, which is actually a cylinder, and no bending of the dork). Second, scattering of light in the ocean, while probably small, is really unknown (it varies widely from place to place, and with depth). I am particularly suspicious about the far blue region just below 400 nm where many measurements made with instruments employing collimated beams have shown a steep rise in the attenuation coefficient.

Recent results by a Canadian group at NRC[1] show that the attenuation coefficient for pure water in the far blue (below 450 nm) is much smaller than given by previous data in the literature. In fact we had earlier measured an attenuation length (about 30 m) for 337 nm light in the IMB detector that we could never reconcile with the literature[2]. The relevance for the SPS is that it might be that the diffuse attenuation coefficient for far blue light in the ocean is actually better than given in the literature[3], but that there is much scattering. We simply dont know, and a full program to measure spectral attenuation and scattering functions would be a major research effort in itself. Thus I believe we are much safer to simply illuminate the ocean isotropically and not have to worry about motions of the laser ball or about the complexities of scattering.

In summary, I do not see any problem with the use of the laser calibrator with an isotropic scintillator source. The tubes can be calibrated over a goodly dynamic range, and by using the fitted intensity at each pmt using all 8 filter/laser combinations one can get a good handle on the attenuation. (With a 40 m attenuation length the attenuation ratio between the nearest tubes at 3.2 m and the farthest at 22.6 m is a factor of 1.6, which should certainly be measurable). There is enough redundancy in the measurements to assure ourselves of the assumptions of stability of timing, pulse height and slewing calibrations, to check on the angular response of the pmts, to be assured of the stability of the calibration system itself, and to know that the water conditions were stable over the duration of the measurement.

References

1) L. P. Boivin et al, "Determination of the attenuation coefficients of visible and ultraviolet radiation in heavy water", Optics (I have a galley proof only), Mar 1986.

2) R. Bionta et al, Rencontres de Morionds, 1982, and discussion in IMB collaboration internal note by W. Gajewski and J. Learned, UCI 1982

3) R. C. Smith and K. S. Baker "Optical Properties of the clearest natural waters (200 - 800 nm)", Appl. Optics 20, 177 (1981).

NOMINAL RESPONSE FOR CALIBRATOR SETTINGS