STATUS OF THE DATA

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Dear Colleagues,

Theo has completed a first analysis of the data from the September run and the relevant (or at least a sufficient set of) data are shown in Fig. 1. What is plotted is the number of recoil electrons (per pulse) in an ECAL row divided by the momentum width of the row and divided by the number of forward γ's for that pulse. Note that

$$\frac{1}{N_\gamma}\,\frac{dN}{dp} = \left[\frac{1}{(N_e N_\omega/A_e)\cdot(OF)\sigma_0}\right]\left[\frac{d\sigma}{dp}\,(N_e N_\omega/A_e)\cdot OF\right] = \frac{1}{\sigma_0}\,\frac{d\sigma}{dp} \qquad (1)$$

Here σ_0 is the total cross section (for our photon and electron energy $\sigma_0 = 3\times 10^{-25}$ cm^2). N_e (N_ω) are the total number of electrons (laser photons), in the pulse; A_e is the electron beam area and $0 < OF < 1$ is a measure of the overlap of the two pulses in time (the longitudinal direction).

The great advantage of Eq. (1) is that we do not need to know the details of the interaction as long as we measure N_γ (reliably). Since σ_0 is well known, $(1/N_\gamma)(dN/dp)$ immediately gives the differential cross-section $d\sigma/dp$ for multiphoton scattering. Note that $d\sigma/dp$ depends only on the peak laser intensity, and not on the laser energy; of course it is a function of p.

For each run, events were selected (a) in the region $0.5 < OF < 1.5$ where we set $OF = 1$ at the peak of the distribution (b) for a narrow band of laser energies $0.9U_L^0 < U_L < 1.1U_L^0$ where U_L^0 was chosen near the peak of the distribution for the green energy. The energy distribution in the rows shows a clear peak and $dN = < E > /p$. There are sufficient events after these cuts to provide better than 5% statistics; we have however assigned a uniform 20% error to all points. Typically, only the topmost 4 or 5 rows have been used in each ECAL position.

The graph contains 12 runs which correspond to the following intensities

9029, 9030, 9032	$I = 4 \times 10^{15}$ W/cm^2
9051, 9052, 9054	4×10^{16}
8036, 8038, 8042, 8048	3×10^{17}
9089, 9090	8×10^{17}

I used a nominal focal area $A = 50$ μm^2 and pulse duration $\tau = 2$ ps. Obviously there is some uncertainty in these intensities and the intensity may vary from event to event.

Figure 2 shows Christian's numerical integration of the Narozhnyi et al. formulae for circular polarization, normalized in a similar fashion. The three graphs correspond to intensities

$$I = 8 \times 10^{15} \quad , \quad 6.4 \times 10^{16} \quad , \quad 3.2 \times 10^{17} \text{ W/cm}^2$$

The calculation does not agree with the data and this can be due to several reasons:

(a) The ECAL saturates. This occurs at about ~1000 particles/ row and this level does not seem to have been reached (one can see this from the dN/dp plots); I also heard stories about partial depletion of the silicon.

(b) The laser intensity is miscalculated. While there is room for error here these errors cannot account for the observed differences.

(c) The data is corrupted. Not probable, given the reasonable overlap for widely different runs and calorimeter positions.

(d) The model is incorrect.

I tend to believe (d) and provide the following interpretation. In the perturbative regime an increase of n (number of absorbed photons) by one, multiplies the cross section by a factor of α. Thus $\sigma(n=2)/\sigma_0 = \alpha$ and

$$\frac{1}{\sigma_0}\,\frac{d\sigma}{dp}\,(n=2) = \frac{\alpha}{\Delta p} = \frac{1}{137}\,\frac{1}{30 \text{ GeV}} = 2.5 \times 10^{-4} \text{ GeV}^{-1}$$

should be a saturation limit for the $n = 2$ plateau. This is not far from what we observe for runs with $I \gtrsim 10^{16}$ W/cm^2; at $I \sim 4 \times 10^{15}$ saturation has not as yet been reached. Similarly the $n = 3$ data should saturate around $(1/\sigma_0/(d\sigma/dp) \sim 1.8 \times 10^{-6}$ GeV^{-1}. In

general the experimental data have a broader slope than the calculation and this may be due in part to spectrometer resolution or to dominance of multiple scattering in the laser focus (eventhough the calculation says that the two effects are comparable).

In conclusion we now have a presentation of the data from the September run. It will be further refined but I doubt that it will change drastically. We may learn a little more in the forthcoming run, especially since we will also study scattering from the IR beam (in this case the $n = 1$ recoils stay in the pipe). Since this is the first observation of nonlinear Compton scattering I propose that we publish now the results for the two sets of low intensity runs. These are non-controversial in that they are in qualitative agreement with the model. A discussion of the high intensity results can be given at a later time. My view is that we present our data as such, as long as we have confidence in it; the model has never been put to test before and could be deficient.

I have prepared the attached draft of a Phys. Rev. Letter for your comments and discussion. Please let me know your views on the draft and your suggestions for proceeding from here on.

Cheers, Adrian