CH/Tech/98/02

PSD Signal Timing - II
Addenda

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Further Timing Aspects of Sheet–Resistor PSDs

This document should be read *after* CH/Tech/98/01

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From mf@np.ph.bham.ac.uk Mon Mar  2 12:36:15 1998
To: oeb@yksc.york.ac.uk
Subject: Re: PSD Signal Timing
(1) It is clear that for the energy signals as the charge
collcetion time increases the peak from the amplifier becomes less
sharp and the Silena peak detect circuits fail, and so the David Smith
peak detect circuits should be used. You indeed show a figure of the
Energy signal where the peak detect circuit has failed (resulting in
the unipolar pulse being transmitted to the CAT), but is the spectrum
OK with the David Smith (DS) circuit?
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Yes, with the DS circuit the 10μs shaped energy signals are picked up OK.

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(2) There is an interesting correlation between leakage current
and the time t_ad (figure 10). There appear to be two separate
regions, one low leakage, one high leakage, do these correspond to
two different detector batches (a check on serial numbers might
reaveal this). It would be interesting to see how t_ad changed for
a given detector as I_leakage changed. Do you have such data?
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Not different batches. This simply reflects the amount of damage the PSD has sustained. The more damage/particle-hits the higher the leakage, and the slower the timing due to the presence of charge trapping centres. The high leakage current PSDs were from the central module closest to the beam, whereas the low leakage PSDs were from the corners of the central module.

"`...how` t_{ad} `changed for a given detector as` $I_{leakage}$ `changed`". *There are unfortunately no data to this effect. However, the approximate behaviour should be* *given by the solid lines in figure 9 of CH/Tech/98/01. The variation about these lines is due to the slightly differing detector characteristics.*

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(3) Personally, I would be extremely surprised if the position signals
were failing in the peak detect circuits. They are shaped to 2us
and we know that the circuits work for shaping times close to 6us
(slightly less). So the question arises if figure 9 is the whole
story, and I think that it is not. First of all are you sure about the
time scale (i.e. there is not a factor of two missing), the TDCs are
8K and we need to be sure that you are getting the right number of
ns per channel. Also, I think what you should plot in figure 9
is not t_ad, but the time difference between the first signal to
arrive (which provided the trigger) and the last. Since if the
last signal to arrive falls outside the 1.6 us time window then
it will be lost. This would be a most useful spectrum. Further,
if it is the peak detect circuit failing, then you should not
see a zero position but a position which is reduced in amplitude
by some factor, as in the case of the energy signals.
This the counts should not be missing from the position spectrum
but displaced, probably towards the centre of the detector. Is
it possible to compare the idevidual position signals to see
if this has happend, since if you know a,b,c you should know
the value of d (as the energy is contant).
Also I am aware that in the last run that there were timing spectra
which were cut off at 1.6us, which seems to be disputed in the
present report.
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No factor of 2 compression.

The greatest timing difference occurs between two diagonally opposing position signals, eg. t_{ad}. At the points of maximum difference (where the halo holes appear) signal 'a' fires the DA trigger and signal 'd' is last (or vice-versa for the other half of the PSD).

If any of the position signals have been missed due to a peak detect fault and have been reduced by some factor, then I cannot find them in the data. Could it be that they have fallen below some conversion threshold and thus do not appear? These data were for 5MeV α–particles, the lowest position signals for which lie very close to the ADC and amplifier thresholds.

"`...timing cut-off at 1.6`μs*"... Having gone back to look at the timing spectra again I have noticed the following. The position timing signals all lie in the range 732*

$\rightarrow$ 1732 channels. ie. a dynamic range of precisely 1μs and NOT 1.6μs. Where the 732ns comes from I am not sure. Therefore the maximum timing difference equals 1μs exactly. In figure 9 of CH/Tech/98/01 t_{ad} reaches this value (the timing resolution – at a guess this is probably of the order of 200ns – must be added to the average behaviour). So the halo effect could be explained by various permutations of signal timing differences falling outside of the magic 1μs barrier.

But, the conclusion drawn in section 4 of CH/Tech/98/01 is the only scenario in which I can account for the energy independent nature of the halo effect – unless the MEGHA MAs do not operate with leading edge discrimination. If the discriminator fires on the shaped pulse then the time–walk should be relatively energy independent. However, this is clearly not the case since the time–walk would also be position independent!

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From mf@np.ph.bham.ac.uk Tue Mar  3 10:14:48 1998
To: oeb@yksc.york.ac.uk
Subject: Re: PSD Signal Timing Update
I think that your timing spectra indicate where the problem lies.
The dynamic range on the mpp pulse appears to be 1us rather than 1.6us
I have just spent an hour on the phone to Dick and he agrees
this is the most likely scenario.
I have a scheme from Dick to extend the mpp pulse by changing
a capacitor in the Tail unit.
From my recent experiemnce with the MEGHA amps at ANL it would seem
clear that the discriminator signal is not derrived from the shaped
pulse, but from somewhere near the input stage, and hence the
energy independence hypothesis goes out of the window.
I still would like to see the time difference spectrum between the
first of the five signals, and the last, I think that this will be more
revealing.
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The timing difference between the signal that triggers the DA and the slowest signal recorded are plotted in figure 1 as a function of X. The spectrum in the background shows the count density for this data.

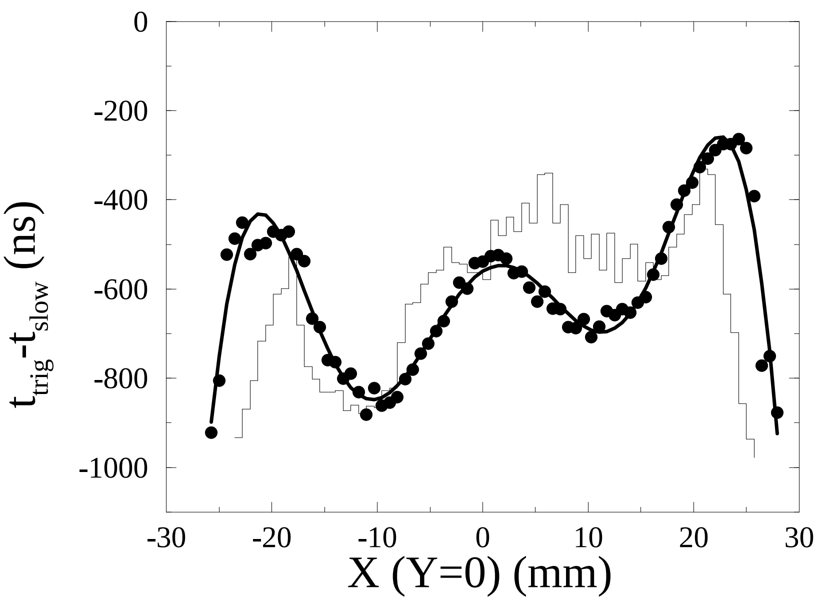

Figure 1:

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From mf@np.ph.bham.ac.uk Wed Mar  4 12:10:15 1998
To: oeb@yksc.york.ac.uk
...the deficit in the position spectra and missing part from the
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time spectrum, given all of the uncertianties, look to
be of the same order of magnitude which I feel happy about.

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From bw@np.ph.bham.ac.uk Wed Mar  4 13:45:20 1998
Subject: Detector/DA matters
To: oeb@yksc.york.ac.uk
We are fairly happy that we now understand why we have been
losing events from our data sets, and that we have mechanisms
for avoiding this happening next time.
However, there are other factors we need to consider. It does seem
that as the detector is irradiated, the shape of the signals changes.
As noted above, we can probably make changes to the trigger
method and to the DA system which avoid this losing data.  However,
two questions remain:
1. Does the effect keep on happening ie does the leakage continue
to increase and the accompnaying delays continue to lengthen,
or do they reach some plateau?  If they plateau, then we can
arrange to have timings set in the DA to accomodate this
maximum.  However, if the continue to degrade, we may eventually
get into the same problem again.  Any thoughts on this?
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I reckon the signal delays will continue to increase, or at least I cant think of any reason why they shouldnt. This is a little pessimistic, but there is no data - data with good detector bias that is - to support an argument either way.

The CDF/GD run in Nov/Dec96 and the recent alpha tests were performed at 120V bias. To alleviate these problems the nominal operating bias should be increased to at least 150V.

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2. Also related to the changes, what is the effect on the energy
and position calibration?  It may be that we can solve our
problems and ensure that all the data gets to tape, but if
it has continually changing energy and position responses, we
may be no better off!  I guess we could perhaps cope with the
energy response changing (presumably the PHD changing) by making
sure we run some elastic calibration data in every 4 hours
or so.
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The energy response changes can be corrected as long as it is assumed that the position response is constant. However, it is not...

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However, it is less clear how we could cope with a
changing position response.  Now, one could argue that, to first
order, the change is effectively a decrease in the charge
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collected, so since this would effect all 4 position signals
equally, the position response will not change.  However, there
must be an effect since the time delay on each signal depends
on how far it is from the contact.  Indeed, we know already
that we get a different position linearity for different
shaping times.   So, the question is, do we have data which
can show us how bad this effect is?
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Yes. Three PSDs used for my thesis run in July96 were placed in the central module for some tests in Nov96. One was annealed and the other two (one more heavily irradiated than the other) were left as they were - the response of the more irradiated PSD however was destroyed by a 'faulty' preamp. Figure 2 shows the responses of the annealed PSD, the un-annealed used PSD and a new PSD. At the start of the July96 running the used and annealed PSDs looked like the new PSD. By the end they were horribly distorted as you mention. But after 4 months of peace in which the leakage currents halved, and under proper bias in Nov96 the response was as in figure 2.

The used PSD shows characteristics of both the 'pin–cushion' distortion near the PSD centre and 'barrel' distortion near the edges and corners. A similar distortion is apparent in the annealed PSD, but to a lesser extent. These effects cannot be described by the PSD-model, but may be associated with a combination of the injection of the leakage current into the PSD to alter the charge division ratios used in the position determination, and/or most likely the extra position signal ballistic deficit due to the delayed charge collection in the damaged silicon.

*The distortion is not too great, but is a problem. In addition, he distortions *immediately* after the Jul96 run and the distortion for the more heavily damaged detectors are unknown, and are likely to be (much) worse. Obviously the calibration grids can not be placed in front of the PSDs willy-nilly, and I think that there is absolutely nothing that can be done about this problem in the Hamamatsus, but believe that the response of the newer Eurisys PSDs would be much better. I would certainly be much happier using them in future.*

Getting back to the timing problem; is there any way in which a delayed signal could be converted if its timing signal fell outside of the 1.6us DA acceptance? I was thinking along the lines of a 'trigger-link' in which, say, IF the PSD energy signal from a particular detector is sampled and held THEN the position signals associated with that PSD would also be held. There would be no timing (WALK) pulse from the position channels if they fell outside of the 1.6us, but the energy signal WALK could be multiplexed to the position channels to get around this.

What is so special about 1.6us anyway? As I understand it there is 8us from the MPP to the start of the CAT operation.

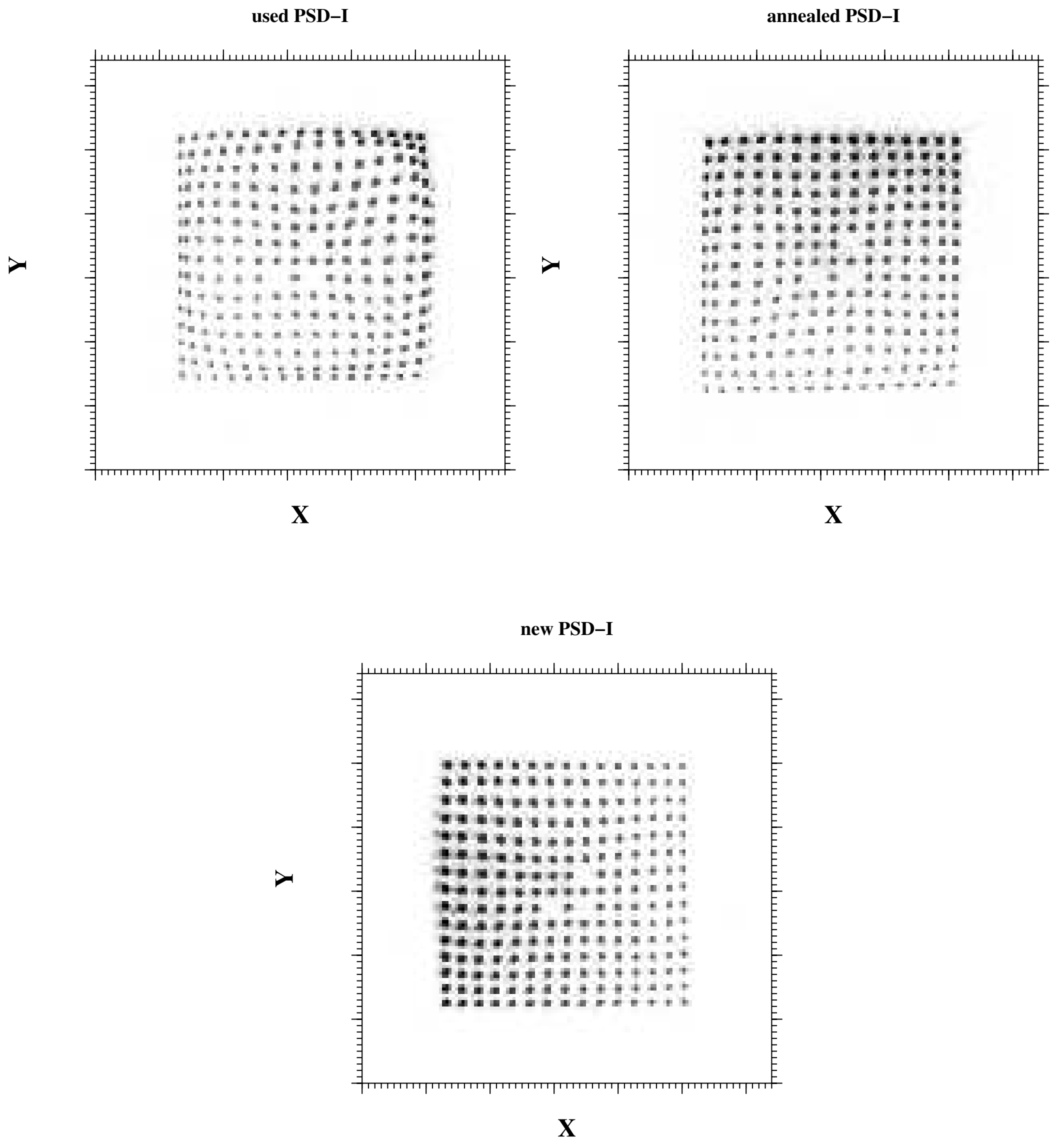

Figure 2: Comparison of the position patterns for the used PSD, the annealed PSD and the new PSD.

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From mf@np.ph.bham.ac.uk Thu Mar  5 09:05:24 1998
To: oeb@yksc.york.ac.uk
Subject: Re: Detector/DA matters
... the plan for the running in March/April is to modify the TAIL
so it produces a 4us MPP pulse which should get around the problems
we are seeing. Also as a last resort I am getting Paul to build
a FIFO so we can take the energy discriminator signal and send
it into the position trigger input so that they will always
be included in the event.
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