MASSACHUSETTS INSTITUTE OF TECHNOLOGY

Laboratory for Nuclear Science

PROCEEDINGS OF THE SECOND INTERNATIONAL COLLOQUIUM ON PEPR

CAMBRIDGE, MASSACHUSETTS

MAY 5 – 7, 1970

TERENCE L. WATTS, EDITOR

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MASSACHUSETTS INSTITUTE OF TECHNOLOGY

Laboratory for Nuclear Science Technical Report

Proceedings of the Second International Colloquium on PEPR

5-7 May, 1970

Terence L. Watts, Editor

Organizing Committee:

B. T. Feld, R. I. Hulsizer, I. A. Pless, T. L. Watts

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This technical report is number 90 in a series of technical reports issued by the Laboratory for Nuclear Science since its inception.

Preface

The First International Colloquium on PEPR was held at Nijmegen, Netherlands, in June, 1968. Since that time several PEPR systems have changed their method of operation or have begun to process film for the first time. Irwin Pless suggested that the time seemed propitious, therefore, to hold another colloquium to facilitate the exchange of information among the groups owning or proposing to own PEPR devices.

The conference was held at Endicott House, a mansion owned by MIT, in Dedham, Massachusetts. Attendance was made by invitation only, partly in an attempt to make the sessions small and informal, and partly because of the limited facilities at Endicott House. A banquet was held on Wednesday evening, May 7th, 1970, after which V.F. Weisskopf gave a short but entertaining and encouraging speech. This speech has not been included in this Proceedings.

The first day of the conference was reserved mostly for the status reports which had been requested from each of the owners of PEPR devices. Wednesday afternoon was reserved for a discussion of bubble chamber physics problems by representatives of the major bubble chamber facilities including NAL. Talks from other CRT devices for processing film were given but no systematic effort was made to include all such devices, and apology for limited space and time must be made to those not represented.

Discussion after talks was recorded and has been edited to make the spoken word readable and to conform to the comments written out by participants on the forms supplied during the conference. When the written comments appeared as abbreviated versions of the spoken comments, the extra detail was left in the transcript. Occasionally, drastic editing was necessary to remove confusion which developed during discussion. A few talks have been typed up from the recording since no typewritten version was available. Misrepresentation of the views of participants will have occurred during this re-writing and the editor takes full responsibility and offers apologies to those who have suffered.

Grateful acknowledgements are due:

I.A. Pless for organizing the Round Table Discussion.

I.A. Pless and A. Nakkasyan for the compilation of bubble chamber picture statistics from data gathered by Irwin Pless from each national laboratory.

R.I. Hulsizer and V. Kistiakowsky for the editing of the transcript of the Round Table Discussion.

H. Baumel, D. Brick, W. Chien, M. Choe, F.T. Dao, M. Hodous, T.C. Ou, G. Schulze, A. Sheng, R. Singer, P. Trepagnier for assistance in editing the discussions after talks.

R. Engler for patient typing of the proceedings.

the staff of Endicott House who gave us a fine banquet and conference, and who found us to be one of their most peaceful large conferences.

B.F. Wadsworth for general consultation.

The Laboratory for Nuclear Science at M.I.T., of which PEPR is part, supplied services and the Atomic Energy Commission supplied support*. The banquet was contributed by part of the registration fee of participants and by grants from Astrodata, Inc., and the Industrial Liason Office at M.I.T.

Terence L. Watts

August, 1970

* AEC Contract AT(30-1)-2098

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"....The conclusions which I have reached are that all PEPR groups are completely independent. They each develop their own hardware and software, and each installation feels that it is the only successful one....."

DEC Interoffice Memorandum

5 May 1970 9:30 a.m.

Session I

Chairman: T. B. Day (Maryland)

Welcoming Speech by Prof. P. T. Demos, Director

Laboratory for Nuclear Science at M.I.T.

It would be difficult in any case to give a cheerful address under present circumstances. I am wondering whether I shouldn't be giving a strike or anti-strike talk of some kind or another rather than welcoming people.

I should recollect a bit. I can go back ten or twelve years, it may have been more and I recall Irwin coming by and asking to develop, I think what was a 1 1/2" bubble chamber. He predicted that there might be some things to come, but I didn't think they would be measured by this kind of gathering.

I must say it has been enjoyable for me, personally. Administratively a few of us have had to enjoy this sort of thing vicariously. We've watched PEPR grow; we've seen it become a very effective tool; I hope it's everything that it promises. It's been particularly pleasant to have had international association and participation on this scale; it's been enthusiastic and I think we wouldn't have been able to get it through the early phases without much, much help. Our Linac, by the way, is being built in a similar way. I'd hate to tell you the number of times we've had people from elsewhere give their time freely to that, so it's an M.I.T. habit. I am just glad that it has this kind of success on occasion.

I should take the opportunity to congratulate all who have been party to the success of PEPR and I should certainly commend and thank Irwin and his colleagues here for a very unflagging kind of devotion to it. Living with them has been a running affair, with me doing most of the running. I don't know if our colleagues from the A.E.C. are here yet, I haven't seen any, but I am sure I speak for them in all respects, but particularly in being proud of the accomplishment. I shouldn't say much more than that except that PEPR has always a high priority with us, it will continue to have that, but my only concern is that we have the scale of suppprt that will enable us to make those priorities effective.

I would stop and wish you an excellent conference, certainly with physics to follow, I hope, and much of it, and again, I am sorry that we are surrounded with tragic events, I hope they will pass. I thank you.

DAY: (Maryland) Well, I can't ever recall being in more pleasant circumstances for a meeting so it's a great pleasure to be here. I am not sure it's a great pleasure to chair the first session, since it was rather short notice, but it doesn't require much speaking. Let me just get on to the first report a little bit early, in fact, which is a good precedent. You'll hear a report from the second oldest PEPR project first. I guess M.I.T. is too shy to be simultaneously host and report first. We have a report from Yale. It will come in two parts. The first one will come from Dixon Bogert.

STATUS OF THE YALE PEPR SYSTEM

D. Bogert, P. Lucas, W. Lund, and H. Taft
Yale University, New Haven, Connecticut

Presented by D. Bogert and W. Lund

This report shall take the form of a progress report detailing achievements and developments with the PEPR system at Yale University since the time of the report to the First International Colloquium on PEPR at Nijmegen in June 1968. This report will be presented in two parts; the first a report on physics measurements to date and plans for the future, and the second a report on some engineering problems and developments accompanying the conversion of the Yale PEPR hardware to the 80 inch BNL film format.

I. Physics Progress

In the proceedings of the First Colloquium[1], the programs then in use for the production measurement of Σ^- events in film from the 30 inch BNL chamber with 400 MeV/c K^- were described. Production measurement of Σ^-, started about 15 Dec 1967, continued throughout calendar 1968 until 1 Feb 1969 at which time 125, 000 Σ^- had been processed. PEPR was operated on a 16 hour/day, 5 day/week schedule. About 13% (or 16, 000) were fiducial volume rejects, leaving 109, 000 PEPR measurements. Of these, 34, 000 were remeasured for failure to make at least a 0c production followed by a 1c decay fit. An additional 5, 000 events which made only 0c production were added to the remeasurement sample at a later date. This means that 70, 000 events made 3c, 2c, or 1c fits at production and satisfactory decay fits to $K^- p \to \Sigma^- \pi^+$ followed by $\Sigma^- \to n \pi^-$. In some sense then, 70/109 is the overall efficiency for Σ^- under the vertex guidance mode of measurement adopted for this first experiment.

The required remeasurements were performed on image plane digitizing machines whose active components were two pieces of magnetostrictive wire strung to form a bipolar coordinate system. After several modifications, this system now works quite dependably. Four tables for 30 inch BNL film and three tables for 80 inch BNL film have been constructed and are operated on-line to a PDP-1 computer formerly used for on-line control of the Frankenstein system at Yale. Remeasurement rates averaging 180 events/shift were maintained with a 3 pt/ 3 view format.

The overall purpose of the Σ^- experiment was to obtain g_A/g_V for the leptonic decays of the Σ^-. The separation of e^- and μ^- events from the overwhelming sample of π^- two body decays was accomplished with the aid of manually measured gap length distributions. It is our opinion that meaningful PEPR ionization measurements on our particular sample of 30 inch BNL film would have been exceedingly difficult, given the great number of subjective decisions that seemed necessary using the digitizing microscope. After necessary cuts on momentum distributions a final sample of 63 leptons including 44 e^- and 19 μ^- was obtained. The decay assymetry $\alpha_{lept} = +0.36 \pm 0.39$ was obtained. This corresponds to a value of $g_A/g_V = -0.33^{+0.39}_{-0.85}$; the sign is in disagreement with the Cabbibo theory, but a positive sign cannot be excluded. A value of α_- based on a sample of 60,000 two body decays of $\alpha_- = -0.067 \pm 0.011$ was also obtained. These results and the PEPR programs and performance are discussed in detail in an article to be published in Physical Review[2], and constitute the first report on physics carried out with the Yale PEPR as the principal measuring engine.

The period from 1 Feb 1969 to 15 March 1969 was occupied with both hardware and software modifications to PEPR. The principal hardware modification consisted of the installation of a new set of DAC's for the main deflection of the CRT beam. Software modifications, mostly carried out by Peter Lucas, were the result of a study of timing of the then existing PEPR programs. These studies resulted in the recoding of all the programs logically 'below' element recognition into machine language. Also, table lookup was employed for sines, cosines, and coded angles. Large banks were zeroed with block transfer instructions.

The coding for track follower was substantially reduced by the elimination of many of the more esoteric logic branches. A net reduction of 1/3 of the time of the programs to process an event was achieved. 103, 000 τ^- events were measured using the basic vertex zone guidance scheme employed for the Σ^- events by 1 Oct 1969. Of these, 13, 000 were fiducial volume rejects. About 60, 000 of the remaining 90, 000 events were found to be satisfactorily measured and made unambiguous decay fits. The remaining 30, 000 were remeasured on the wire measuring tables. The physics of this experiment is a study of the leptonic 3-charged decay branching ratios and should be completed in calendar 1970.

The PEPR programs were further modified by Peter Lucas to permit operator controlled remeasurements, using the DEC display CRT but with no direct film accessibility. From October 1969 to February 1970 such operator controlled remeasurements were performed on a one shift/day basis. About 50% acceptance was found. This indicates that about one half of the remeasurements were of sufficient difficulty that even with an operator, the events were impossible, largely the result of the inability of the hardware to obtain data on some track of these events.

The τ^- experiment may be regarded as intrinsically more difficult than the Σ^- experiment due to the demanding nature of a branching ratio experiment and also the higher multiplicity of tracks at the principal vertex.

Since March 1970, the principal effort has centered on the conversion of both the hardware and the software to measure 12. 8 GeV/c K^- from the BNL 80 inch chamber. We have elected to pre-digitize this film, in contrast to the vertex zone guidance of the Σ^- and τ^- experiments. Three points on each track, the vertex and two others, are measured on the magnetostrictive wire measuring tables. Several reasons, including a large number of short tracks and the high multiplicity of prongs, led to this selection of strategy.

The film will be measured in three different pulldowns per frame, so that the same 1. 5:1 optics and 5 inch CRT may still be used. The new film drive has been installed and a substantial fraction of the necessary programming has been coded and debugged. Simultaneously, a program of increasing the range of the hardware response and the introduction of peak detecting to permit more accurate track center measurements has proceeded. A number of technical problems, centering around the necessity for a wide dynamic range of perhaps 30:1 are discussed in the second section of this report.

The physics of the 80 inch experiment will include a study of all topologies so any interaction found during scanning is being pre-digitized. Also, a total beam count will be performed by PEPR during the measuring pass. At Yale, we do not feel that we will be able to study automatic scanning until we develop an operator to film interface. We do, however, hope to develop a satisfactory approach to ionization measurement for the events in the pre-digitized sample.

II. Hardware Innovations for the Extended Range

The purpose of this section is to report on hardware developments made by the Yale PEPR group towards improving the operating range of the system planned for use with the 80'' reprocessed film.

Fig. 1 shows a sample of the reprocessed 80'' film which is a representative example showing the central portion of the full picture. This film presents a real challenge from the engineering point of view.

Notice the vertical bands of sharply contrasted background tones that range from clear to opaque black. These are, of course, the ''coathangers''. Fortunately, the coathangers are generally vertical and with angle discrimination in the programming, they can be tolerated. Some coathangers digitize as noise smears while others appear as track elements. Serious problems arise with coathangers appearing in the general vicinity of fids as they may digitize as a false vertical arm of a plus fid.

Notice the incremental variations in film background and track strength encountered along any track element. Tracks in flare area tend to be washed out while in some bands are distinctly contrasted against the dark black background. Also, notice there are major differences between fids and, in fact, often element strength varies from arm to arm of fids.

This picture is self-explanatory but one's concern must be for the overall dynamic amplitude range of track signals if this film is to be measured.

Fig. 2 is a bar graph of some representative raw phototube data (w/o AGC) which summarizes signal variations. This graph presents only a select portion of a larger data set which itself admittedly is not a large sampling. But because of a general picture uniformity from frame to frame, we believe this graph is representative. We expect to verify this with future work.

This is a plot of some 30 central angle pulses with total bar magnitudes representing the total pulse and pedestal heights. The cross hatch area within the bar is the track pulse amplitude for that particular sweep, with the clear bar showing the pedestal height for that area.

The signal variations are summarized in the table. Notice the fid pulses vary from 2 to 20 volts (10:1) with track pulses changing from 0. 6 to 7. 5 volts (12:1). Combining all elements (fids and tracks) results in a demanding 33:1 ratio. Also, these ratios are further modified by a 4:1 ratio in the pedestal heights.

This film with its signal variations creates a real hardware crisis. Let's consider even a 20:1 amplitude range. Let's assume you have a discrete component system, needing a 2V of signal (min) to overcome forward base-emitter drops, meaning you must pass a 40V of signal on the ''high side''. With a necessary frequency response to 10-15 mh, amplifier design suddenly becomes a ''nightmare''. We have had success with the Fairchild μA715 IC OP AMP coupled to a voltage booster but to our knowledge there are no commercially available IC or discrete component modules quite suitable for the job. Also, note that if peak detection is used, no clipping can be tolerated, making the problem even more demanding.

Fig. 3 shows the Simplified Block Diagram that is our current Yale PEPR system. It represents our attempts to solve the hardware problem related to this film. It is in the engineering stages but is operational, being used for program debugging but needing some refinements before doing physics. This system utilizes a signal controlled AGC, a new pedestal subtractor and peak detecting.

The raw uncompensated phototube signals are applied to the AGC system which has two Sample and Hold circuits. Their purpose is to locate the most positive portion of the pedestal and hold this voltage as an indication of signal strength. Operating in alternate out-of-phase manner, circuit 1 is Holding with circuit 2 Sampling, and vice versa. Both outputs are combined to generate a control voltage for an FET attenuator. Therefore, the AGC is updated on every other sweep. This is not a new circuit having been used in our 30'' experiments, but seems to be essential for 80'' measuring, when remembering the sharply contrasted point-by-point background differences typical of the 80'' film. The FET normalized signals then pass thru a computer controlled attenuator where further signal strength modification can be made by programming. Signals pass into the new pedestal subtractor where tracks are separated from pedestals. Next the signal is split going first to the Height - Width discriminating delay line TEDs (similar to the Mark V in the Astrodata systems). These TEDs are needed to locate the central angles pulses. Secondly, the track signal peak is located by the differentiator and provides a stop-scaler pulse directly related to the true track center by marking the zero slope with a logic pulse. The signal peak location is stored in a one-shot delay allowing time for the delay line TEDs to discriminate for central angles. By a sequential gating system using an RST flip-flop as an anding circuit, only central angle track peaks are passed on to the controller. A series of 3 one-shots and flip-flops commutate in round-robin fashion eliminating any dead times.

The circuits needed to form a peak detector are diagrammed in Fig. 4. A straight-forward simple method of peak detection is used with a μA715 differentiator circuit driving a μA710 comparator type zero crossing detector. The zero slope of the incoming signal is located by mathematical differentiation and converted into a zero crossing logic level. The zero crossing detector amplifies this logic signal and drives the subsequent gating circuits. A capacitor is added as a parallel feedback element to control high frequency noise normally associated with differentiators. Circuits of this type perform satisfactorily with amplitude ranges of 25 or 30:1.

The actual waveforms of peak detection are diagrammed in Fig. 5. This illustrates a useful feature of a peak detecting system as the delay line TED outputs become noncritical in the timing sense. Because these pulses are used only to gate a F/F, they may occur at any time while the delayed peak one shot is set. The usual timing differences between narrow TEDs, broad TEDs may be ignored.

Also, note that only true track centers are digitized meaning that conglomerate waveforms such as ''camels'' found with element intersections are either totally rejected or otherwise outputted as a true element center position (a peak). This is a highly desirable feature.

The so-called Peak Holder is the heart of the new dynamic pedestal subtractor and is illustrated as Fig. 6. Its purpose is to separate track pulse from pedestal using a sample-hold method. A dummy pulse V_R is summed with the pedestal to transpose the negative pedestal-negative pulse, to a positive pedestal-negative pulse signal. (This will look familiar to those working a clear film format.) The circuit now works as a standard peak follower circuit sampling the more positive pedestal changes but ignoring the negative going track pulses because of reverse bias on the charging diode. The resultant output is some proportionality K times (V_R-Vped). K is determined by resistor values with a derivation included in Fig. 6. This circuit can be made to follow the general pedestal shape rather than hold a constant value voltage if the optional leakage resistor is added. This must be selected to specific needs remembering the capacitor voltage must not follow the negative going track pulses. In effect, we have generated a pedestal pulse without track pulses.

Fig. 7 shows a typical sum and difference amplifier circuit with $E_o = E_2 + E_3 - E_1$. Having generated the (V_R - Vped) signal, it is a simple matter to combine all signals and algebraically cancel all voltages except the track pulse. Also, notice the dummy pulse V_R is conveniently cancelled away by subtraction.

Putting together the peak holder, sum and difference amplifier with a pulse amp generating the V_R, we create the dynamic pedestal subtractor as is shown by Fig. 8. It uses no delay lines, and has wide amplitude range in the mathematical sense as pedestals down to 0 volts are tracked.

Assessment

It is somewhat premature to predict success, but we are encouraged by preliminary results. We need to further expand the range of the system and, in particular, expand the range of the H-W discriminating TEDs which now separate the off angles from central angles. This should be feasible to do as the critical digitizing is done with the peak detector which, by itself, is not the limiting factor.

We believe that while our system theory is elementary -- it is direct and will be effective.

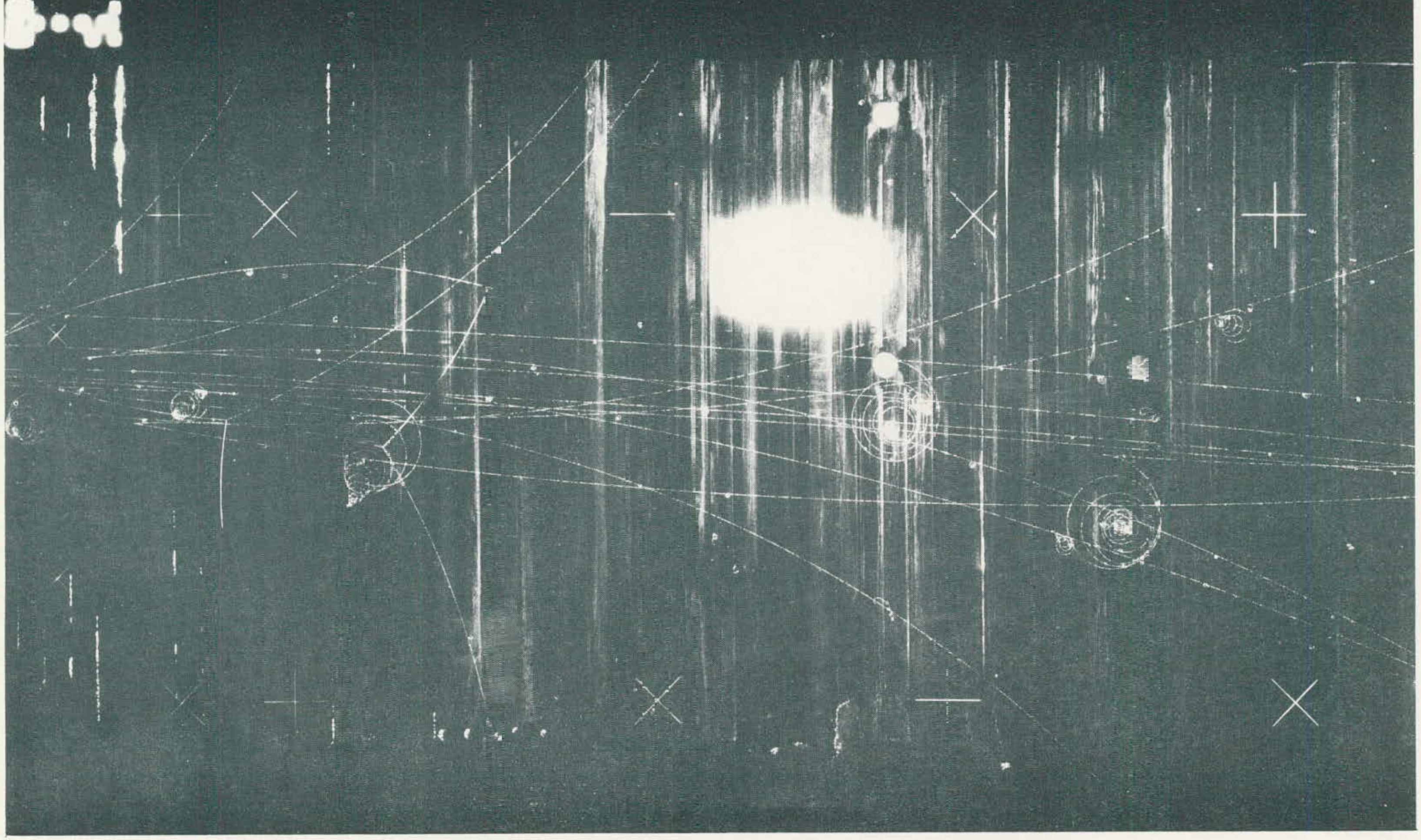
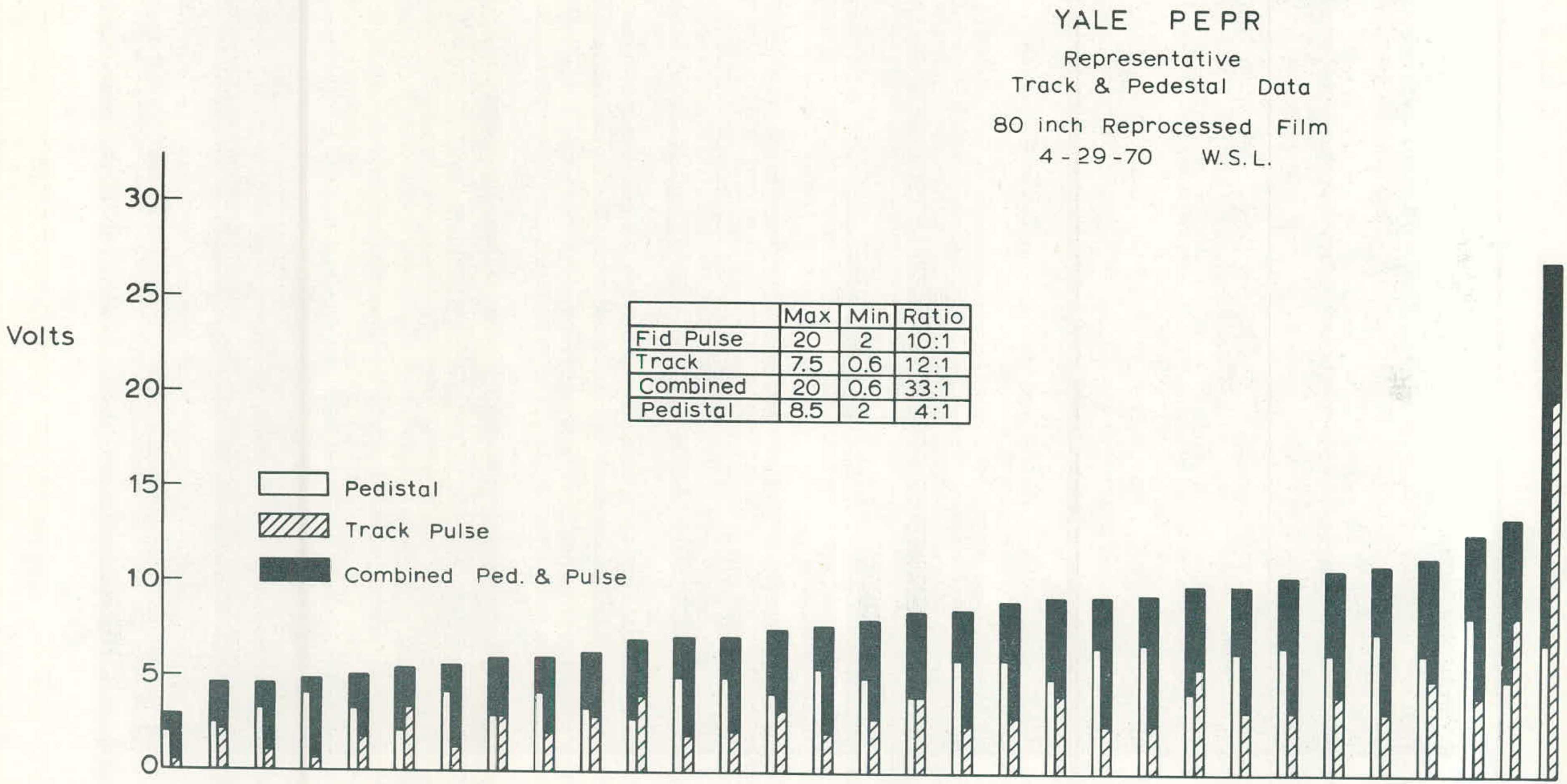


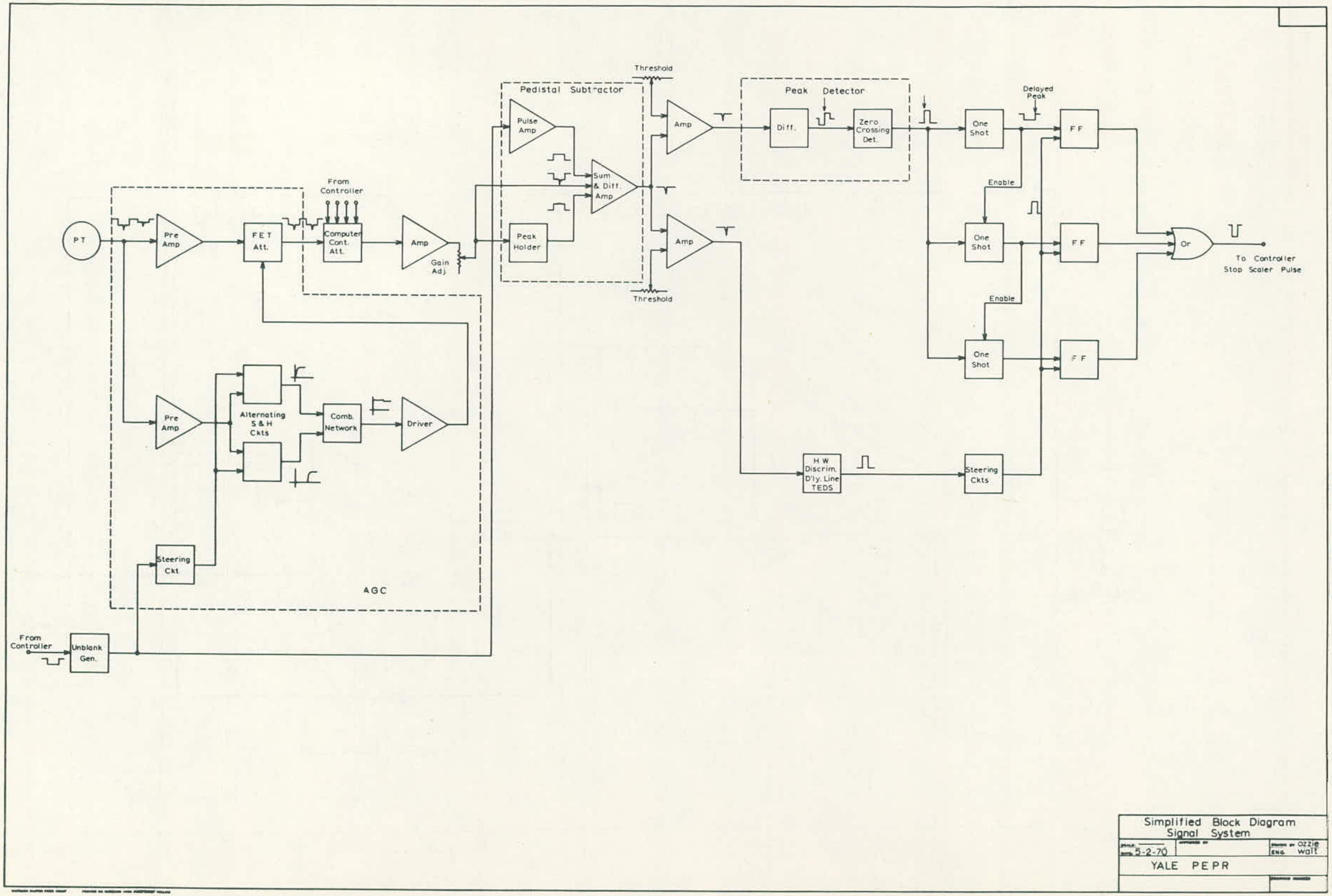
Fig. 1



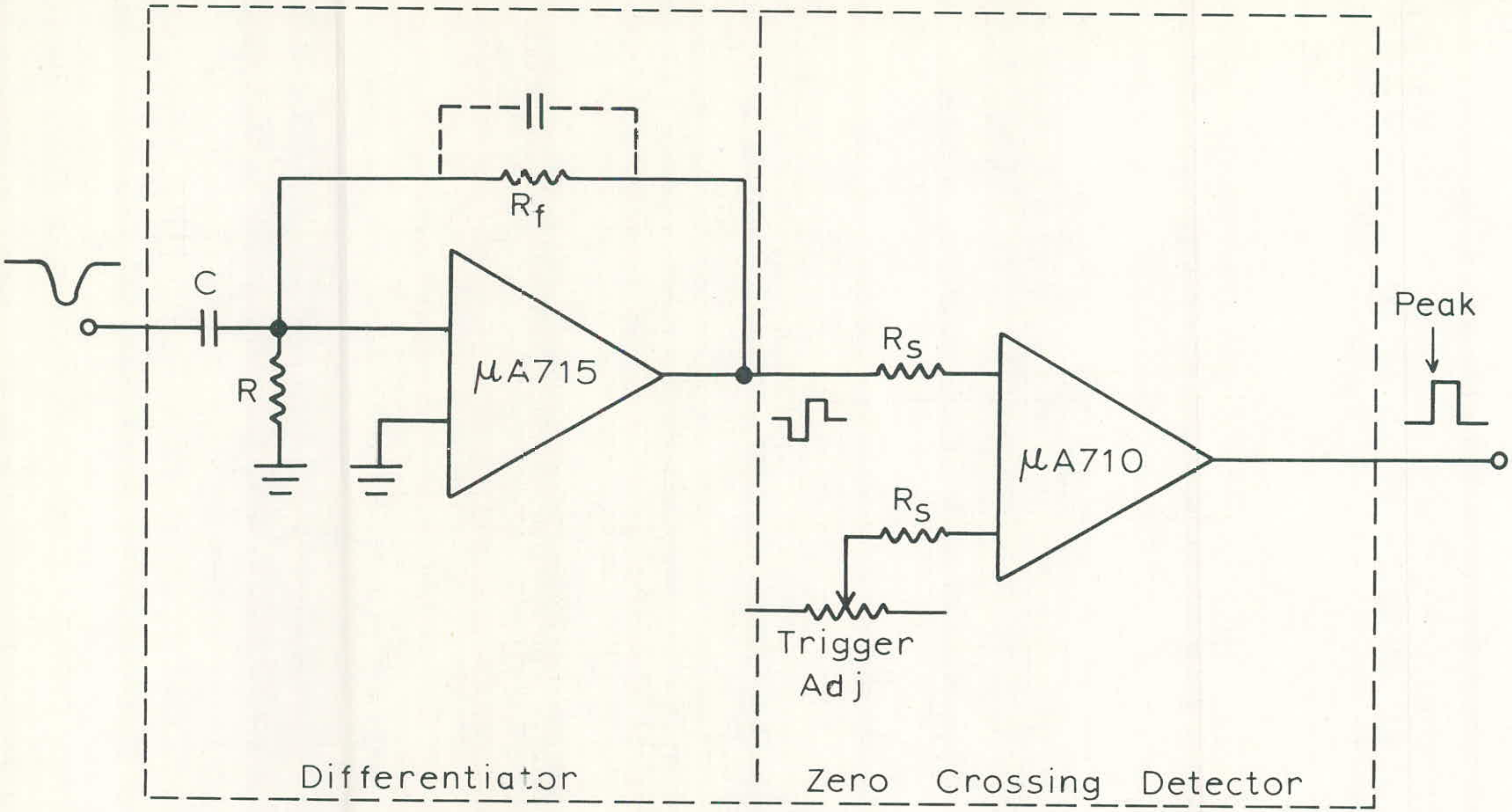
	Max	Min	Ratio
Fid Pulse	20	2	10:1
Track	7.5	0.6	12:1
Combined	20	0.6	33:1
Pedistal	8.5	2	4:1

Fig. 2

Fig. 3



PT
Pre Amp
FET Att.
From Controller
Computer Cont. Att.
Amp
Gain Adj.
Pedistal Subtractor
Pulse Amp
Sum & Diff. Amp
Peak Holder
Threshold
Amp
Threshold
Peak Detector
Diff.
Zero Crossing Det.
Delayed Peak
One Shot
FF
Enable
Or
To Controller Stop Scaler Pulse
Pre Amp
Alternating S & H Ckts
Comb. Network
Driver
Steering Ckt
AGC
From Controller
Unblank Gen.
H W Discrim. D'ly. Line TEDS
Steering Ckts
Simplified Block Diagram Signal System
5-2-70
ozzie
walt
YALE PEPR



Yale PEPR
Peak Detection Ckts
4-21-70 WSL

Fig. 4

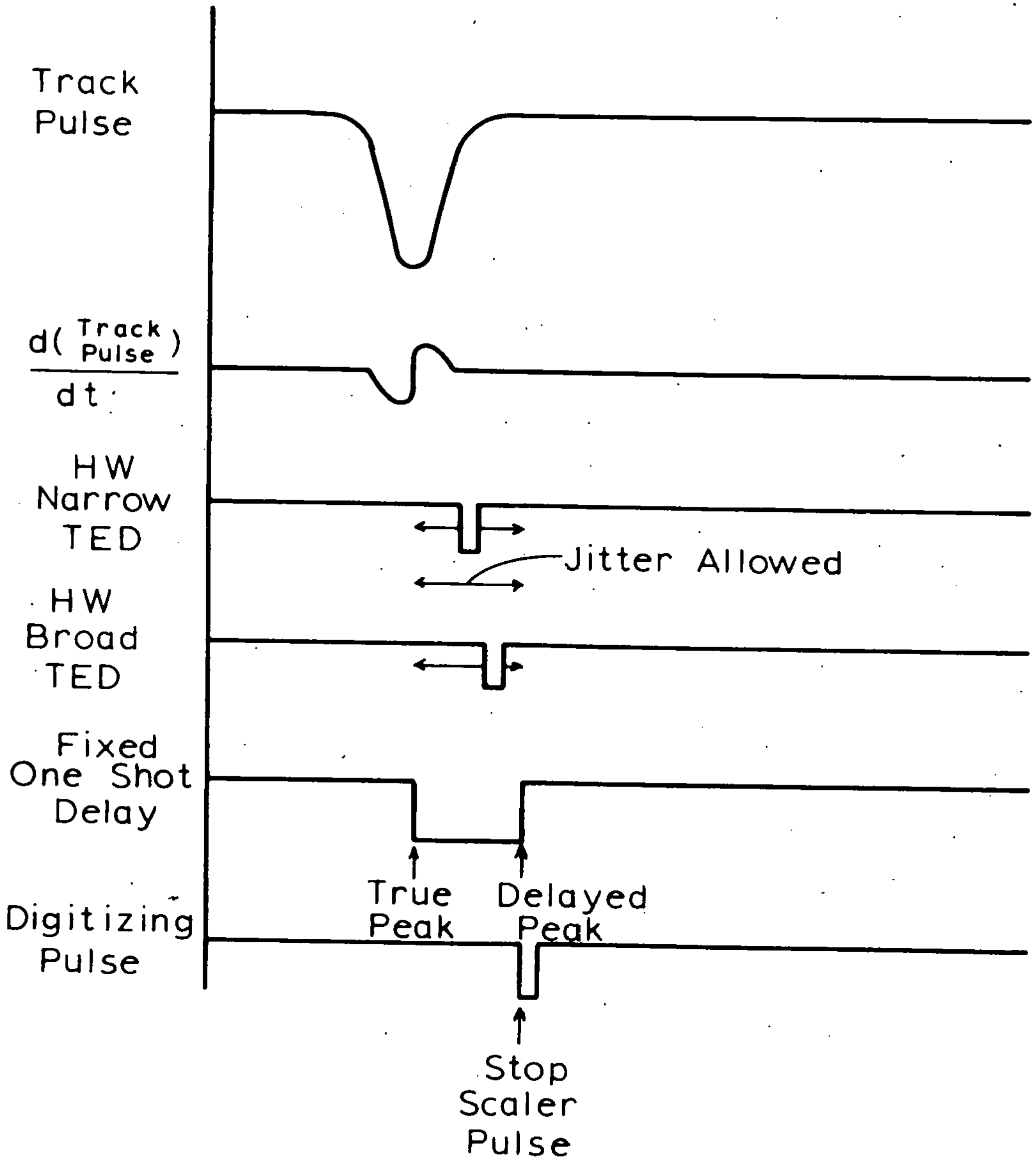
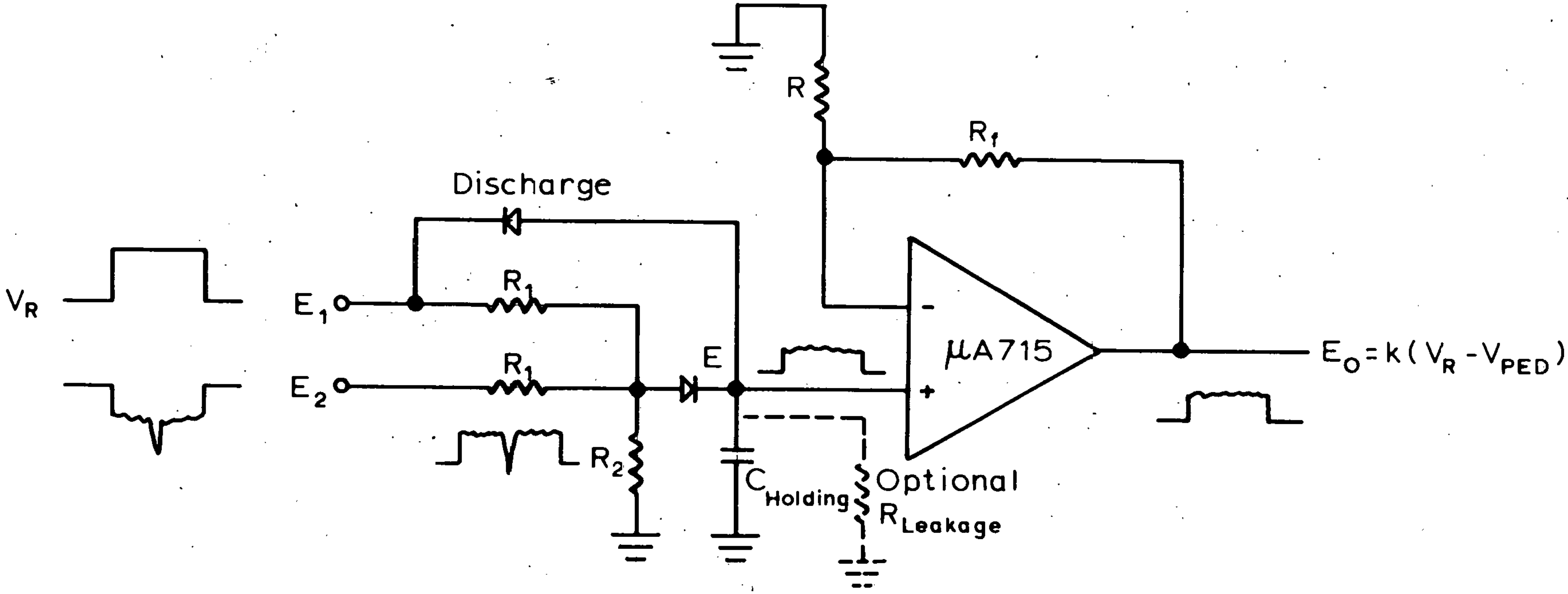


Fig. 5

Yale PEPR
Peak Detecting Waveforms
4-22-70 WSL



$E = k_1(E_1 + E_2)$ $\qquad k_1 = \dfrac{R_2/R_1}{1 + 2R_2/R_1}$

$E_O = k_2 E$ $\qquad k_2 = \dfrac{R_f + R}{R}$

$E_O = k_1 k_2 (E_1 + E_2')$ $\qquad$ where E_2' is ped. w/o pulse

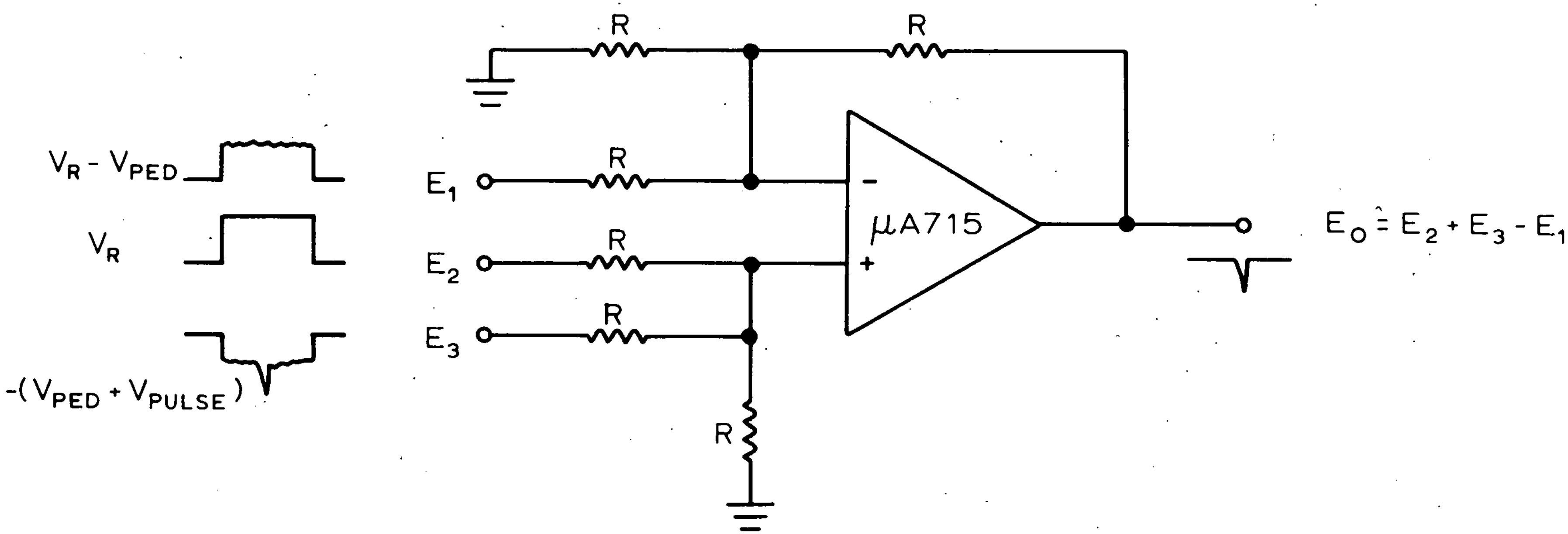
if $E_1 = V_R$

$E_2' = -(V_{PED})$

then $E_O = k(V_R - V_{PED})$

Yale PEPR
Peak Holder
4-21-70 WSL

Fig. 6



$$E_O = -E_1 + E_2 + E_3$$

$$E_O = -(V_R - V_{PED}) + V_R + [-(V_{PED} + V_{PULSE})]$$

$$E_O = -V_R + V_{PED} + V_R - V_{PED} - V_{PULSE}$$

$$E_O = -V_{PULSE}$$

Yale PEPR
Sum + Difference Amp.
4-21-70 WSL

Fig. 7

Yale PEPR
Dynamic Pedistal Subtractor
4-21-70 WSL

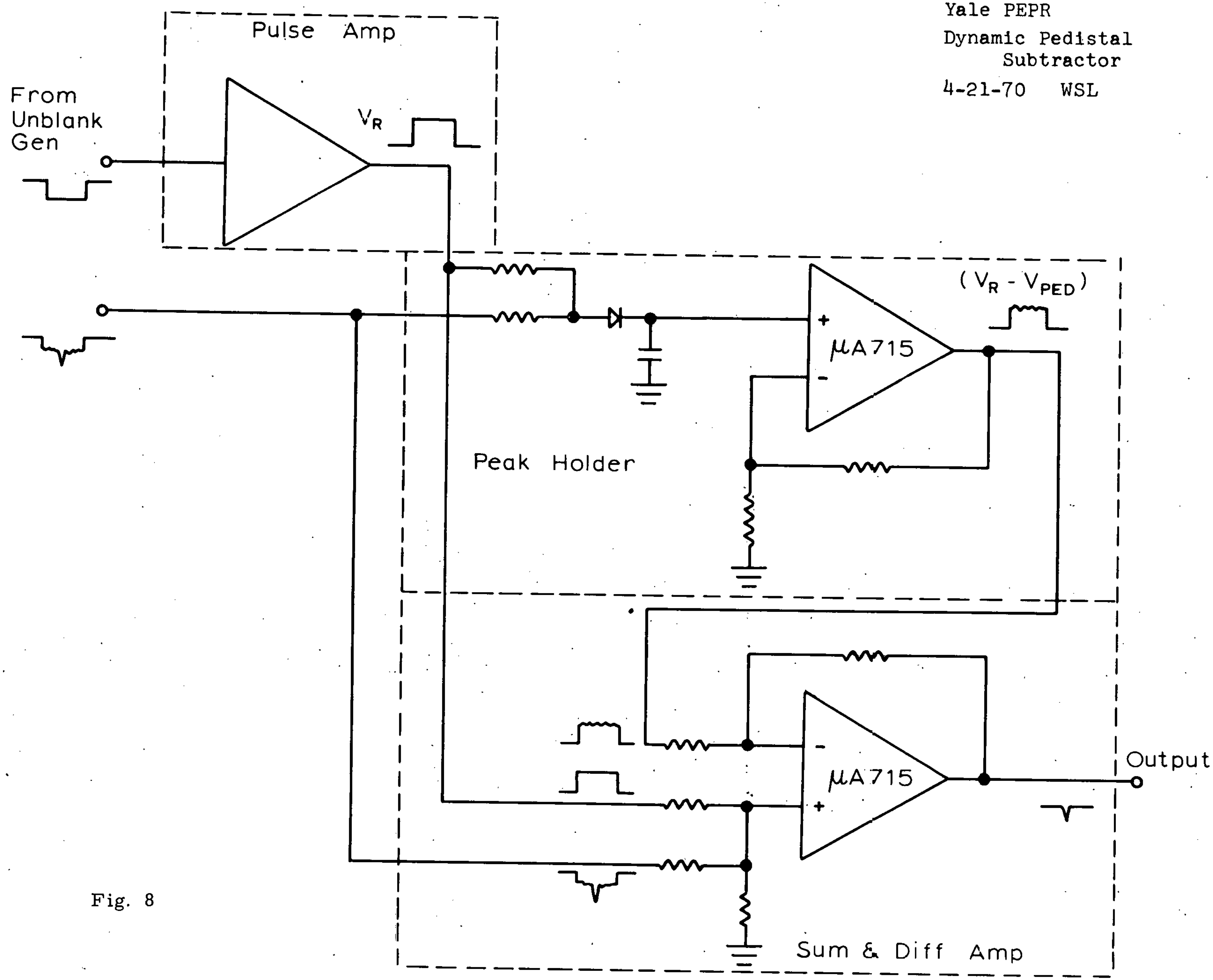


Fig. 8

References

1. D. Bogert, P. Lucas and H. Taft, ''The Status of the Yale PEPR''. Proceedings of the First International Colloquium on PEPR, University of Nijmegen, Nijmegen, The Netherlands, June 1968

2. D. Bogert, P. Lucas, H. Taft, W. Willis, D. Berley, P. Yamin, R. Kofler, G. Meissner, and S. Yamamoto, ''Measurement of the Pionic and Leptonic Decay Asymmetries from Polarized Sigma Minus with a PEPR''. to be published in Phys. Rev July 1970 Dl.

DISCUSSION

DAY: (Maryland) Let me start off with a question to Mr. Lund. I am interested in two things. One is the noise problem and the zero slope that goes with it, I mean, how do you know whether it's noise. The other is, what is the dynamic range of the pedestal remover itself.

LUND: (Yale) Those are fair questions. First of all the noise problem. The differentiator is somewhat helped by a threshold circuit which preceeds it and any signals which fall below the threshold level are simply rejected. Also, there is some frequency composition built into the differentiator circuit which tends to roll off a very high frequency noise. As for the range of the dynamic pedestal remover, again this is a mathematical number in the sense that you put down a zero pulse, but my system using the mu A 715 will go up to about 26 volts pedestal and down to approaching zero volts.

DAY: (Maryland) There any other questions? Yes, Name, rank and serial number?

HULSIZER: (MIT) I was just wondering how accurate the wire things are that you use for supplementing the PEPR data.

LUND: (Yale) 10 microns

HULSIZER: (MIT) 10 microns!

GLASSER: (Maryland) I just wanted to know what the equivalent dynamic range was in the 30" film. How much change did you find?

LUND: (Yale) I would say there was about a 10:1 variation in the 30" film, but you could do a good job with a 6:1 system. As a matter of fact, our old system was working in about the 6:1 range.

DAY: (Maryland) If there are no further questions, we'll call for the next report from Nijmegen.

NIJMEGEN PEPR (M) - Status

May 5, 1970

F.J. Crijns
B.J. Deery
J.H. Hendriks
E.A. Holdampf
M.C. Raaijmakers
D.J. Schotanus
T.E. Schouten
J.J. Timmermans
R.T. Van der Walle
T.N. Warszawer

(Presented by R.T. Van de Walle)

This report is intended to give the present status of the PEPR-project at Nijmegen. Its primary purpose is to inform the other PEPR-groups of our problems, progress and plans for the future. It contains little or no new technical information with respect to hardware or software development.

Let me start off by reminding you of the Nijmegen PEPR-set-up as many of you saw it now approximately two years ago* (See fig. 1). Apart from replacing our TNO-lens by a Leitz-lens f/1.9 (.8 magnification), this set-up has not changed. Some of you might also recall the enthusiastic remarks I made during my talk at the Nijmegen PEPR-Colloquium about the idea of using a satellite computer set-up. I would now like to say that, although we still believe in this idea, at this moment we feel like adding an important proviso; it is a good idea, provided you own <u>both</u> the large and the satellite computers.

You will recall that at the time of the first PEPR-Colloquium we had hardware functioning and some crude area scanning. It took us till September 1969 (i.e. approx. one year) before we got the very first events through the measuring system and geometry. As for some of the reasons why it has taken us so long, I would like to mention the three most important:

a. The time consumed by the very extensive hardware checking procedures. Since Nijmegen had acquired the very first ASTRODATA-hardware, we were at that time rather suspicious about the performance and were writing and running an extensive set of PDP-based hardware testing routines. In retrospect although much of the hardware experience gained in this way was useful in building our 7" C.R.T.-system, it looks as

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* Described in the Proceedings of the International Colloquium on PEPR, which was held at the University of Nijmegen on June 5-7, 1968. Copies of the Proceedings are still available upon request.

if we have been overdoing this somewhat since really nothing very serious has shown up as a result of these tests.

b. The underestimation on our side of the effort required to rewrite and make run our own set of PDP-7 based assembly language programs.

c. Acces-limitations to the main computer. This fact was imposed on us due to the lack of funds to acquire our own large computer.

The events we have been trying to put through are 2-pronged interactions initiated by 4.2 GeV/c K^--mesons in the CERN 2m.H.B.C.. The film was reverse developed.(*)

The software system used consisted of a set of basic assembly language programs (ELREC, Black-Box, etc.) located in the PDP-7 and a set of IBM 360 based FORTRAN IV-programs which take care of the track-following, fiducial location and the overall measurement - strategy. In addition the IBM 360 also stores the IPD-input and the geometry-output programs. The IBM-based programs are partly just M.I.T.-programs (Strategy, Clear-point locator, T.F.) and partly programs rewritten at Nijmegen (Fidloc, Help).

Although we plan ultimately to go to a vertex control strategy, we decided to first start with a clear-point strategy. We essentially started from the clear-point software package in use at M.I.T. around the middle of 1968.

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* The track S/N-ratio for normal developed film varied between 3 and 8; for the same film, reverse developed, the track S/N-ratio went from 7 to 13. In spite of this increase it is doubtful that we will keep using reverse developed film. For our 2m - film at least, the S/N-ratio gain for reverse developed film was largely neutralised by fluctuations in background-blackness (especially those, connected with the change of flash-region).

One of the first things we discovered as we were trying to put our first events through geometry (ie. thresh) was that our calibrations were not as good as we had previously thought. A new effort was initiated around the end of 1969, an effort which is still not yet completely finished, which at this moment gives results in pincushion residuals of ∿ 2.1 μ in the y-direction (active sweep) and ∿ 3.0 μ in the x-direction (inactive sweep).

Using these calibration constants and measuring in a 1.5 mm - sweep PE-mode (Least-count 3μ), we recently measured a couple of rolls (675 tracks). Fig. 2 shows the RMS-distribution for some 340 tracks measured during this run; the RMS-values were obtained by reprojecting the spatially reconstructed tracks onto the film plane. This distribution peaks at ∿ 8 μ. The instant measurement rate was of the order of 60 events/hr. The 20 sec./view instant measuring speed implied by that number was roughly devided as follows: ∿ 5 sec. filmtransport, ∿ 5 sec. I/O, ∿ 1 sec. fiducial location, ∿ 6 sec. track-following, ∿ 3 sec. of various computations. The track rejection rate by THRESH was ∿ 20%.

Let me conclude by briefly describing our hardware and software plans for the future.

1. Our 7" C.R.T. hardware has been tested and is ready to go. These tests have shown that the 7" Ferranti-tube is quite sensitive to the distance between front plate and focus coil. If this distance is optimized we are able to obtain a spot-size ≤ 17 μ over the complete screen. Even with this smaller spot-size the light-output of the Ferranti-tube is substantially greater than for our 5" Dumont C.R.T.

2. A new film-transport is under construction. Our present transport uses a stepping motor serving the vacuum capstan and printed circuit motors for the reels. Maximum film-transport speed is ∿ 3 m/sec. The frame-by-frame speed however, is low (approximately 3 - 5 sec. per frame). The new film-transport will be considerably faster ont the frame-by-frame transport (approximately 1 sec. per frame).

3. We have acquired a larger satellite-computer (PDP-9 - 24 K words core storage - Two 250 K words disk). Although we will again build a data-link between this PDP-9 and the IBM-computer, our hope is to be able to put the bulk of the programs in the PDP-9, so as to be able to measure as independantly as possible from the IBM 360. The link would be kept mainly for I/O reasons (and eventually for geometry on-line).

4. Switch to Vertex-Control Strategy.

Our immediate strategy-goals however, are to clean up and improve the present clear-point versions and do a small statistics experiment with the present 5" hardware.

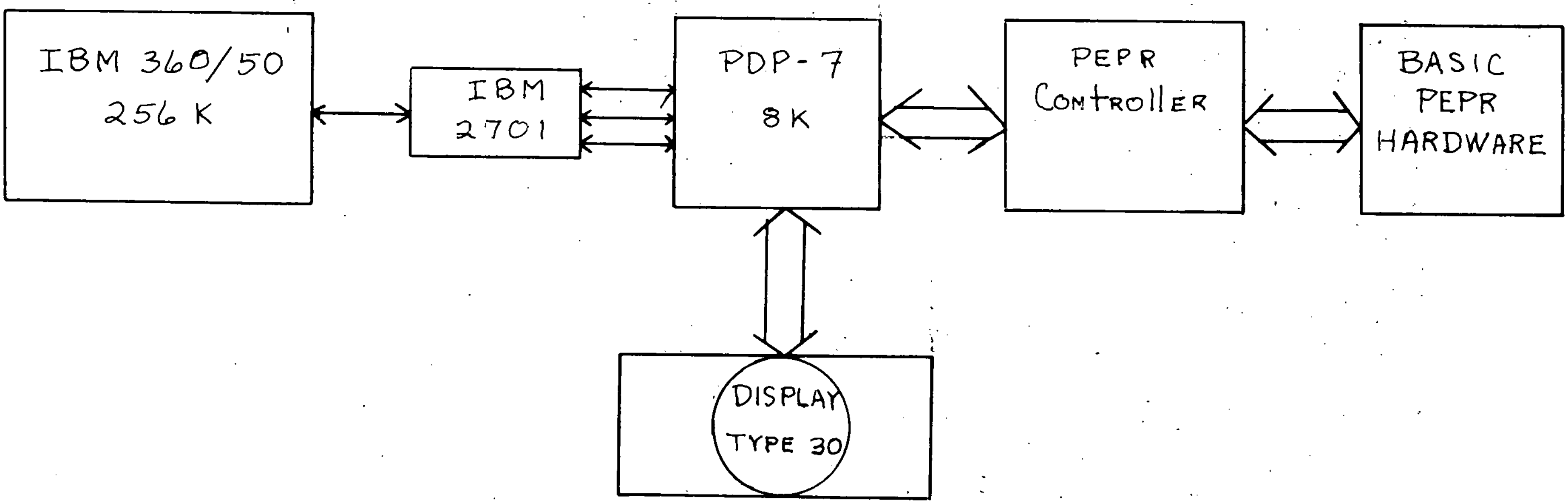
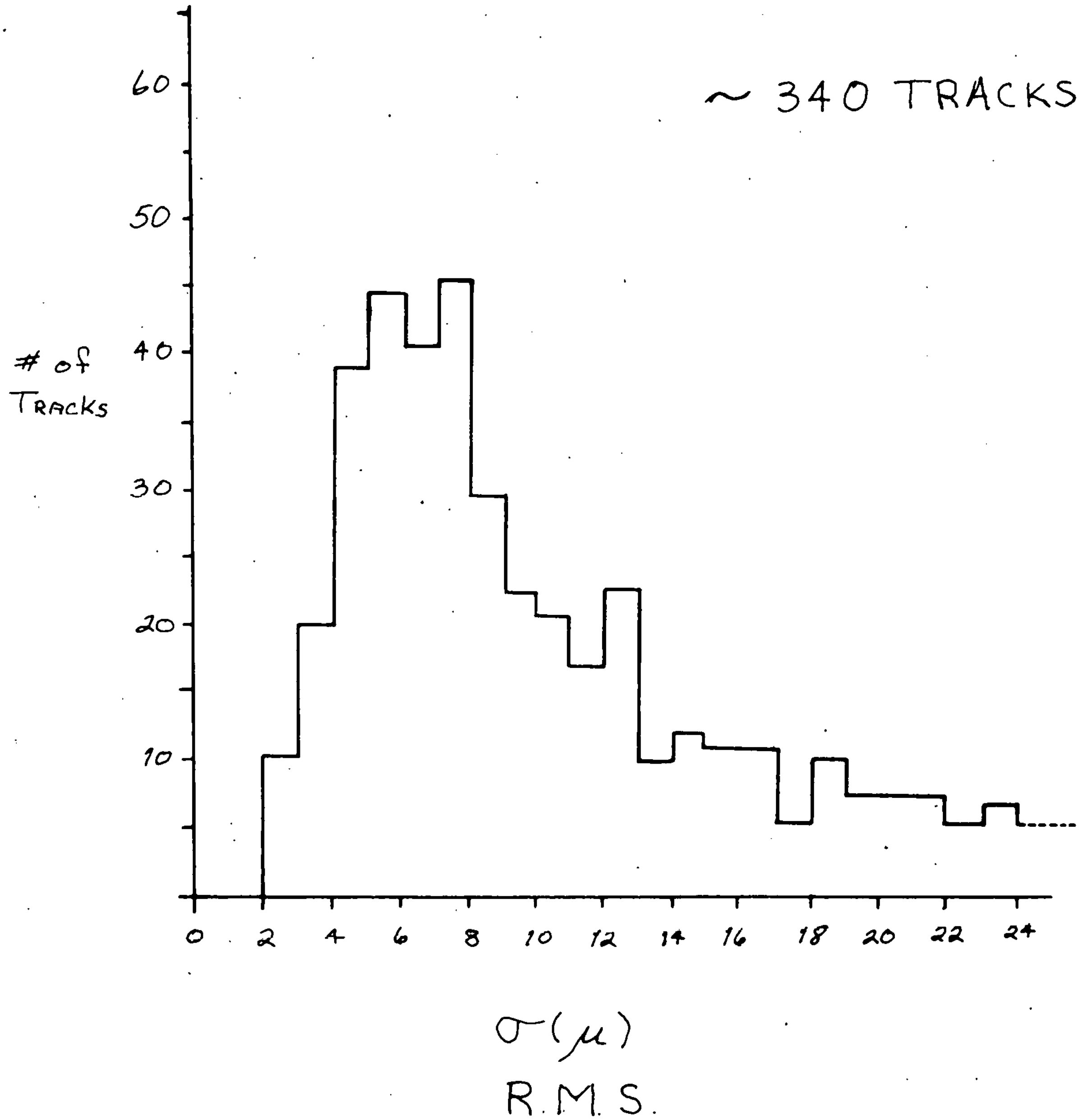

IBM 360/50
256 K
IBM
2701
PDP-7
8K
PEPR
Controller
BASIC
PEPR
HARDWARE
DISPLAY
TYPE 30

Fig. 1

FIG. 2



~ 340 TRACKS
# of TRACKS
60
50
40
30
20
10
0
2
4
6
8
10
12
14
16
18
20
22
24
σ(μ)
R.M.S.

DISCUSSION

TAFT: (Yale) Would you have any comments to make on the feasibility of operating PEPR not with a small slave computer attached to a large computer but rather with control of a small section of a large computer?

VAN de WALLE: (Nijmegen) Well, not really, but maybe I should go into some of the problems we have with the present set-up that might also be there when you use a section of a large computer. It turns out we had so many frustrations with changing systems under our feet without knowing the details and changing timing and channels, you know, little things which they usually don't tell customers, that we got a little bit frightened of using a computer which we don't have complete control of.

LUBATTI: (Washington) I must say that I agree with that. I looked at this at Seattle, the possibility of using the U.W 6400, and I think it would be absolutely disastrous for anyone to try to do PEPR by taking over part of a large computer. I think one would end up just with this problem, because the computer centers are forever changing systems and one really needs to know the system and have control over it when one is doing PEPR. I will say again, I think it would be an error if people were to move in this direction.

VAN de WALLE: (Nijmegen) At Nijmegen, there were many changes made by the IBM engineers which were noticed by no user of the computer except us, timing changes for instance.

5 May 1970 11:15 a.m.

Session II

Chairman: R. J. Plano (Rutgers)

PLANO: (Rutgers) I encourage questioning, especially on troubles, and hope the speakers will spend some time for my own personal information on staffing problems and on other problems which may help us avoid some of the troubles we are having.

Contributed paper for the Second International
Colloquium on PEPR. 5 - 7 May 1970.
M.I.T., Cambridge, Massachusetts, USA.

HEIDELBERG AUTOMATIC RECOGNITION OF BUBBLE CHAMBER EVENTS

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April 1970

Introduction

The aim of our group at Heidelberg is the automatic recognition of bubble chamber events. Figure 1 displays the hardware equipment which is at our disposal: We have an ASTRODATA PEPR-device with an f/2 lens and a one view film transport delivered by LEITZ. This is on line to a PDP-10 computer. For the mayor computing load an IBM 360/65 with 128 K 32 bit words is available.

Encouraged by the success of R. G. MARR and G. RABINOWITZ[1)] in automatic track following with the HPD we decided in 1967 to try an automatic film analysis using the PEPR device.

As in principle PEPR is fast enough to pick up all line elements on the whole picture, we wanted to avoid any roads or reduced search areas.

The PEPR device seems to be specially suited to such a purpose due to its built in data reduction by scanning with a line element. As access to calibrated coordinates in core memory is very much faster than the access to the equivalent information on film, we do not use the film as a random access memory.

Our main strategy is to extract all information from a bubble chamber picture in form of a sequence of line elements (LE's). Each line element consists of a precision measured and calibrated x and y coordinate, direction angle φ, ionization measurement, and some quality information. These line elements are uncorrelated.

From these line elements the physically interesting information is reconstructed afterwards:

For each of the three views the LE's are linked together to form tracks. From the relative position of tracks vertex candidates are derived. Then going to three-dimensional space genuine space vertices are filtered out of the vertex candidates. On the basis of the space vertices the 2 D-tracks are matched. After matching vertices and tracks the tracks are geometrically reconstructed in space, thereby resolving ambiguities in the matching procedure. Remaining ambiguities are eliminated on the basis of topological checks. All vertices and all tracks that might be interesting for further analysis are output onto magnetic tape, ready to go into the kinematics program GRIND[5] (after some selection).

When we were in the process of writing our software, we learned that the DAPR-Group at Berkeley[2] (using the HPD) were already very much advanced in the envisaged direction. We profitted a lot from their ideas and experiences and their results challenged us.

Our software consists of mainly two programs (Fig. 2):

a) The line element acquisition program ARNULF* running on the PDP-10 computer.

b) The event recognition program BERTA running on the IBM 360/65.

Acquisition of Complete Film Information by Use of a PEPR Device

A program, called ARNULF*, has been written to extract all information from bubble chamber film in form of a sequence of line elements (LE's). The picture on the film is devided into a net of scanning cells of typical size 1.5 x 4 mm^2. Within each cell all LE's are detected, precision measured, bubble measured and calibrated.

The cells are treated independently from each other. Apart from this restriction full generality is maintained in choosing the different features of the PEPR device (e. g. length of LE, speed and amplitude of sweep, threshold etc.).

The information of one LE, consisting essentially of x and y coordinate, azimuth angle, number of bubbles etc., occupies two 36-bit words. Optionally one or two words of bubble bit

* developed by Institut für Hochenergiephysik

pattern can be added. 500 - 1000 LE's per view are output onto magnetic tape. The LE's from one roll of film (1000 frames, one view only) fit onto one reel of magnetic tape. The output is analysed by the event recognition program BERTA.

Human prescanning is not required. If it is done, the scan information is transferred to the output tape and is only used for frame number selection.

General input parameters can be supplied in the format of CERN-TC "titles". The total size of the program is 20 K PDP-10 words. The speed depends on input parameters and film size and is typically 4 seconds per view on 80 cm chamber film.

BERTA: A FORTRAN Program for Automatic Recognition and Reconstruction of Bubble Chamber Events

TWO-DIMENSIONAL OPERATIONS

Combining line elements into track pieces

The data of one view are handled according to the measuring sequence in which they were written on the input tape (Fig. 4). The program starts by combining LE's (Line Elements) into track pieces: Looking at the program at a time when some LE's are already processed, there are 3 classes of processed data: TB's (Track Banks), isolated LE's, and removed LE's. The TB's are built up as LE's connected by "Pointers", i. e. to each LE belongs one pointer pointing to the next neighbouring LE (Fig. 5). TB's to be elongated are members of a left-right-list (Fig.5). "Sitting" on one TB end this list allows fast access to neighbouring TB-ends. For each TB an extrapolation circle or straight line is computed. Each new LE is tested to fit into all near by TB-ends. If it fits into one and only one free end, it is built in. If it fits into more than one end, the LE is removed. If it fits into one end, and this end is not free (i. e. a LE of the same column was built in earlier), the better fitting LE is taken, and the poorer one is removed. To decide whether the LE fits into a TB which consists only of 1 LE, a special test is performed to see if both LE's are compatible with a circle. Combinations with small angle differences are preferred.A LE that fits nowhere creates a new TB, which is at once ordered into the left-right-list.

After handling the LE's of one colum all extrapolations are recomputed, and TB's having not been elongated over a certain distance are removed from the left-right-list. The left-right-list is updated finally.

Special features are:

Extrapolation circles are normally computed though three carefully chosen points. In regions with complications a least squares fit is used.

When trying to build in the third LE into a TB the TB will be broken up if the LE fits with only one member of the TB.

Fiducial search

Within predetermined regions in the film plane TB's and isolated LE's having the known angle orientation are selected by using ordered lists of track and coordinates.

Straight lines are fitted along supposed fiducial arms. In case only one LE for a fiducial arm exists the x, y-coordinate and the known angle orientation is used to determine a straight line. The angle of orientation is corrected by a small rotation which is computed by the comparison of the connection line between the most distant predicted and reconstructed fiducial marks. Crossing points are computed and a first set of transformation coefficients is determined. The transformed fiducial positions are checked against the theoretical one. Bad fiducials if existing are removed and the transformation coefficients are recomputed.

Having good transformation coefficients the true fiducial positions on film are determined and all LE data compatible with a fiducial mark are deleted.

Linking track pieces

Starting with the longest track piece attempts are made to continue track pieces with more than 2 LE's at both ends. Only shorter track pieces are used for elongation attempts.

Extrapolation is done exclusively by fitted circles. Candidates for TB continuation are compared against each other and the better fitting one is selected. Continuation candidates with 2 or 3 LE's are broken up if only a part of them fits into the TB to be continued.

The continuation procedure at one TB end is performed as follows (Fig. 6): First a test is made to determine whether an overlap region (45^{o} or 135^{o}) of "active" and "inactive" scan may lie on the end. In the case where this happens the search starts inside the track; otherwise it starts at the track end. In steps of LE-lengths, regions on the extrapolation circle are computed. Respective angle groups are used to select, together with the x, y coordinates of the search region, candidates for continuation (Fig 6). Up to 5 LE's of the candidate are tested to lie on the extrapolation circle. If a TB candidate is built-in, a new extrapolation circle is computed.

Search for kinks

On account of large tolerances needed when forming TB's out of LE's some TB's may have undetected kinks.

A search for kinks is undertaken by testing carefully compatibility of all LE's with a circle.

Labeling beam tracks

Tracks having beam direction and beam curvature are labeled. Beam tracks running through are removed and from now on processed in a reduced form.

Ionization, data reduction and transformation to z = 0 plane

The bubble density at each track end is computed from bubble density information of respective LE's.

The number of LE's per track is reduced to a standard number, typically 12 - 16.

The coordinates of the reduced number of LE's are now transformed into a common coordinate system in the plane z = 0 (inner side of front window of chamber).

New extrapolation circles at both track ends are fitted in the plane z = 0.

These operations are not applied to beam tracks running through the whole chamber.Only 3 - 4 untransformed coordinate pairs are kept instead.

Search for vertex candidates

The search for Vertex Candidates starts after labeling those track ends, which lie within certain film boundaries.

Principle: VC's are possible intersection points of tracks complying with two conditions. The point may not lie (Fig. 7) on one of the tracks, and the distance between intersection point and end point of a track must be sufficiently small. The position of a VC which is formed by more than two tracks, is chosen as the intersection point of the two most favourable tracks.

Favourable properties are:

- large length of a track
- intersection angle close to 90^{o}
- small distance between the two track ends

All further tracks are attached to the founded VC by distance test. No intersection points are computed for the latter one.

Logical sequence: All relevant tracks are sorted according to decreasing lengths. Around each track end not lying outside certain picture edges, a rectangular zone (Figure 7) is established. A zone reference list of all other tracks which end in such a zone is created.

Starting with the longest track, selected from the lengths list, VC's at both ends of this "primary" track are searched for as intersection points with the "secondary" tracks as already catalogued in the zone list of the respective primary track end.

To find the most fabourable tracks to create a VC the tracks of the zone reference list are classified hierarchically under A, B and C (A better than B better than C):

Class A	-	secondary tracks lying very close to the end of the primary track,
Class B	-	secondary tracks not belonging to A but having an intersection angle of more than 45^{o} with the primary track,
Class C	-	the rest of the tracks.

The tracks of the classes A and C are sorted according to decreasing lengths; the tracks of class B according to decreasing intersection angles. Tracks of class B having a length below a certain limit or ending too far away from the primary track end are placed on the end of the list.

The tracks from the classified zone reference list are now tested for intersections with the primary track in hierarchical sequence. A VC is founded if 3 conditions are met.

1. One or two intersection points of secondary and primary track exist, or when the missing distance is very small.

2. The intersection point lies outside both tracks; an uncertainty of the positions of the track ends is tolerated however.

3. The intersection point lies within the zones of primary <u>and</u> secondary tracks.

Once a VC is found, the tracks of the zone of the primary track end and the tracks of the zone of the secondary track end just used to form the VC, are tested for belonging to this VC. Further tracks are attached to the founded VC if their missing distance is compatible with a certain tolerance.

Tracks that have been used as primary tracks are removed from the zone reference list of their secondary tracks, so that they are not used again as secondary tracks. If several tracks are secondary tracks of a common vertex they are also removed from their mutual zone reference lists. One track is permitted to be attached to any number of VC's. This procedure is repeated until each track has been used as primary track once.All remaining isolated tracks are marked. This method guarantees the registration of all intersection points that can be interpretated as VC's. Recognizing VC's which form a vertex or not and the final attaching of tracks to vertices is done in the matching section.

<u>Search for short vertex track candidates</u>

Within a distance of about 2 LE-lengths forming an area around each vertex candidate a search for short tracks (isolated LE's) takes place. If the angle of revealed LE coincides (within tolerance) with the angle of the connection line between LE center and VC, the LE is kept. A middle point half way along the connection line is calculated to take over the angle information form the LE's. Then point coordinates are transformed to z = 0 plane. Later, after the matching procedure, those LE's which belong to an established vertex are selected for further processing.

THREE-DIMENSIONAL OPERATIONS

The matching of vertices

The matching of a point situated in a bubble chamber is obtained by a simple geometrical construction: The 3 views taken from this point form a triangle, similar to the triangle defined by the three parallel optical axes of the cameras. The direction from one corner of the triangle to another will be called stereo direction for the corresponding pair of cameras. The triangle contracts to a point if the space point lies on the front glass of the bubble chamber and is largest when the space point lies on the back glass. This largest triangle defines a "slit" whithin which the two images of a space point must fall. The matching of space points consists of finding the stereo triangles formed by the VC's in the three views.

A list of possible pairs of VC's is first compiled for the three view combinations 1-2, 2-3 and 3-1. The deviations tolerated for the pairs of VC's are derived from the error parallelogramms determined during the VC search. A loop over the three lists of pairs selects the VC triples. The remaining VC pairs are completed to triples by estimating the position of the vertex in the failing view. We obtain in this way a list of vertex triples which are at this point considered to correspond to vertices in space. All remaining isolated fortuitous track intersections are rejected. It must be noted that several vertex triples may share commen VC's .

Extended track search

The aim of this step is to find those tracks which have not yet been attached during the 2-D operations to a saved vertex. These tracks may have ended too far from a VC. They may also have been registered as not having ended in the chamber or the VC may have lain within the track. These tracks will then be cracked into two separate ones. Neighbouring isolated LE's pointing to a saved VC will now be inserted in the track bank.

Matching of tracks

The track match operation processes simultaneously the tracks attached to one space vertex. However, if several vertex triples share common VC's all track images connected to these triples will be considered. Already matched track triples are not sent again to the matching routine; matched pairs must be reprocessed pending the possible discovery of the failing view. All combinations of track images (all pairs) for each of the three view combinations pass through a sequence of tests which develop a complete list of acceptable track pairs. A pair is immediately discarded if one of the following tests fails:

1. Test on sign of curvature.

2. Front back test: Both tracks must point at the vertex to the same side of the stereo axes.

3. Test for track length compatibility: The length of both tracks must not be too different from each other.

4. Slit-test at the end, middle and beginning of the tracks: The slits at the end, middle and beginning of one of the tracks are constructed in turn. The test consists of ascertaining whether or not the other track view intersects the slits.

5. Helix-test: The separation, defined as the distance between corresponding points of a space point on the track, varies along the track pair linearly with the arc length if they

represent the same helix in space. The prediction of the midpoint seperation is compared with the observed one.

6. Turn-over test: This test is based on the fact that the turn-over points, i. e. points on the track image where the tangent to the track image is parallel to the stereo axes, determine a corresponding point in space. Distances between the turn-over points and the common stereo axes have to be equal on both views.

Connection of space tracks to space vertices

Once the matches of the tracks attached to a VC triple or a group of related triples are known, fortuitous attachments of track images to a VC will be removed:

1. Single views of tracks are automatically detached from the VC.

2. Pair or triple matches are also detached from a VC triple if the requirement is not fulfilled that the images of a space track belong to the images (VC's) of the same space vertex.

This step cleans most of the ambiguities of related vertices, since VC triples left with less than two matched tracks (triples or pairs) will be eliminated. The remaining vertices are then reconstructed in space, using the coordinates of the VC's. The result serves as first approximation for a more elaborate vertex fitting procedure.

Matching of isolated tracks

The track views that have not been attached to a space vertex (zero prongs, recoil protons etc.) can be sent all at once to the track match routine. Some extra tests are applied in this case.

Geometrical reconstruction of tracks

A full scale geometry program has been integrated into the BERTA-system for the following reasons:

a) To give the final and most reliable decision in cases of ambiguity left over in the matching procedure.

b) To obtain results ready to go into the kinematics program GRIND. Such results are more easily read and checked than input for a geometry program. Extra tape handling can be saved and the feed-back time is shortened.

c) It was felt that using careful procedures which are free of iterations the number of tracks failing for computational reasons could be reduced as well as computing time.

d) To be able to reconstruct tracks having a large turning angle (greater than 180°).

The method employed is that of "corresponding points" applied in a similar way as in the Hamburg geometry program WELAGA.

Computation of improved vertex position

The spatial position of vertices is carefully recomputed from the pertinent space reconstructed tracks. Precise vertex coordinates are important to decide whether tracks matched in connection with the vertex are really belonging to the vertex.

Output

The results are written onto a magnetic tape called "Film Summary Tape". For each frame it contains a FRAME-bank, POINT-banks and TRACK-banks conformal to the corresponding banks used in GRIND. Unambiguous events can be sent into GRIND directly. Delicate configurations will first need some experiment-dependent topological interpretation.

Remarks

Some of the guidelines observed in writing the programs were the following:

Speed: Since we have to handle a huge amount of data, we have put special emphasis on efficiency and speed of all programs. E. g. where ever possible iterative procedures have been avoided; if possible the evaluation of functions was replaced by a table look up.

Flexibility: The entire BERTA program was coded in FORTRAN IV language in order to make it transparent and machine independent.

Comfort for the user: All experiment dependent parameters are input in the form of the CERN TC-"titles" (where possible identical title blocks have been used as in THRESH[4)] and GRIND). Comments within the FORTRAN source statements are extensively used.They are meant to be the main and exhaustive documentation of the programs. By the aid of runcards the user can request various printouts and plots of intermediate results to gain an insight into the operation of the program at critical points. An elaborate system of remark and error words has been incorporated reflecting a great number of conditions that occurred during the analysis. Statistics about the mentioned remark and error conditions and about time consumption of individual BERTA operations give a valuable survey on the tuning of the experiment.

Limitations

Though it was intended to develop a largely experiment-independent analysis program, there are a few limitations in the present version.

a) The program has been based on the very helpful presumption that the optic axes of the cameras are parallel.

b) Handling of more than three views has not been forseen.

c) Tracks shorter than about the length of the PEPR-line element (1 - 2 mm on the film) will not be detected. However, provisions have been made to introduce pre-measurements or post-measurements of these critical tracks, which are easily measured by two corresponding points.

Time and core requirements

Time checks show that one frame (3 views) will be processed within 10 - 20 seconds on a 360/65. The complete program occupies some 90 K 32 bit words of core.

Status

The first events giving good geometry results went through the program in March 1970. We estimate the time needed to make the program run for one special experiment to be a few man-months. We intend to have a production version of our programs that should be usable for a wide variety of experiments, ready by autumn 1970.

Acknowldegements

We are very much indebted to Prof. H. Filthuth for his continuous interest in our work and his personal encouragement. We greatfully acknowledge the stimulating sympathy Dr. W. Kattwinkel exhibited for our project. We should like to express our thanks to the Heidelberg PEPR-hardware group: Dr. P. Mokry, H.Ströbele, P. Krause, J. Novak and H. Becker for their excellent and untiring work.
It is a pleasure to thank M. Bergen for his valuable help in proof reading. We acknowlege the computer staff and the secretaries of the IBM Scientific Center and of the Institute for High Energy Physics for their gratifying cooperation.
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Captions of Figures

Fig. 1 Hardware used by Heidelberg Automatic Event Recognition.

Fig. 2 Organization of programs and data.

Fig. 3 Information given by ARNULF for each LE.

Fig. 4 Distribution and using sequence of scanning cells. The shown sequence also represents the sequence of processing when forming TB's out of LE's.

Fig. 5 Administration of track banks during build up procedure.

Fig. 6 Use of sorted lists when linking track pieces to tracks. Lists of integer words are built up having the value to be sorted in the left half word and the TB- or LE-address index in the right half word.

Fig. 7 Search for vertex candidates.

Fig. 8-11 View 1 of 1.6 GeV/c K P interactions in the 81 cm saclay H_2 chamber showing consecutively: film picture, line elements as they are input to BERTA, track pieces formed from these data and complete tracks.

Fig. 12 Film coordinates of a simulated bubble chamber picture, generated with an extended version of the program FAKE . The following figures were produced with a CALCOMP-plotter.

a) view 1 b) view 2 c) view 3

Fig. 13 The 3 views of Fig. 12 plotted together in a common coordinate system (G_x, G_y coordinates). Corresponding track ends are connected by stereo triangles.

Fig. 14 Input data simulation to the program BERTA from the picture Fig. 12(view 1). The line elements (LE's) are created along the tracks with gaussian distributed random errors on position and angle. A fraction of LE's is suppressed at random (mainly in the zone of vertices) another fraction is split in two. Background LE's are distributed over the whole picture.

Fig. 15 Plot of the line elements of Fig. 14 connected to tracks (view 1).

Fig. 16 Plot of the tracks from Fig. 15 after the vertex candidate search on view 1. The error parallelogramms drawn are enlarged.

Fig. 17 Simultaneous presentation of the three views of the bubble chamber corresponding to Fig. 13. The stereo triangles of matched vertices are drawn in the plot (a predicted VC-position, estimated from a VC-pair, is marked by an octogone). The circular arc running across the figure represents a far track connection to a vertex triple, detected during the extended track search.

HARDWARE USED BY HEIDELBERG AUTOMATIC EVENT RECOGNITION

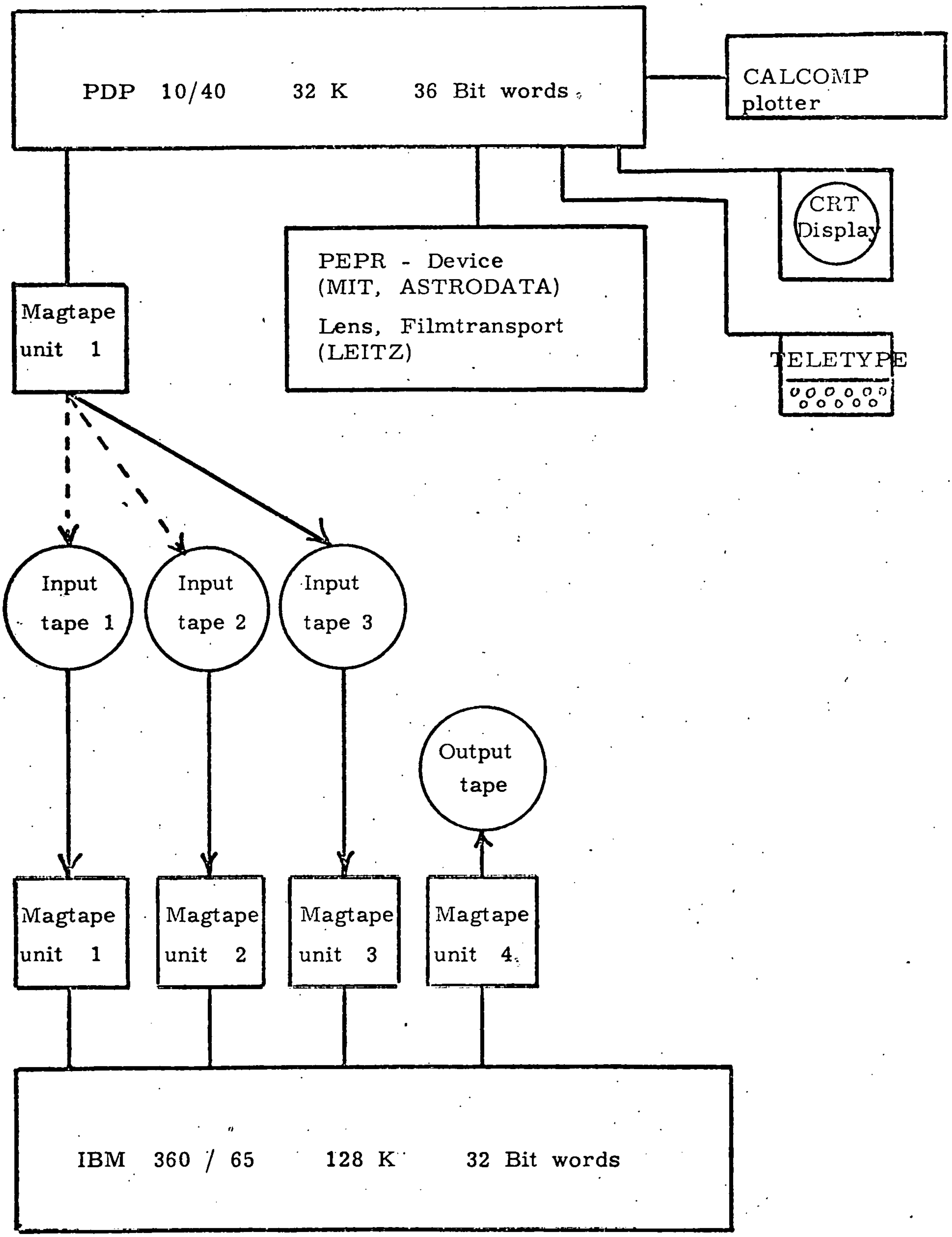


Fig 1

ORGANIZATION OF PROGRAMS AND DATA

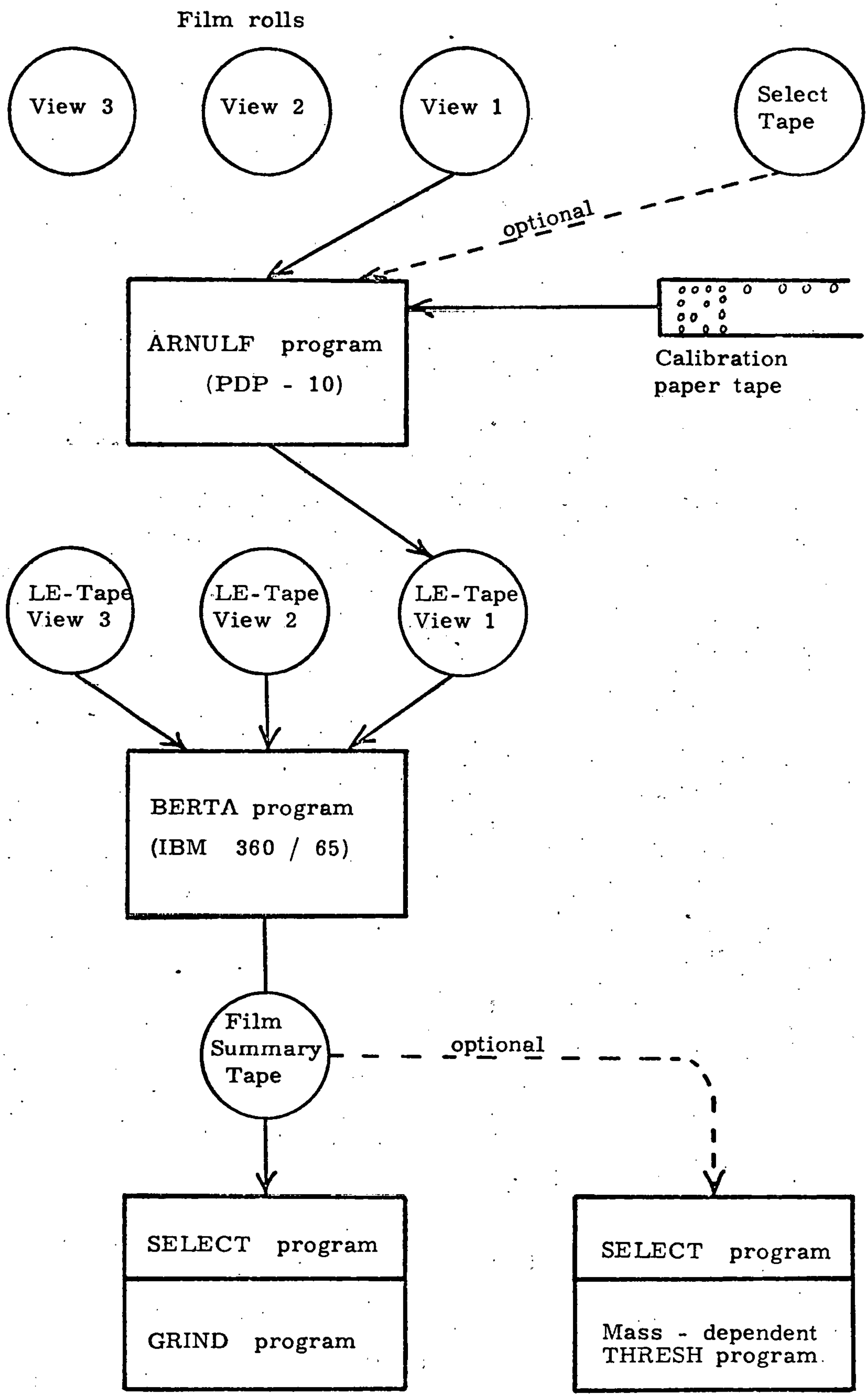


Fig 2

INFORMATION GIVEN BY ARNULF FOR EACH LINE ELEMENT

Word 1

18 bits	18 bits

X - Coordinate
averaged PE-measurement calibrated

Y - Coordinate
averaged PE-measurement calibrated

Word 2

	1	1	1	3	4	4	4	7	9 bits

critical threshold

no. of LE s in scan cell

bubble count

orientation angle in 1 degree or 1/2 deg. or 1/4 deg.

max. no. of hits at one sweep

no. of hits for this LE

flag "no PE"

flag "unmeasurable"

flag "inactive LE"

Word 3 optional

36 bits

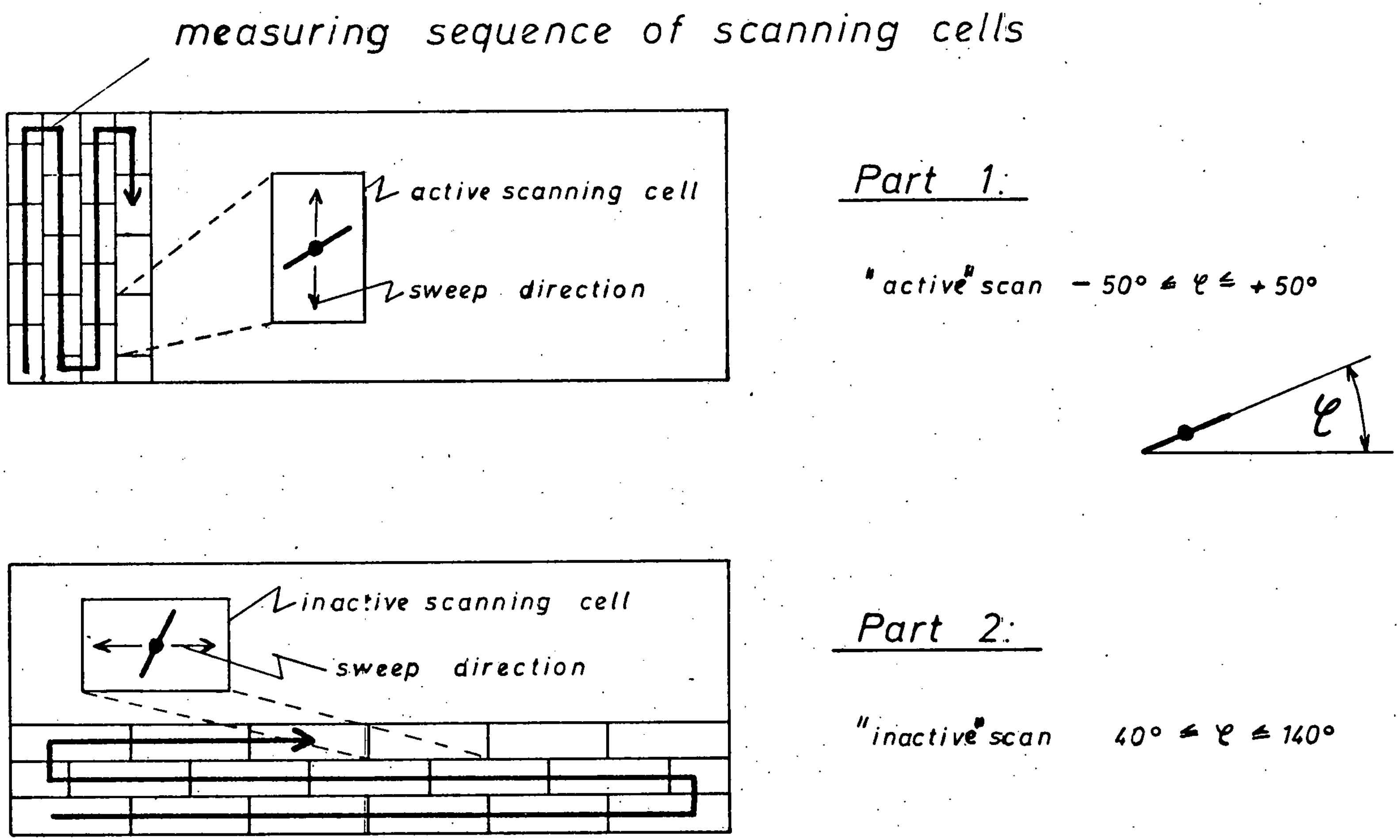
bubble bit - pattern 1

Word 4 optional

36 bits

bubble bit - pattern 2

Fig 3

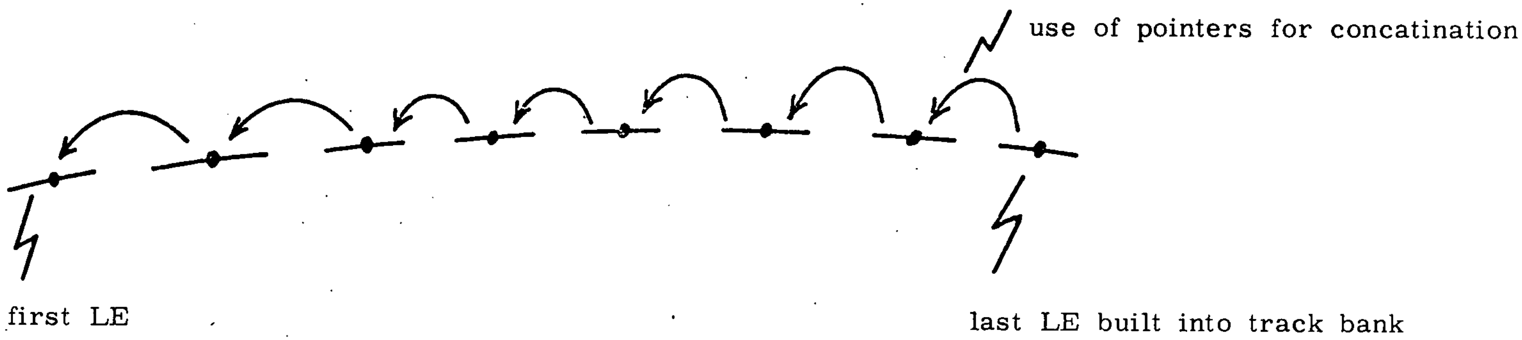


typical size of scanning cell: 1,5 × 3 mm
typical step width for LE angles: $\Delta\varphi = 1°$

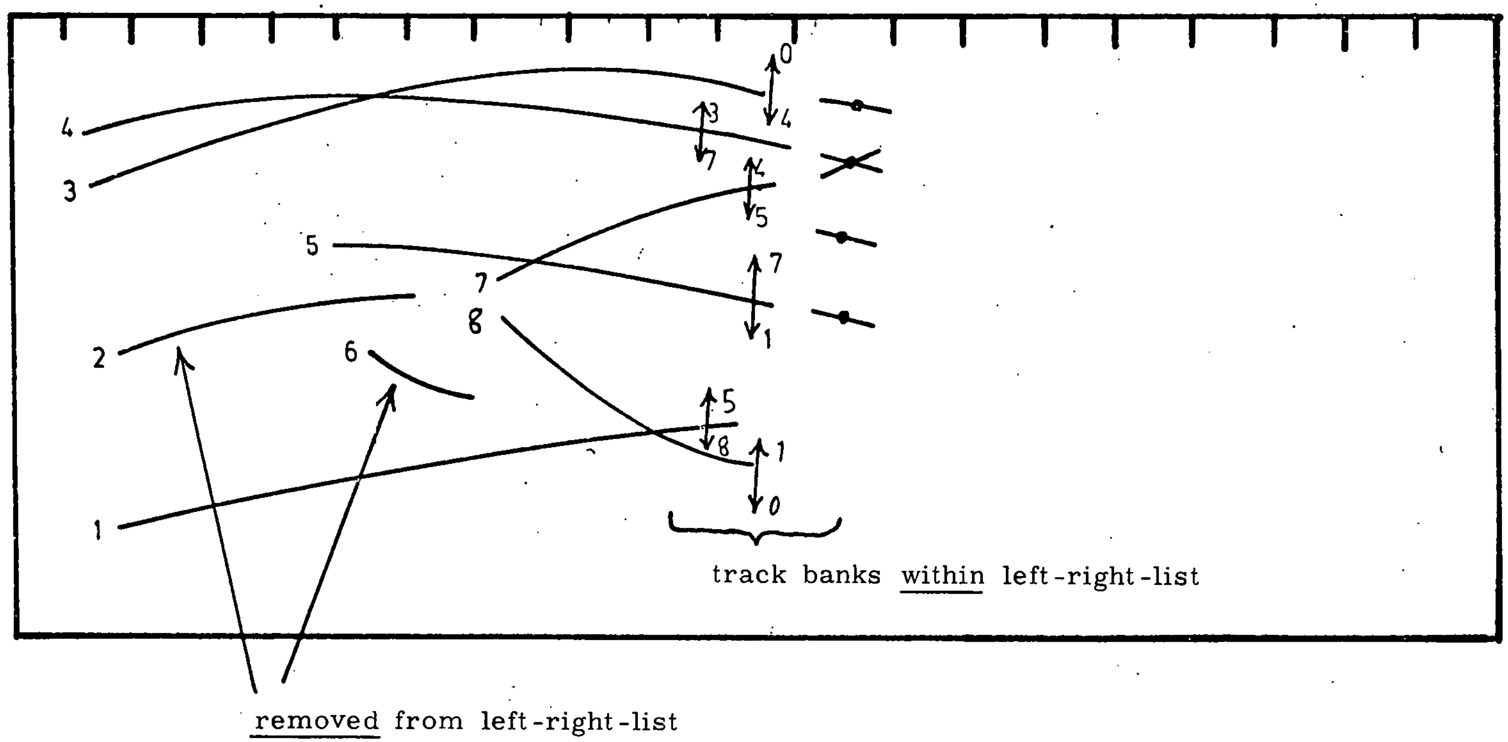
Measuring sequence for one picture

Fig 4

Connection between LE's belonging to one track bank

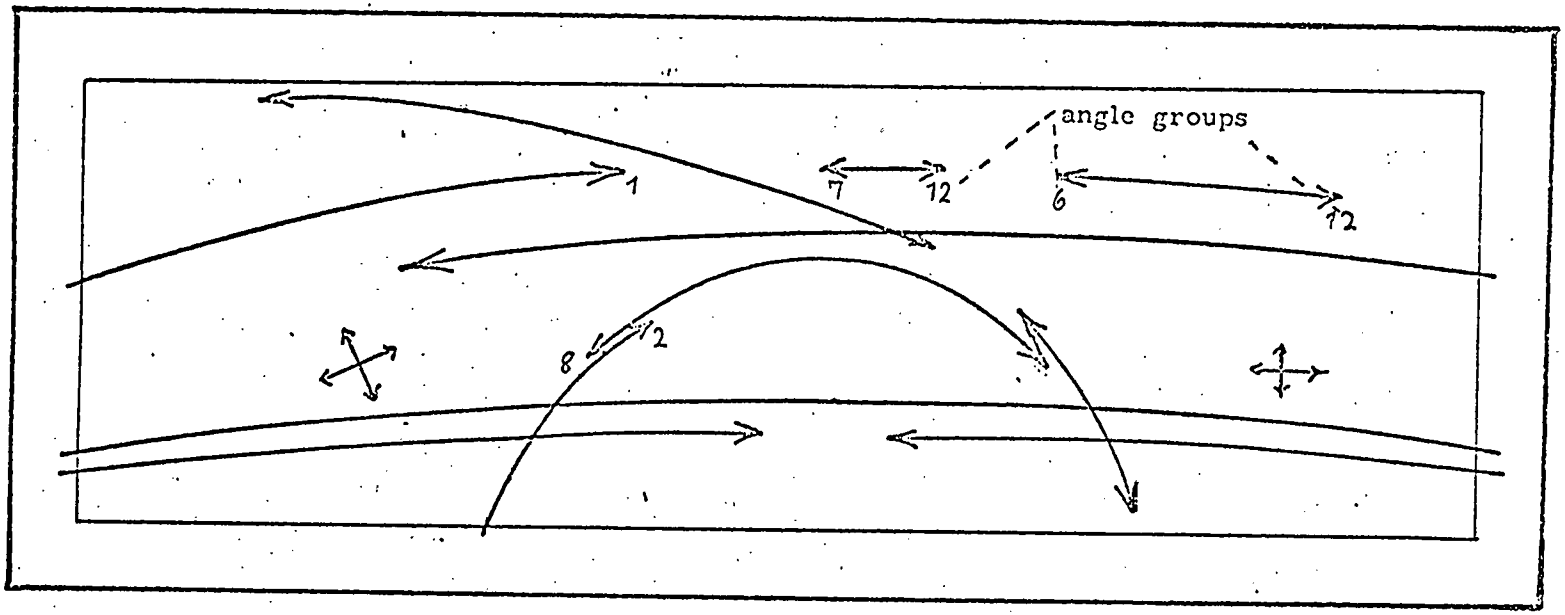


Left-right-list



Administration of track banks during build up procedure of tracks

Fig 5



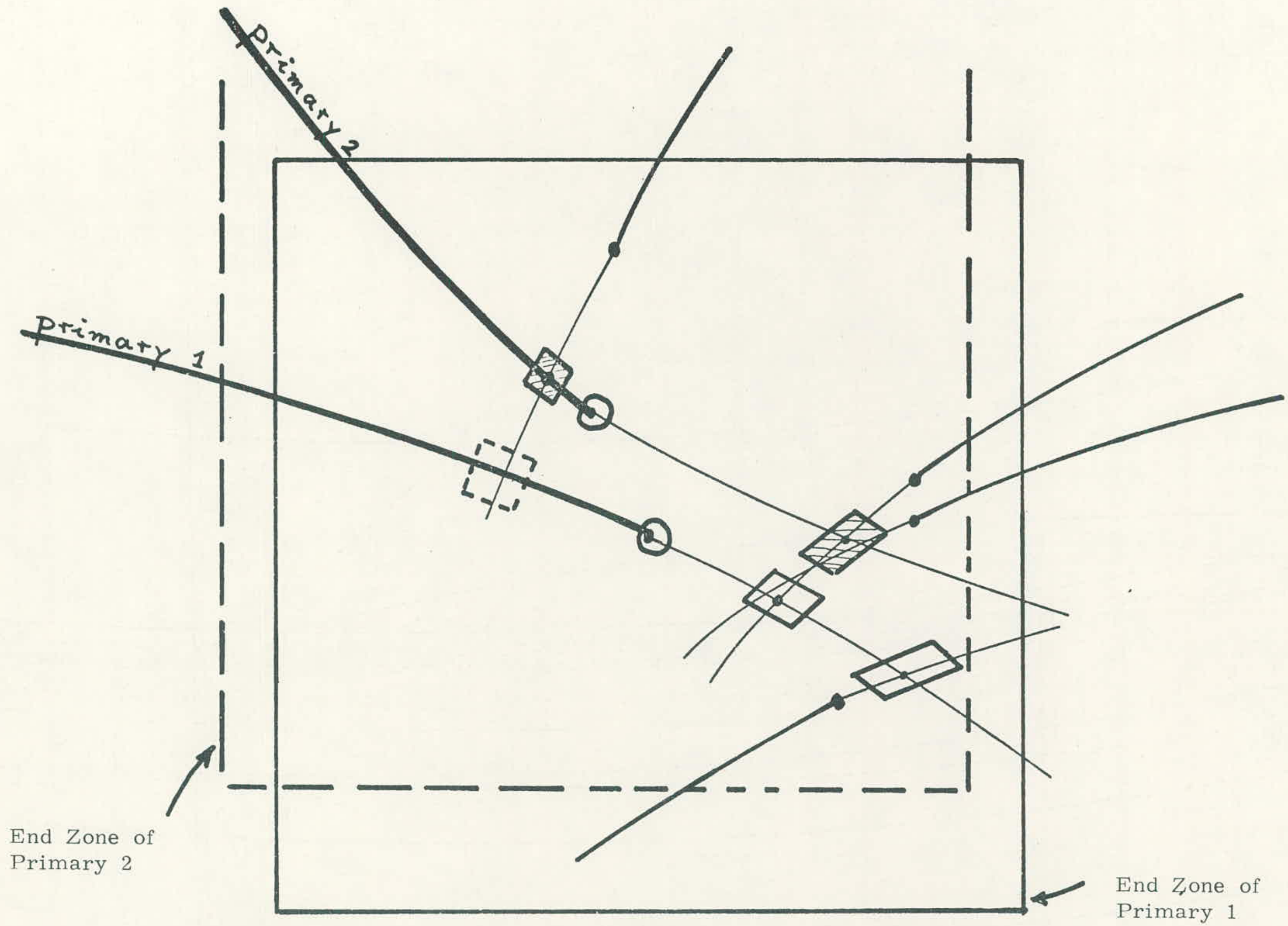
1.) a sorted list according TB lengths

2.) 12 sorted lists of TB-ends sorted according X and Y coordinates, one list for each angle group of 30 degree

Starting with the longest track piece shorter track pieces are tried to be connected with the longer one

Linking track pieces to tracks

Fig 6



- SEARCH FOR VERTEX CANDIDATES -

Fig 7

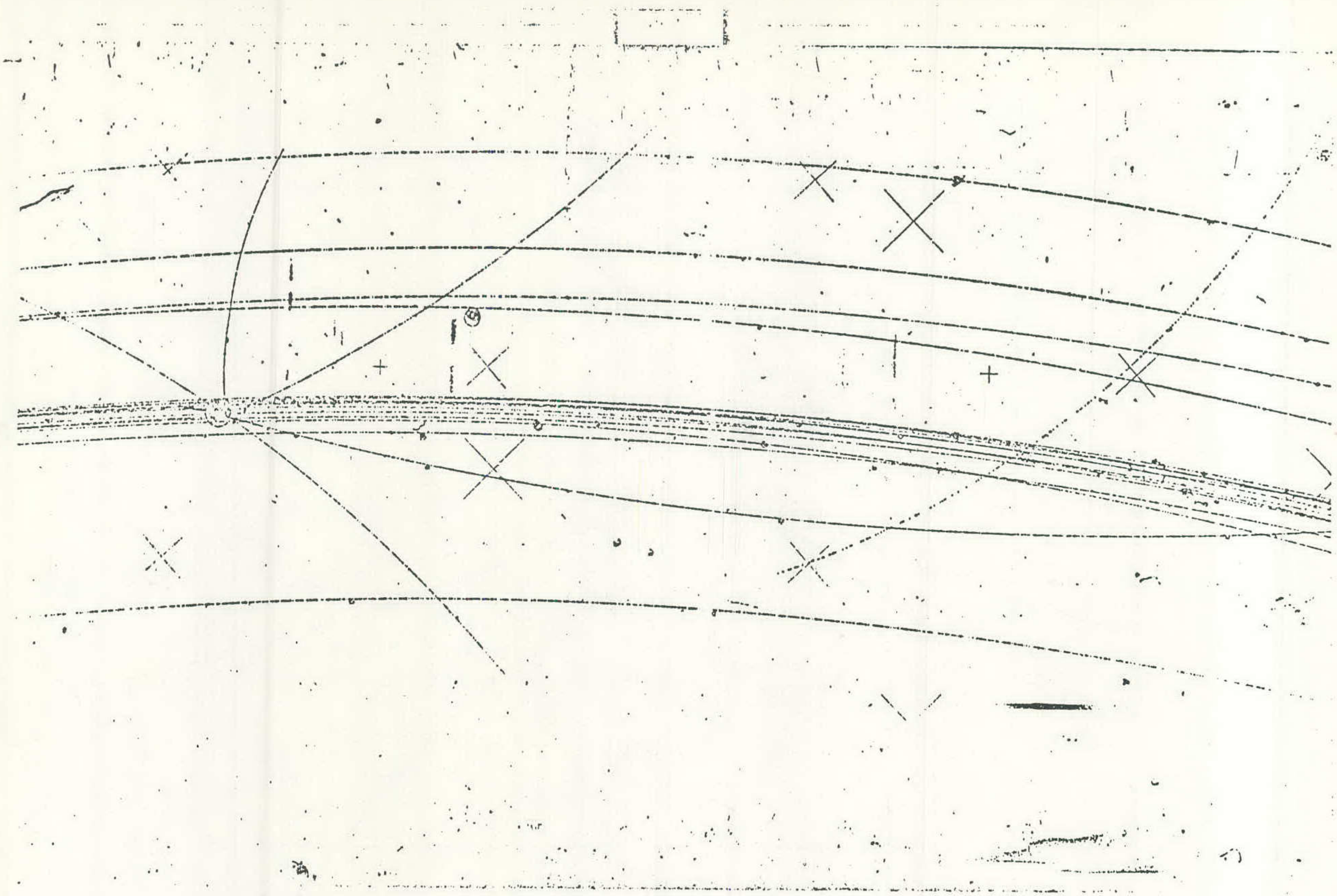


Fig 8

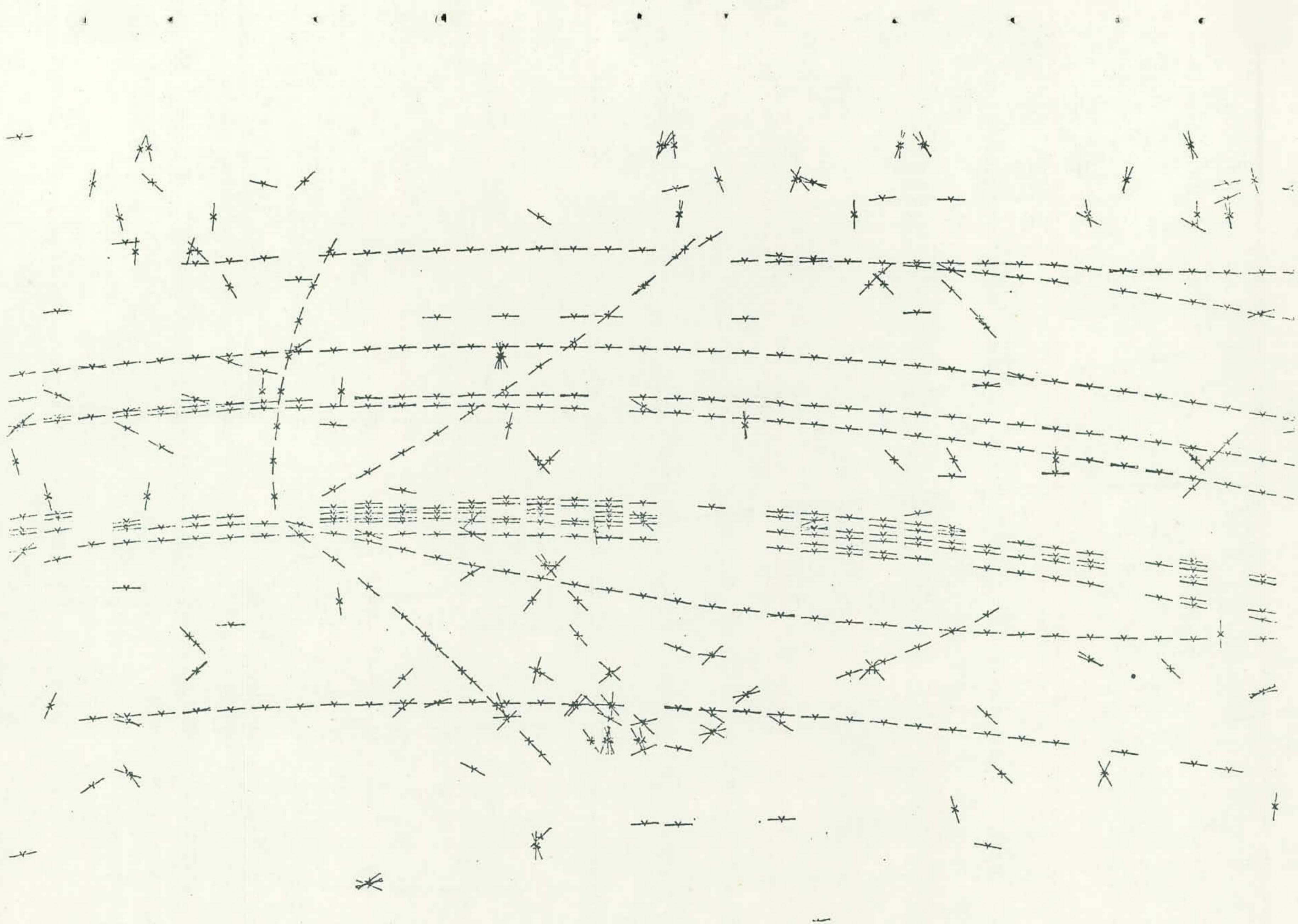


Fig 9

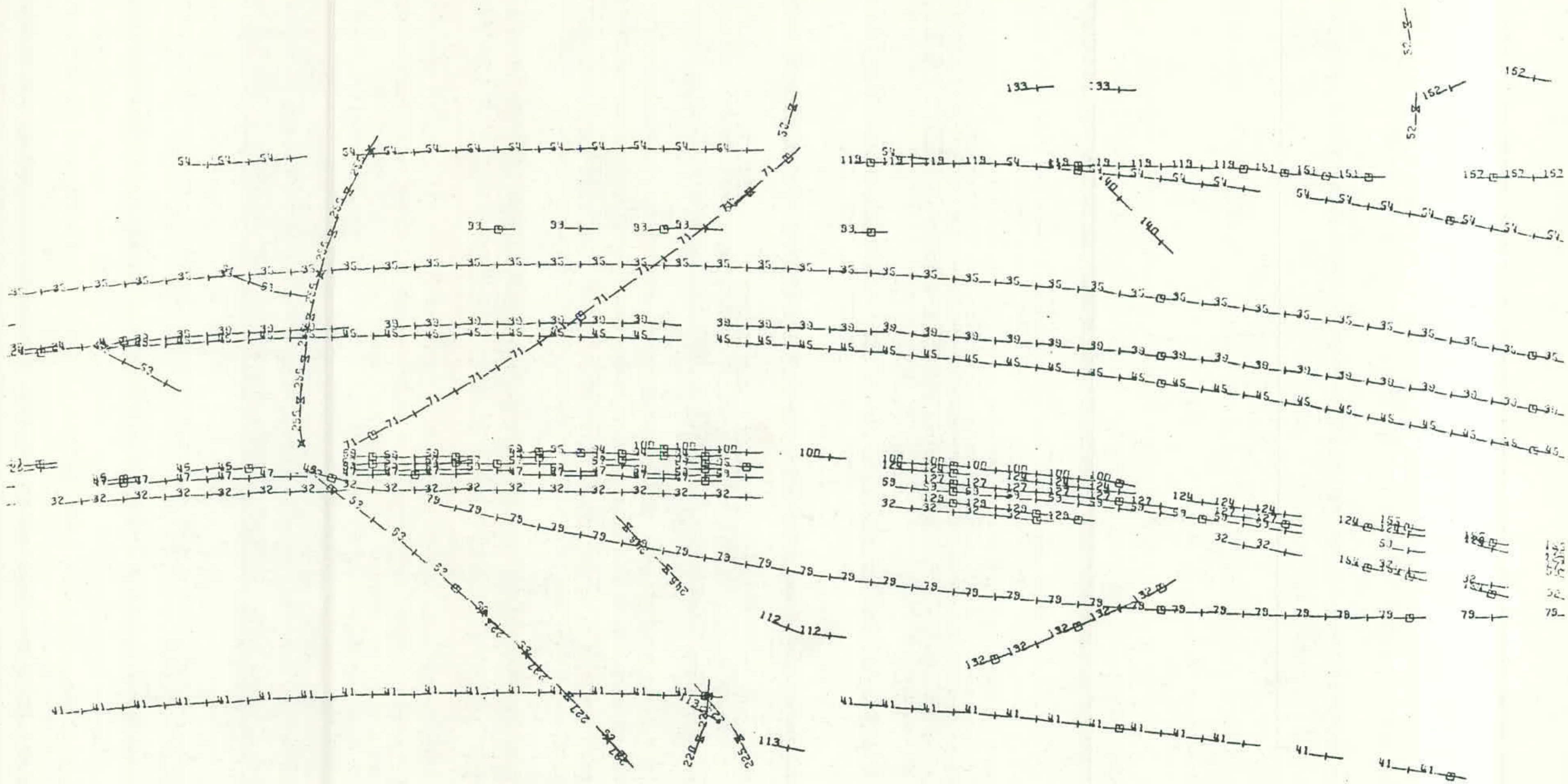


Fig 10

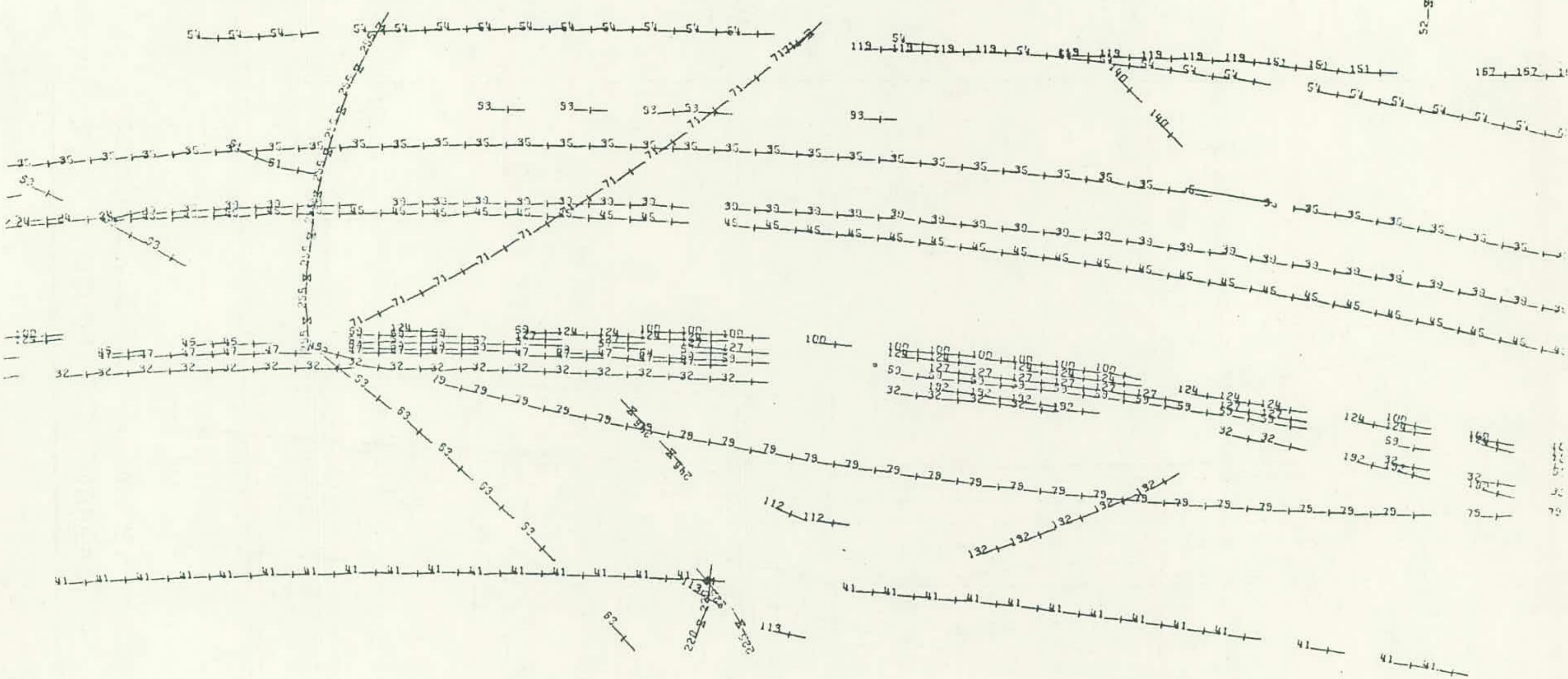


Fig 11

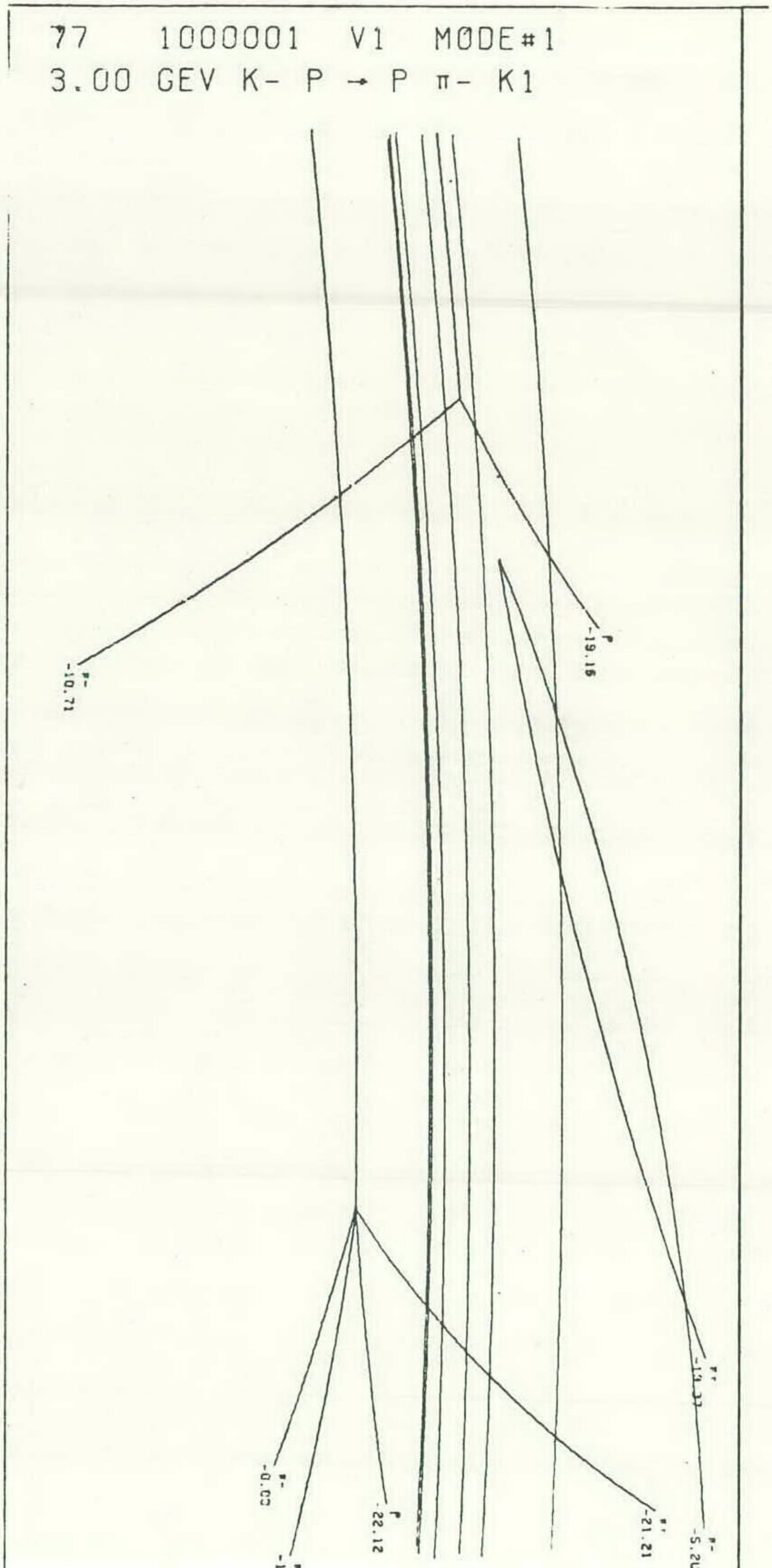


Fig 12a

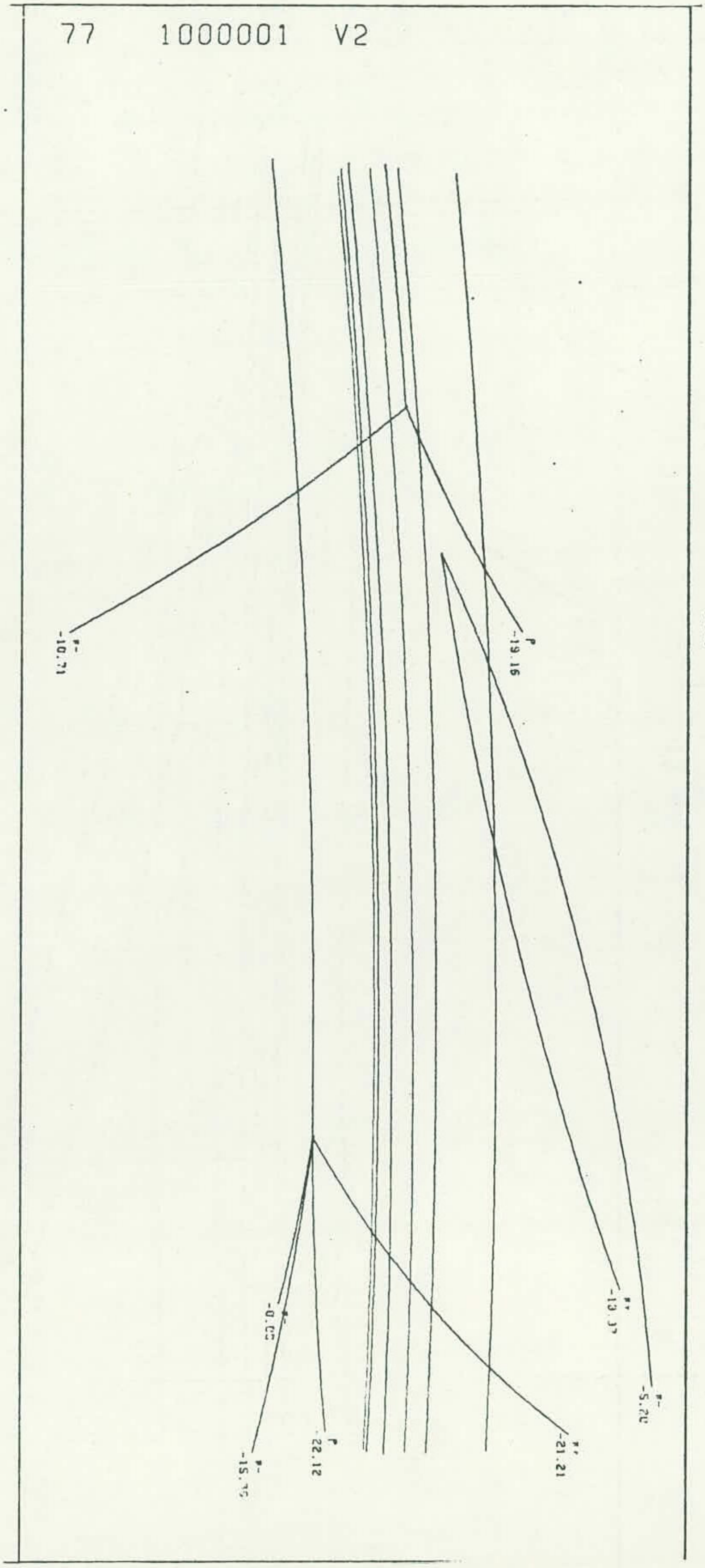


Fig 12b

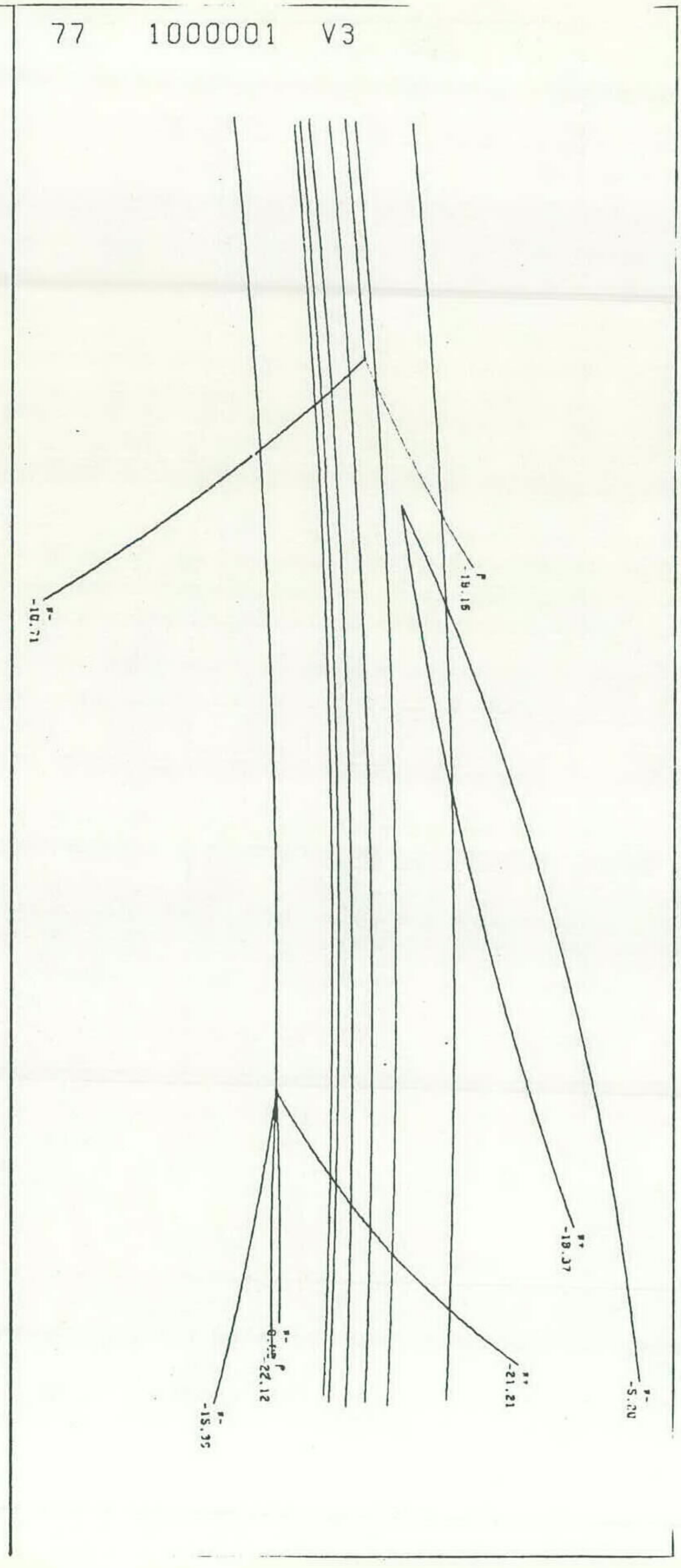


Fig 12a

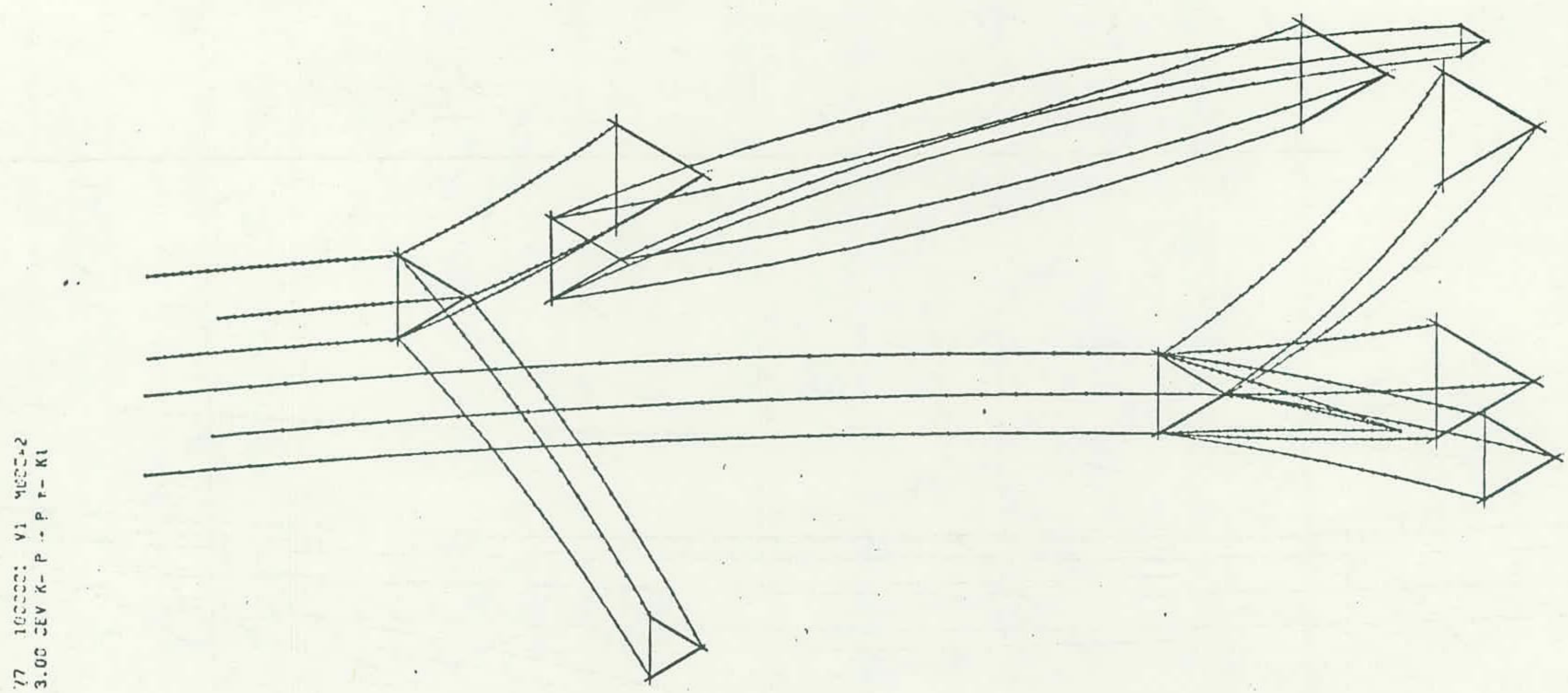


Fig 13

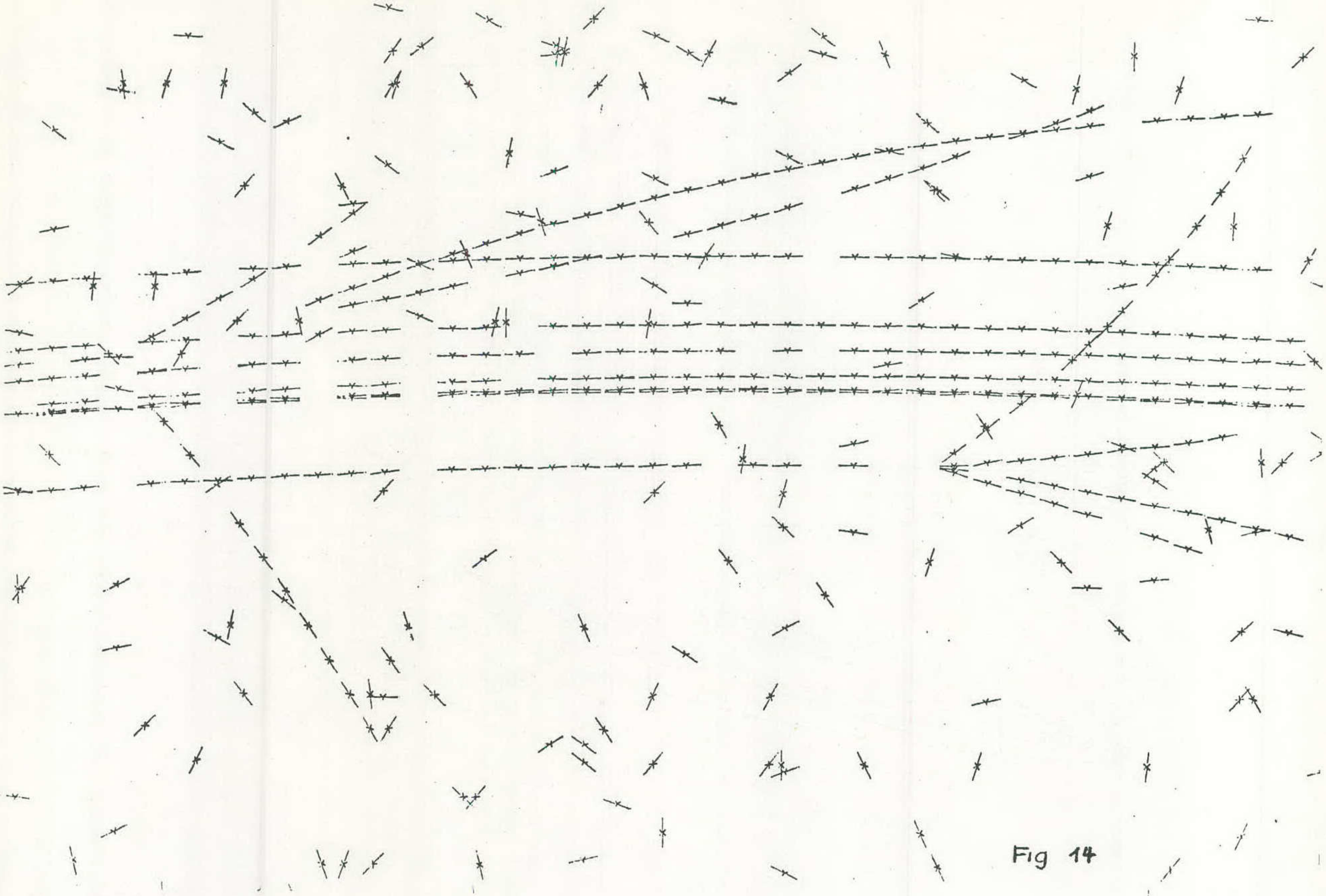


Fig 14

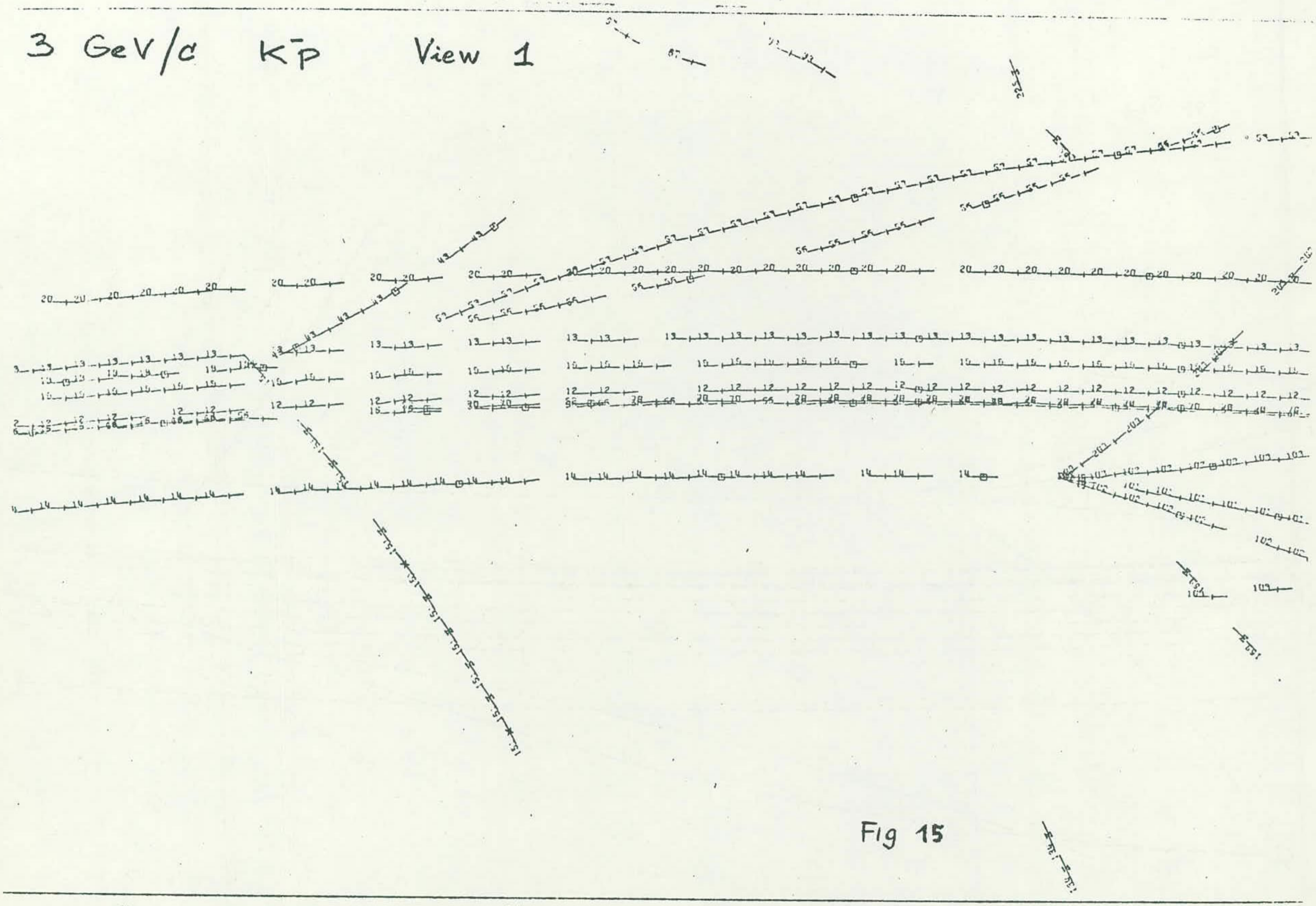


Fig 15

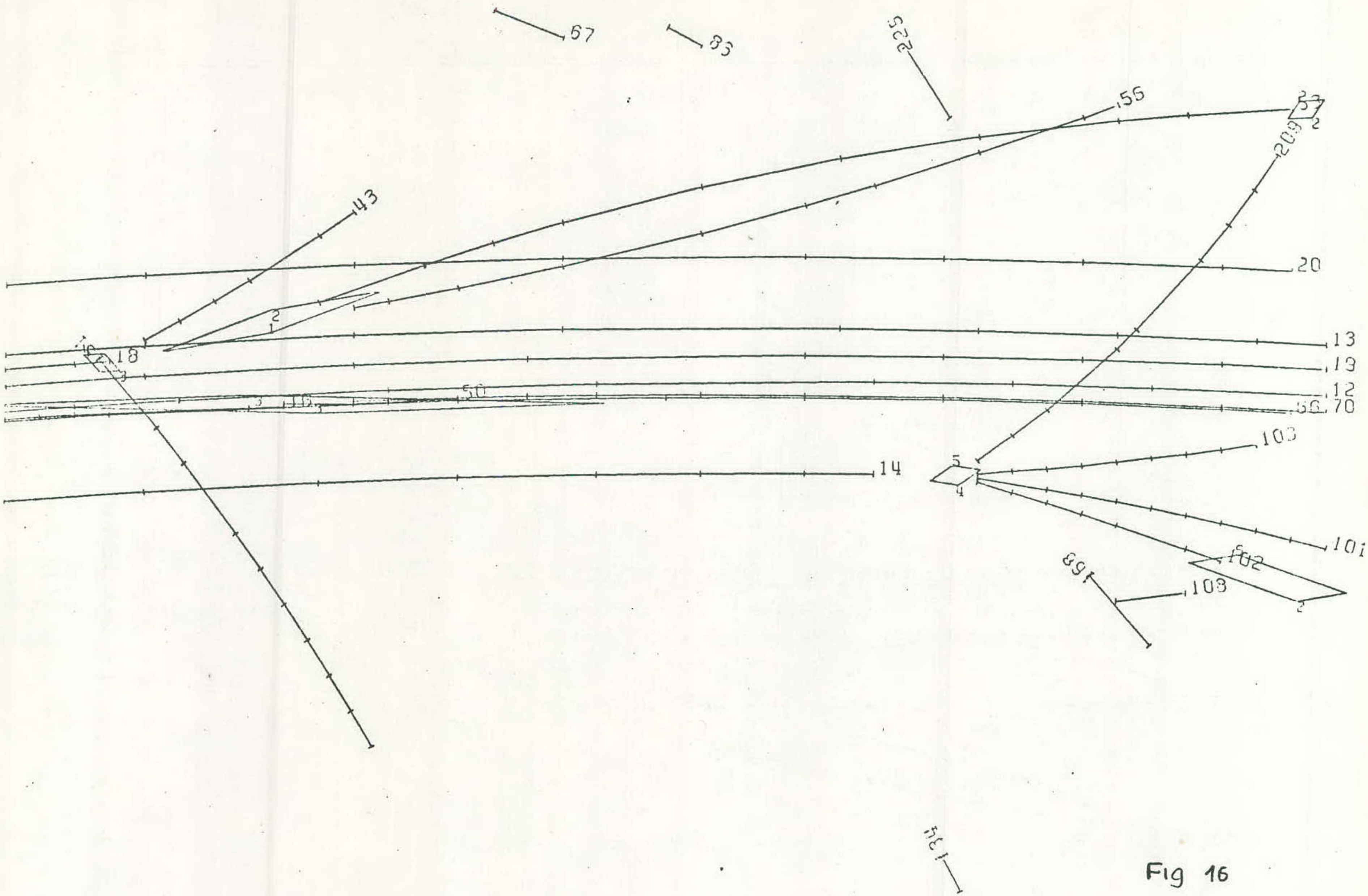


Fig 16

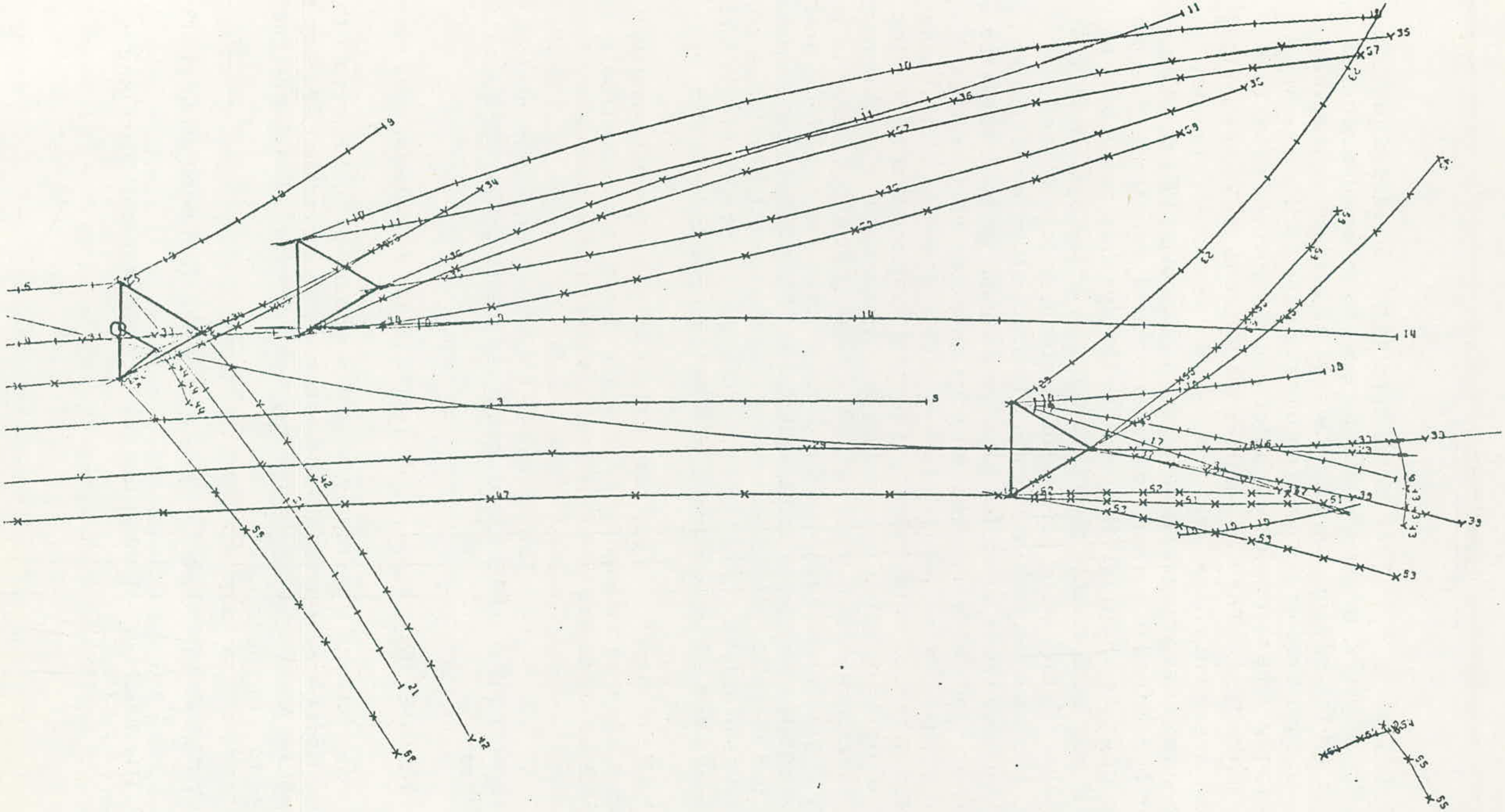


Fig 17

DISCUSSION

PLESS: (MIT) I think this approach is a classic question of whether it is better to use the film as a memory or whether it is better to get the information into the core as quickly as possible. For the second case one could then use the core as the memory since access to the core is presumed faster and more economical than access to the film. The two comments I'd like to make are on that classical argument. One, that this a one pass system, that is, whatever you have you get on the first pass through PEPR. You lose the possibility of making computer control sensitivity changes. For example, in a difficult film, where the dynamic range may be very large, it may be desirable to change the gain of the system as you track follow (in fact some successful systems in fact do that). Second, you lose the possibility of analysing ambiguous situations. For if you have these so-called gaps, you cannot go back to where the gaps are and change hardware to try to decide whether in fact there is a gap or not. The question of gain is answered, or at least discussed in the following terms. On most systems that I know the amount of time you actually spend on the film is about 5% of the total time. Therefore even if you reduce the amount of access time to your "memory" you cannot increase the overall speed very much (at most by 5%). So the question of additional speed of the 360/65 core memory against the so-called film memory (which takes about 12 microseconds to get 3 bits of information) is in a certain sense irrelevant. Would you like to make some comments to those two points?

KROPP: (Heidelberg) Yes, may I start with the second question? One idea of chosing thresholds is to preset them, since contrasts on respective views are more or less constant in fixed areas.

The access to calibrated line element data, which we want for the BERTA program, is faster by having the data in core memory.

VAN de WALLE: (Nijmegen) Do you have any results on real measurements?

KROPP: (Heidelberg) We only have 3-6 simulated FAKE events through the whole BERTA system and these are not very accurate. We are still in the testing phase and don't have any final results. Track residuals are close to 5 microns.

VAN de WALLE: (Nijmegen) Has any calibration been attempted?

KROPP: (Heidelberg) I think the calibration residuals lie close to 3 microns.

PEYROU:(CERN) How much time does one event take?

KROPP:(Heidelberg) Measuring one view with a PEPR PDP-10 combination takes about 4 seconds. This means 12 seconds for 3 views plus 10 to 20 seconds to process this data on the 360/65.

GLASSER:(Maryland) How many track elements do you put out per view?

KROPP:(Heidelberg) 500 to 1000.

OUANNES:(France) How do you get the bubble density formation level?

KROPP:(Heidelberg) Yes, after the program has recognized a line element, the element is scanned by a spot. The density is then averaged over two regions which are near the two ends of the track.

H. J. Martin
June 5, 1970

TALK AT PEPR COLLOQUIUM
May 5, 1970

There are three topics that I wish to discuss in this status report on the PEPR system installed at Indiana University. These topics are (1) a description of our system, including its history--installation date, operation status, etc.; (2) our current experimental plans and our projected timetable for processing these data on PEPR; and (3) some of the details of our software system.

Figure 1 is a block diagram of our system. The Astrodata PEPR is connected to an XDS Sigma-5 computer. The computer configuration is shown--two specific comments can be made concerning it:

(1) The 32K memory is a constraint that has influenced the design of our PEPR software. Approximately 9K of the memory is used by the XDS Batch Processing Monitor; our PEPR programs are about 20K words long and the remaining storage is reserved for de-bugging and program changes. After a software routine has been de-bugged and used for some time, we re-examine it and frequently rewrite it removing those sections of the routine that have not been useful in practice. This continuing clean-up of our software is necessary with the limited core.

(2) The I/O facilities are minimal. The RAD can be used effectively when handling relatively small amounts of data. A second magnetic tape is needed to facilitate merging and sorting of the PEPR output data.

The system was put together over a period of about nine months. The PEPR hardware was accepted in October, 1968 and the Sigma-5 installed in January, 1969. We designed and built the interface to couple the two systems. It was completed and tested by April, 1969. The film transport and optical system were also constructed at Indiana University and were installed by June, 1969.

What has been our experience during the year that we have had a complete system? The PEPR hardware is reliable and well-documented. We have not encountered any major maintenance problems. The TED package has required slight modifications and some realignment. Our major effort has been concentrated on PEPR software and the development of an operating system based on the vertex guidance concept used at Yale.

The first experiment planned for our system is a 300K-picture exposure at ANL of π^+p interactions at 6.5 GeV/c. The exposure was completed in February, 1970; there are about 10 beam tracks per picture, and we expect to measure approximately 150,000 2-, 4-, and 6-prong events during the next 12 to 15 months. We have started to process data and several thousand events are now at different stages in our system. None of the events have reached TVGP. PEPR measures about 200 views/hour and we collect information for three views. About 80% of the 2-prong events are complete (all tracks seen and measured by PEPR). The numbers for 4-prong and 6-prong events are 60% and 30% respectively. These percentages are sensitive to the current status of our programs and are particularly affected by the initial search pattern for outgoing tracks.

The final part of this report describes some of the details of our data processing system.

We scan two views of the film and collect pre-digitization information on one view. The scan information that we label as necessary includes (a) coordinates of the vertex and one fiducial, (b) a comment indicating close beam tracks, i.e., within four track thicknesses of the interacting beam track, and (c) a comment indicating a short track, either stopping or scattered (the comment differentiates between these cases). We also obtain information about the directions of the outgoing tracks and these data are used in searching for track elements.

The vertex and fiducial coordinates are measured on the scan table using T-squares. These data as well as the other scan data, are punched on paper tape. The scan and pre-digitization rate is 20 to 25 events/hour. The weekly rate is about 4,000 events.

We use vertex guidance for the PEPR measurements. The software system was influenced by the limited memory on our Sigma-5 computer, a limitation that became particularly important when we tried to include high-level filtering routines that were topology-dependent and when we tried to include operator intervention. Our current system divides the measurement and filtering process into several stages. There are four aspects of this system that I want to describe:

(1) Scan Points. A scan point defines the center of a region on film where PEPR will do an area scan. Other parameters, e.g., dimensions of the area, number of scan cells, etc., can also be specified. The line elements found in the area scan are used to initiate track following.

A pattern of scan points is established to search for track data. The pattern may cover a circular annulus around the vertex point; it may include circular arcs at different distances from the vertex; it may include several scan points along the expected beam track direction; it may include scan points along the extrapolated direction of a track segment found earlier. The operator can switch to more complex patterns of scan points when the standard search pattern is not adequate.

(2) Track Segment Processing. A simple track follower is used. When it is terminated by a congested region, then scan points are generated along the extrapolated direction and additional segments of the track may be found. All track segments that extrapolate back to the vertex are retained.

After the data for all of the tracks are collected, then the segments are examined for overall smoothness and kinked tracks (decays, scatters, and track follower errors) are separated into several segments. When several segments of the same track have been stored, then these segments are linked together to form a single track. The output at this stage in the data processing consists of track segments that extrapolate to the vertex or pass through the vertex. Nearby beam tracks are retained.

(3) Off-Line Guidance. This stage in the data processing takes place after the measurements from the three views have been merged. The program includes a set of filter routines designed to remove extraneous beam tracks and to select the tracks associated with the event. If an outgoing track is missing in one or more of the three views, then the whole event is rejected. The data for the three views

are displayed on a CRT and an operator can intervene to override a filter decision, initiate additional filtering, or manually filter the data.

(4) <u>On-Line Guidance</u>. This stage is being developed. It allows the operator to add scan points, to track follow in a manual mode, and to select scan point patterns. The operator normally makes the decision to intervene. If the program anticipates operator intervention, it delays the film advance for several seconds and signals the operator. If there is no response, then the program proceeds to the next event.

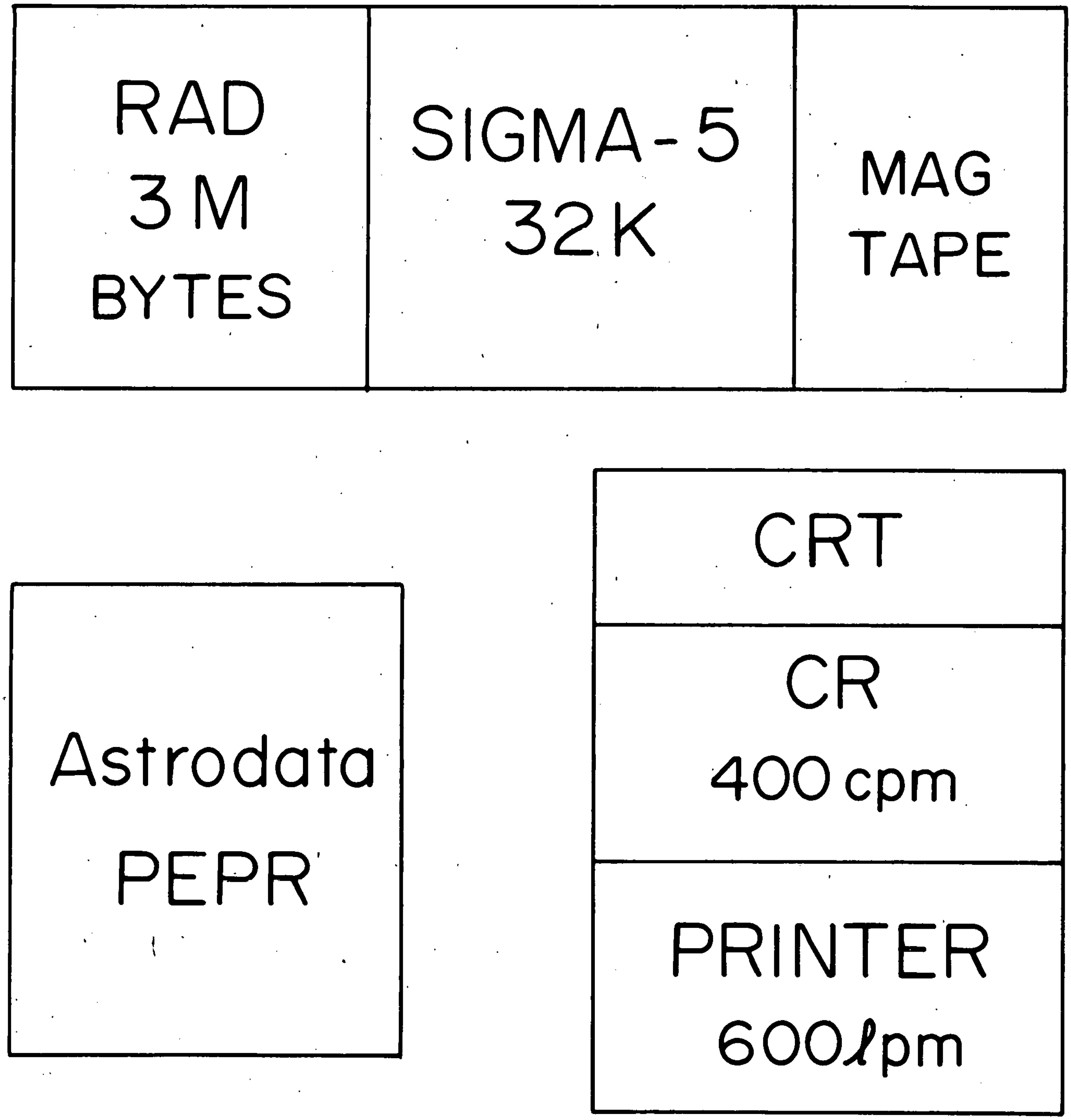

RAD
3 M
BYTES
SIGMA-5
32 K
MAG
TAPE
CRT
CR
400 cpm
PRINTER
600ℓpm
Astrodata
PEPR

Figure 1

DISCUSSION

MARTIN: (Indiana) Maybe I'll say something about staff. We have an engineer, a technician and a programmer plus 3 or 4 undergraduate students who also do programming and some graduate students. My main comment would be that we've had our apparatus for about a year during which we have worked on test events. Also the details of the programs have taken a long time. When we got an exposure at the end of February we decided that we'd have to make an effort to measure these within a certain time. However, we found that many of our developments that we thought would be of great aid in some ways blocked us. We really didn't improve our efficiency by on line guidance, so that we've taken this out for the time being. In general we found two things happening: one was that our concentration earlier had been on individual events and we hadn't really looked to see how important these things were in handling a large number of events. Secondly, we found that once we started some events in we were working a lot harder and pushed a little more.

Question from audience: What about on-line guidance?

MARTIN: (Indiana) We are not abandoning it, but we have found that it is difficult in our limited core to make a decision as to when we needed it. So we were essentially putting it in and letting the operator look at every event that came in. On two prong events there was no need for him to do that and all he had to do was push a button that said go on to the next event we found that on the events where we thought the operator would be useful that on 50-60% of these he really doesn't make a useful decision.

MULVEY: (Oxford) What experience do you have with the on-line guidance?

MARTIN: (Indiana) We had difficulties due to the limited core and found that a useful decision was not often made.

LACH: (NAL) Would a visual projection for the on-line guidance have made a difference?

MARTIN: (Indiana) It would have made a difference, that's right. The operator would have been able to make decisions much more rapidly. With our on-line guidance the operator can look at a line scan of the whole picture on the CRT display and then magnify it and move along the track or along various regions to follow what's going on. But we've found that by putting the scan points a bit further out we can do as well as the operator can.

PLESS:(MIT) I think your on-line guidance is similar to HELP at M.I.T. ?

MARTIN:(Indiana) Yes, it is HELP.

PLESS:(MIT) In our system we start out with a lot more information. The operator is called to interact only about 10% of the time. Probably due to the fact that we use clear points. Since we are using the same film as you, I feel if you put in clear points your difficulties would be fixed up. Also, it seems that the operator can't say that this is a clear point and so can't track follow.

MARTIN:(Indiana) Yes, he can track follow.

PLESS:(MIT) O.K., in other words he doesn't painfully go point by point?

MARTIN:(Indiana) No, he can go painfully point by point but usually doesn't. He usually puts in a scan point which will generate track following.

PLESS:(MIT) The thing that I don't correlate between your experience and ours, and it may be the fact that we've been in the business 5 more years, is that, in fact, the operator is required only 10% of the time. Also, in our case, when the operator is called for help, it's usually reasonably efficient and quite quick. The difference may be in the approach, and the type of our HELP programs as compared to your on-line programs. Finally I doubt it would be an enormous help to have had, at least at the M.I.T. experience, an optical image, because if we needed it, we would have put it in. One has the TV scan and line scan; and reasonable contact with the film gives no great necessity for implementing an optical image.

MARTIN:(Indiana) Let's say we're watching on the CRT and we see it's missed a track. Now the operator can stop it at that particular point, to help it or just to go on. He has that choice. We usually find that there were good reasons why it didn't work in the automatic mode and the operator is going to have to work very hard to improve it. I felt that a visual aid would help the operator to judge whether it was worthwhile to intervene.

PLANO:(Rutgers) It seems to me that 32K of 24 bit memory is a severe limitation. Could you comment on that?

MARTIN:(Indiana) We have 32K of 32 bit memory, and this is enough if the programs are written efficiently.

PLANO:(Rutgers) Are you planning to use your rapid access discs to read in subroutines as needed or is this not a limitation?

MARTIN:(Indiana) We tried doing some overlaying of and it slowed things down terribly. So we develop programs up until our limit of core and then we go through and throw out what we replaced, compress the program back down and let it expand up to the limit again.

ALLISON:(ANL) On POLLY we do have an optical projection and it might be useful in this discussion to add our views on the subject. We also have a fairly detailed display for blowing up small areas of film and for looking for small stubs and local features. We think that a TV type display or some substitute as we have is suitable and is of much use when you want to look at some narrow angle feature, where you need to look at the whole picture in order to see the separation of the track. The optical display also has some advantages in that if you can't see something on it then you can tell the operator to give up.

VAN de WALLE:(Nijmegen) Could you be more specific about the rejection rate due to the track follower's problems?

MARTIN:(Indiana) The rejection rate is primarily due to a loss of tracks which occurs when we have tracks going in the forward direction (we have a narrow angle between the tracks). What will happen is that you really won't see the tracks separate from each other until they've gone out maybe for 3 or 4 mm from the vertex. That's our problem and it's a very physics oriented problem.

Status of Maryland PEPR

T. B. Day, R. G. Glasser

First let me describe our set-up for those who may be unfamiliar with it. We have an Astrodata PEPR that is hooked to a PDP-10, the only notable feature being that the State of Maryland bought the Astrodata device. At the moment we are looking at 70 mm film in the 30 inch hydrogen chamber using a K^+ beam of about 500 MeV/c. Unfortunately we had the wrong cameras, so that we had minor problems with the number of sprocket holes but that's a minor technical point. I would like to point out two important facts. One, we're looking at 2 prongs, where the protons tracks are short. Secondly, the background is nominally black while the tracks are white. This background has given us the most trouble so far.

With our PDP-10 we have 48K of core along with a resident time-sharing or quasi time-sharing system. We don't seem to be bound too much for core, although Glasser may speak to that later, mainly due to the use of the POLLY programs, which are relatively small. We are trying to develop a system which will be nominally fully automatic. In other words we'll load film directly onto PEPR, scan across beam tracks and follow them out to the end. We will then have good vertex points and so will then search around for outgoing prongs. We're spending most of our effort developing this aspect at the moment. In the coding as a backup system we can also put the data for each view on cards (or magnetic tape also). We hope we don't have to use the cards, but now we do have a lot of scanning going on down the hall. They basically scan against the 30 inch fiducial grid and record what zone the data is in. Finally we have a great deal of help kind of programming built into this system. This allows us to magnify the film, to go one point at a time in order to watch the track follower and to do all kinds of interesting things. Right now it is used mainly for debugging.

We do not have an optical display although we do have a PDP-10 CRT display which is essentially a very fast TV scan. At the moment we perform TV scan in real time every time we want to look at anything. We have the largest sweep possible, even giving up a little bit of the linearity of the sweep for this purpose, so that at one sweep we can go 3 to 5 mm roughly and our TV scan therefore is very fast in heartbeats. At the moment the only thing I worry about is the lifetime of the tube. Now let me speak about the film's quality. On one frame that we've been hung up on for months, we can see two tracks which are almost lined up in angle, and the ratio of the height, which is in a certain sense a measure of dynamic range in the film, is almost 10 to 1.

The general background is very bad so that we had problems in the signal to noise ratio. Also we had troubles with the pedestal since as one went from one side of the film to the other the background changes brightness. Even within the sweep we had this range. In order to handle this I might just mention a few things that we have built in. First we have tried as much as possible to use the features of the Astrodata controller. This has helped a great deal, although we feel that we haven't explored all its features yet. We have, for example, in our track following found it quite useful to change the line length and sweep range as we go along. We have also regularly changed the threshold of the program due to a sort of a secular drift of the background as one moves across the frame. We also can make changes between the real time TV scan, which is a spot scan, and the line scans that are used for track following. We regularly alter the angle range, and in order to get around some of the noise problems we go back and forth between a 2 micron scan or a 10 micron scan. We have looked at some of the calibrations, but not too seriously since our attitude has been to see first of all what we can do with sort of 10 micron mode. We have measured a few tens of events, put them through geometry, and reconstructed them. These give back beam tracks which are around 500 MeV/c and that's as far as we've taken it.

Let me say one final point. Because of the dynamic range problem and also because of the wide variation in the track resolution (in the track contrast) we've cranked up our CRT beam current to the point were it worries me. The problem with this is that although we do need this high current for weak tracks, the stronger ones may saturate out the amplifiers. We had two CRT's which developed a permanent glowing feature due to secondary emission of elements down in the gun. However the current CRT seems to be holding up so far.

We have held off ordering a new transport, since the film format in this part of the country is going to 35 mm. As I mentioned we have 70mm film now, so that we hope we can get a transport which makes use of the fact that 2 x 35 is equal to 70, as a transport which can handle two simultaneous views of 35 mm film. Then, if we can ever do the physics first, we can look into a two dimensional geometry track following kind of problem.

Now in line with the general gloom, let me just mention some of our problems. Our main problem is that we consider terribly bad film, Yale said that their 80" film was bad due to the coat hanger effect, but compared to our's theirs looked beautiful. Of course we can't state what the difficulties will be when we go from 10 to 100 to 1000 to 10,000 events since there are all sorts of order of magnitude problems.

We haven't felt too badly about working on this bad film because if we can get anything out (with this film) then we'll be in good shape when we get good film. Also I emphasize that help features like looking for short protons are so naturally built into the system that we anticipate that the problem will be how to wire them out selectively.

Finally, a word about staff. We deliberately decided to understaff ourselves in order that the overall physics production of the laboratory not be harmed. The people working on the project are Bob Glasser, an engineer technician, George Harris, a graduate student, Jerry Fram, and myself.

DISCUSSION

HULSIZER:(MIT) I'd like to propose a criterion on the question of automatic scanning. If you figure that the computer costs roughly $100/hour to run, and a scanning girl plus the equipment maybe costs $10/hour, a computer has to do the automatic scanning about 10 times as fast as the girl in order to make it economically feasible. If you figure some number like 30 seconds to process an event(that may be wrong by a factor of 5 depending on whose system you're talking about), then it seems to me the automatic scanning shouldn't take more than 3 seconds, if you're going to justify the automatic scanning on economic grounds. I wonder if you could give us some estimate of how your time consumed compares with the figure of 3 seconds plus or minus whatever error factor you want to put in.

DAY:(Maryland) I deliberately avoided giving any times, since right now we are looking carefully at each event and displaying as we go. For example, when we go across beam lines we're also displaying the points one by one. I think that slows us down considerably, and as soon as I finish the comment, I'll turn it over to Bob Glasser since he can probably give you more useful information. From where I stand behind him, the time it takes to find a beam track, follow it out, and then store it is about 3 seconds. I define the above scan since this is what replaces a scanner.

GLASSER:(Maryland) I would not disagree with any of the numbers that Professor Hulsizer is quoting. I would just disagree with the criterion. My feeling is that justification for automatic scanning cannot lie in its economic feasibility, but what kind of information you get back. We all have the problems of looking at variable efficiency in the scanners, and I won't try to say it will ever be more economical to scan automatically than to multi-scan five times. But at least you have the hope that you can make the criterion you are using understandable and therefore quantifiable, so you can use that to get your physics more accurate. That's the only justification I can give for going to that mode.

DAY:(Maryland) How much time does the display cost us?

GLASSER:(Maryland) Anywhere between 90 - 98% of our time.

DAY:(Maryland) Thus it is the display which is killing us since it is in the seconds category.

ALLISON:(ANL) We have been, for the past year processing an experiment in an automatic scan mode using basically the same idea as at Maryland, and therefore I feel that we should be answering this question. Basically we find that small variations in film quality, which hangs over you all the time, completely outweigh whether the film is pre-scanned or not. Thus with 10% variation in film quality a few bad rolls which are being pre-scanned will go more slowly than the same number of rolls which are slightly better and are automatically scanned. It is not obvious, as I'll show you tomorrow that looking at the various experiments that we've processed on POLLY, you can tell which of them are automatically scanned and which are pre-scanned. I'd also like to add that one or two other things. Automatic scanning gets you more physics in the sense that you get cross sections and beam track lengths. You also reduce the amount of data handling in that you don't have to invest any time and money involved in converting scan cards to tape. Since scanning data is produced in real time and one measures straight from it, there are no problems due to different magnification or other day to day problems associated with handling pre-scanned events. We found it better than we expected.

DAY: (Maryland) Excuse me, can I introduce a comment? Since economy has been introduced let me just briefly point out that times have changed and the situation now is not to try to compete with the human operation but how to live with it as human operation is subject to attrician.

BASTIEN: (Washington) Did you say that it costs $100/hour for PDP-6 time.

HULSIZER: (MIT) I was just using a order of magnitude figure for what it would cost to buy and run a computer.

LUBATTI: (Washington) I would like to comment on Bob Hulsizer's suggestion that full pattern recognition may be uneconomical. I tend to agree that one should make use of human intervention, since a complete algorithm is quite difficult without additional help. Of course if one has counters in the chamber then these do a great portion of the operator's job.

HULSIZER: (MIT) I raised the economic consideration partly because it's a number and numbers are easy to think about. But it seems to me the assumption that you immediately return with when you counter the economic argument is that automatic scanning is more objective. However, I don't think we have any evidence, considering the general nature of bubble chamber film, that an automatic scanning device is going to be anymore reliable and quantifiable than a human scanner. In fact, most the troubles that people have, lie with the noise level in automatic following devices. This is going to give the most unpredictable and unquantifiable trouble in the scanning process. So I will now add the scanning quality argument to the economic one.

ALLISON: (ANL) I like to throw on some more facts here, when one is automatically scanning down beam track. As you go, you can build into the program scanning criteria which may involve rejection of 10% of the beam tracks in an unbiased way. In fact, our experience is, that automatic scanning is looking down beam tracks and I wouldn't say the same thing of looking for vee's. That's another problem though that would be interesting to try. In a sense of looking down beam tracks, automatic scanning is much easier than we expected and is certainly much, easier than measuring events once you found them, however you found them. It produces data with a well defined reliability for a negligible increase of processing time (≤ 25%).

CAW: (Johns Hopkins) Tom I'd like to know what your beam current is and what you're scared of.

DAY: (Maryland) Well, it corresponds to about 1 1/2 micro amps D. C. Let me just make a comment back on this other point, we have never gone into the prior rough measurement situation, not even of the vertex. Our alternative to finding the beam tracks and following them is to use a scanned zone so that, of course, is a very fast operation too.

PLANO: (Rutgers) Any other comments?

VAN de WALLE: (Nijmegen) Concerning the comment about zoning and vertex control, I think it takes about as much time to get the zone correctly as it takes to get the correct vertex. Secondly, I can possibly see another economic reason for doing full automatic scanning in that one sometimes has to cope with stupid but real walls between budgets for equipment (computers) and budget for salaries.

PLANO: (Rutgers) I'd like to add one comment. As computers get cheaper and people get more expensive the above argument is bound to change. Any other comments?

GLASSER: (Maryland) I'd just like to say that the argument on scanning certainly is at the moment (given Plano's warning) true, but I think that's probably not the right denominator. The right denominator to use is probably the time you spend measuring. The automatic scanning time if it's a small fraction of the measuring time, is still worthwhile. In other words, you may always be talking, on some kinds of experiments, about a negligible addition in cost no matter how you do your scanning, and I call 50% negligible.

5 May 1970 2:00 p.m.

Session III

Chairman: H. J. Martin (Indiana)

The Johns Hopkins PEPR System

Carl Chatzky

1.1 COMPUTER CONFIGURATION

We have a Sigma-7 computer and an Astrodata PEPR. The computer has a fixed point add time of 1.4 microseconds, 48,000 words of 32 bit 850 nanosecond memory; a 3 million byte fixed head per track (512) disc with an average rotational delay of 18 milliseconds and transfer rate of 140 kilobits per second; two 9 track tape drives with 800 BPI packing density and 90 inch/sec. tape speed; a seven track tape of similar characteristics; a 450 card/second reader; and a 450 LPM line printer.

1.2 DISPLAY SCOPE

All the display scopes I have seen on other PEPR systems have required CPU attention for each point displayed. This is unsatisfactory for two reasons: first, it competes with the execution of real calculation; and secondly, the refresh rate is held down and consequently the number of points which may be displayed without annoying flicker.

We have a display scope built by our group with ITT and XDS components. It has a built in channel which enables it to pick up x,y coordinate pairs stored in memory and display them without interference to the CPU. Since the computer memory is segmented into "doors", there is in general no conflict over memory accesses. The buffer in memory for holding the x,y pairs is not allocated until execution of a given run, at which time all space which is not used for anything else is used. About 2,000 points can be displayed without flicker.

We routinely display a T.V. scan or enlarged T.V. scan of the vertex region with the current sweep cell center indicated by a brightened spot. The N/Phi plot can be simultaneously displayed. The various banks can be displayed and superimposed on the T.V. scan, especially the output bank. Of course, the T.V. scan is not done in production. We feel very strongly that a good display can be a great help in software development and maintainance of machine performance.

1.3 FILM TRANSPORT

We have an extremely general high-powered film transport which can handle 35, 46, or 70 millimeter film sprocketed or sprocketless.

The system has a film acceleration of 1400 in/sec^2 (3.65g) to a maximum speed of 300 in/sec. (25 ft./sec). However, we currently limit the top speed to about 12 ft/sec.

Film position is controlled by the computer through a 24 bit 2's complement bidirectional counter. Once this register is loaded, film motion is initiated, rotating an incremental shaft encoder until the count is reduced to zero. One count corresponds to .56 mm of film motion. The computer, of course, is free to do other calculations during this time.

We do all calculations of film position on a dead reckoning system. The results of a film motion are confirmed by the measured locations of the chamber fiducial marks.

The film Brenner marks are not used.

Since the spacing of frames on our 46 mm sprocketless film is non-uniform because of slippage in the camera, the program must "learn" and continually adjust to this variation.

Positioning of the film to a close tolerance is necessary since our 5" CRT can see only 2/3 of a frame and we do not want to move the film during the measurement of one view.

Film is held in the film gate with both mechanical and vacuum hold down.

1.4 LENS AND SPOT SIZE

We have a 1.5 to 1, F2.8 lens manufactured by Applied Optics and Mechanics Co., which we use in the demagnifying mode. The demagnification of the spot and the aberration of the lens just about cancel out, so we have about a 25 to 30 micron spot at the film plane.

1.5 PEPR - SIGMA INTERFACE

Our Sigma-PEPR Interface was built by our group from standard SDS logic hardware, and mounted within the PEPR controller Bay 5. Each communication with the controller is handled via a single CPU cycle. The data path is 32 bits wide. Fortunately, no 36 bit PEPR register has more than 32 bits of significant information. The rearrangement of the 32 bits into the appropriate 36 bit format is handled by the interface.

1.6.1 MARGINALITY OF PR (10 micron) MODE

In regard to the Astrodata setup, we feel that, especially for our film on which the tracks are about 25 microns wide, the PEPR hardware is very marginal in PR (10 micron) mode. We have two reasons for saying this. First, there is the behavior of the modulation function with precision grids of different sizes. The precision grid has alternating dark and light bands with either 25 micron or 50 micron spacing. When PEPR looks at these in PR we have almost 90% modulation with the 50 micron grid, which falls off to under 50% modulation with the 25 micron grid. In PE, on the other hand, the modulation stays close to 90% on both the 50 and the 25 micron grids, which we think indicates that the signal pulse height is being lost in the response time of the analogue circuitry of PEPR. One of our biggest improvements occurred when we began track following in the PE mode, and now we try not to operate in the PR mode. The second problem - and this problem was perhaps more serious than the first - was the following: given two tracks of the same density, lying within 150 microns of one another, when sweeping across them in PR, we found that in only one out of seven trials did the TEDS recognize the second track. In PE mode, however, the second track was detected in better than seven out of ten cases. While not perfect, this can be used for track following.

One way to avoid the band pass limitation, while maintaining the field size of the PR scan, is to change the parameters of the PR scan from total count 256 resolution of 10 micron to 512 total count, 5 micron resolution. This change has been made in our hardware, but we have not as yet evaluated how well it eliminates the problem.

1.6.2 DATA FLAG

Another modification deals with the strategy for handling detected elements. We modified the action of the data flag so that it does not come true immediately upon detection of the first element of a given sweep, but waits until the end of the sweep. This modification in logic is effected by introducing a new flip-flop whose input terms are the old data flag and the "not sweeping" signal. The advantage of this arrangement is that the program loop which unloads the track elements need only look at the angle associated with the last element unloaded and can then assign that angle to the previous elements.

1.6.3 THRESHOLD

We have modified the controller to give us six bits of threshold control. The three gain bits function as low order threshold bits and are wired into a six bit converter. We use the automatic threshold facility very heavily during all parts of the program.

1.6.3.1 SIGNAL CONDITIONER OUTPUT

In general, we found the actual pulse amplitude out of the Signal Conditioner to be on the order of 1-2 volts, while we have the capability of processing a much larger signal. Therefore, we changed the feedback resistor around the signal conditioner output amplifier by a factor of 2. This resulted in a greater dynamic range of useable threshold values. Typically, the center of our operating range shifted in value from 16-18 to a value of 36-42, giving a useful dynamic range of zero to 64 rather than zero to 36.

2.0 PROGRAMMING APPROACH

The ultimate end of PEPR research is measuring without input or operator intervention. At the opposite extreme from this is predigitization of some points on tracks and of the vertex. In the middle is prescanning to indicate which frames contain an event and roughly in what area the vertex occurs. We decided to start at this middle level for two reasons. The first was economic necessity: we could not afford the necessary operator expense. The second was that Yale University had a program successfully operating at this level, and we thought we should be able to get it to work for our film.

2.0.1 SONIC PEN

The sonic pen scanning table now nearing completion of construction in our laboratory may change some of these considerations. A sonic pen is a pen-shaped device which emits a click (spark discharge) when its tip is pressed. Two long (3 ft., 5 ft.) capacitance microphones mounted at right angles to each other on the scan table transduce the click, and the x,y coordinates can be determined from the delay. The high accuracy (25 mils) and ease of use may be something to consider when planning the next generation of software.

2.1 PRODUCTION PROGRAM FLOW AND OPERATOR INTERVENTION

Assume that the Nth frame has been measured and passed by the operator, then:

1. The film transport is told to move to the next frame (N + 1th) to be measured.
2. An overlay is brought in from the disc.
3. Calibration of all points measured in frame N is done.
4. Calibrated points from frame N are output to tape.
5. If necessary, a new scan card is read. Calculation about the distance to move for frame N + 1.
6. Initialization calculations for frame N + 1.
7. Wait for film to come to rest.
8. Find and measure fiducials; possibly recondition film.
9. Do area scan about vertex (as given on scan card).
10. An overlay is brought in from the disc.
11. Short track following of all tracks discovered by the area scan

and possibly associated with the event.

12. From information obtained in 11, find plausible vertex.
13. Follow tracks associated with event as far as possible.
14. Make sure an incoming beam is present in event and, if not, look for one.
15. Try to be sure that no outgoing track is really a close passing beam.
16. If not enough tracks for event topology, reexamine measured but rejected tracks (coat hangers and beam suspects), possibly loop back through 11.
17. Display all found tracks with event candidate brightened and blinking. Allow operator to pass event or add or delete tracks. If insufficient tracks for event topology remain, re-try program from 9. one more time.
18. Recycle to 1.

The routines for 8, 9, 11, and track following are generalizations of Yale work; that for 12 is from POLLY.

There is considerable amount of calculation done between 11 and 16 concerned with being sure that two seemingly independent tracks are not simply two measurements of the same track.

2.1.1 FIDUCIAL FINDING ROUTINE

The basis of our fiducial finding routine is taken from Yale's system: however, we require a more elaborate search strategy. The center of a fiducial is determined by four measurements, two on each arm of the fiducial. The two points determine a straight line and the intersection of the two lines determines the center of the fiducial. When three or more fiducials have been measured, a least squares fit is made to the known positions of those fiducials with a rotation, translation, and magnification as parameters. If the Chi-square of the fit is sufficiently small, a good fiducial measurement is assumed.

Notice that recognition of a single fiducial is assumed after the existence of only four line elements at appropriate angles is ascertained.

When searching for the first fiducial, a series of long linear searches is carried out in the x direction (direction of film motion) with slowly varying y. This type of search is plausible since the largest unknown is the motion. The other fiducials are searched for with a spiral search around the nominal center.

2.1.2 AREA SCAN

This routine organizes an area scan in a rectangular region around the known approximate position of the vertex, and detects elements whose projections fall within an area such that they could lie on tracks of the required event; i.e., they must point towards the vertex region.

It is known that the vertex lies in a box 2 mm. square on the film. Using the centre of this box as reference, scan lines are set up asymmetrically around the vertex. The active scan lines are spaced 2 mm. apart and much weight is given to the search of forward tracks by allowing 6 of the 8 scan lines to be on the forward side of the vertex, while only 2 are used on the beam side. (This is especially important in our experiment, where the forward tracks are often indistinguishably close for a considerable distance due to their high momentum).

The inactive scan on the other hand is symmetrically organized around the vertex, the eight scan lines being 1 mm. apart.

No looking is done in the active region within 3 mm. of the nominal vertex since that region is usually densely populated with tracks so that measurement is difficult and interpretation unreliable.

2.1.3 PRELIMINARY TRACK FOLLOWING

This routine takes the data found by the area scan and, using the furthest out elements as starting points, tries to follow and identify all tracks in the vertex region. Having found a new element, the Phi, x, y of this element are used as a starting point and the track follower is called to follow the track away from the vertex in steps of .5 mm, sufficiently far to obtain a reasonable parametisation. This track follower is then asked to reverse the direction of following and to follow the track in towards the vertex. Tracks at steep angles to the beam direction near the vertex are followed right through the vertex, if possible, and then noted as being not very likely candidates for inclusion in the event. For tracks at small angles to the beam, it is important that the track follower be prevented from following them close to the vertex as:

1. It is a confused region.
2. The vertex position is unknown, and there is a danger of the follower sliding onto an outgoing track. If the follower has failed to adequately measure a track, the step size is reduced to .25 mm, and the track follower called again. This process is repeated until every element found by the area scan is accounted for.

2.1.4 THE TRACK FOLLOWER

We use the Yale track follower almost intact. One change is implemented when starting to follow a track: we try to find an appropriate threshold to use particular to this track. If your threshold is too low, you get many hits, but the hits will cover the entire angular window (± 5 degrees) and you get essentially no information about what the true angle is. Consequently, it is a good idea to spend some time when getting started on the track to decide what is a really appropriate threshold. We sit at the beginning of the track and we sweep over it until we find a threshold which gives us between five to seven hits and we decide that that is the nominal threshold with which we will follow that track. There is also a signal to low level programs when we are in track following mode (or when we are in certain parts of the fiducial finding routine) that there really is a piece of the track expected. The low level routine then makes repeated attempts to make a good measurement, lowering or raising the threshold as appropriate. The attempt at remeasurement is especially valuable in light of the phenomena noted in 1.6 and costs very little in time.

The second change is in the prediction routine. What is an optimum piece of track to use when doing predictions? We do a prediction on the basis of five points as does Yale. However, we do not use the last five points. We update the bank on which we do the predictions only on every other point so we actually have the most recent point, but the other four points come from every other measurement on the track. Thus, we have a longer piece of track on which to predict. This is a very clear advantage if you are doing a circle fit. If you are doing a straight fit, it serves the purpose that one wrong measurement does not tend to make you really start looking in the wrong place.

It might be noted that at our energies (6.9 GeV/c) the program decides (on the basis of sagitta) to use a straight line prediction more than 70% of the time. With this method of doing predictions, we were able to tighten down our tolerances on what the track follower would accept as a continuation of the track when looking at the predicted point. Thus, rarely do we ever see the track follower slip off onto a neighboring track.

2.1.5 VERTEX CALCULATION

We first find all intersections of all pairs of tracks. If the intersection lies within a reasonable area of the nominal event vertex, this intersection is a vertex candidate. A confidence level for each intersection is calculated such that long tracks intersecting at large angles bear more weight than short tracks intersecting at small angles. Then a weight is calculated for each intersection based on the sum of 1/(squares of the distances between the intersection in question and all other intersections).

Then the vertex position is calculated using a combination of the confidence level and weight of each intersection. Now the distance of each track from this vertex is calculated and up to ten of the closest tracks are remembered in an array in decending order of closeness to the vertex. An appropriate number of these (depending on event topology) are used to calculate a true vertex where the weights described above are not used, although the confidence level is. If the distances of the tracks from their common vertex are such that their average RMS error is less than 50 microns, the vertex is considered good.

2.1.6 CALIBRATION

The calibrations applied to the final points are in the form used by M.I.T.: a pincushion distortion; a central interpolation count distortion; an interpolation count significance distortion; a spot-to-line distortion; and an angle distortion.

Although our calibration routine measures and fits polynomials to all of these distortions, the last two are not used in the production module. We are planning to use the spot-to-line soon.

The residuals after calibration to predicted straight line seem to be something under 3 microns in both the x direction and the y direction. This is done with a spot in the PE mode.

2.2 IONIZATION MEASUREMENT

The ionization program which is at present not incorporated in the main body of the program is taken from an M.I.T. method, which is that once you know the location of the track, form a line element as close to parallel to the track, and sweep over it many times and look at the ratio of misses to number of trials as a function of threshold. If there were no noise this would be a step function. Of course, there is bound to be noise in the system, so the step will have some slope, and, in fact, I was surprised to see that our slopes were much steeper than those seen at M.I.T., typically four or five thresholds out of sixty-four.

This was a fairly time-consuming process and we finally developed an algorithm which, since it knows the approximate threshold, looks at values close by and then steps in either direction until all thresholds in the transition from 80% hits to 20% hits are covered. Since our slope is fairly steep, ionization of the track is defined as that threshold at which there are 50% misses.

We have had a graduate student measure a series of tracks with this ionization program which were also measured on the scan tables, and while we do not have a quantitative number on what ionizations can be resolved, the agreement between hand and PEPR measurements was very good.

3. SUMMARY

The film that we are using is 46 mm. unsprocketed film from the 82-inch bubble chamber at SLAC. It is $K^{+}P$ film of 6.9 GeV/c. The tracks are rather straight. The format of the film produces a fiducial area of 100 mm. by 45 mm. We think that we are essentially in production. We do not have a match program working and at the present moment we do not have a TVGP set up with its chamber. We did have a make-shift version of TVGP about a month ago. We chose a sample of twelve events in which the outgoing tracks did not overlap, so that we could simply sort them by outgoing angles. We did test those through TVGP and SQUAW and got fits that compared with hand-measured events, so we are fairly close.

We have measured about two full rolls of film in essentially automatic mode from scan cards with an operator present who just had to hit the button at the end.

DISCUSSION

LUCAS: (Yale) In operating PEPR at Yale AJOIN followed tracks often more than once, I wonder if you could comment on whether this is still true or not, for your program?

CHATZKY: (Johns Hopkins) Absolutely, I think that if you noticed in that second slow event 80 or 90% of the time was spent in AJOIN. I think there are two problems that account for that. One is that, as I pointed out, I think it is fairly important in order to make a good measurement, that you find a suitable threshold. You don't always get a good measurement of the track unless you take the time to do a preliminary scan of a cell to look at how many hits there are in the various elements and then to look at them again with a good threshold. Very often a single element won't match very well when you come to take it out of the bank and that's one problem.

I think basically it's a bad idea to scan a whole area, it is much better, as many people, such as POLLY, have done, not to do an entire area scan but to do some smaller scan, eg. a circle scan, but not necessarily a circle because it takes a long time to calculate sines and cosines, maybe a polygon or a square or a rectangle and look at everything along some line. Then pass through the rest of your program and see if you already have enough information. If you don't then go back and scan another part, there is no sense in doing the area scan all at first. I think the idea is to do a couple of line scans surrounding the vertex and then only go back and do another line if needed afterwards, and that I think is the other problem. That as you can see is a fairly significant change in the flow of the whole program and as soon as we find that MATCH and TVGP do confirm that we are in production, we'll go ahead and do that. In the meantime, in the past month while waiting for MATCH to come up we have just been fiddling with one little point or another.

The Rutgers PEPR

R. J. Plano, Rutgers University

We have the standard Astrodata hardware and a PDP-6 computer. We are following the M.I.T. point guidance programs quite closely. So far the calibration is working, and we can interact with the scope to put in guidance points, we have measured an event, and that's the report. However, I do hope that some of you are interested in some of the details so I will go on. The Astrodata hardware is in particular number 7, and it arrived on Thanksgiving Day in 1968. We have interfaced it ourselves to the PDP-6; we've done many of these little jobs ourselves. The hardware, I'd like to say, behaved very well and we've had essentially very little trouble with it. We've had more trouble with the point guidance software, but it is now beginning to work and I'll say more about it. Later I'll also discuss a little what we hope to do in the future once we get into production.

We've had the PDP-6 computer since 1965. It was originally purchased to do on-line checking of standard measuring machines and that is still doing this. It has 64K of 2 microsecond memory, no fast accumulators, 4 mag tapes, 6 dec tapes and 8 teletypes. We are a strong believer in time sharing and this is an important component of the whole plan. We also have a disk coming this month. On this computer we do not only PEPR, but we do all the computing associated with the bubble chamber group. In fact we allow other people in the Physics Department, and even a little from outside of the Department, to use the computer, since the time is as yet available. This is, I think, of some help to us, as I feel a little guilty if the computer is not used much, and the disk for example, was obtained partly to improve the availability and usefulness of the PDP-6 to teaching and other research areas and was justified partly on this basis. We've told all these people that the PEPR project has top priority and they are used to being bumped and conceivably could be permanently bumped some time in the future. However, I don't think that will happen. The Rutgers PEPR is not just a Rutgers PEPR. We are now forming a joint group with the Stevens Institute of Technology, including Leonard Koller and Snowden Taylor with graduate students. The entire staff we have is as follows: The physicists involved include Koller and Taylor at Stevens, and myself, Peter Yamin, Robert Knop and E. Byerly Brucker at Rutgers and about 9 graduate students. We used to have a senior technician, and if any of you know of a senior technician who would like to come to Rutgers would appreciate your help very much. We do have a junior technician. With this staff we do all the maintenance work on the computer and on PEPR, since we have no maintenance contract for taking care of the computer. Two senior professors, as Tom Day said, are half a research associate, and one is even less.

A few technical details may be of interest here. We have a film advance made by the same company as the John Hopkins film advance. It is quite a different design in that it takes sprocketed film only, is driven by a stepping motor with steps of 1/4 sprocket hole and goes at a top rate of 2000 steps a second which comes out to about 500 feet a minute. It is also trivial to change between 35, 46 and 70mm. Apart from some initial troubles, whereby whenever you touched it or closed the door the electronics blew out $300 or $400 worth of $50 transistors, the film advance has worked very well. We haven't been in production yet so I really have to qualify that. But it does look very good, apart from the fact that it takes one half second to open and close the film gate. It cost $11,000 and I am told the next one of this design will cost $15,000. Another difference from most groups is that we operate our PEPR not in direct user or I/O mode but through a service routine. This is done partly because of all the other time-sharing users to try to avoid killing the system as much as possible and I believe that has been reasonably successful but it does slow up the PEPR by a factor of perhaps 2 or 3; I don't have any exact numbers on this. In our production, once we are finished debugging I am quite certain we will take this feature out and do direct user I/O from the program like most other groups do.

Another difference is that we don't have a standard DEC scope, but a Tektronix 611 storage scope. This again is motivated partly by the fact that we have lots of other users and we don't wish to tie up the central processor any more than necessary, but also because it's cheap. The basic scope cost $2,500 and we interfaced it for an additional $1,500. What you save with this, of course, is the refresh time, you use no computer or other hardware time to do it and there is obviously no flicker. You can use a light pen, which may not be immediately obvious because they have a clever feature of a write through spot which doesn't store and which you can move around at will. This is an analog of a light pen hit. You can, of course, either use this directly or, as we are currently doing, search through the bank of coordinates which were displayed and find the one closest to the light pen "hit". This is slower but works very well.

Our calibration is a direct copy, as with almost all the software, of the M.I.T. version and we get a standard deviation of about 5 microns on the scope. This takes about 9 minutes and I believe is the best measure of the fact that we go two or three times slower by using a service routine. I am sure we'll have no trouble speeding that up when we get going.

The major hold-up at the moment is the bookkeeping system. We can now, if we enter points in the scope, measure events quite reasonably well and rapidly, but we have not yet interfaced this entire system with the IPD's. This should be quite easy since our IPD measurements come out directly on mag tapes avoiding card problems. I hope that this will be done in the not too distant future.

The physics we had in mind was originally to do 25 GeV protons in the 80" chamber. We got an additional run on that which we thought would be very useful in that we could compare the results with the many events we already measured by standard techniques. Unfortunately the new film was taken just before the shut-down a year ago and turned out to be of very bad quality. The 80" was extremely non-reproducible and although we sent a roll to Rutherford to be reverse developed, we decided that either way it was essentially impossible for PEPR. We are now doing it by hand.

We have currently a proposal for a $\bar{p}p$ exposure at around 1.3 GeV in the T meson region which we hope to do on PEPR. This is proposed both at Argonne, which is unlikely due to the low beam intensity, and at Brookhaven in the 31" chamber. We are asking for 500,000 pictures and we'd like to do a complete detailed study of this region which means that we hope to measure of the order of up to a million events. This, of course, is a somewhat ambitious program as so far we've measured only one event.

I believe that once we get into operation with the current equipment we will be able to do something like 200,000 events a year and to go much faster than that we'll have to do something more desperate. What we hope to do is one of the following or some combination thereof: one is to get a three-view PEPR, second is to get a faster computer which is an obvious statement and third we are thinking of becoming more automatic by following beam tracks to find events as done by POLLY. We've done no detailed thinking about this and I have nothing to add to the discussion. In closing I would venture to say that I see no reason, apart from the usual reasons, that we cannot be in preliminary production sometime this summer.

5 May 1970 3:30 p.m.

Session IV

Chairman: R. I. Hulsizer (M.I.T.)

STATUS OF THE VISUAL TECHNIQUES LABORATORY*

P. L. Bastien, L. A. Dunn, R. G. Kenyon

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*Presented at PEPR Colloquium, May 5-7, 1970, M.I.T.

by P. L. Bastien

CONTENTS

This paper describes the current status of the Visual Techniques Laboratory of the University of Washington Physics Department and also presents results on a vertex guidance system that has been developed in the past six months at the University of Washington.

I. STATUS OF THE LABORATORY

The charter members arrived last October 1st and have accomplished the following objectives:

I.A. Facility

Laboratory funds were obtained from the University to build a facility within the Physics Building. This was considered essential to maintain close contact with the Physics Department. Construction began November 1, and was completed March 15. The total office and laboratory floor space is about 3000 square feet. The computer room has a raised floor and is air-conditioned. There is also a scanning room with sufficient area for four IPD's, and an electronics shop.

I.B. Computer

The PDP-10 arrived on November 1, and became operational about three weeks later. The computer has 32K of 1 microsecond core, 16K of 1.6 microsecond core, a 600 line per minute line printer, 4 DEC tape drives, 2 magnetic tape drives, and a VR10 CRT display.

I.C. IPD Machines

Two machines are being constructed. The design is similar to that of the Berkeley SP5 model. The frames have been built and assembled; optics, film transports and digitizing arms are mounted and being tested. The read out, which will consist of one incremental tape drive hooked directly into the IPD machines, is now being wired and tested.

I.D. PEPR Hardware

The PEPR Astrodata hardware has arrived; the electronics was debugged at Astrodata. The interface between the Astrodata and the PDP-10 has been wired and on line tests with the PDP-10 will begin around June 1. The film transport is being built by the Bucone Corporation and will accept 35mm film. The lens has a magnification of 1:1 (for SLAC film), a resolution of at least

40 line pairs per millimeter and an aperture of f/1.9. It is now being mounted on the film transport. These will be shipped sometime during June.

I.E. PDP-10 Software

The PDP-10 is equipped with a time sharing monitor with duplex software and is compatible with the M.I.T. MACRO subroutines. The bubble chamber chain of programs (TVGP, SQUAW, ARROW, and KIOWA) has been implemented and tested. The M.I.T. point guidance system has been partially tested even though our PEPR hardware is not yet working. In order to do this we obtained, by courtesy of Professor Pless of M.I.T., fine mesh PEPR data of 98 events of $\bar{p}p$ at 5.1 GeV/c in the 30" Argonne Chamber. The scans consisted of an average of 1500 (x,y,ϕ) triples. In addition, the other programs in the chain such as PREP which takes IPD measurements and transforms them into a format acceptable to the M.I.T. Point Guidance System were also tested. Figure 1 shows a bubble chamber picture of a $\bar{p}p$ event. Figure 2 shows an area scan of this event and Figure 3 shows the results of track following off-line with the M.I.T. point guidance system.

The PDP-10 is approximately twice as fast as the PDP-6 for TVGP and SQUAW, and approximately 1/3 as fast as the 360-65 for the same programs. In Table I is shown a comparison of actual timings for the three machines.

II. VERTEX GUIDANCE SYSTEM

With the fine mesh PEPR data we have been able to develop off-line a vertex guidance system based on a global approach.[1,2] The system has worked with a very high success rate on the limited sample on which it has been tried. The input to the system is a fiducial mark and the vertex for each view. These are measured off-line on an IPD machine (Image Plane Digitizer). We make area scans in a circular region around the vertex. The scan is not made at all angles as will be seen later. The circular region has a radius of 1 cm on film. To all elements (x,y,ϕ) found in the area scan, two transformations are applied (See Section II-A): for each element in the area one computes potential curvature $(\equiv\rho)$, and an angle $(\equiv\psi)$, based on the vertex point and the (x,y,ϕ) coordinate of the element. When the $(\rho,\psi)_i$ values corresponding to each element $(x,y,\phi)_i$ in the circular scan are plotted on a two dimensional scatter plot, circular tracks emanating from the vertex are reduced to a point. Tracks which do not go through the vertex are spread as background over the scatter plot.

To make absolutely clear what is meant by these two transformations, we choose to show the results of the program on the γ-ray of Figure 4. An area scan of the gamma ray is shown on Figure 5a. The program is given the position of the vertex of the gamma ray; it then obtains the ρ and ψ values for each element and plots them on a two dimensional scatter plot (See Figure 5b). The two concentrations of points on Figure 5b correspond to the electron and the positron. A simple program can easily isolate these two concentrations and obtain whole tracks at once. The resulting tracks are shown on Figure 5c.

We will consider the following topics:

- Basic global transformations
- Description of the current program
- Performance of the Program on 98 events
- Latest improvements
- Conclusions

II.A. Basic global transformations

II.A.1. ψ transformations

The geometry of the ψ transformation is shown in Figure 6. If an area scan is made around V it is easy to show that, for all the PEPR elements (subscript n) belonging to a circular track (superscript i) emanating from V, (and ignoring errors), the following relations hold:

$$\psi_n = \delta_n + \phi_A = \psi_v^i \qquad (1)$$

ϕ_n is the PEPR angle; δ_n is the difference between the chord from the vertex point to (x_n, y_n) and ϕ_n; ϕ_A is the angle between the x-axis and the chord; ψ_v^i is the angle between the tangent to track i at the vertex point and the x-axis.

The error on ψ_n is constant over the whole PEPR scope, except very near the vertex, and is approximately equal to ±1.5°:

$$\Delta\psi_n \simeq \Delta\phi_n \simeq \pm 1.5° \qquad (2)$$

The IPD vertex point, known to ±60μ on film, is assumed to have been refined to ±10μ by a program described in Section II.B.2. If we histogram all ψ_n's for the area scan, circular tracks emanating from the vertex will show as pulses of width 1.5°, independent of the curvature of the track.

II.A.2. ρ transformations

On Figure 7 we show a configuration which arises in the case of a gamma ray. Both tracks have the same tangent at the vertex point, and therefore will fall in a single ψ pulse. It is possible to separate them by taking the elements belonging to the pulse and making a histogram of the curvature (ρ_n) for those elements. The curvature is computed by the following relation:

$$\rho_n = \frac{2 \sin \delta_n}{d_n} \tag{3}$$

The error on ρ is not nearly as well behaved as the error on ψ:

$$\Delta\rho_n = \frac{2}{d_n} \cos \delta_n \, \Delta\delta_n \tag{4}$$

However, one notices that:

$$\frac{d\rho}{d\psi} = \frac{2}{d} \frac{d}{d\psi} \sin(\psi-\phi_A) \simeq \frac{2}{d} \tag{5}$$

Therefore, the errors on ρ and ψ are very strongly correlated, even though ρ and ψ themselves are 'orthogonal' quantities (See Figure 5b). Consequently, it is better to histogram the quantity ρ'

$$\rho'_n = \rho_n + \frac{2}{d} (\bar{\psi} - \psi_n) \tag{6}$$

where $\bar{\psi}$ is the average value of a ψ pulse.

II.B. Current Program

We have implemented these ideas in a program which is now capable of doing a physics experiment. This program has the logic for handling the most general topology. Three view events processed through this program have been successfully used as input to MATCH.

II.B. Input to the program

The coordinates of a standard fiducial mark together with the coordinates of each vertex and the number of prongs emanating from it are given. The generation number of each vertex is also given; hence, the whole topology of the event is known. An example will clarify this statement. If we look at Figure 8 we see an event with 5 vertices and 2 trees. It is possible to completely specify the topology of the event by the following string:

```
(A,1,3) (B,2,3) (C,2,3) (D,3,3) (E,1,3) (F,2,3)
 ↑ ↑ ↑
 | | └──Number of tracks
 | └─Generation Number
 |
Coordinates
```

Vertices B,C,F are second generation vertices and vertex D is a third generation vertex.

II.B.2 Accurate determination of the vertex point

An area scan is performed in Region 1 of Figure 9. This region has a radius of approximately 2.5 mm on film. The segments in this area are usually well defined, hence it is possible to group the elements into segments. Having done this, segment intersections should then yield a satisfactory vertex approximation. The problem separates into two distinct steps:

1) find the segments
2) find the intersection point (vertex)

Step 1) is best described as the following sequence of substeps:

a) Area scan to get all elements within distance R_2 of the IPD'd vertex point (Region 1 in Figure 9). The distance R_2 is about 200 MDC. (1 MDC=12.5μ on film). If there is a second vertex closer than R_2 then reduce R_2 to the distance between vertices (but not less than about 100 MDC). Exclude all elements whose angle, ϕ, differs by more than say 15° from the direction of the line between the IPD'd vertex and the element position (x,y), unless the distance between the element position and the IPD'd vertex is less than about 60 MDC.

b) Now order the elements by decreasing distance from the IPD'd vertex.

c) Take the element farthest from the vertex and use it as a point from which to perform the segment recognition sequence (ψ-transformation, pulse recognition and circle fit, see Section II-B-3). Use the previously calculated distance of the element from the IPD'd vertex as the maximum radius for element selection from the ordered bank. Each element selected from the bank will have its distance value replaced by its segment number. Then take the next unused element from the bank and again perform the segment recognition sequence. Do this until the element bank is exhausted.

This completes step 1). Any segment recognized that does not pass within about 30 MDC of the IPD vertex is excluded from further consideration. Also note that a beam track (or other) passing through but within 30 MDC of the IPD vertex would be split into two segments. A test on end points and end angles can exclude some of these as matching segments. The angle test will not always get a match since the parameter must be very tight in order not to match segments of the event, although occasionally this can happen. Unless the event is a 2-prong, it is still possible to get intersections and obtain a vertex position.

Step 2) is now performed in the following sequence:

a) Intersect all remaining pairs of segments whose end points are within say 50 MDC of the IPD vertex and whose end angles differ by 10^{o} or more.

b) Save the intersection point unless it is more than 30 MDC from the IPD vertex <u>or</u> unless its position is within the end point of either segment by more than 5 or 10 MDC.

c) Now calculate the average X and the average Y of the intersections and obtain minimum and maximum values for both X and Y.

d) If the X dispersion is greater than say 5 MDC then exclude the point whose X is farthest from the average X and return to c).

e) If the Y dispersion is greater than say 5 MDC then exclude the point whose Y is farthest from the average Y and return to c).

f) Take the calculated average as the vertex point.

If less than two segments are found or if the coordinate dispersion still exceeds the limits after reducing to two points then the IPD vertex is not changed.

The results with this algorithm are quite good even with a sizable error (up to 20 or 30 MDC, i.e. 250μ to 400μ on film)on the IPD vertex position as transformed to the PEPR coordinate system. There are some problem situations, especially 2-prongs with an outgoing track parallel to the beam that also have a close beam track passing through. However, the current success rate is 90% on the events considered to date (about 50). It would appear that if the hardware performance is reliable, i.e. if the information content of the film is obtained, then it is possible to process this information with highly reliable results.

II.B.3. Annular scan and pulse recognition

An area scan is made around the vertex in the annular region shown on Figure 9, Region 2. This annulus has an inner radius of approximately 200 MDC (2.5mm on film) and an outer radius of R_{max} of approximately 650 MDC (1 cm on film). The M.I.T. digitizations happen to have been made along tiers separated by approximately 20 MDC (250 microns on film) and with a PEPR line segment length set to 1000μ. However, the particular values for the parameters R_{max} and tier size are quite likely to change in the future. Note that the area scan time is inversely proportional to the square of tier size. The area scan is not performed at all angles, but rather in an angular range given by the following form:

$$\Delta\phi = \frac{\sqrt{(x_{cell} - x_{vertex})^2 + (y_{cell} - y_{vertex})^2}}{R_{max}} \Delta\phi_o \qquad (7)$$

In other words, $\Delta\phi$ will be a linear function of the distance of the cell center from the vertex; $\Delta\phi_0$ is currently set to 50^{o}. This type of scan eliminates a large number of unwanted elements, particularly from nearby parallel beam tracks and also takes considerably less time. Figure 10 shows a bubble chamber picture containing a 4 prong and a 2 prong events and Figure 11c shows the elements which remain in the annular region around the 2 prong after the above cuts have been made. Before histogramming, the elements are weighted by a factor $1/\cos\phi_n$ (active), or $1/\sin\phi_n$ (inactive), to account for the fact that the scan is made along the x and y direction, i.e. tracks at 45^o get less elements. This weighing helps but is not essential. The elements are now histogrammed with a bin size of 0.5^o and the histograms are examined by a routine called SUPTED, the software equivalent to the TED circuits. This routine isolates peaks in a histogram. The elements in each ψ peak are now ρ' histogrammed. SUPTED is applied to the ρ' histogram and will in general find one peak plus a few background hits, but occasionally 2 peaks (such as in the case of a γ-ray) or even 3 peaks. It is an experimental fact that the track segments have become very clean at this stage by virtue of this double sorting. The reader may be able to convince himself of this by looking at the ψ-ρ scatter plots on Figure 5, 11b and 18b.

Finally, we note that if the program of section II.B.2 has been unable to find an accurate vertex point, the original IPD point is used to make the ψ-ρ transformation.

II.B.4. Track fit.

It is clear that there will be pulses which are made up of two parallel tracks running very close to one another. In order to eliminate the unwanted parallel elements, we use a slightly modified TVGP circle fit routine (CIRCLE).[3] Since the wanted track has more elements than the unwanted track, the first fit is weighted toward the wanted elements; then CIRCLE eliminates all bad residuals. The parameter governing the maximum percentage of points which can be removed has been increased to 50%. This procedure very rarely fails, and an example of this is shown on Figures 12b and 12d although in this case there is only one bad residual. Segments are now absolutely clean. The circle fit routine also gives us a fitted center and radius for the segment. This information is used to eliminate track segments which do not go through the vertex point.

II.B.5. Outer area.

For all track segments in the annular area which reach the outer boundary we can define a road with the center and radius given by CIRCLE. An area scan is made in the narrow road (see Figure 9, Region 3), and the same ψ-ρ-CIRCLE technique is applied to the elements found in the road. This method of global tracking in a road is extremely reliable and simple and could certainly be used in point guidance schemes. Its advantages are simplicity and lack of special cases.

II.3.6. Joining the segments

Once the segments have been obtained for a vertex, they are joined together into tracks and the number of points on a track is reduced to approximately 10 or less for TVGP input. The event data bank is then put on tape.

If there are several vertices, steps 1 to 5 are repeated for each vertex. In this case redundant segments will have to be eliminated; this part remains to be coded although we anticipate no particular difficulties since we know the position of all vertices.

II.B.7. Brief description of the logic and some subroutines

This section is strictly for people interested in details of the program. Since the logic is not trivial it may be of interest to describe quickly the method used to keep track of the segments in a multivertex event. All digitizations x,y,ϕ for segments are stored in a large bank, RSAFE[4] and a pointer to the beginning address of a segment in RSAFE is stored. The number of elements in a segment is also saved. A word is kept for each segment specifying its track, tree, vertex, region (as on Figure 9), and where it begins and ends within the region; CIRCLE information is also saved. 'Dead' segments can be removed from RSAFE by means of subroutine POISON. Routines which manipulate segments have the following type of calling sequences:

```
CALL SUB (RSAFE(IPOINTER), RSAFEY(IPOINTER), RSAFEP(IPOINTER),
          RDST(IPOINTER), LENGTHSEGMENT)
SUBROUTINE SUB (RX,RY,RP,RDST,L)
DIMENSION RX(1), RY(1),RP(1), RDST(1)
```

In addition to (x,y,ϕ) we save d, the distance of the element from the vertex, so that the track elements can be arranged in order of increasing distance from the vertex (for TVGP).

II.C. Performance of the Program on 98 events

In this section we analyze the performance of the program on 98 events. These events were scanned at VTL with scanning criteria less stringent than those of the $\bar{p}p$ experiment. All topologies were taken (2,4, and 6 prongs). In some cases we made a point of choosing events which would have otherwise been rejected by standard scanning criteria to see how the program would perform under these more difficult conditions.

We will first lead the reader in reasonable detail through four events then go through the results for the total sample.

II.C.1. Detailed examination of 4 events

The first event is the previously discussed gamma ray. On Figure 4 we have the original bubble chamber picture for the event. On Figure 5b we see the results of a ψ-ρ scatter plot, on Figure 13a and 13b the ψ and ρ pulses, and on Figure 5c the resulting tracks.

Figure 10 contains two events: a four prong and a two prong. The coarse area scan for the picture is shown in Figure 14a and the resulting tracks from pulse recognition for the 4-prong on Figure 14b. On this frame the two prong event is rather difficult because the forward track crosses several beam tracks. The ψ-ρ scatter plot for this event is shown in Figure 11b, and the resulting annular area scan is shown in Figure 11c. The segments found in the annular scan are shown in Figure 11d. For the forward going track ψ-ρ histogramming found the elements shown in Figure 12b. Then CIRCLE removed the bad residual as can be seen in Figure 12c; the distance between two dotted lines corresponds to 1 MDC (12.5μ). The residuals are quite scattered since no pin-cushion corrections were applied. Finally, in Figure 12a, we show all the segments including those found in the road scans (Region 3 of Figure 9). Note that in the road scan for the beam track, which is narrowly surrounded by two other beam tracks, there are no bad residuals: The CIRCLE fit routine has removed them.

Now we wish to show a particularly difficult pair of events (Figure 15). These events are two six prongs, where both incoming beam tracks happen to be superimposed; ten prongs are going in the forward direction. If we look at the area scan in Figure 16a carefully we see that the PEPR hardware did not digitize large sections of a forward track. If it were not for this, all the tracks in the forward direction, in both events, would have been found (see Figure 16c and 16d). An extra track (vertical arm of the fiducial) has been found in the second event. This extra track will be removed by MATCH. These events are particularly interesting since they show the power of the method for unraveling events which are very difficult for a scanner to disentangle, particularly in a point guidance scheme. We also notice that the backward curving track on the first event has failed; the Argonne chamber has a 30 kilogauss field, and when we took the area scans at M.I.T., not anticipating this difficulty, we used a 1000μ line segment. This was clearly too large for such curved tracks and digitizations were either non-existent or very poor on such tracks (look very carefully at the left hand side of Figure 16a). This means that we have systematic failures on curved tracks throughout the entire sample. Despite this, we will see that the overall performance of the system is excellent.

II.6.2. Performance of the program on 98 events

We eliminate from the tally (Table 2) seven events which are outside the fiducial volume. Of the 91 events considered in all three views, 75 pass all tracks or fail at most one track in one view. Five events fail completely, i.e., they are classified as unrecoverable. Eleven events fail a track in more than one view because the track is either very curved (1000μ line segment problem) or very short (lacking TV scans we cannot find stubs). These two problems will be solved in the next version of the system (see sections II.D.2 and II.E). It should also be noted that the angle of elements at the scan's angle boundaries are skewed since hits which could have occurred past the boundary would shift the average angle toward the boundary. A new version of ELSCN has been written to properly handle this case and will be included in the next version of the program. Since the boundaries happened to be -45° and $+44^\circ$ for the current data all angles from 40° to 48° and from -49° to -41° are incorrent. Furthermore, some of the events which fail have two tracks in the same ψ and ρ pulses. These are now separated by a subroutine, SPLIT, discussed in the next section.

The conclusion is that on this particular film, despite the above handicaps, an 82% operating level is attained. Including 50% of the 'recoverable' failures raises the operating level for this type of film to 90%.

II.D. Latest Improvements

II.D.1. Track splitting-SPLIT

On the area scan of Figure 18a we see two very close tracks emanate from the vertex; these tracks have the same ψ and ρ. The track circle fit technique will normally eliminate one of the tracks; however, we see on Figure 18c that all double elements have exactly the same x coordinate because of the very nature of the PEPR scan. The y coordinates are separated in this case by approximately 100 microns (8 MDC). These two tracks are therefore close to the limit of hardware resolution. The TEDS will not fire if two tracks are closer then 5 MDC. When SUPTED finds a very large ψ and ρ pulse, we split the elements in the pulse into two segments by linearly separating pairs of elements having nearly identical x coordinates. A routine has recently been written by Maria Rosa Pignotti to handle this case for up to five tracks. (This is analogous to the element recognition problem). SPLIT is called before CIRCLE; on Figure 18d the split upper track is shown.

II.D.2. Improved Angle Resolution

We now discuss a way of improving the angle resolution. It is clear from Equations (1), (2), and (6) that any improvement in ϕ_n will enhance the efficiency. It turns out that in ELSCN, while making PE digitizations, it is possible to reposition the line segment along a line defined by the PEPR angle, at points spaced approximately 20 main deflection counts to each side of the central point, with little cost in time. We can fit a straight line through these points and get an angle which is considerably better than the standard 1.5^o resolution for the PEPR angle. In fact, if the least count is approximately 3 microns on film at 40 main deflection counts one would expect a resolution of approximately .5 degrees instead of 1.5 degrees. We have tested this idea using the fine mesh digitizations; the resolution does improve by the expected factor. We intend to incorporate this into ELSCN very shortly.[5] SPLIT along with improved angle resolution will make the system effective on high energy experiments, where tracks are strongly peaked in the forward direction.

II.E. Conclusion

We feel that the method of global track following that we have presented here is extremely powerful because it avoids special cases and is relatively simple to implement. We are currently inserting the improvements that we have mentioned and are preparing to take data for 600 4-prong and 6-prong events to process through TVGP and SQUAW. These will include T.V. scans around the vertices (to find stubs) and 1 mm and 1/2 mm area scans to find very curved tracks. It would appear that if the hardware digitizes properly these global transformations are capable of extremely high efficiency, possibly 95%, on good film such as we have. The method can probably be used even more fruitfully at high energies where the circle approximations are even better. We estimate that the program will process 150 events per hour. The success rates are sufficiently high to warrant consideration of an off-line help system discussed in the next section.

On-line display systems were written to determine program performance. These displays are quite extensive and have been found essential in generating new ideas and in debugging. We are, in fact, firmly convinced that direct interaction with extensive displays via teletype is the only way to understand complex programming systems. Some of the displays were photographed and have been used as figures.

III. Off-line Help With a Disk

We suggest that a disk can be used very effectively to eliminate re-measurements. This will allow the PEPR system to measure a roll of film in a single pass. In other words, no further measurements have to be made as is customarily done.

Let us very briefly review the 'front end' programs of a standard bubble chamber data reduction system; the flow is shown in Figure 19. The IPD machines (Image Plane Digitizers) are used to digitize points on each of three views of an event. These points are input to the PEPR programs which also have access to the film. All the tracks of the event are measured by the PEPR system, usually one complete view at a time. The PEPR measurements are input to the MATCH program which labels corresponding tracks in the three views, TVGP which reconstructs tracks in space, and SQUAW which does kinematic fitting.

We now discuss a disk system for which a block diagram is shown on Figure 20. As a view is being processed two things can happen to an event-view:

a) It is unambiguously solved. In this case, the tracks are written out on the disk at the average of 1000 words/event (based on 2 to 6 prong events).

b) There is a track missing (maybe even more) or there is some doubt as to whether the event has been successfully measured. In this case, a fine mesh scan of the whole view and a TV scan of the region around the vertex are output on the disk. We estimate that we will write out at the very most 10,000 words/bad event, and this data will be used in the off-line help system.

For a typical roll of film and assuming a success rate of 85%, we estimate that 3 million words will go onto the disk per 3 view roll of film.

Now MATCH, TVGP, and SQUAW are called and all 'good' PEPR events are processed through these programs and the results are returned to the disk. At this point we know which events have failed TVGP, MATCH, and SQUAW, and we bring in an operator. The great advantage of this system is that the operator has available three views of the film on a scanning table. Next to that scanning table is a CRT display and a teletype through which the operator interacts with the PDP-10 and with the disk by remote control. All three views of all events which have failed are now examined with the aid of very elaborate displays. Most of the displays have already been developed to test and debug our vertex guidance system. If some tracks of a view have been failed by PEPR, the light pen can be used to push them through. If the event fails MATCH, TVGP or SQUAW, all the tracks can be examined for bad points or various other difficulties and mended. At any time the operator can swap in programs like MATCH, TVGP or SQUAW to see if his efforts have been successful. The swapping time for a 35,000 word program is approximately 0.5 second.

Ionization is measured <u>only</u> for those tracks in which there can be an ambiguity. Taking into account dip this means that for positive particles, any track with a curvature > 3.0 GeV/c will not have an ionization measurement. Since we always have available curvature information from CIRCLE we can determine when an ionization measurement should be made. For those few cases where there is still some ambiguity the operator can decide by looking at the event on the scan table. Of course, the details of what one does about ionization are strongly experiment dependent. For example, if we are studying diffraction dissociation of the incident particle at 20 GeV/c, we will very rarely need ionization measurements and the above system will work very well. If on the other hand, we are studying the π^+p interactions at 4 GeV/c, the above system will almost always require an ionization measurement for the positive tracks. For such experiments, it will be interesting to determine if ionization measurements can be made on one view only.

All events are completed at this stage and no further remeasurements will be necessary. The events are then ordered by frame number while still on the disk and written on a magnetic tape for further processing.

This system is very much like having a three view system without having to code very difficult programs like three view tracking. Although from the experience one gains from the human operator, one might develop a three view tracking routine operating off-line with the data on the disk. We point out on

Figure 2, that one of the views could remain in the PEPR film gate at help time if the need arises, although at the moment this appears to be unlikely. However, random access to one view at help time might be useful and one might also consider doing ionization in one view after SQUAW, if this proves workable.

Such a system cannot be operated without a disk, since three magnetic tapes would be required to store the data for a roll, and access to a magnetic tape is prohibitively slow, as is the swapping of programs from DECtapes. The disk we propose for this system is the DEC RPO2 Disk Pack, which is a storage disk containing 5×10^6 words per disk pack. The average access time to the first word of a bank of words is 62.5 ms. Once there, the transfer rate is 15μsec/word; thus it requires approximately 0.5 sec. to transfer 35,000 words.

Finally, the data transfer rate for a disk is about four times that for magnetic tape and offers a large advantage, especially for KIOWA runs where most of the time is spent on I/O.

IV. PLANNED DEVELOPMENTS

We have plans to work in several other areas of Visual Techniques:

a) We are starting a collaboration with the University of Washington Microbiology Department. We propose to design a scanning device which will automate tests which determine a patient's sensitivity to various types of antibiotics.

b) We have started a small pilot program on teaching elementary and intermediate physics with a computer. Some programs have been written where the student interacts with the computer through a teletype and is presented with displays on our VR10 scope. The problems treated are collisions (elastic and inelastic) and scattering in any central force field.

c) We are designing a system which will automate the measurement of γ rays in heavy liquid chambers.

d) Our vertex guidance system will be originally used on $\pi^- d$ film at 15 GeV/c in the SLAC chamber which we hope to obtain in the near future.

	PDP-6	PDP-10	360-65
TVGP time	21.0 min.	10.5 min.	4.7 min.
SQUAW time	23.0 min.	9.7 min.	2.6 min.
TVGP core	27 K	27 K	∿ 38 K
SQUAW core	26 K	26 K	∿ 38 K

Table I.- Timings for 107 TVGP events and SQUAW events (2 hypotheses) through the PDP-6, PDP-10 and IBM 360-65

	Number	Percent
Good events (1)	75	82%
Possibly recoverable events (2)	11	12%
Failures	5	6%
TOTAL (3)	91	

(1) At most one track missing in one view.

(2) Several tracks missing because we do not have as yet a stub search or because the track is too curved (See Section II-C); most of these events will be recovered.

(3) There were 98 events in all but 7 had been chosen outside the fiducial volume to see what would happen and have not been counted (most of them failed).

TABLE II. - Performance of the Current ψ-ρ Program

REFERENCES

1. M. Bazin, A Global method for Pattern Recognition of Bubble Chamber Photographs, PPAD-534E, (unpublished)

2. P. L. Bastien and L. A. Dunn, A new approach to Vertex guidance VTL PN-3, (unpublished)

3. F. T. Solmitz, A. D. Johnson, T. B. Day, Three View Geometry Program, Alvarez Group Memo, P-117

4. P. L. Bastien, L. D. Kirkpatrick, Format of SAFEK in Vertex Guidance, VTL PN-24 (unpublished)

5. These modifications have actually been implemented at print time see P. L. Bastien: Modifications to ELSCAN, VTL PN-26 (unpublished)

FIGURE CAPTIONS

Note: The magnification may vary from figure to figure. Using the fiducials as reference marks may help the reader.

Figure 1 Bubble chamber picture of an antiproton interaction in the Argonne 30" chamber. In the upper left hand corner a four prong event is seen; an anti-neutron star appears in the middle of the picture.

Figure 2 Coarse mesh area scan of bubble chamber picture on Figure 1.

Figure 3 Performance of the MIT point guidance system (using the simulated ELSCN) on the bubble chamber event of picture 1.

Figure 4 Bubble chamber picture of a gamma-ray event.

Figure 5a Coarse mesh area scan of the gamma-ray event shown on Figure 4.

5b $\psi\rho$ scatter plot of the gamma-ray event shown on Figure 4. ψ values run along the abscissa and ρ values along the ordinate. The two concentrations of points at the same value of ψ and different values of ρ correspond to the electron and positron. Notice the strong correlation between the ψ and ρ errors. For this picture ψ ranges from -20^{o} to 50^{o} and ρ from -0.003 to 0.003 MDC^{-1}.

5c The track segments corresponding to the concentrations in the $\psi\rho$ scatter plot of Figure 5b. The two tracks are clearly separated.

Figure 6 Geometry of the ψ transformation. V is the vertex point, track i is assumed to be circular, $(x,y,\phi)_n$ is a PEPR element, d_n is the distance between the vertex point and PEPR element n, ψ_n is the transformed ϕ_n. ψ_n is equal to ψ_V^i, the angle of the tangent to track i at the vertex point.

Figure 7 An example of the ρ transformation. When two tracks have the same tangent at the vertex their ϕ_n transform into the same ψ bin.

Figure 8 IPD (Image Plane Digitizer) measurements input to the ψρ system: a fiducial mark (F_d), a measurement for each vertex point (A through F), the generation number of each vertex and the number of tracks emanating from each vertex (see Section II.B.1).

Figure 9 Three types of scans are made for each vertex. The data from the area scan in Region 1 are used to locate the vertex point accurately (the IPD vertex is known to only 60 microns). In the annular region (Region 2), the ψρ transformations are performed to find track segments. If a track segment reaches the boundary of the annular region, as at point E, a road (Region 3) is defined by the center and radius of the circle fitted through the track segment elements. An area scan is made in the road and the elements found are ψρ histogrammed to get the remainder of the track.

Figure 10 Bubble chamber picture showing two events: a four prong on the left and a two prong in the center. Note that the forward prong of the two prong event is crossing two beam tracks.

Figure 11a The three short segments were found when making the Region 1 scan (see Figure 9) around the IPD vertex point for the 2 prong of Figure 10. From these three segments the vertex point scan is determined to ±10μ.

Figure 11b ψρ scatter plot for the elements in Region 2 of the two prong in figure 10. One notices at 70 degrees the point concentration corresponding to the track going up on Figure 10, and around 10 and 190 degrees a number of concentrations which correspond to beam tracks and the forward prong.

Figure 11c Area scan in Region 2 for the 2 prong of Figure 10. These are the elements used to obtain the ψρ scatter plot of Figure 11b. The $\Delta\phi$ cut defined in II.B.3. Eq. (7), has been applied.

Figure 11d The track segments which remain in the annular region (Region 2) and central region (Region 1) for the two prong event.

Figure 12a The two prong eventin Regions 1, 2 and 3 of Figure 9.

Figure 12b The forward going track of the two prong event before removal of bad residuals.

Figure 12c Residuals for the forward going track in the two prong event after removal of bad residuals. The distance between two dotted lines represents 12.8 microns on film (1 Main Deflection Count). Residual pin-cushion has not been removed.

Figure 12d The forward going track in the two prong event after removal of bad residuals.

Figure 13a The ψ pulse corresponding to the gamma-ray event of Figure 4.

Figure 13b Histogram of the ρ values of the elements in the ψ pulse of Figure 13a. The ψ pulse splits into two ρ pulses.

Figure 14a Area scan for the picture of Figure 10.

Figure 14b The tracks found for the four prong event of Figure 10.

Figure 15 A bubble chamber picture with two six prong events. The incoming beam tracks are almost superimposed and there are ten prongs in the forward direction.

Figure 16a An area scan for the bubble chamber picture of Figure 15.

Figure 16b Track segments resulting from $\psi\rho$ histogramming for the second six prong on Figure 15. We see that all the tracks have been found except one which was not digitized by the PEPR hardware.

Figure 16c Track segments resulting from $\psi\rho$ histogramming for the first six prong on Figure 15. All the tracks in the forward direction have been found. The backward track has not been found because its curvature is too large. The PEPR hardware could not digitize properly on this track with a 1 mm line segment.

Figure 16d Same as Figure 16b, but we show the central points (x,y) of the elements (x,y,ϕ) instead of showing the elements.

Figure 17 Four events which have been successful: top right is a six prong event, top left is a four prong and a three prong (same as that on Fig. 3, but through our system), bottom left is a six prong with a scatter, and bottom right is a two prong with a scatter.

Figure 18a Area scan of an event where two of the forward prongs nearly coalesce.

Figure 18b $\psi\rho$ scatter plot for the same event.

Figure 18c The elements in the ρ pulse for the two nearly coalescent tracks. Splitting these tracks is easy since the x coordinates of the 'double' elements are the same.

Figure 18d The top track in Figure 18c, after separation.

Figure 19 Standard Bubble Chamber data reduction system using PEPR.

Figure 20 Proposed disk system.

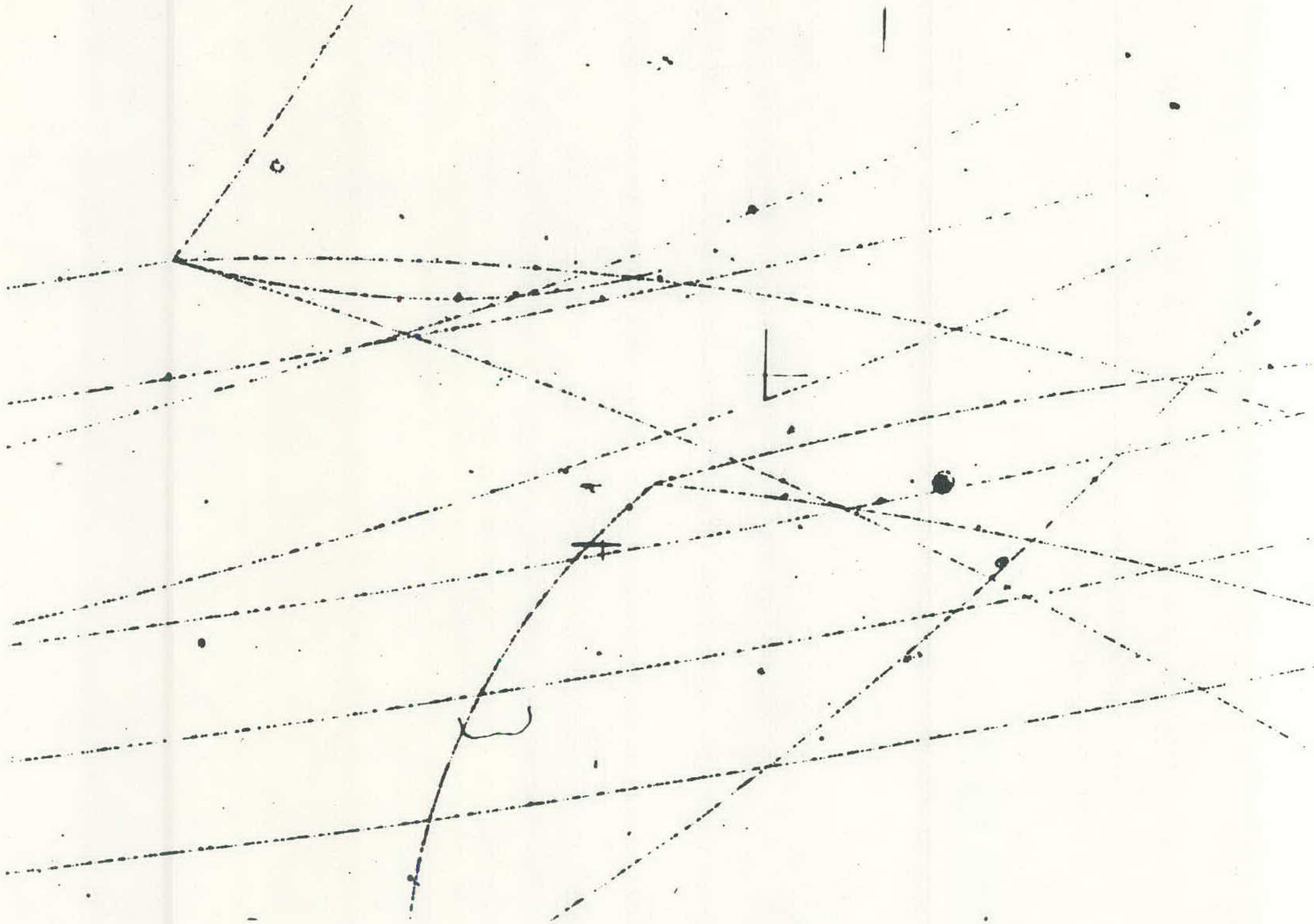


Figure 1

Figure 16d	Same as Figure 16b, but we show the central points (x,y) of the elements (x,y,ϕ) instead of showing the elements.
Figure 17	Four events which have been successful: top right is a six prong event, top left is a four prong and a three prong (same as that on Fig. 3, but through our system), bottom left is a six prong with a scatter, and bottom right is a two prong with a scatter.
Figure 18a	Area scan of an event where two of the forward prongs nearly coalesce.
Figure 18b	$\psi\rho$ scatter plot for the same event.
Figure 18c	The elements in the ρ pulse for the two nearly coalescent tracks. Splitting these tracks is easy since the x coordinates of the 'double' elements are the same.
Figure 18d	The top track in Figure 18c, after separation.
Figure 19	Standard Bubble Chamber data reduction system using PEPR.
Figure 20	Proposed disk system.

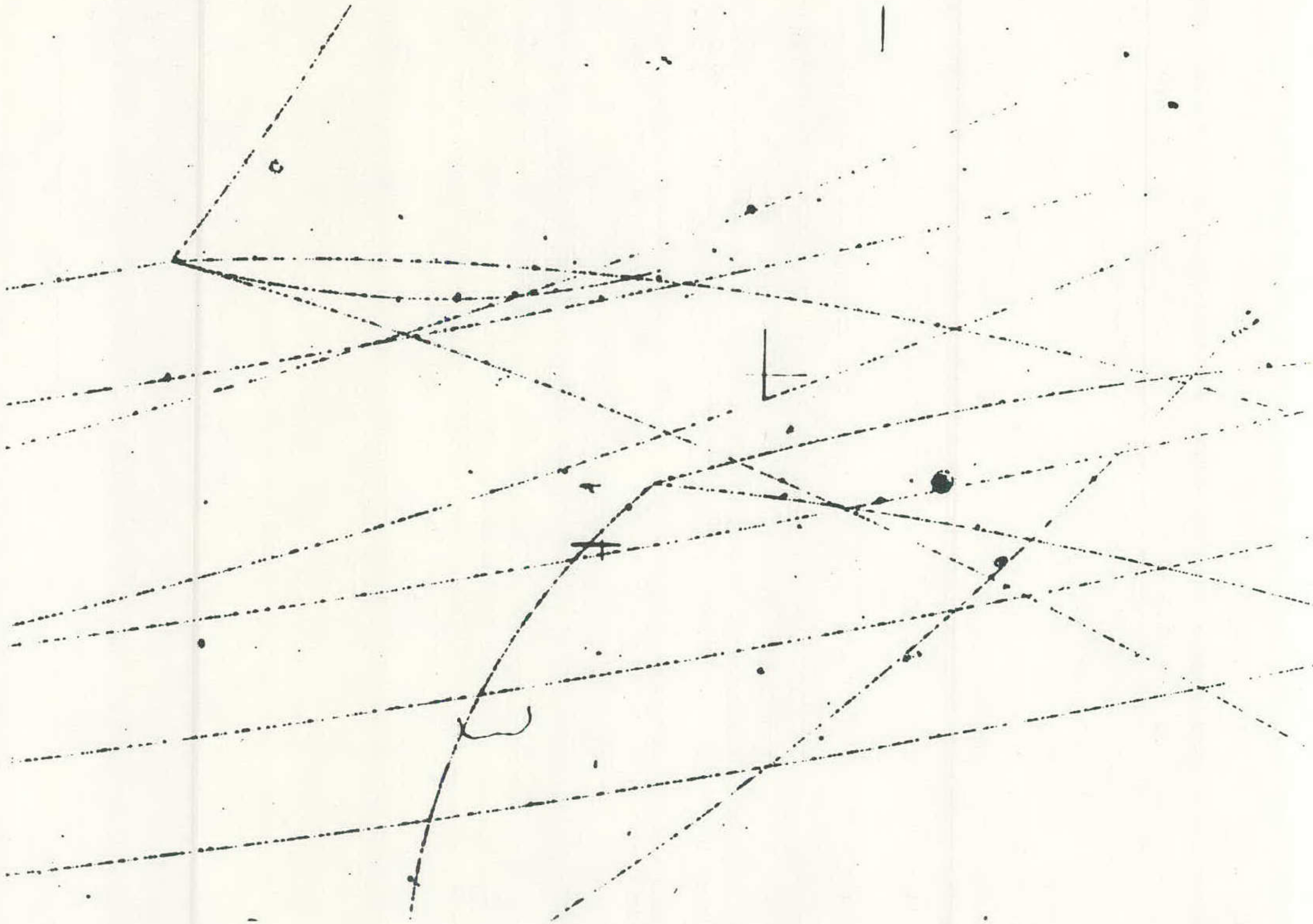


Figure 1

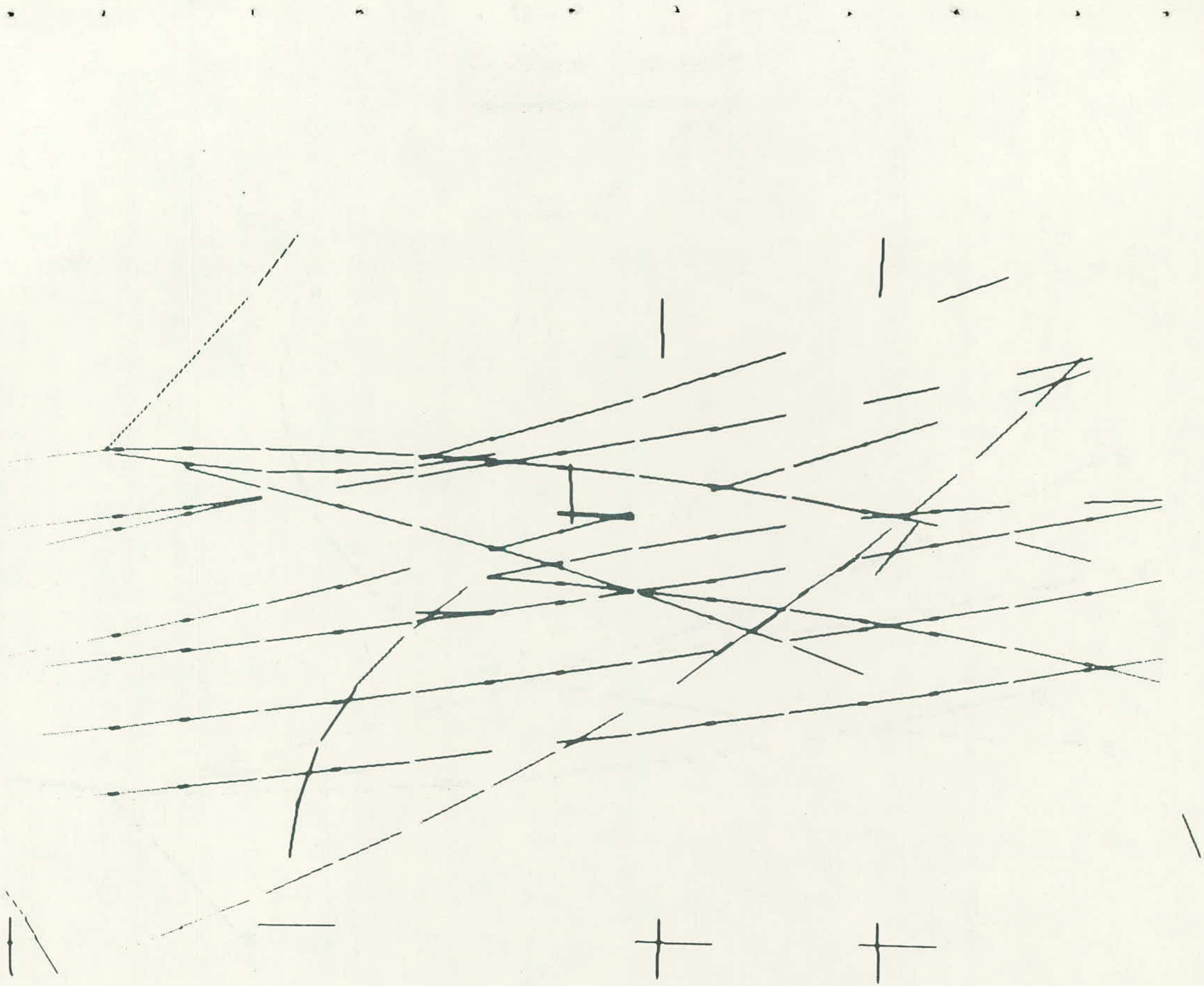
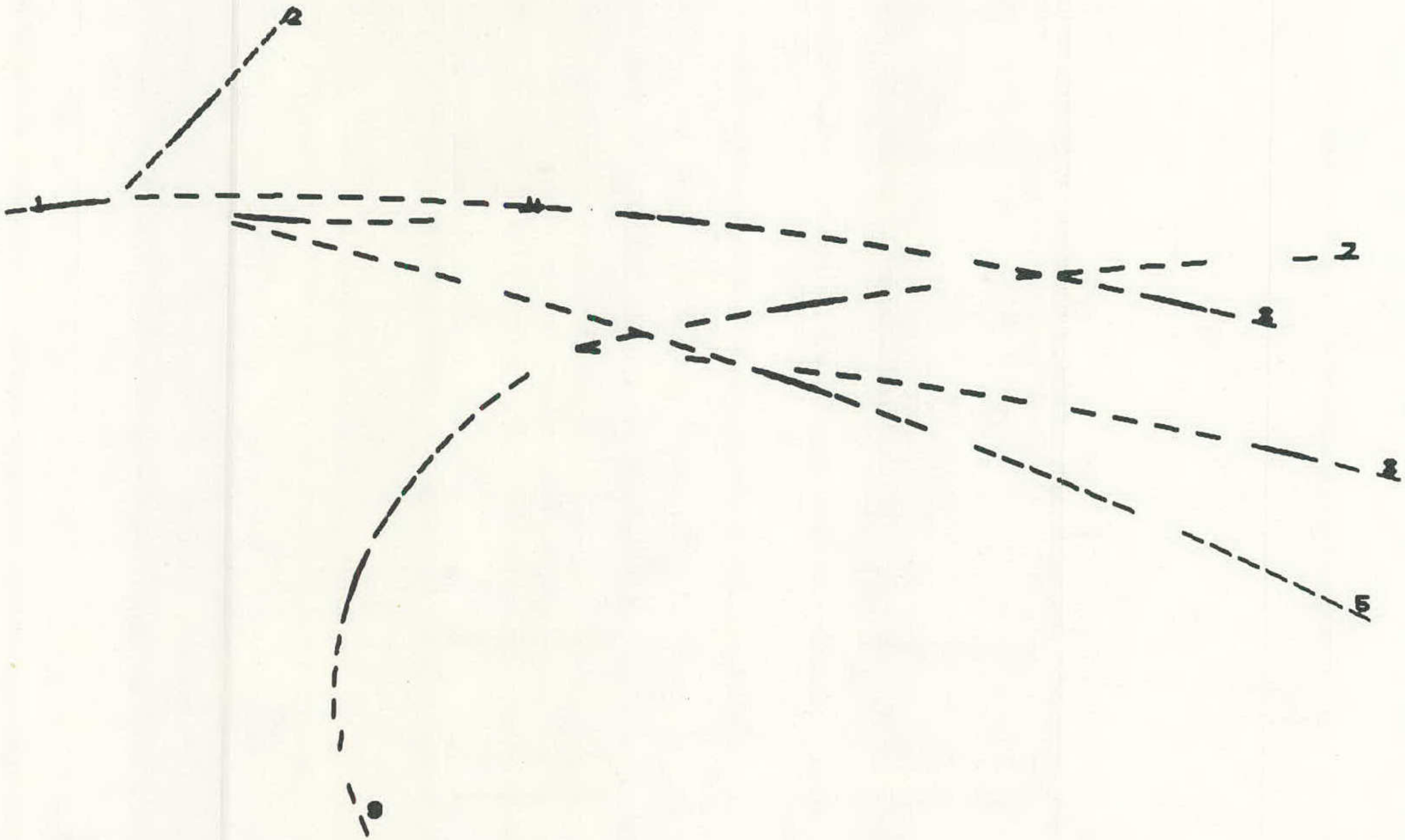


Figure 2



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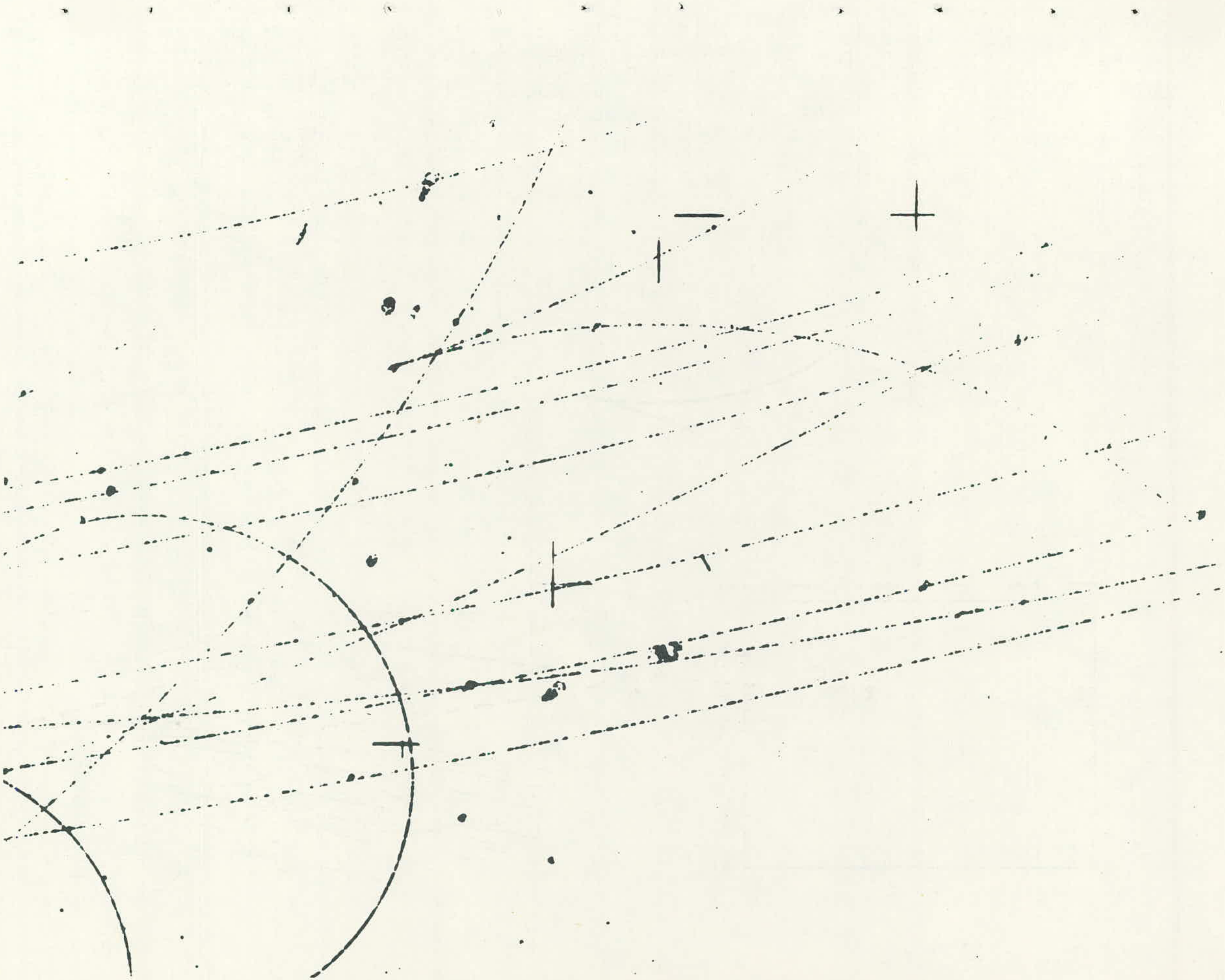
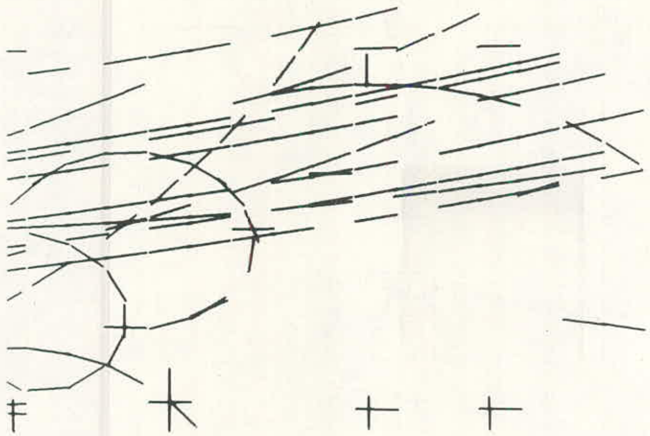
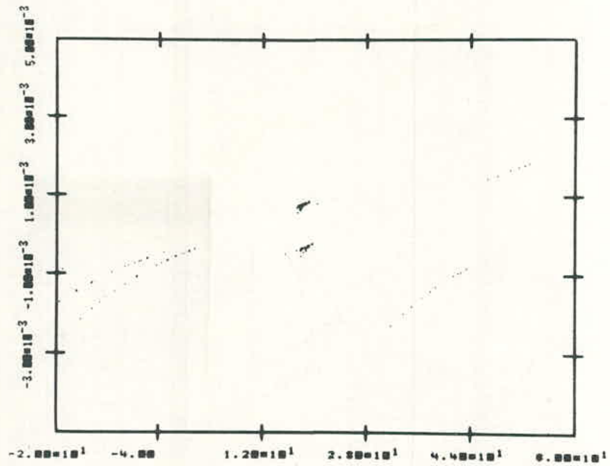


Figure 4



(a)


5.00×10^{-3}
3.00×10^{-3}
1.00×10^{-3}
-1.00×10^{-3}
-3.00×10^{-3}
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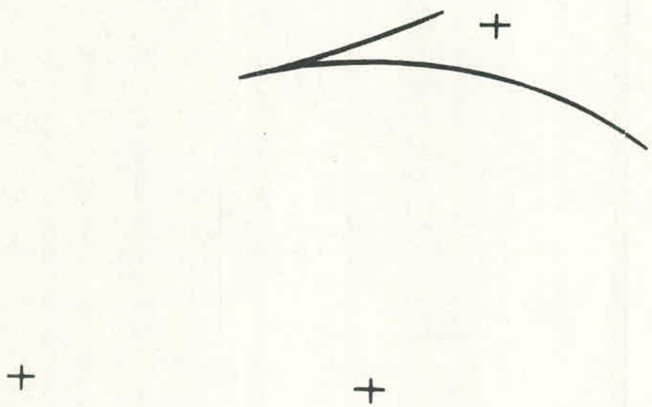
-4.00
1.20×10^{1}
2.80×10^{1}
4.40×10^{1}
6.00×10^{1}

(b)



(c)

Figure 5

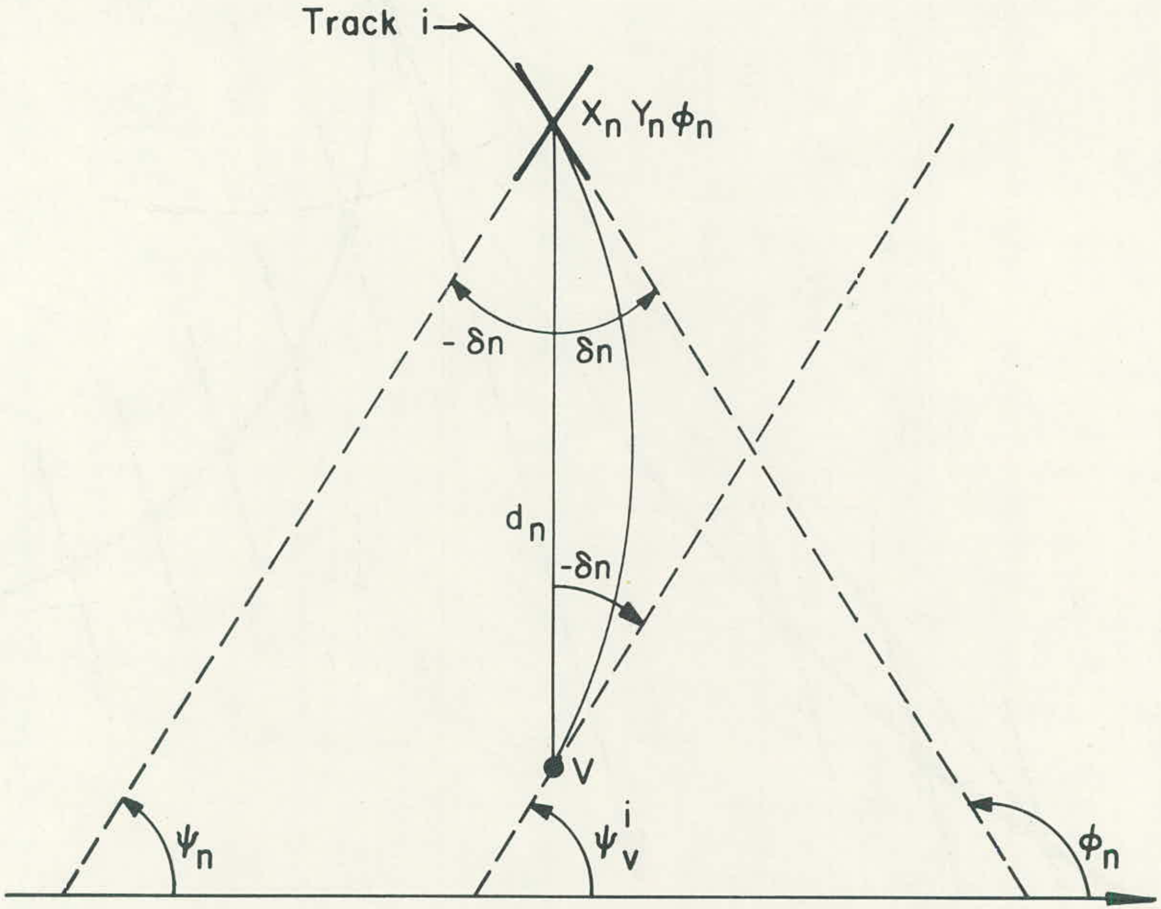


Figure 6

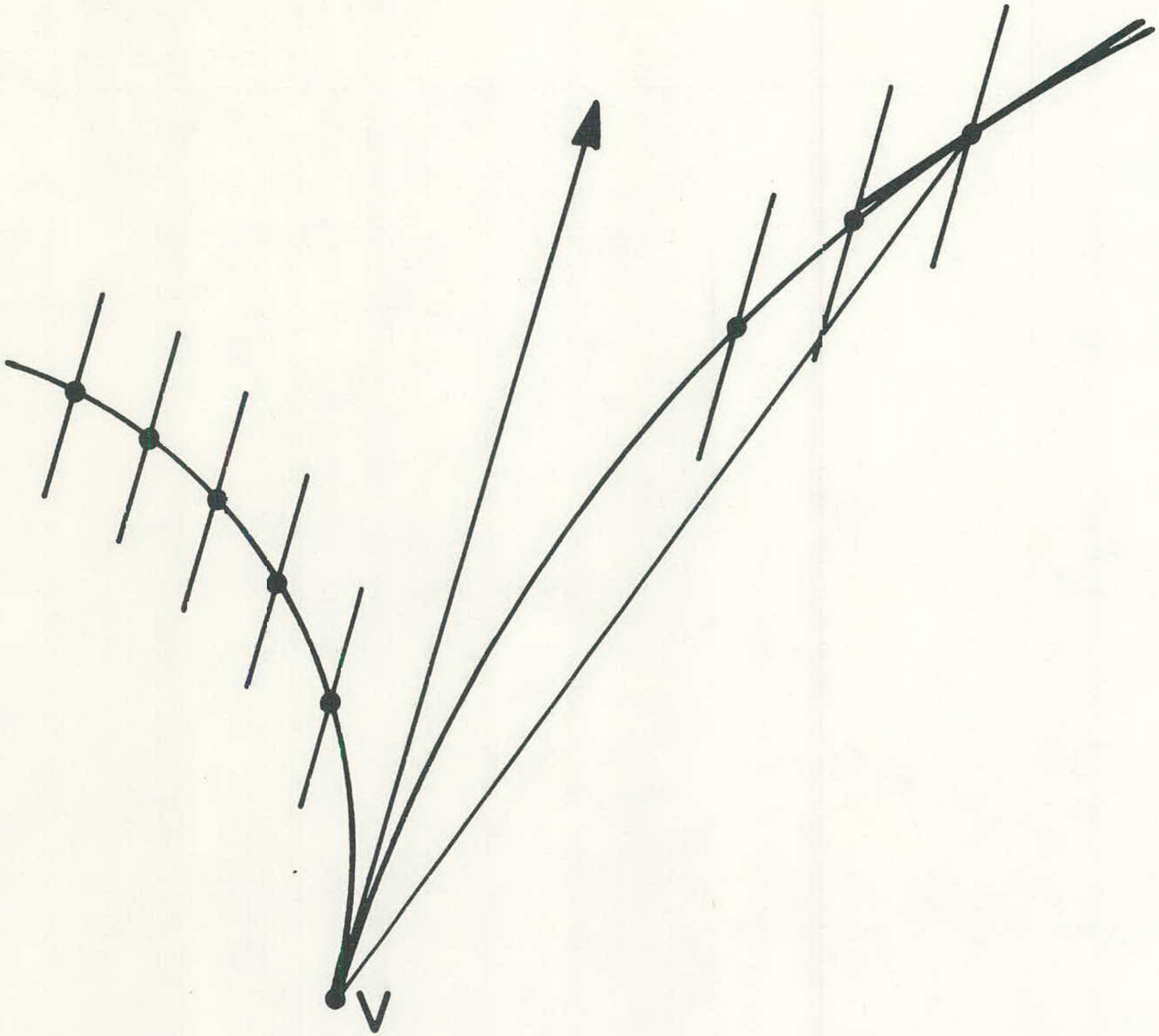


Figure 7

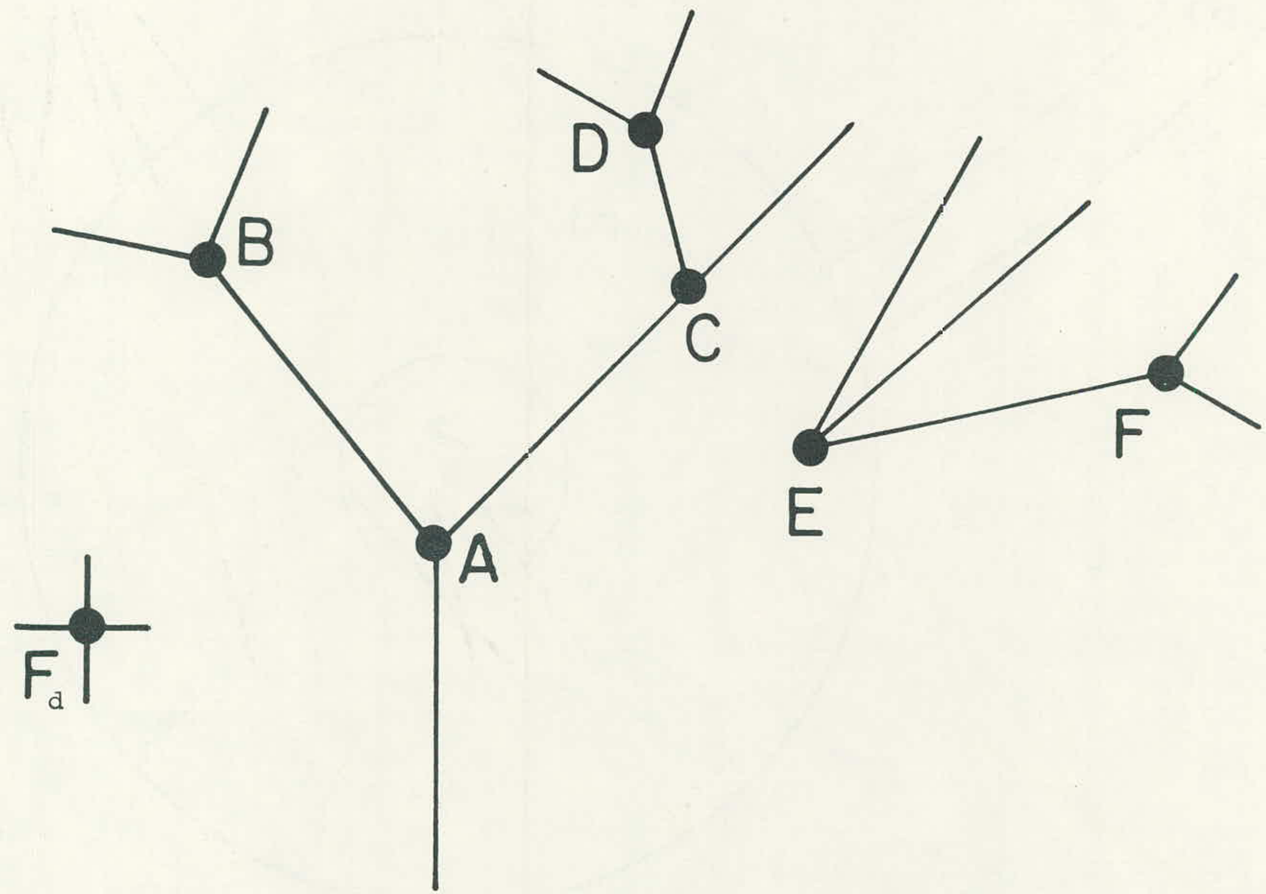


Figure 8

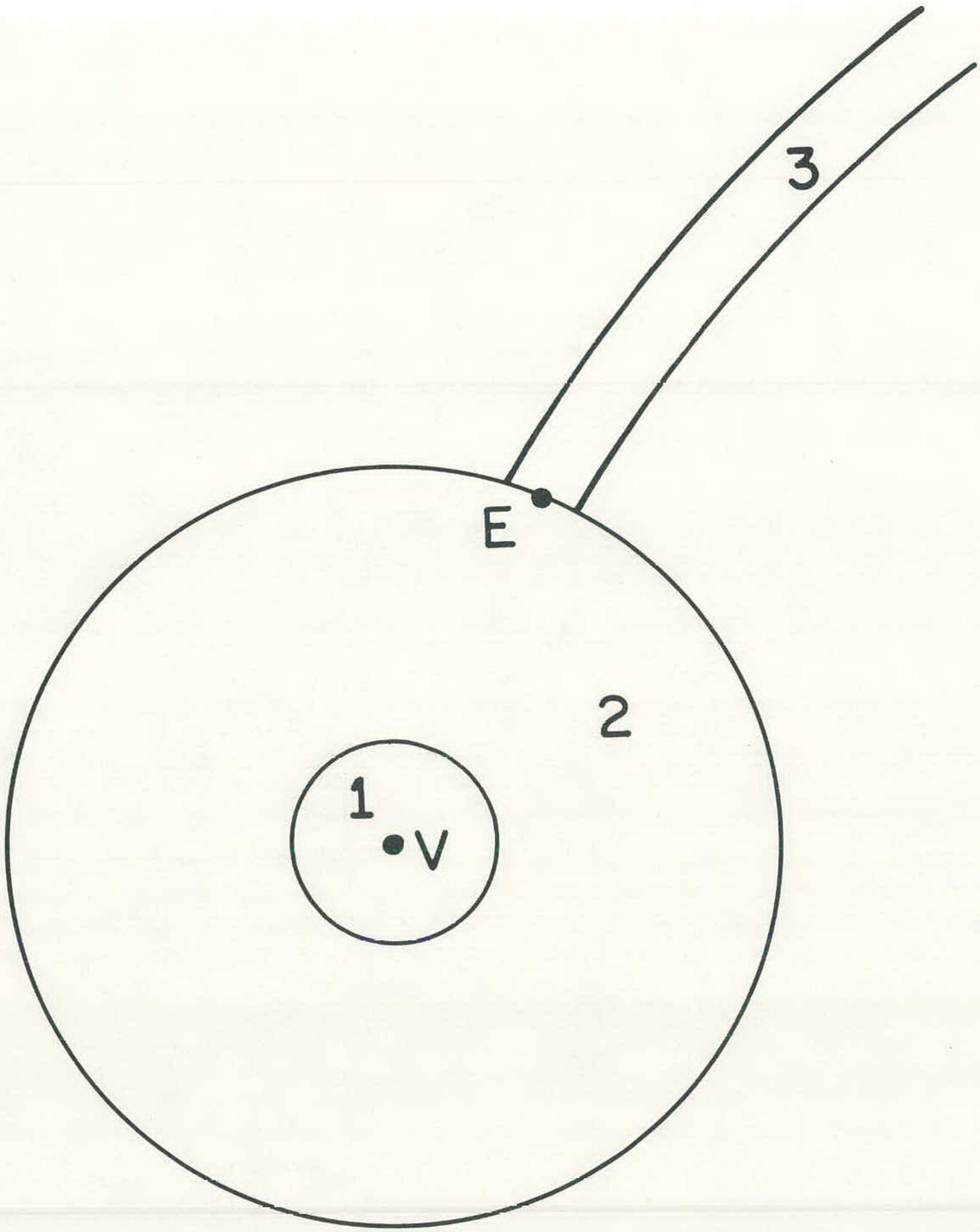


Figure 9

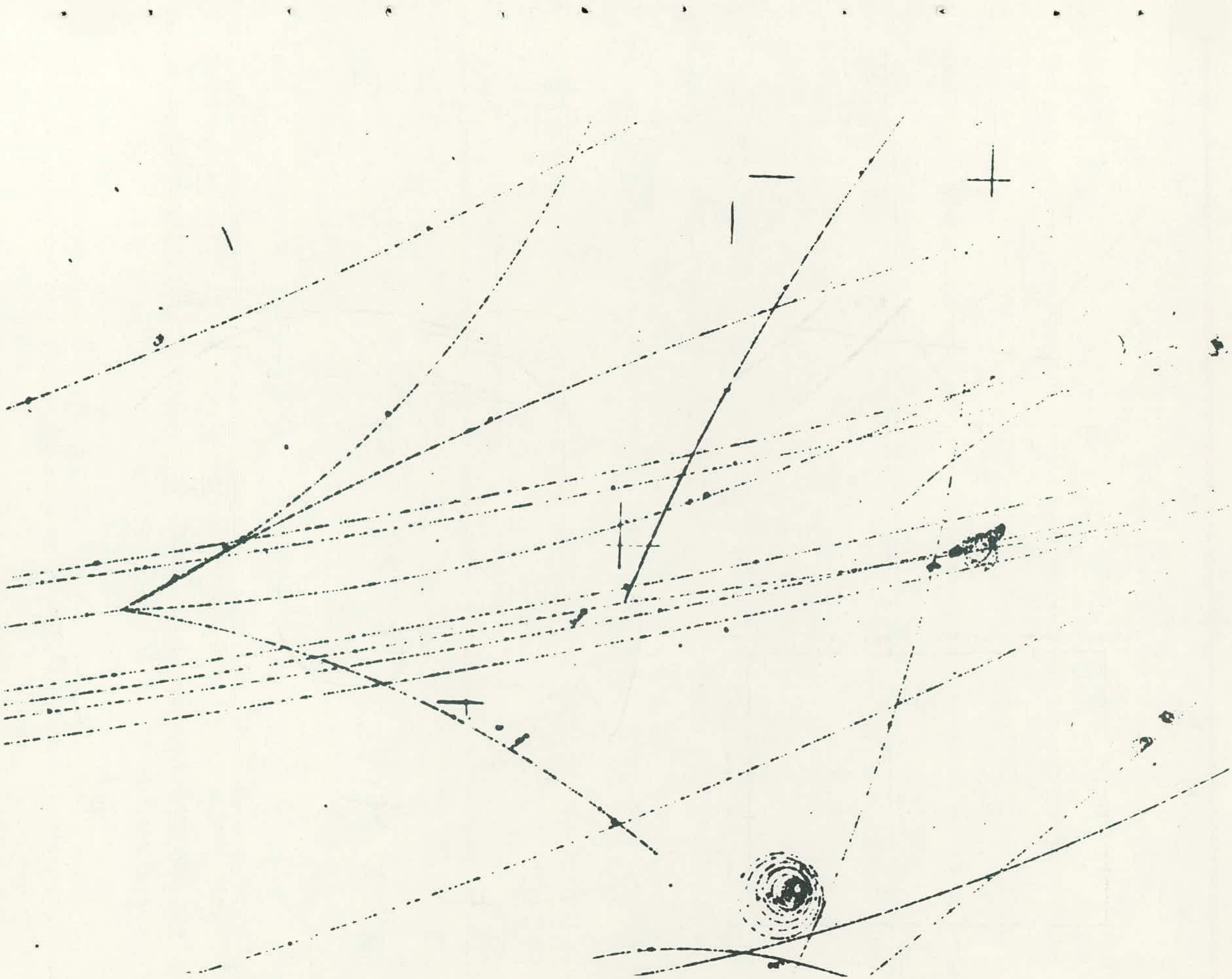
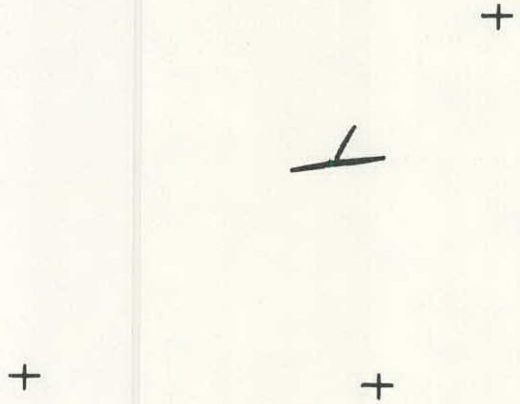
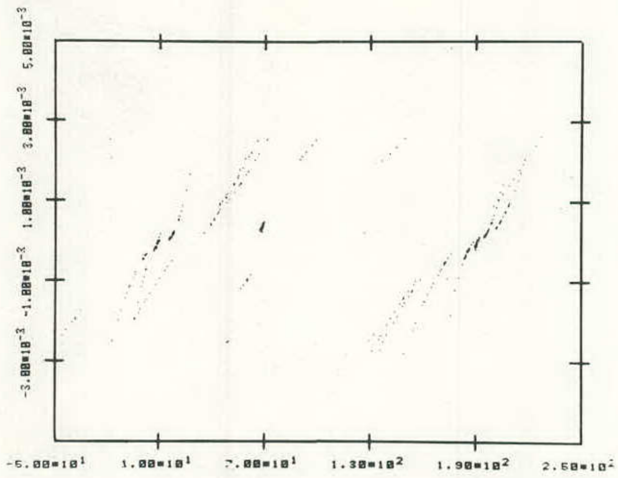


Figure 10



(a)



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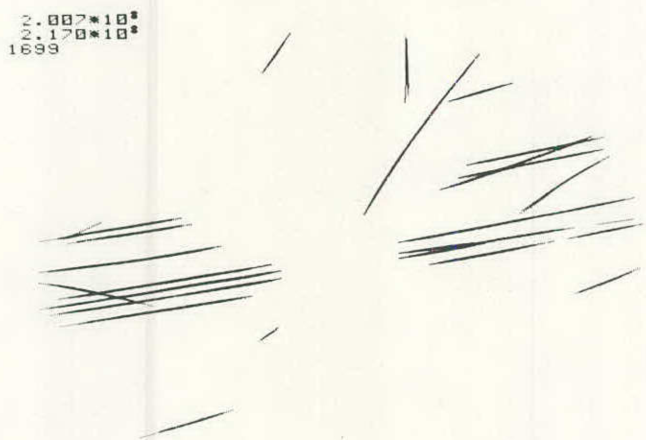

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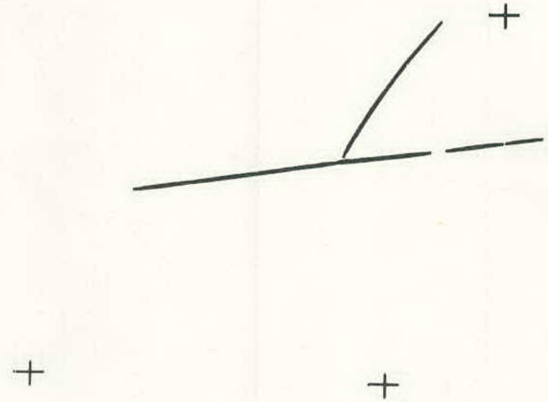
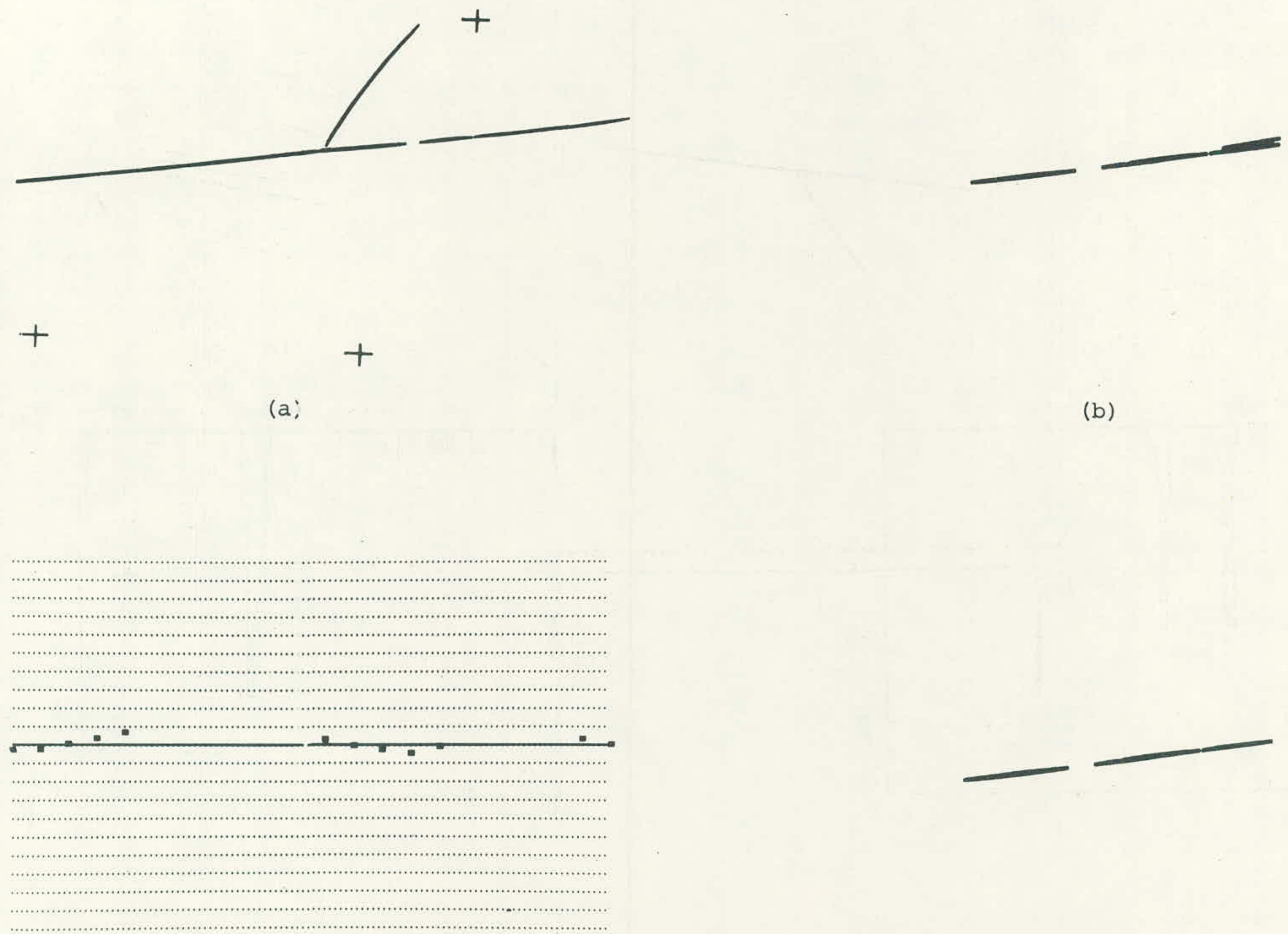


(d)

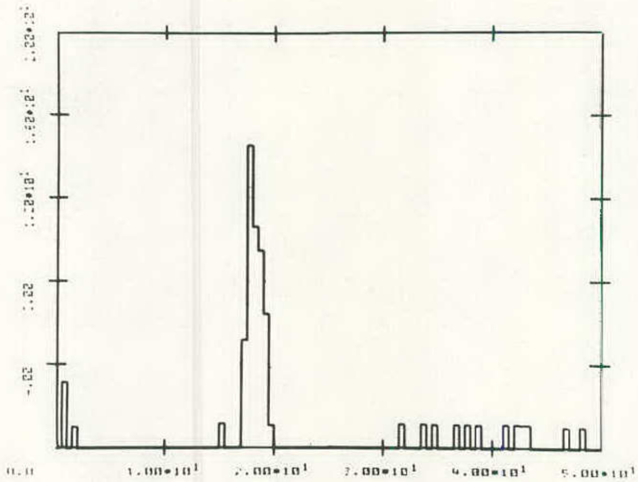
Figure 11



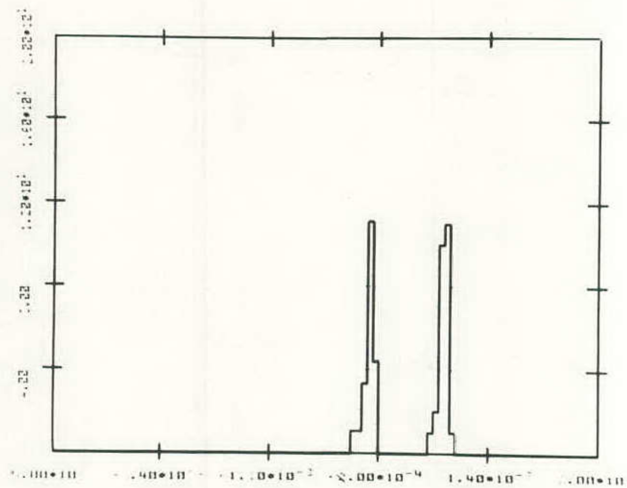
(a) (b)

(c) (d)

Figure 12

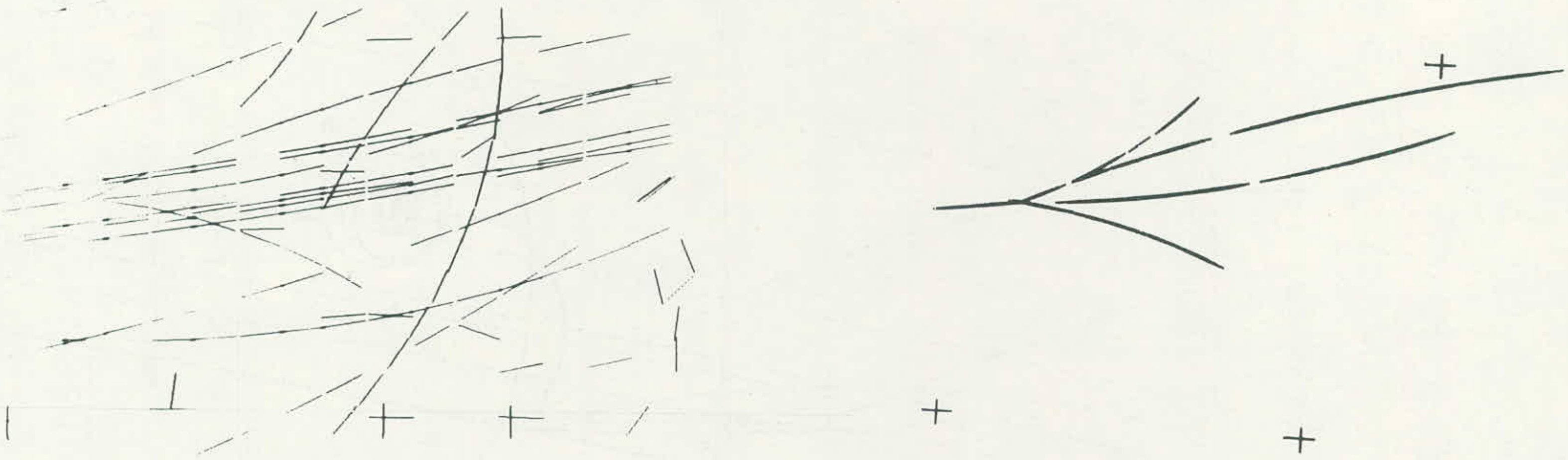


(a)



(b)

Figure 13



(a) (b)

Figure 14

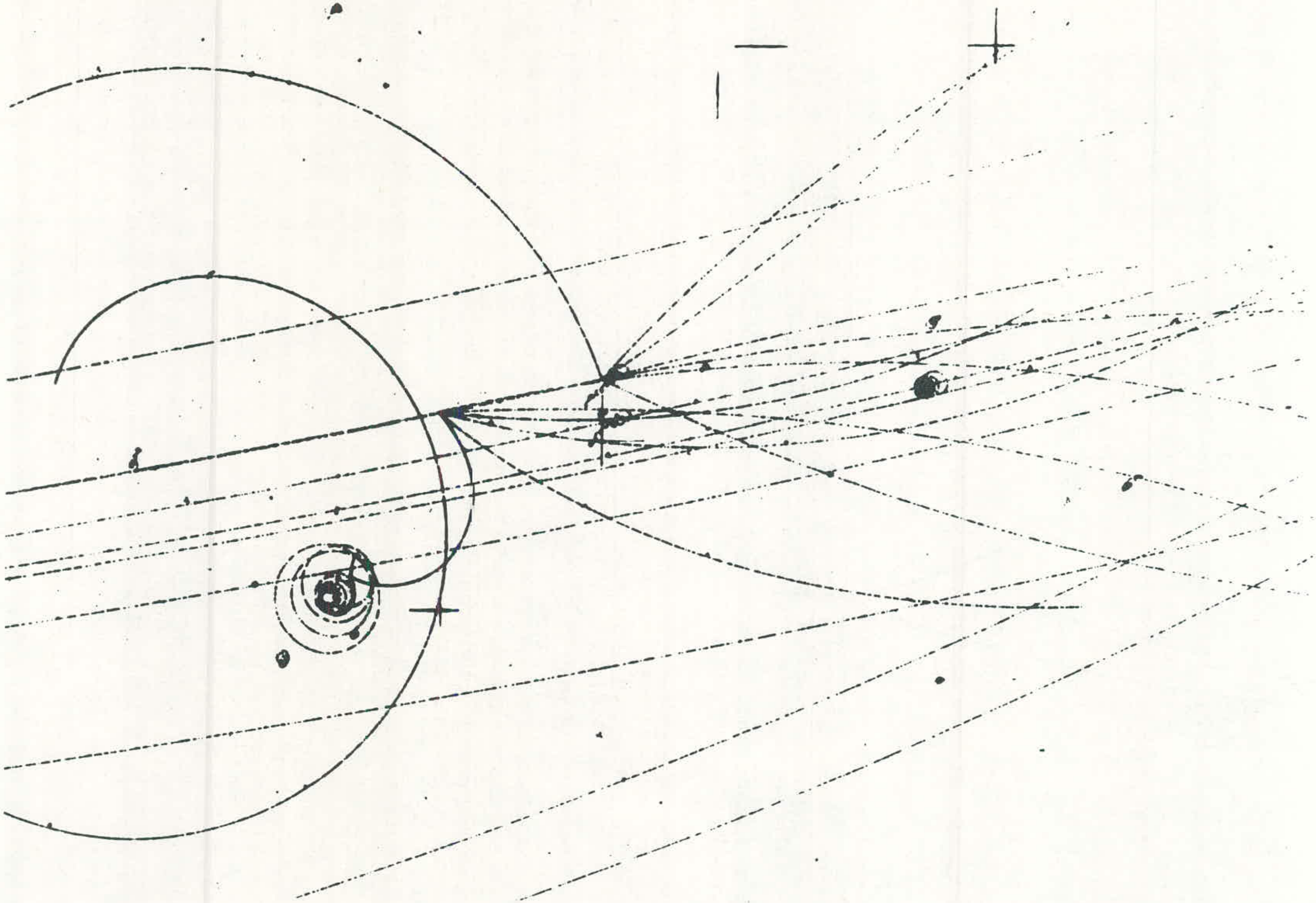
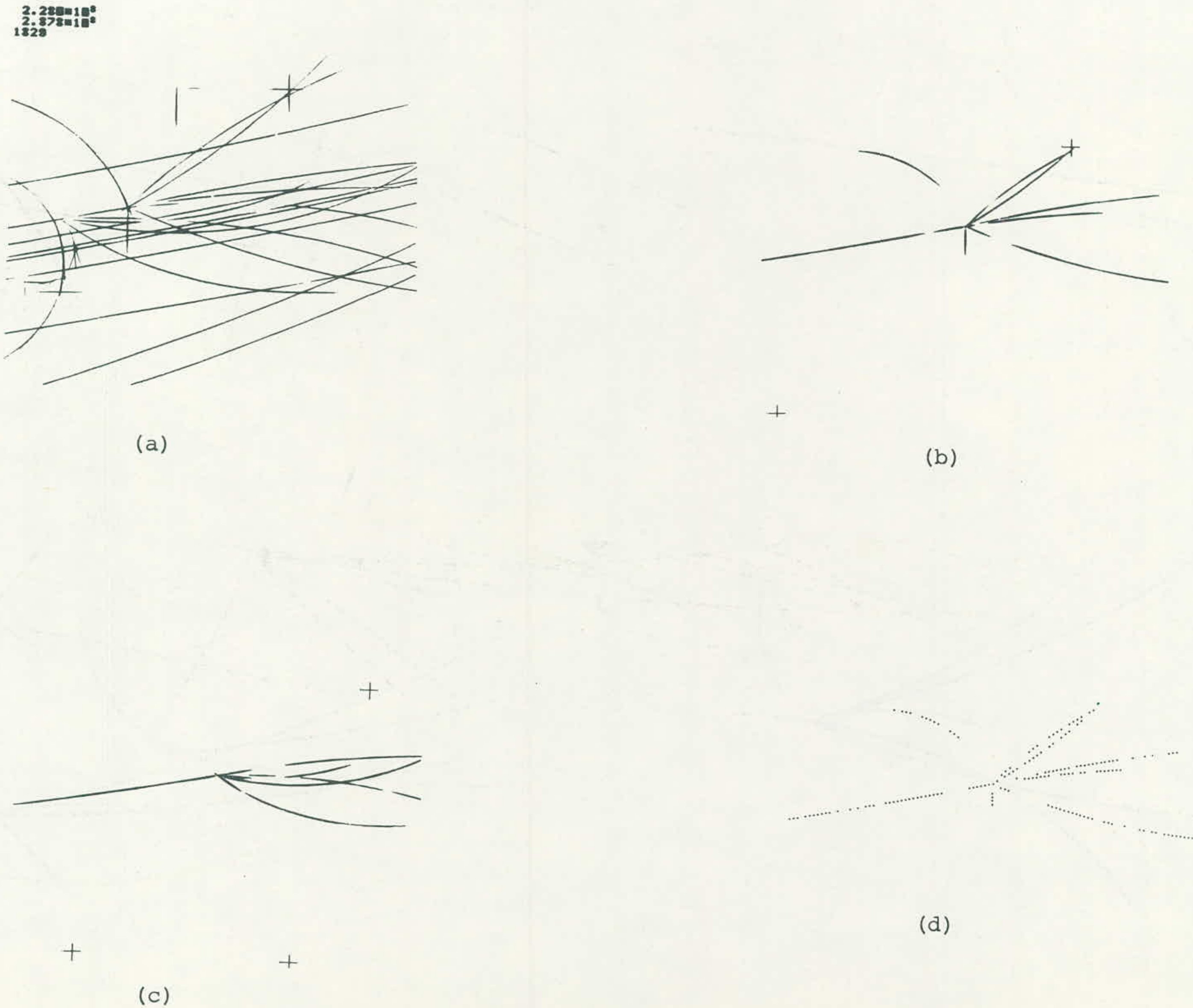


Figure 15



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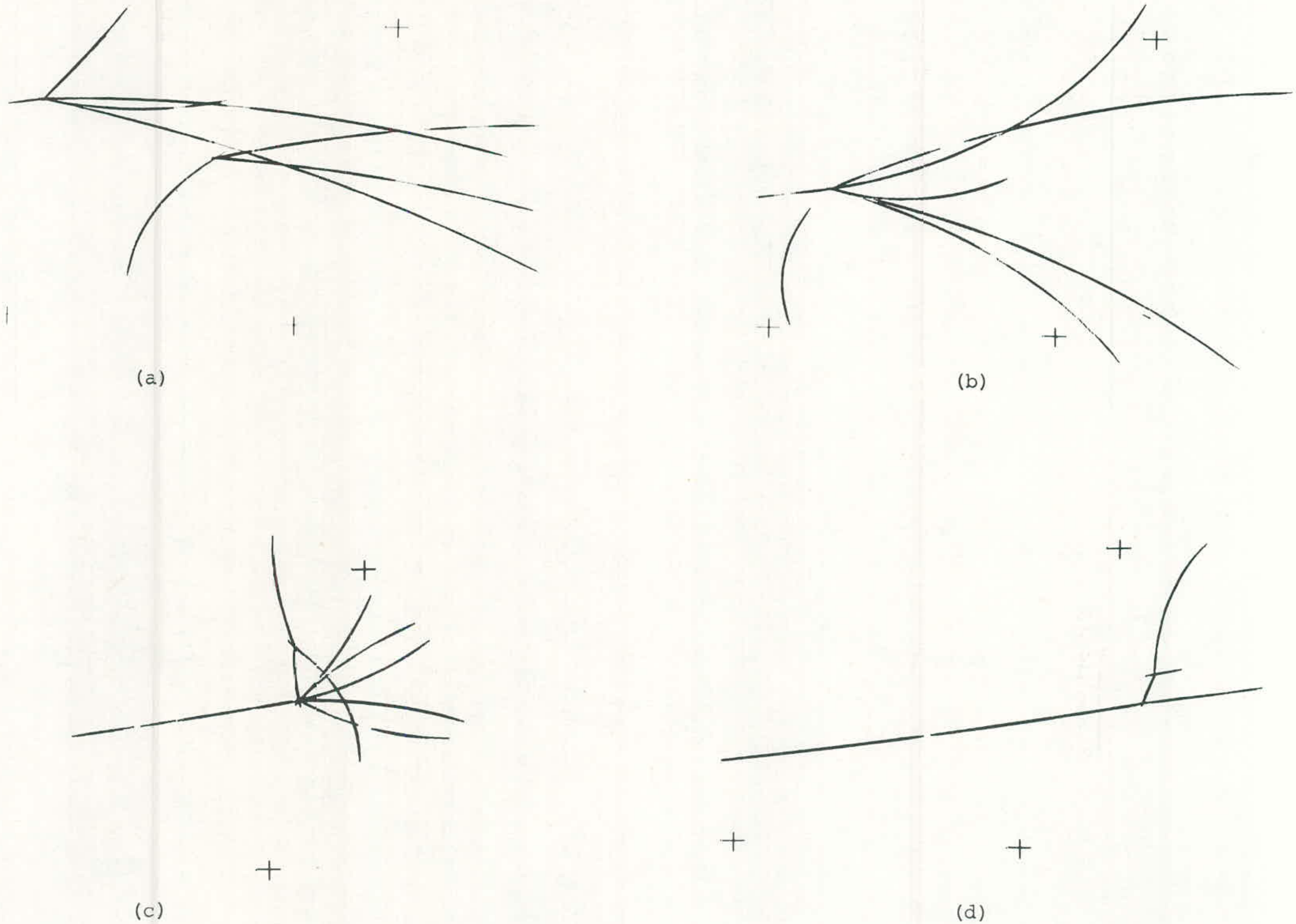
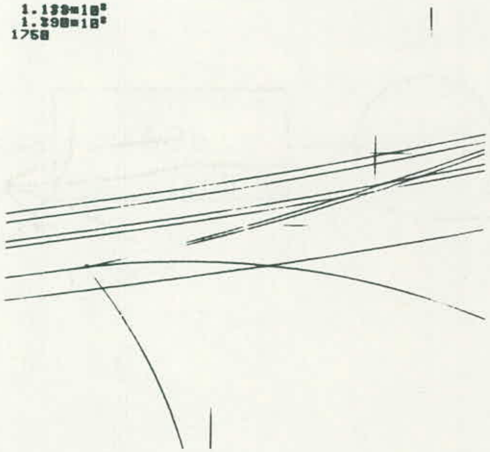
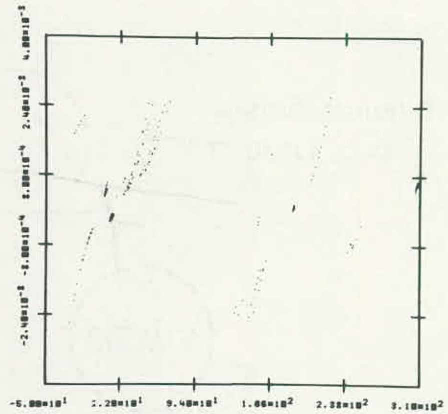


Figure 17



(a)



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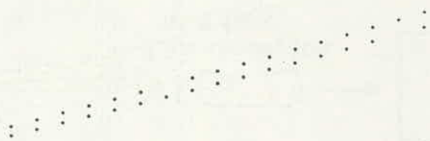


(c)

(d)

Figure 18

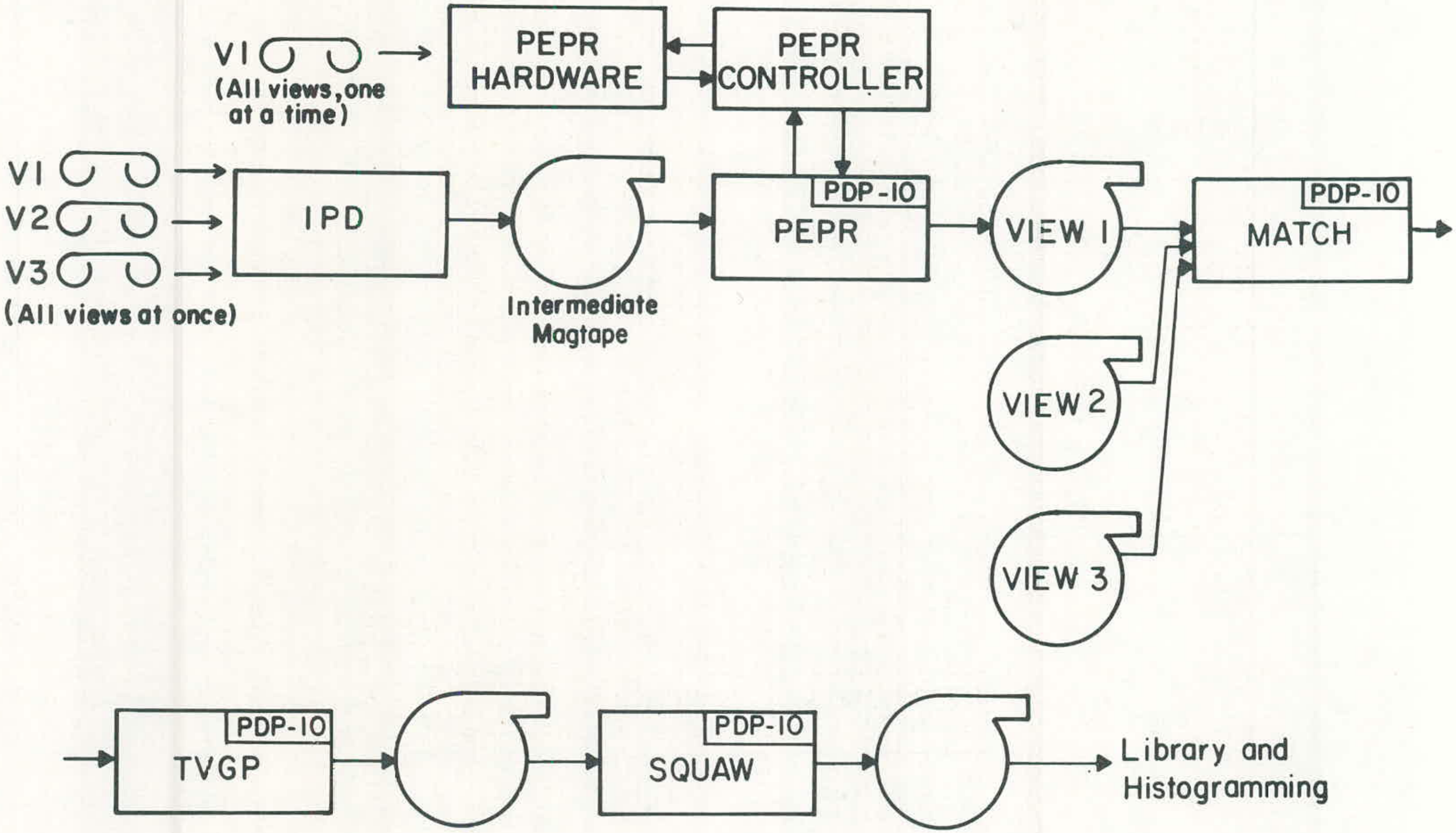

V1
(All views, one at a time)
PEPR HARDWARE
PEPR CONTROLLER
V1
V2
V3
(All views at once)
IPD
Intermediate Magtape
PDP-10
PEPR
VIEW 1
VIEW 2
VIEW 3
PDP-10
MATCH
PDP-10
TVGP
PDP-10
SQUAW
Library and Histogramming

Figure 19

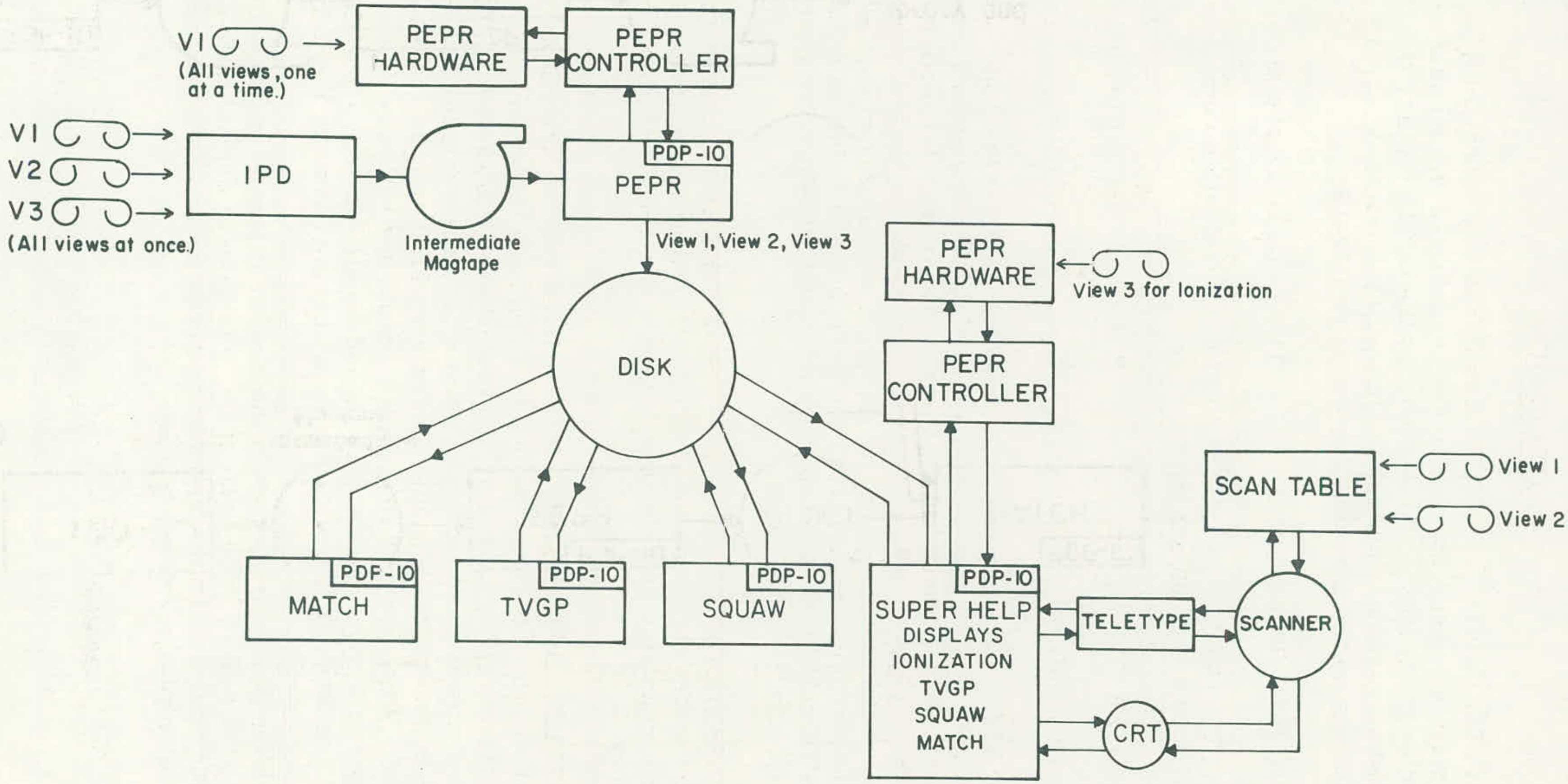

V1
(All views, one at a time.)
PEPR HARDWARE
PEPR CONTROLLER
V1
V2
V3
(All views at once.)
IPD
Intermediate Magtape
PDP-10
PEPR
View 1, View 2, View 3
PEPR HARDWARE
View 3 for Ionization
PEPR CONTROLLER
DISK
SCAN TABLE
View 1
View 2
PDF-10
MATCH
PDP-10
TVGP
PDP-10
SQUAW
PDP-10
SUPER HELP
DISPLAYS
IONIZATION
TVGP
SQUAW
MATCH
TELETYPE
SCANNER
CRT

Figure 20

DISCUSSION

WATTS: (MIT) I'd like to ask two technical questions. How sensitive is your phi plot to the accuracy of the measurement of the vertex point first, and second, on the circle display that you displayed, you had residuals plotted - what was the worst residual? How much in microns on the film was the worst residual?

BASTIEN: (Washington) The residuals were about 6 microns.

WATTS: (MIT) Why did the residuals go upwards on one side and downwards on an other?

BASTIEN: (Washington) These are not calibrated for pin cushion.

WATTS: (MIT) O.K. How about the first question.

BASTIEN: (Washington) We have a vertex point which is known to 100 microns on film and we make the scan in region 1 which eliminates a lot of background, and then we compute an intersection from the tracks. Then, we have a vertex search that searches for the vertex from all the tracks that we want, by moving the point around. You compute an intersection by minimizing the perpendicular distances to the tracks, it's an old Frank Solmitz routine called MINPRP, after that you have an accurate vertex point.

GLASSER: (Maryland) How sensitive is this to the case of reasonably steep tracks where the curvature on a projection changes quite markedly and also the coulomb scattering which also has the same effect.

BASTIEN: (Washington) I think that we are really going to have to look at a lot of tracks before I can answer this question, we have only 80 events we've looked at and the scheme works very well. The few steep tracks work up to the point where the curvature changes. Also, the magnetic field in this film is very high and the tracks curve quite a lot. When we took those area scans, we had only 2 mm line segments, which is a very bad thing to do and so all the very curved tracks did not register, but they certainly will if we have the 1 mm line segment.

PLESS: (MIT) On the classical beam track too close, this does not give a very good resolution, you have to depend upon knowing that the vertex point is on the right beam track because the rho and the angle are the same for all beam tracks.

BASTIEN: (Washington) Yes, what happens is that you have to locate this vertex by means of the Frank Solmitz fit, if you have very close beam tracks. May I say it is fortunate that sometimes if the tracks are too close they won't register at all in the PEPR hardware.

PLESS: (MIT) The point I am really getting at is that this system for a number of reasons hinges crucially on knowing that vertex and if, for example, in beam tracks too close, which incidentally in your roll of film, I suspect you don't have, because they were eliminated at the scan table.

LUBATTI: (Washington) Not true.

BASTIEN: (Washington) Not only that, we took all the events, in other words we took the $\bar{p}p$ scanning rules that were passed out for our $\bar{p}p$ collaboration and we took all the events in the fiducial volume, plus some difficult events to see how the system would perform.

LUBATTI: (Washington) Pierre, your point is that when you make your small scan at 100 main deflection counts you have that 3 degree cutoff automatically throughout.

BASTIEN: (Washington) Yes, since two tracks will not register if they are closer than about 40 microns, we chose a 1000 microns radius which corresponds to approximately 3 degrees and so this is where we have chosen the 3 degree cut. All other beam tracks are certainly going to be wiped out by this count unless they are really very close. Of course, if they are overlying then there is nothing you can do.

PLESS: (MIT) It also is just a question about how close is close.

BASTIEN: (Washington) How close is close is probably 50 microns. There are other ways to work on this problem. You can use something very equivalent to element recognition to separate two tracks that are very close, by replacing N vs phi space by x vs y space. For tracks 30 microns apart we use a trick that is exactly like the element recognition and we can separate close tracks and afterwards go to MINPRP and calculate the best intersection. From this you will get the best possible vertex intersection by minimizing the sum of squares of the distances. This is very powerful.

VAN de WALLE: (Nijmegen) On these phi histograms that you showed using data measured by the M.I.T. PEPR, did you use the IPD vertex or the PEPR measured vertex to make them?

BASTIEN: (Washington) We used the IPD vertex. This is a whole new system, we measured the fiducials from the area scan, in fact we wrote our own fiducial locator, and then we computed the expected vertex. For those familiar with it, PREP is not included in this system, it's now a very short subroutine in the system.

PLANO: (Rutgers) I don't understand your numbers, if the vertex is known to 100 microns and line element is a 1000 microns away, you'll have an error of 6 degrees, you cut at 3 degrees I believe?

BASTIEN: (Washington) Yes, 2 or 3 degrees.

PLANO: (Rutgers) You pick up the wrong beam track do you not?

BASTIEN: (Washington) I think maybe I am wrong in the number of 100 microns, it's more like 5 main deflection events namely 60 microns, 100 microns is the worst case. When this happens, the elements close to the vertex will be wiped out by the cut.

COX: (Johns Hopkins) You said that you need 100 microns accuracy, you don't know quantitatively how much the phi plot spreads with that inaccuracy?

BASTIEN: (Washington) I don't know yet.

COX: (Johns Hopkins) Your method can be used in absence of knowledge of vertex just taking any point on the track it seems to me, although the number of calculations you have to do expands by considerable amount. The question is, what kind of time do you need, typically, on the kind of events that you're looking at?

BASTIEN: (Washington) To process these events here takes 4 seconds, but I don't know what that number means, because some parts of it are very much shorter than if we were on-line and some parts of it are very much longer for reasons which I don't want to develop. I don't really know how long this whole thing is going to take but I suspect it would take conservatively 10 seconds on the PDP-10.

COX: (Johns Hopkins) This is just to do the sorting with the 100 micron knowledge of the vertex point?

BASTIEN: (Washington) No, this includes everything.

COX: (Johns Hopkins) The question I am asking is specifically, how long does it take to do that calculation to form the psi and rho histograms?

BASTIEN: (Washington) I am sorry, I don't know, but we'll find out.

COX: (Johns Hopkins) Do you have any idea how this compares with, say, point guidance or with a more standard system?

BASTIEN: (Washington) I am sure the point guidance would be faster.

COX: (Johns Hopkins) But the methods of track following and track element grouping that are used standardly say by Yale, would be much slower in your opinion?

BASTIEN: (Washington) I have no feeling for what those programs do, but I would certainly say that the point guidance system would go much faster.

DAY: (Maryland) Could you tell me what are the distinct differences between this vertex guidance, and just searching in circle around the vertex more or less perpendicular to the arc with subsequent following of all the tracks back? That is, like POLLY does or like we do?

BASTIEN: (Washington) I think the point is that this transformation gets you all the tracks, straightens out all the tracks.

DAY: (Maryland) If you look along the radius to a circle around the vertex then doesn't that also filter out just the tracks that are pointing more or less back to the vertex?

BASTIEN: (Washington) That's what we're doing.

DAY: (Maryland) What is the transformation doing, that recognizing a radial line from the vertex with plus or minus 5 degrees with the PEPR hardware doesn't do automatically for you?

BASTIEN: (Washington) But these tracks can curve 60 degrees, how are you going to find those?

DAY: (Maryland) No, in close.

BASTIEN: (Washington) It is like a spiral reader scan, except we are slightly better off because we have the better angle resolution, but in the small area it is exactly like a spiral reader scan.

DAY: (Maryland) Then why not follow those tracks out, or do you leap way out?

BASTIEN: (Washington) Fine, you could follow the tracks out.

HULSIZER: (MIT) I'd like to ask the same question another way. Is it, am I correct in assuming that you're saying that your scheme, this global scheme would follow tracks out through difficult areas better than a system that started from a vertex and had to track follow all the way to the end of the track?

BASTIEN: (Washington) Yes, let me make something very clear, other people have developed the systems that follow the tracks and I think these systems are fine, I wrote one like that a very long time ago, in fact, we had a vertex guidance scheme that was working reasonably well in 1963. I have experience with the point guidance system and I liked it too. Personally, I think this is simpler, because there are no special cases.

MULVEY: (Oxford) Perhaps make one comment. Horace Taft and I have been talking and it may be that although we have the situations in which, given the vertex, we know there are lots of techniques which have been successful at finding tracks from that vertex, POLLY and others, perhaps this could be a very powerful method if you have unfortunate tracks which are split up by lots of very bad regions so that you don't get good leverage on any small section. But I think, one comment I was going to make and which other people have made, is that if you use this for your total strategy, it seems that you are spending a lot of time picking up data on tracks which eventually aren't going to be any use to you.

BASTIEN: (Washington) That's correct.

MULVEY: (Oxford) Now if one starts out knowing the vertex then, as POLLY and other systems have tried, you might as well make use of that fact. If you get into trouble then you might try this scheme in a given direction to see if you can find the track that's really going through all the piles of mud.

BASTIEN: (Washington) Now what you're saying is fine, in fact, Prof. Plano pointed exactly the same thing out to me. He said if you have the segments of tracks close in to the vertex, why not go immediately into a road scan defined by each of those segments, instead of making a complete area scan. I think that's a fine idea. We will sort out the elements found in the road scan using our transformations.

COUNTER CONTROL OF BUBBLE CHAMBER PHOTOGRAPHY

I.A. Pless

MIT

The work that I am going to talk about is to try to solve the pattern recognition problem from the other end. In all the effort to date you are given the film and then you try to solve the problem. We have tried to do something about the film before it is taken. The work I am going to talk about was done by a collaboration of people from M.I.T., SLAC, and the University of Washington. The program was conceived at M.I.T. and was under the direction of Henry Lubatti and he continued directing this work when he moved to Washington. Joe Murray, Roger Gerhard were the collaborators at SLAC and Bernie Wadsworth Don Goloskie and Eugenio Sartori, Dave Brick and myself were the collaborators from M.I.T.

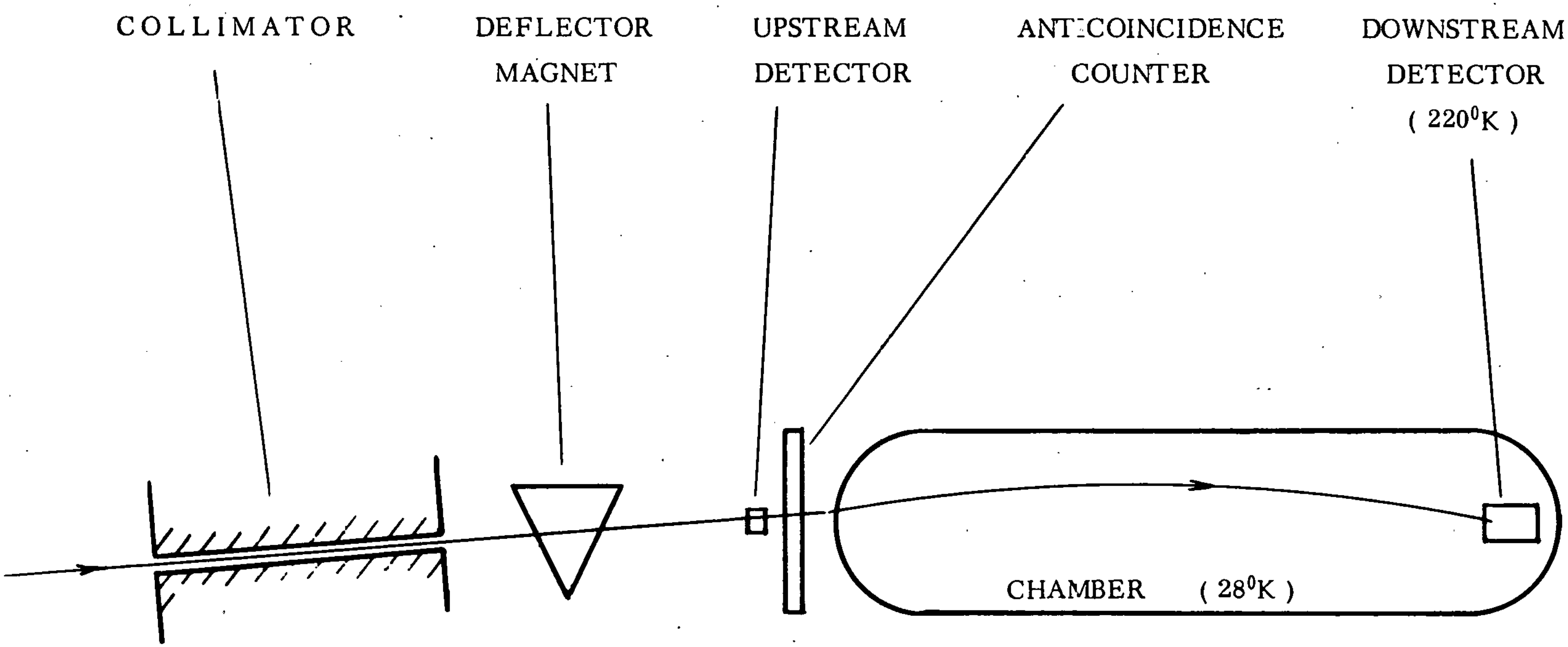
The idea of trying to do something about bubble chamber film before you take the picture is not new, both at CERN and at Berkeley people have tried to build stepping magnets that were pulsed every time a track came through the chamber so that you'd get an equal spacing of tracks. The attempt here is to do the same thing, but do it not with any stepping magnets but by selecting out pictures before you flash the camera. The test of this idea goes as follows: figure 1 shows the 82" chamber at SLAC which has a 6 inch stainless steel exit wall, so no particle will get through that wall without being slightly battered. Therefore it was required to put a detector inside the liquid itself, the only detector that had any hope of working was a solid state detector. Such a detector doesn't like to work at liquid hydrogen temperatures, so therefore it was protected from the liquid hydrogen by a little can with super insulation. We call this the inside out beam finger, since this technique was used, inside out, as a liquid hydrogen target in a methyl-iodide chamber, where the bubble chamber was hot and the target was cold. So we have a solid state detector essentially at 220 degrees Kelvin, the chamber at 28 degrees Kelvin. Upstream we have a collimator, (a long iron slit that has essentially 1/4 inch width and 1/2 inch height) and we have a deflecting magnet that steers the beam through the defining counter. We have a big anti-counter so that if there are any muons, which in fact there were, that slip by outside the defining counter but come through the chamber simultaneously with the beam, we know about it. Finally comes the solid state detector. As you can imagine, since this solid state detector is only 2 cm. in diameter, and the defining detector was ultimately 3/8 inch in diameter, there was a lot of maneuvering around with respect to the position of the slit, the magnet and the defining counter. You stick the solid state counter into the bubble chamber in some undefined place and then you move these others around until you hopefully land the beam upon it.

The logic is the following: If you have one track that comes through the defining counter, no particle through the big anti-counter and no count in the solid state detector then you take a picture, the idea being that if you have no count in the solid state detector, that particle must have interacted and must have been deflected from its path. If you are 100% successful, every time you take a picture you'll have an event in the chamber and nothing else - an incoming track and an event. Figures 2-6 show some of the results. It took 20 expansions to get each of these pictures. Figures 2, 3, 4b, 5a for instance, show a single track and an interaction. Figure 4a shows two tracks; in other words, in this sense, this picture, although it has a lovely event, is a failure because we have two tracks in this view. Figure 6a has an interaction in the wall of the chamber which you can't help. Figure 6b shows a failure, probably there may be an interaction in the wall, but nevertheless you do not have any visible interaction and so the system didn't work. Figure 5b shows a straight track. The anti-counter did not completely cover the whole entrance of the chamber so this track sneaked by the anti-counter.

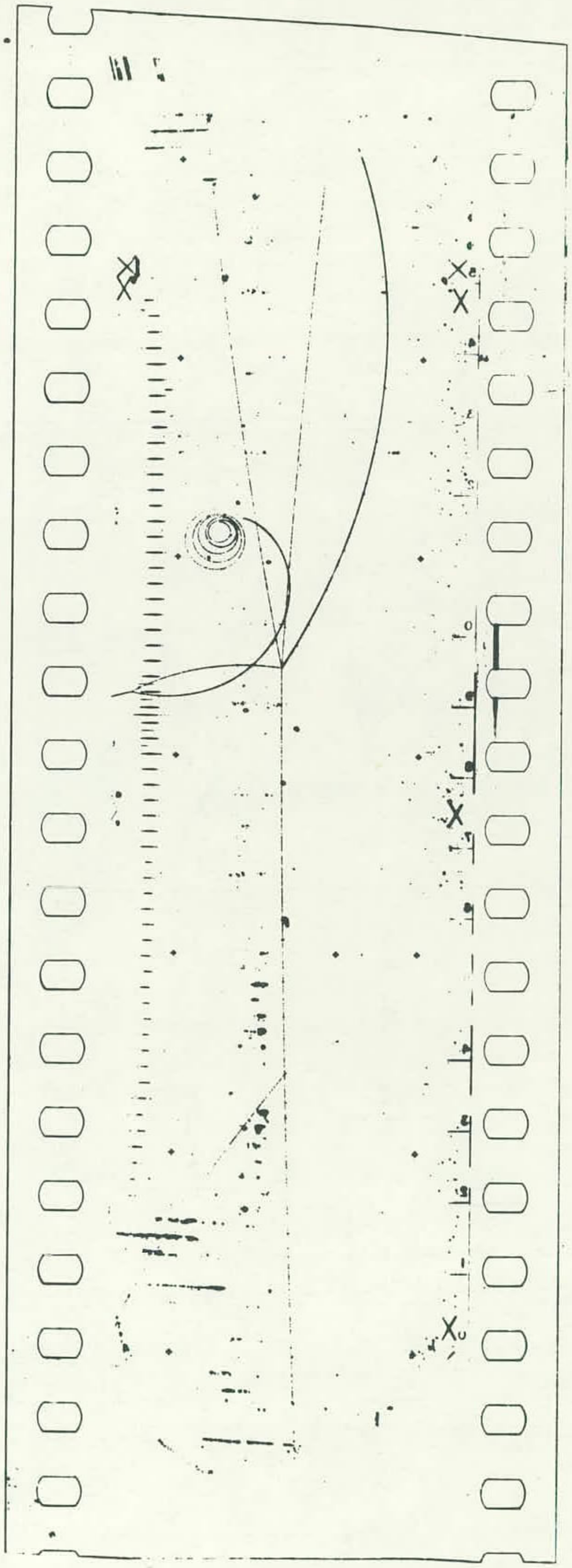
The comment that I want to make is that we set up this system with one counter just to test the feasibility of aiming the beam at the solid state detector. If you had a chamber that would go 40 times a second or 20 times a second then you would get such a picture once a second and that would be a very nice event rate. Since the SLAC machine goes 360 times a second it wouldn't bother it at all if we took 40 pulses per second. However, the chamber cannot do that, so therefore in terms of chamber time, the present system is not useful, it just takes too long to get the number of events. Therefore the concept that we are now pursuing is to put 5 of these solid state detectors equally separated across the back end of the chamber, with 5 collimators and some logic. The main idea now would be if that in any one channel you have an event you take the picture, and you might have up to 5 equally spaced tracks going through the chamber. If you work through the numerics that doesn't happen but basically what we would expect is about once every fourth expansion, therefore, on the SLAC chamber, once every 2 or 3 seconds, one would be able to get a picture of this same quality. Therefore the

the problem of beam tracks too close, or confusion of the beam track with a downstream track, hopefully would not be so important as it is now.

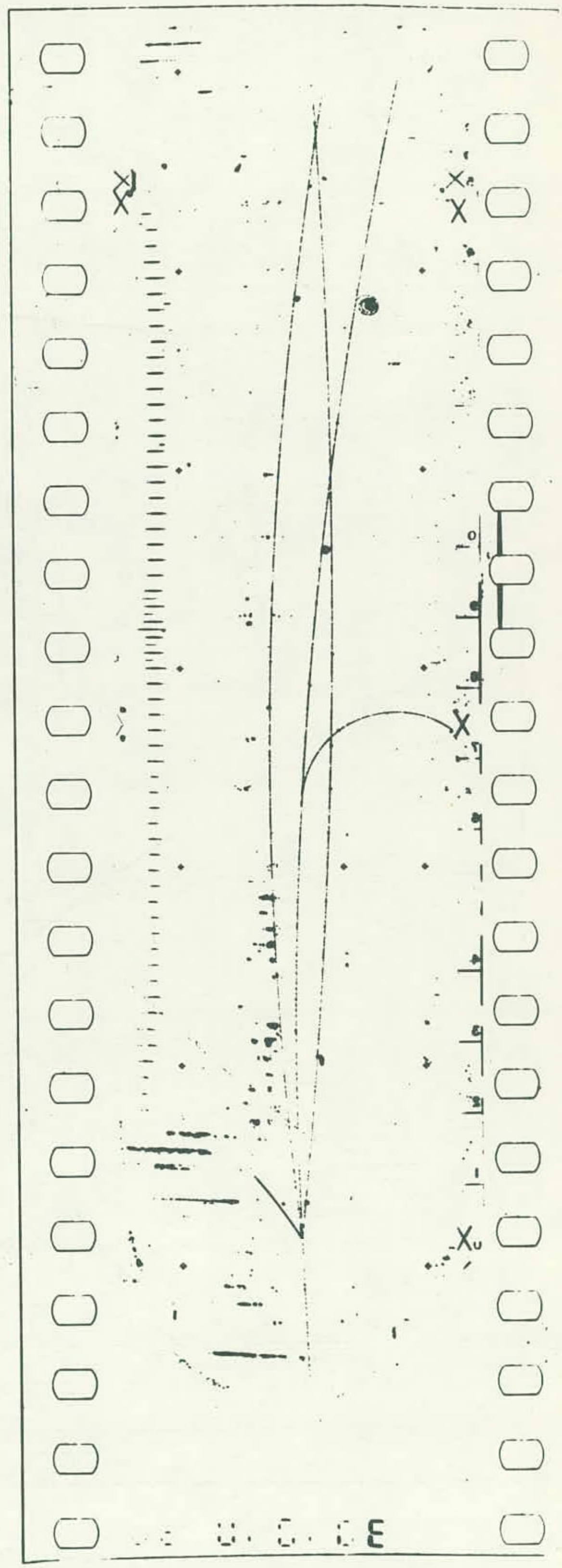
FIGURE 1



ARRANGEMENT OF COUNTERS FOR PHOTOGRAFHY CONTROL
AT SLAC 82-INCH CHAMBER.

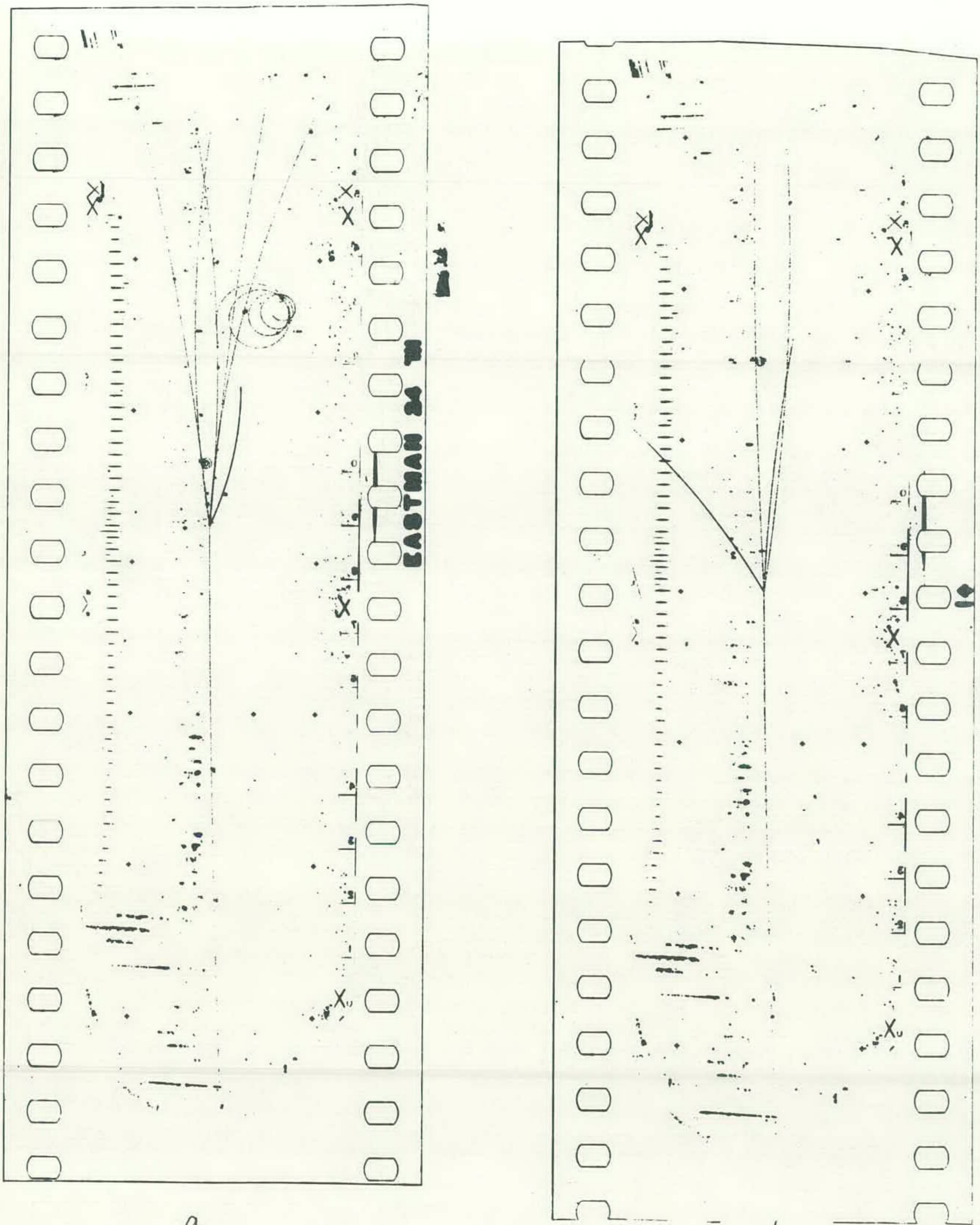


a



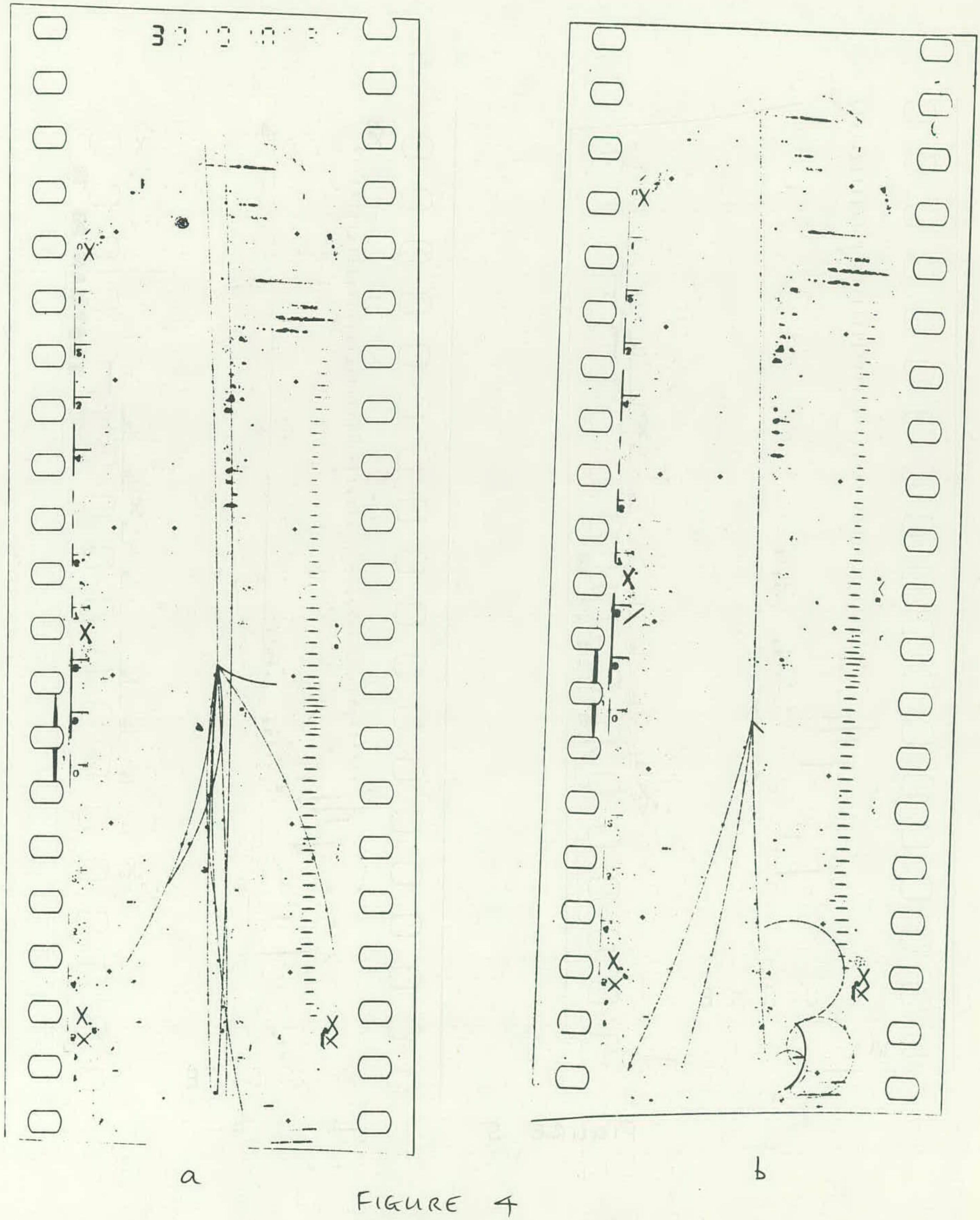
b

FIGURE 2



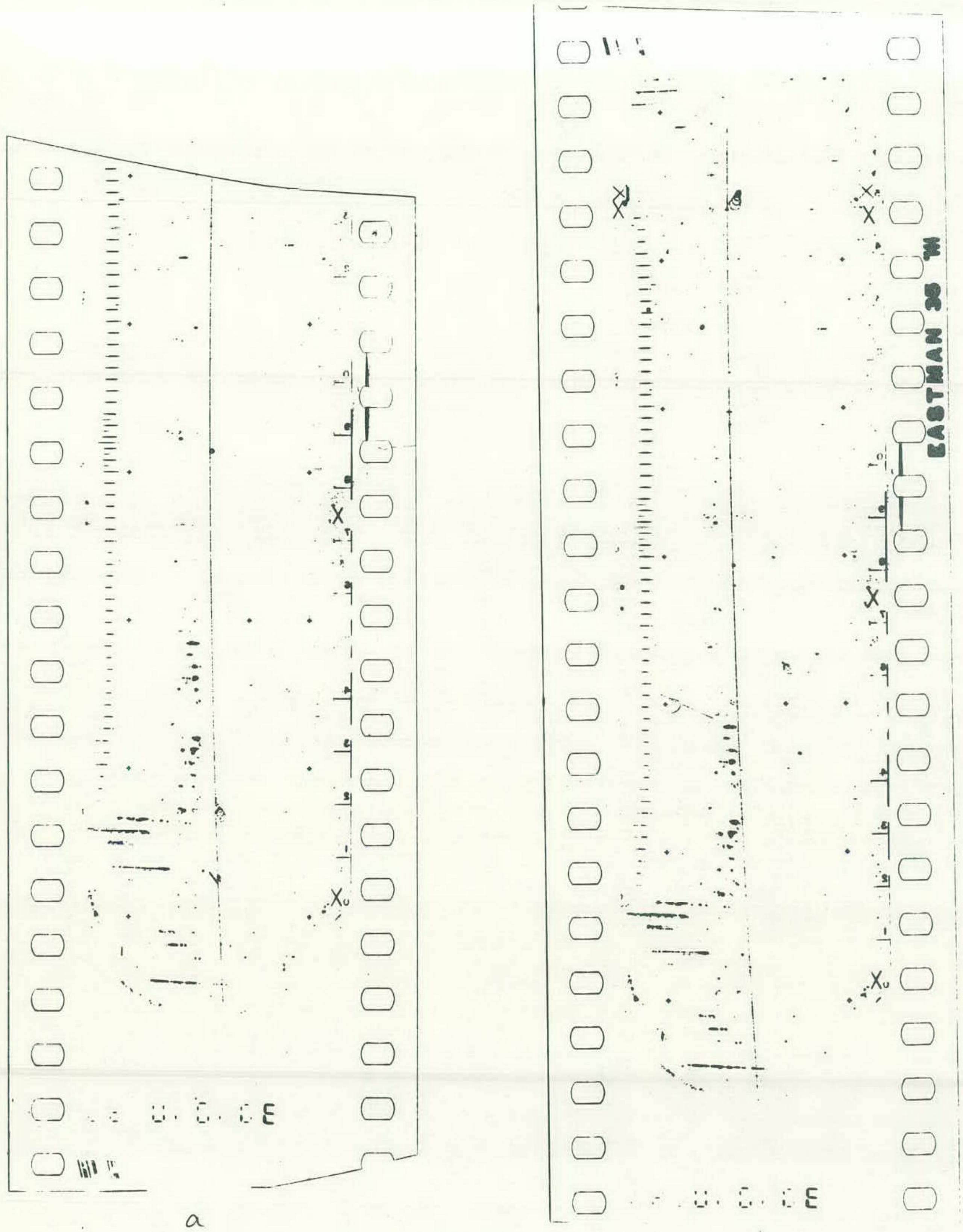
a b

FIGURE 3



a b

FIGURE 4

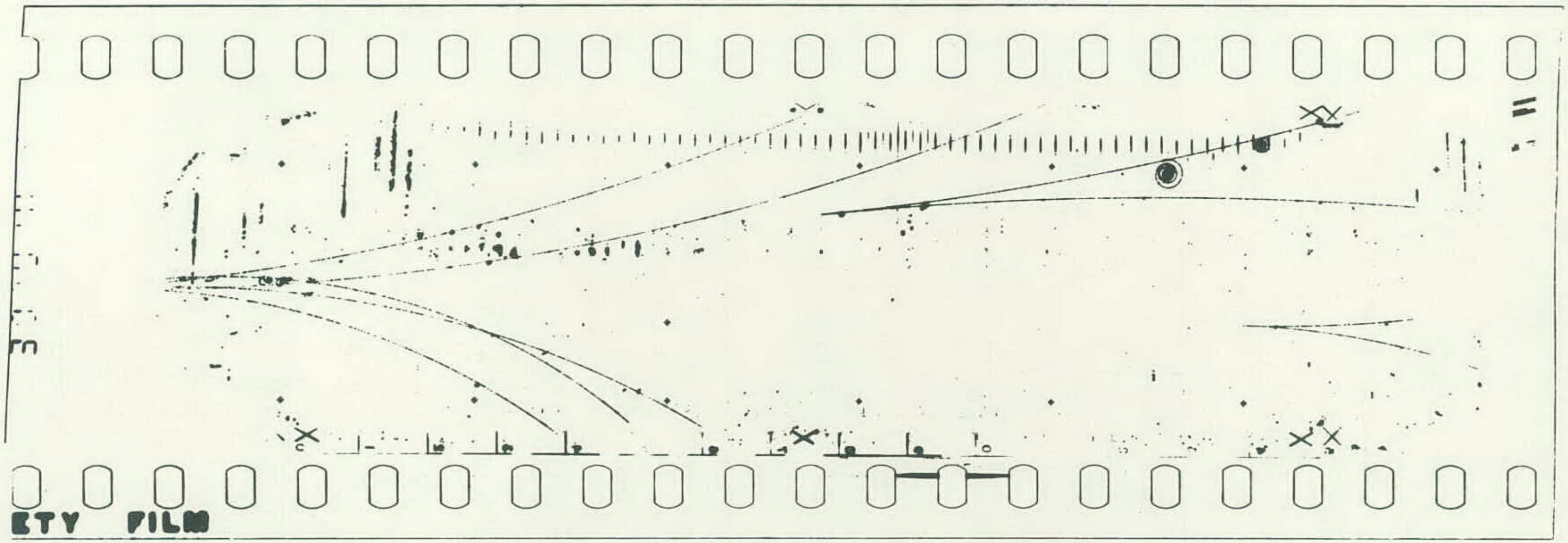


a

b

FIGURE 5

a



b

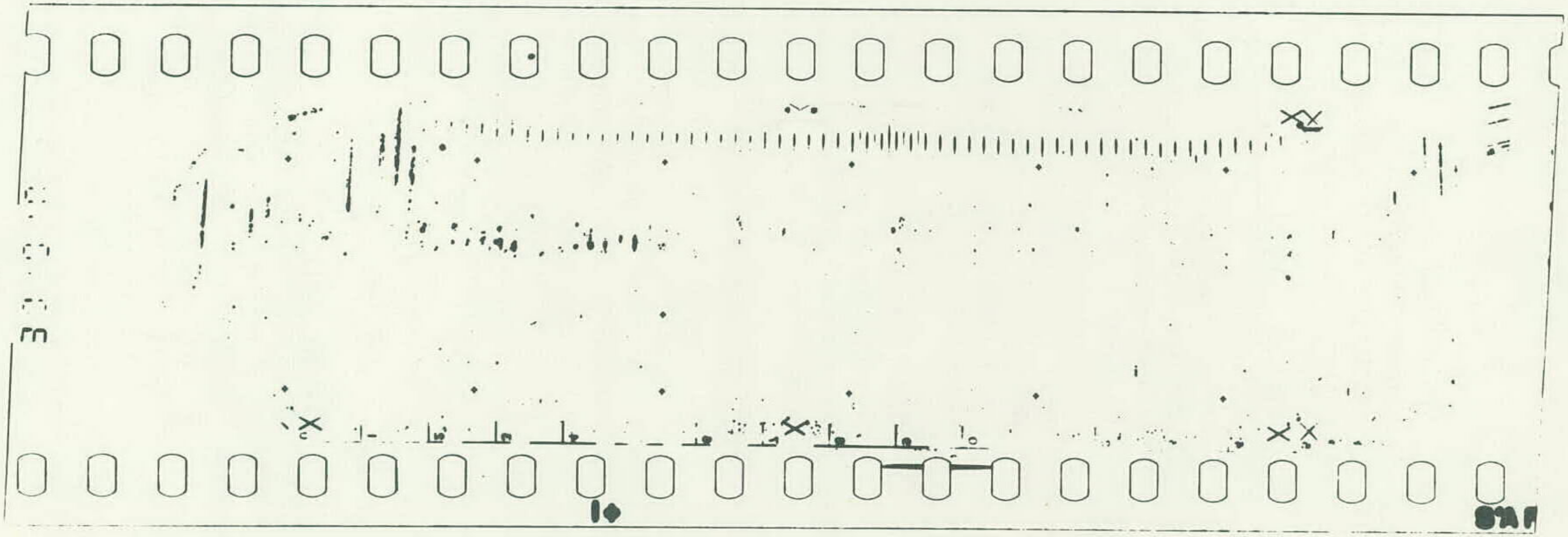


FIGURE 6

DISCUSSION

MULVEY: (Oxford) On some events there will be some sort of bias presumably. For instance you wouldn't do this for elastic scattering but then you'd say you wouldn't do that in the bubble chamber anyway.

PLESS: (MIT) No, the answer to that question is somewhat surprising. Basically you have to look at the area of the solid state detector compared to the area of the back of the chamber. We went through 1000 events and the thing that's surprising is that the detector is better at detecting small angular scatterings like elastic scattering than a person is, namely, this system sees kinks and tracks which people do not. Everyone of the tracks that missed the detector, when we looked very carefully, a very large percentage of them had a kink which we would never have seen on even a careful scan. So, in fact, if you wanted to use a bubble chamber for elastic scattering this is much better for elastic scattering than without.

SLATTERY: (Rochester) I was wondering how you were going to arrange for each film to have 5 tracks going through your counters for each frame. If you are not having a stepping magnet in the front why wouldn't you have more tracks.

PLESS: (MIT) It will be very rare indeed that there would be as many as 5 tracks plus an event. On the average there would be about 3 tracks, sometimes there would only be one track, according to Poisson statistics. Usually you'd ask that, on the average, each channel contain one track; that means on some pictures you'd have two in that channel. If there are two in that channel you'd ignore that picture. If you don't use on the average one per channel, but if you take the optimum number for the average per channel then you'd get the numbers I quote, namely a good event every 4th or 5th expansion.

SLATTERY: (Rochester) How would that rate compare if you actually had a stepping magnet which, say, put 5 tracks in each time.

PLESS: (MIT) Charles Peyrou is here, and he probably can answer that better than I can, but stepping magnets have gone out of style because they don't work.

PEYROU: (CERN) Stepping magnets have gone out of style because they have never been tried.

ALLISON: (ANL) Concerning biases, it seemed to me that the question is not of the pictures you did take, ie. how many of them didn't have events in, but of the pictures you didn't take, how many had events in because one of the secondary particles went into the detector.

PLESS: (MIT) These were 16 GeV pi minuse's that we were looking at and they were chosen simply because we were parasiting on an other experiment so we could get the machine time free. If you take the area of the back of the chamber, which is probably the correct area at 16 GeV, compared to the area of the solid state detector, this will give you a rough but pessimistic idea.

ALLISON: (ANL) It depends very much on the multiplicity.

PLESS: (MIT) Sure, you have to take into account that area and the multiplicity. Incidentally, there is a magnet downstream from the collimator to sweep low momentum particles from the collimator out of the beam.

CRESTI: (Padova) Is the beam focussed on the upstream detector or is the collimator selecting only a small cross-section of a spread out beam?

PLESS: (MIT) The beam is focussed on the collimator. The flux of pions at the energy used in this experiment is 10% of the maximum flux that the machine can produce. The flux of K^+ mesons is also large enough to be used instead of the pi meson as the interacting particle.

6 May 1970 9:30 a.m.

Session V

Chairman: R. T. Van de Walle (Nijmegen)

Status of MIT's PEPR's*

P.L. Bastien, H. Baumel, W. Chien, M.K. Choe, R.I. Hulsizer
P. Marcato, T.C. Ou, I.A. Pless, E. Sartori, G. Schultze, J.N. Snyder,
B.F. Wadsworth, T.L. Watts, J. Wolfson, R.K. Yamamoto**

Laboratory for Nuclear Science

Massachusetts Institute of Technology

Cambridge, Mass. 02139

* Work supported by U.S. AEC

** Talk delivered by T. L. Watts

FILM PROCESSING

This is a status report on the film processing at M.I.T.

The point guidance scheme used at the M.I.T. PEPR has been described at previous conferences. Briefly, a point is measured on a vertex of an event, and a middle and end point on every track independently for each view. These points are measured on image plane digitizers simultaneously with the scanning for events. PEPR processes each view separately, track following each track from the measured point using the vertex and end points of the track as goal points. Tracks are associated between views in program MATCH, and then go into the usual sequence of TVGP and SQUAW.

All events in the film are measured usually, including strange particles, except on one experiment ($\bar{p}p$) where 2 prongs are ignored. On some events, the beam track is not measured if another beam track is too close. The momentum and angles of the missing beam track are inserted from a map. If a beam track is not measured, or is short, another beam track is digitized in each view to act as a minimum ionizing reference track.

Event throughput is conveniently studied at two places -- PEPR, and scanning and pre-digitizing stage. Overall throughput is limited, for us, by the scanning and pre-digitizing operation. We operate five image plane digitizers somewhere between

8 and 16 hours per day for 5 days per week. Their instantaneous rate is 20 - 45 events/hr/machine.

It is of more interest here to show the throughput of events at PEPR. Figure 1 shows the weekly throughput for the last year. PEPR operates 40 hours per week with one operator on a PDP-6 computer. On 5 - 25% of events the operator may have to restart the track following routine from a different place on the track, or retry fiducials or note that a scanning and digitizing mistake was made, or reject a track too short for the present program, or time threshold parameters for different rolls of film. The rate as seen from the figure, varies between 50 and 125 events/hr. The instantaneous rate was 120 events/hr before January 1970 and 180 events/hr afterwards, for pp 5 GeV/c film. It was 210 events/hr for $\pi^- p$ 3.9 GeV/c film after January 1970.

The film being processed is shown along the bottom of Figure 1. All the film came from the 30" HBC at Argonne National Laboratory. All events are measured in πp film but not 2 prongs in pp film. Scanning and digitizing rates fall somewhere between the two extremes shown in the figure so that the two phases of operation are roughly matched. $\pi^- p$ film processes at an appreciably faster rate than $\pi^+ p$ film and pp film.

In January 1970, we interleaved and rearranged the memories on our PDP-6 and obtained a 50% increase in speed. Figure 2 shows the sizes and cycle times of our memory blocks. The two

1.65 μsec 16 blocks were interleaved and the 2 μsec blocks also. The interleaving is on the most and least insignificant bits. The increase in speed is caused partly by faster memory and partly by interleaving. As far as we know, all programs which are heavily used were not in the 5 μsec core before the change.

An attempt was made, on all the film we have taken, to control picture quality. A test strip from every view of every roll was inspected with a microscope and flying slit for track quality, and the number of beam tracks was kept to 6 per picture where possible. This was possible on most runs although the early batches of film had large variations in beam count. Event processing rate is dependent on picture quality (no surprise).

Figure 3 shows the status of the film we have taken as of December 30, 1969. The last column gives the number of events on DST as of that date.

Figure 4 shows the reasons for lost time in the last three months. PEPR trouble consisted of such things as a glass plate dropping out of the film transport; such troubles are quantized in units of about 5%, because there are about 20 working days per month. This last week it has been found necessary to rejuvenate the cathode in the CRT because the electron beam current had fallen too low. It is pleasant to report that the 4 year old CRT responded satisfactorily.

Figure 5 shows an analysis of the rejected events for the various batches of film that we have processed. The failure rate caused by PEPR varied from 12% to 23% of all scanned events.

At present we are changing to a PDP-10 processor keeping the same memory blocks and peripherals. This will be complete very shortly. A second scan table has been built for processing SLAC 82-inch film. This table uses the same driving circuits and digital electronics as the prototype PEPR which processes ANL 30-inch film. The changeover takes about 2 hours including 1 hour of warm up time before calibration. This new table has been calibrated and will shortly be our main production effort.

Our Astrodata PEPR has no film transport yet and is being used for testing a three view program. Three views of ANL 30 - inch film can be positioned in the film gate simultaneously using a 1.2/1 lens magnification. The three view program involves the simultaneous scanning of 3 views of an event and will be reported on by J. N. Snyder tomorrow. The program is being implemented by J. N. Snyder, T. C. Ou, and J. Wolfson. The program has been tested up to the space track following on one event. It occupies about 60K of core in the PDP-6 without DDT or symbol table.

I have been asked to include a short statement on how we measure ionisation. A full statement on the method we use was presented at the Ninth Data Processing Conference held at Argonne, October 1968. Briefly, we scan across the track with a spot about the size of bubble at a spacing of about a bubble size. We scan the whole length of a track and count the scans with no data ("misses") and the total number of scans; the ratio is called the lacunarity.

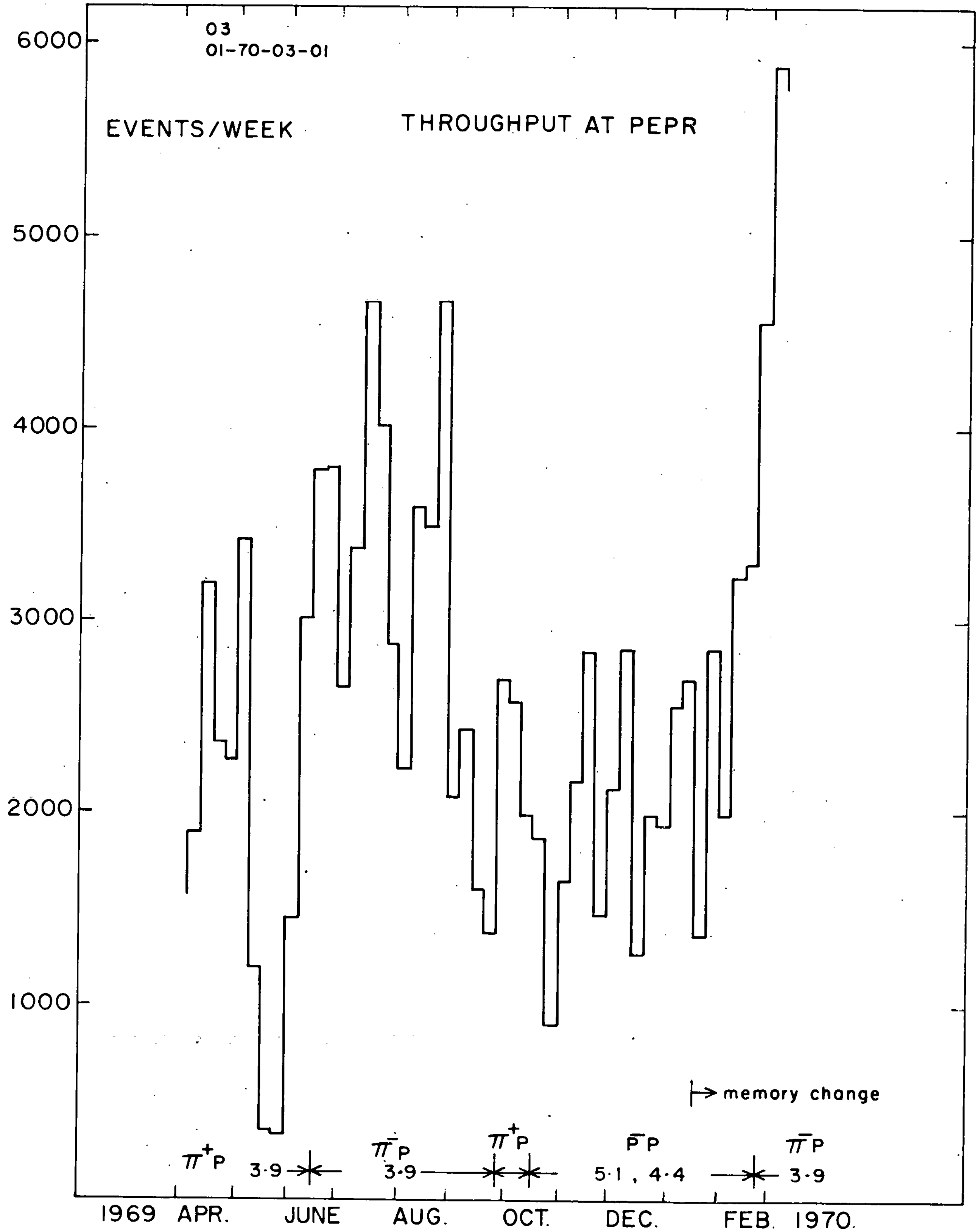
A minimum ionising track is used to set the threshold discriminator level which gates the track signals. The level is adjusted until the minimum track gives a lacunarity of 40 ±2%. It has been found that such a threshold setting gave the largest separation in lacunarity between minimum and heavily ionizing tracks. After the discriminator level is set lacunarity is measured on all tracks in the event in all views.

Once tracks are matched from view to view, an overall lacunarity for each track is computed from total sweep and total misses in all views. Hypotheses allowed by kinematics are tested for consistency with the ionization measurements by constructing a sum of squares

$$\sigma = \frac{1}{N-1} \sum_{i=1}^{N} \left(\beta_i^2 - \frac{a}{\log \ell_i} \right)^2$$

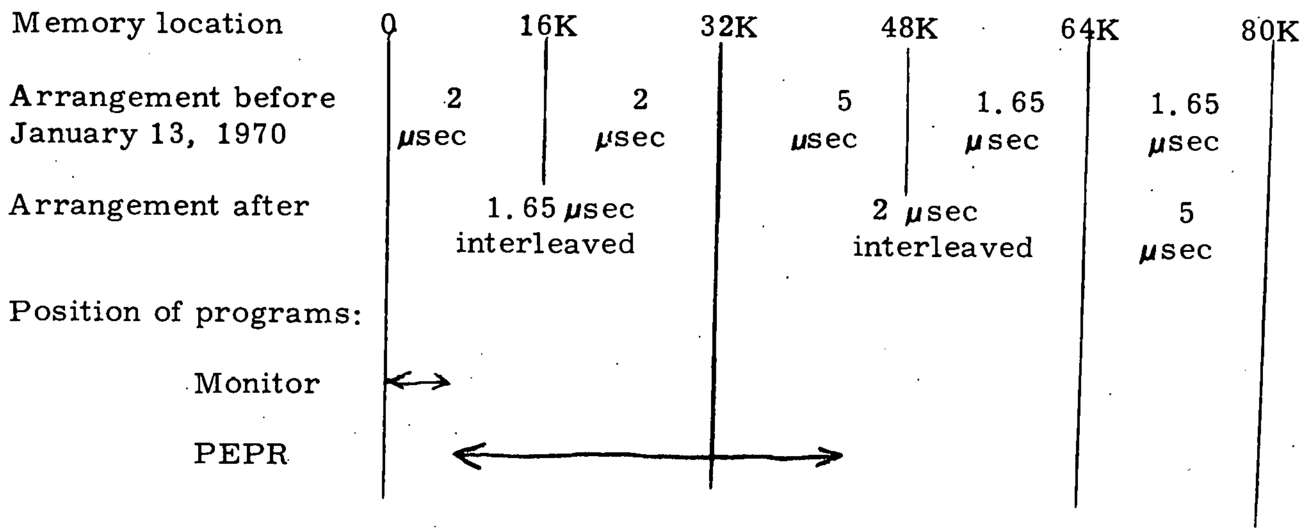
where N is the number of tracks in the event and β_i is the velocity calculated for the i^{th} track in this hypothesis; σ is minimised with respect to a. We chose $\sigma = .06$ for the division between good and bad hypotheses. In a test, no events were assigned wrongly by this criterion because a bad hypothesis gave σ inside the cutoff while a good hypothesis gave σ outside. However 23 events out of 99 contained at least one wrong assignment of a hypothesis, 4 because no good hypothesis fell inside $\sigma = .06$, and 19 because bad hypotheses fell inside along with the good hypotheses.

This method is good for resolving pions and protons to just above 1 GeV/c. Systematic effects make the measurements too inaccurate above that. We have not studied these effects in detail. They only seem to affect about 4-10% of the events so we look at those on the scan table.



03
01-70-03-01
EVENTS/WEEK
THROUGHPUT AT PEPR
6000
5000
4000
3000
2000
1000
memory change
π^+p
3·9
π^-p
3·9
π^+p
$\bar{p}p$
5·1 , 4·4
π^-p
3·9
1969 APR.
JUNE
AUG.
OCT.
DEC.
FEB. 1970.

Core Memory on PDP-6



03
01-70-03-02

Figure 2

Film Processing Status 31 December 1969

Experiment	Laboratory	Reaction	GeV/c	Pictures: Taken	Pictures: Measured	Events: To Measure	Events: Measured	Events: On DST
1	ANL	$\pi^+ p$	3.9	222K	222K	0	44K	20K
2	ANL	$\pi^+ p$	5.8	124K	124K	0	30K	17K
3	ANL	$\pi^- p$	3.9	298K	200K	20K	40K	0
4	ANL	$\pi^- p$	5.8	279K	0	56K	0	0
5	SLAC	$\pi^+ p$	14.0	36K	0	36K	0	0
8	SLAC	$\pi^- p$	8.0	130K	0	130K	0	0
9	ANL	$\bar{p}p$	5.1	220K	116K	19K	20K	0.02K
10	ANL	$\bar{p}p$	4.5	220K	0	39K	0	0
						300K	134K	37K

03
01-70-03-03

Figure 3

Distribution of PEPR Time

	Dec. 1969	Jan. 1970	Feb. 1970
Operate	75.5	75.2	82.3
Lost:			
Film change	11.2	10.7	12.9
Calibration	1	1.1	1.5
PDP 6 trouble	2.8	.5	.75
PEPR trouble	5.2	4.6	.7
Magnetic tape unit trouble	4.3	5.7	1.7
Other	0	2.2	.75
	100%	100%	100%

03
01-70-03-04

Figure 4

Analysis of Rejected Events

Film	Events in Sample	Successful	Rejects caused by PEPR	Rejects on Scan Table	Miscellaneous Rejects
$\pi^+ p$ 3.9 GeV/c	3452	80.8%	11.8%	2.5%	4.9%
$\pi^+ p$ 3.9 GeV/c	3230	76.5%	14.5%	3.5%	5.1%
$\pi^- p$ 3.9 GeV/c	2874	80.3%	14.3%	3.2%	2.6%
$\pi^- p$ 3.9 GeV/c	3897	71.6%	17.9%	6.4%	4.2%
$\pi^+ p$ 5.8 GeV/c	29083	63.8%	22.8%	4.4%	8.6%
$\bar{p}p$ 5.0 GeV/c	2867	63.2%	22.9%	9.9%	4%

Successful events pass through SQUAW
PEPR rejects are failures discovered by MATCH and TVGP.
Scan table rejects vary from experiment to experiment, and include short tracks less than 1/2 mm on film, and pictures crowded by too many beam tracks.
Miscellaneous rejects include machine failure on image plane digitizers, accidental errors by operators.

Figure 5

DISCUSSION

KOLLER: (Stevens) You had something like 10% for film change, do you include film transport in the 75% of PEPR working, and if so what fraction is film transport.

WATTS: (MIT) It was back in about 6 months ago, when we timed the film transport and it was occupying about 10% of the time if you just let things run instantaneously and without any pauses for operator intervention or anything like that. Now it's undoubtedly gone up since then because, a) we interleaved the memories, and b) we now have a PDP-10; does anyone from MIT know the recent timing?

WADSWORTH: (MIT) I believe the measurement we made at one time was 25% but I am not quite sure whether you are overlapping it these days with the mag tape writing and whether you are taking that into account when you say 10%.

WATTS: (MIT) No, when I said 10%, with that time we do overlap the mag tape writing; that used to be about 5% before we've made the recent changes.

PEYROU: (CERN) I am a little surprised about your procedure about ionization. Yesterday we heard a talk which was saying that there were difficulties even for normal amplifiers because there was a dynamic range of 1 - 22 or something like that in some chambers. What are you going to do if you ever process the 80" of Brookhaven for instance.

WATTS: (MIT) I think one does experiments, you fit the programs you've got to the experiments you have in hand. What we have is adequate for the experiments we are doing right now. If one has more difficult film then one certainly has to look into changing programs, and also probably changing hardware.

PEYROU: (CERN) I don't understand. Normally 40% seems to me a very low level in the following sense, normally bubble chambers are operated with something like 15 bubbles per cm and therefore the lacunarity that you would expect is something like 20%. You are passing many bubbles without counting them, that might be all right, I am not criticizing that it is not empirically all right, but it is not a priori what you should expect if you had a perfect bubble chamber; if you had a perfect, very black bubble chamber you should have 20% or 30% of misses but not 60%.

WATTS: (MIT) Yes, in this chamber, if you run it at a very low threshold, the lacunarity would be about 10%, on a minimum ionizing track, ie 10% of misses over total sweeps. But we're not sensitive then to the differences between the minimum ionizing and the twice ionizing and the third ionizing tracks, we are not as sensitive as we are at 40% and that's why we chose 40%. If one plots the ratio of the log of the lacunarities versus threshold, you find that it's largest at this 40% level.

PEYROU: (CERN) That will mean that the size of the bubbles is dependent on ionization and I don't believe that.

WATTS: (MIT) The other way around, our ionization depends on the size of the bubble.

PLESS: (MIT) Just let me make two comments about this question, one comment, we obviously try to run the chamber at slightly less than 15 bubbles per cm, which is very hard to do but we aim for sort of like 9 bubbles per cm. but are not always successful. We have found from experience that if you run the bubble chamber light, the discrimination between particles, between beta's is much better, however, when we fail to get less than 15 bubbles/cm the thing we found experimentally, and later we can make theories on why it works, is that if you took four prong 4C events where you knew what the particles were and we took the large quantity of these and ran these events with a different thresholds checking the separation, and we have display routines and all sorts of nice programs to be able to do this conveniently, checking the separation as a function of threshold then as a function of the measured although fakely measured lacunarity of the incoming beam track, we found that we could properly identify these four prongs events most satisfactorily when we set the threshold so that at the beam track we were measuring a lacunarity of 40%. Now the statement that is made, that the size of the bubble so to speak is independent of the ionization, that the size of the bubble for a heavily ionizing track, providing you don't get into the case where the bubbles merge together, is the same as for a minimum ionizing track is a correct statement. However, if you then take a look at the quality of the film, if you take film and underdevelop it, one of the things that you notice is that the heavily ionizing particles, because of the non-linearity of the film, stay preferentially as compared to the minimum ionizing tracks. Now obviously the film is already developed before we get it, on the other hand, the sensitivity of the equipment is such that as you increase the threshold, the heavily ionizing tracks remain, the lightly ionizing tracks disappear, I mean,from the display, and this is an experimental fact. We therefore take advantage of this experimental fact which is not too well understood theoretically, and we checked it against 4C events for which we did not have to know the ionization to identify the particles and it seems to work exceedingly well for the 30" chamber at Argonne. When we get to work on the 82" chamber at SLAC we may be in terrible difficulties and this is one of those things we have to find out.

LORD: (CERN) Could you say how much time the ionization measurements add to the time to measure an event.

WATTS: (MIT) Let's say I chose a figure like 6 seconds per event in a view and then track following is probably about 3 seconds something like that, ionization is probably about a second and a half to two seconds maybe, sometimes it is as long as the track following.

VAN de WALLE: (Nijmegen) It may be easier if you would give a fraction of the time, how does it influence your rate for instance?

WATTS: (MIT) On the PDP-10 the instantaneous rate of 322 events an hour changes to 410 - 440 events an hour if you leave out ionization.

BETTINI: (Padova) Do you measure ionization on every view?

WATTS: (MIT) Yes every track, every view

BETTINI: (Padova) I don't understand the reason. On an average track you have something like 300 to 500 independent measurements. How does the systematic error on the ionization compare with the statistical one. All these numbers are not independent because of the same image of the bubble in the 3 views.

WATTS: (MIT) There is some statistics associated with a cathode ray tube device as a random number generator, well it's very hard to convince programmers of that, so there is some statistical work associated with just merely just doing the thing over again at the same point, I admit it's not too large but it's there. On systematics, we are affected by systematics, for instance we have noticed a difference between views, and I am sure I've actually seen on some tracks, a difference along the track, so there are systematics but we close our eyes right now.

BETTINI: (Padova) How often do you scan to reduce the statistical error to have many hits and how do you control the systematic error.

WATTS: (MIT) Every 25 microns.

BETTINI: (Padova) Why 25 and not 50 or 100?

WATTS: (MIT) That's about our bubble size, that is about the size of the bubbles.

BETTINI: (Padova) Do you need that accuracy, do you need that many scans?

WATTS: (MIT) Oh, I understand, I am sorry, you mean couldn't we do this on half the track or 2 mm of track here, 2 mm there; I don't know, we haven't tried it, probably we could.

Automatic Scanning and Measurement of Bubble Chamber Film on POLLY II*

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Argonne National Laboratory, Argonne, Illinois 60439

ABSTRACT

An exposure of 600,000 hydrogen bubble chamber pictures of 2.3 GeV/c $\bar{p}p$ interactions in the 30-inch Argonne-MURA chamber is being scanned and measured for 2, 4, 6 and 8 prong interactions by POLLY II, a computer-controlled CRT device. The performance of the automatic scanning and measuring is assessed on the basis of the first 160,000 events processed. The control program searches for beam tracks and finds

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*Work supported by the U. S. Atomic Energy Commission.

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‡Present address: Nuclear Physics Laboratory, Keble Road, Oxford, England.

⁑Present address: Scientific Control Systems Ltd., London, England.

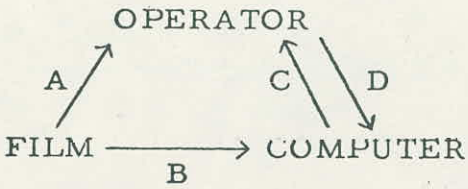
interactions without prescanning. It digitises the tracks, measures bubble density and outputs master point information ready for direct input to the geometrical reconstruction program. The operator is an integral part of the system and is available to give assistance when required. Film is processed at 70-100 events per hour.

1. INTRODUCTION

POLLY II is a precision CRT flying spot digitiser used for the automatic scanning and measurement of bubble chamber film at the Argonne National Laboratory. The device, of which POLLY I[1)] was a prototype, is on line to a 48 K Sigma 7 computer. Extensive innovations in the FORTRAN control program[2)] have enabled us to measure film which has not been prescanned at all -- thus in a single pass of the film, under computer control, the stages of scanning, measurement, and track density determination are completed. Full-scale production on a 2.3 GeV/c $\bar{p}p$ experiment started in April 1969. By January 1, 1970 a total of 160,000 events had been scanned and measured in 2135 hours (an average of 75 events/hour). In this paper we discuss the performance of the POLLY system on the basis of this experiment. It should be pointed out, however, that POLLY II measured four other experiments during the year (1.5 GeV/c π^+d, 5.5 GeV/c K^-d, 5.5 GeV/c K^-p, 5.5 GeV/c $\bar{p}d$) for which the film was prescanned (vertex position on the first view only). This amounted to a further 80,000 events. During the year continued program development has led to further improvements in both the quality and the rates of scanning and of measurement.

2. THE SYSTEM

The POLLY system is concerned with the interfacing of three elements: the film, the operator and the computer. Information has to be extracted from the film by the computer -- the operator is a backup system. The interface structure is shown below:



We now discuss the nature of the four interfaces A - D.

Interface A is an optical projection of the film allowing the operator to look directly at the whole picture.

Interface B is a 9" Ferranti precision cathode ray tube (type 9B/71Q0). A spot on the tube is imaged with 2:1 demagnification onto 70 mm film and swept through a raster scan of a small area at the required angle by magnetic deflection coils around the neck of the tube. The photomultiplier output is compared with a discriminator level to yield digital data about the position of bubble images on the film. This operation and the resulting digital data is called a "slice scan"[1,2].

Interface C is an IDI display CRT (type 21EM10P7) with character and vector generators. It is used to show the operator what the program is doing, what information it has obtained already, what its problems are and to display areas of unfiltered digitisings relative to which the operator may make precision measurements. An example of this is shown in Fig. 1. The area displayed is 3.8 mm in diameter on film.

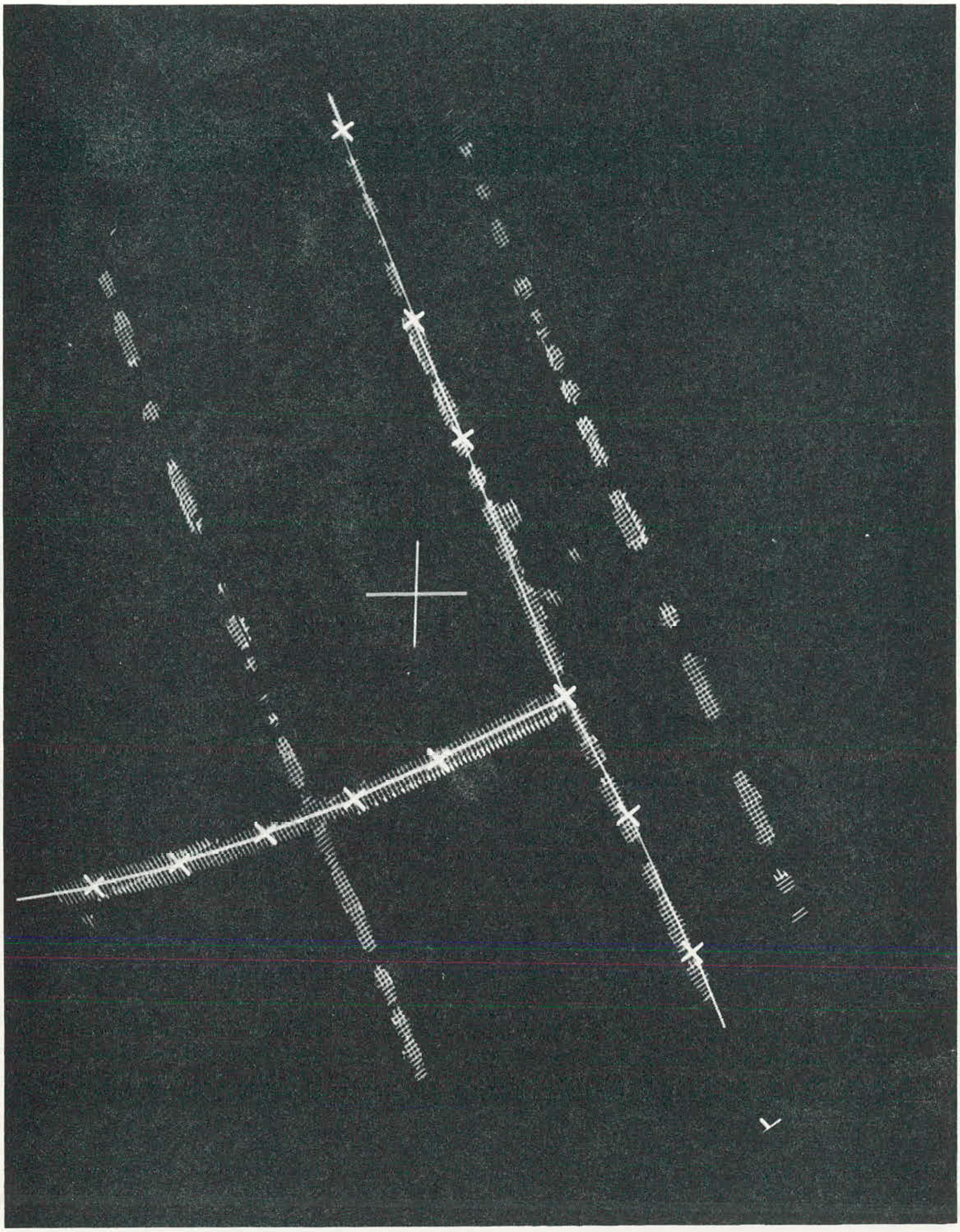


Fig. 1. A display built from real time unfiltered digitisings. The area shown is 3.8 mm in diameter on film. The small crosses represent final track master points for output to TVGP. The display can be "moved" under the central cross by the operator's track ball.

The picture is built up by individual slice scans to form a real time picture. The operator can "drive" the picture under the cross-mark to make a precision measurement of a vertex point or whatever may be requested by the program.

Interface D consists of an array of 32 buttons forming a word which can be read by the program, a track ball driven pointer which is associated with a reticle on the optical display, and an orientation knob. The track ball feeds an X-Y register which can be loaded or read by the program. The reticle is used to indicate features to the program, but not to make precision measurements. The program can selectively illuminate buttons so that the operator is presented with a meaningful selection of possible courses of action.

The logical structure is summarized in Fig. 2.

During operation the system is in one of four states. They are distinguished by whether the computer or operator is in control and by whether the optical display (interface A) or the precision CRT (interface B) has access to the film.

State	Control	Computer	Operator	Interfaces
I	Operator	Idle	Active	A, C, D
II	Operator	Looping	Active	B, C, D
III	Computer	Active	Watching	B, C
IV	Film transport	-	-	-

Fig. 3 shows a photograph of the hardware with diagrams of the light paths for State I ("Display Mode" - Interface A) and States II/III ("Measure Mode" - Interface B).

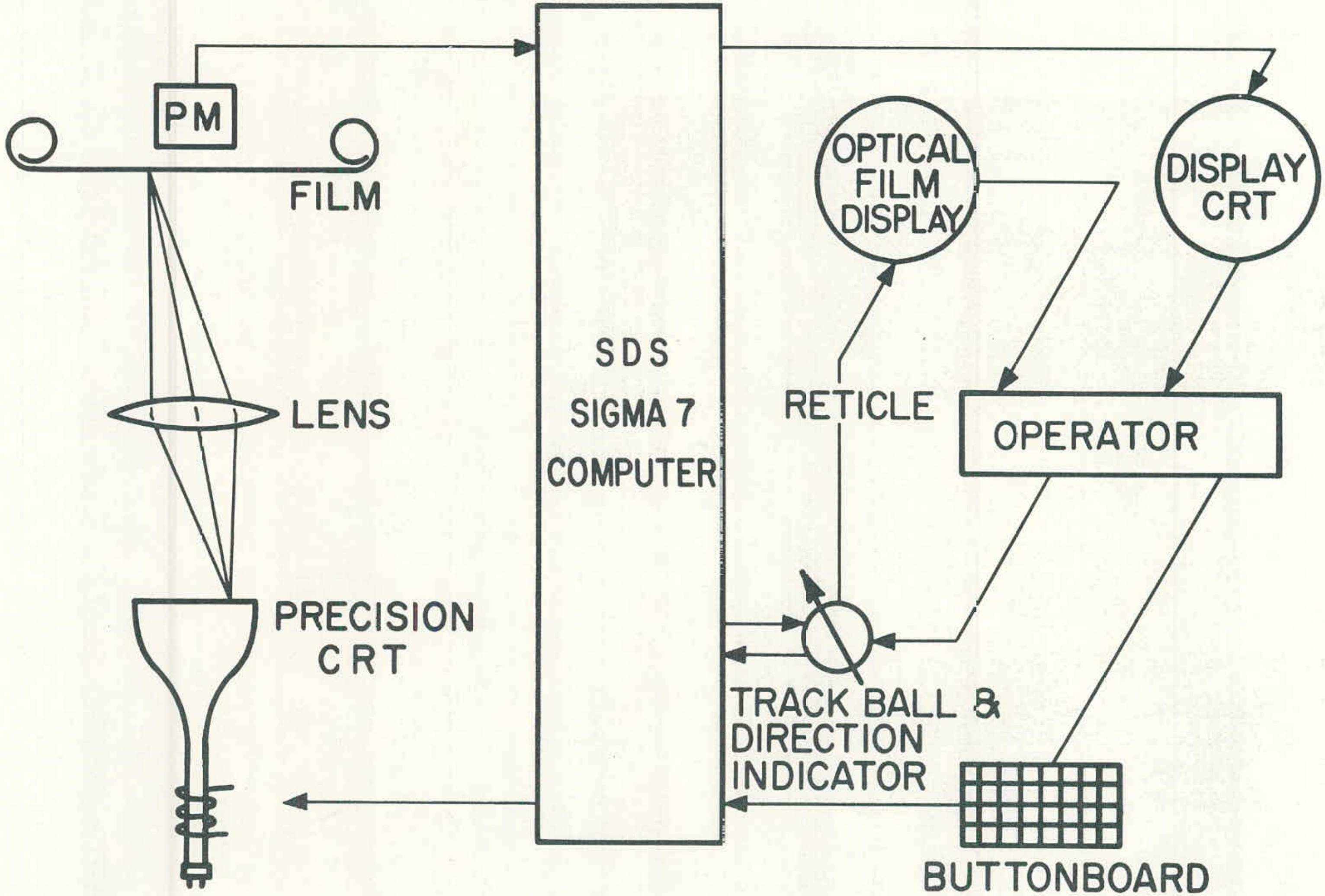
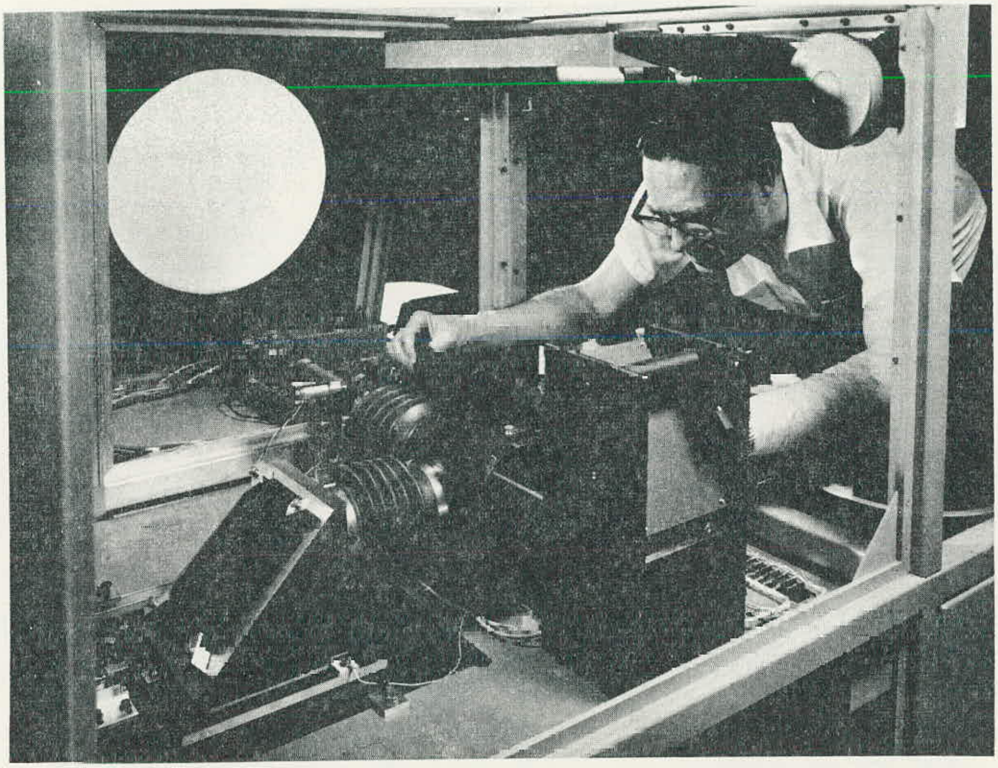
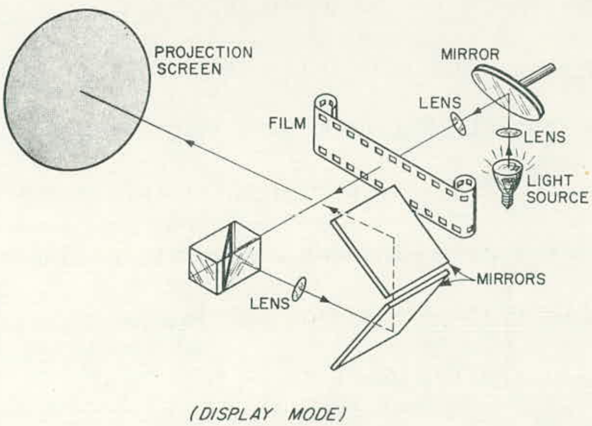


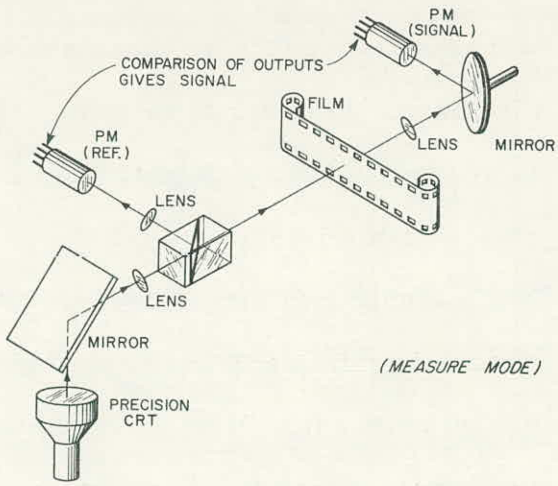
Fig. 2. Block diagram of the POLLY system.



(a)



(b)



(c)

Fig. 3. (a) Main elements of POLLY hardware.
(b) Light path for operator view of the film.
(c) Light path when digitising film.

Fig. 4 is a view of the operator's console. The screen on the left is the optical projection; the one on the right the display CRT. The track ball is in the center and the array of 32 special purpose buttons is on the right.

Most of the time the operator is powerless and the system runs in State III or State IV. Only when the control program diagnoses a possible problem does the system drop down into States I and II (~ 25% of views measured, depending very much on film quality). Once, however, the operator is given control, he can guide the program, slice scan by slice scan if necessary, to overcome problems which are beyond the current program's logic.

3. PROGRAM STRATEGY

Most bubble chamber events are easy to measure and only a small fraction of tracks causes problems. This leads to the use of fast simple logic until trouble is encountered. The operator is viewed as a very sophisticated, relatively cheap and very slow "peripheral" whose role is to fix up those situations which do not yield to this approach. In order to keep him fully informed and to enable him to react as quickly as possible, a great deal of thought and effort has gone into his interface with the program. This man-computer interface has a significant side-effect; it allows the programmer elaborate real-time debug displays which play an important part in the development of the program. Of equal importance is the feedback from full-scale production which has been proceeding in parallel with further program development. This philosophy has allowed us to "bootstrap" our way

Fig. 4. View of the operator console.

into the use of more sophisticated methods, so that now the operator is idle for significant periods of time (depending on the film quality).

An important advantage of the CRT is the use of the random accessibility of data on film. Thus the total data storage in core is less than 3,000 words excluding the display buffer. In this way we avoid extensive bookkeeping operations and save computer storage.

To avoid reading in large quantities of experiment-dependent data, many constants in the program are "learned". These include the shifts and magnifications of the chamber image on the film, the depth of the beam plane in the chamber, the density of minimum ionising tracks, the angle of beam tracks entering the chamber, etc.

A single I/O instruction to the POLLY hardware produces digitisings from a set of scan lines forming a "slice scan". Each scan line is represented by one word in core which has a capacity for data from one track crossing. A slice scan, therefore, appears in core as a vector with one element per line. The software uses the slice scan in two different ways. These may be called "searching" and "following" and are illustrated in Fig. 5(a) and (b) respectively. In searching the program is trying to make contact with a new track as quickly as possible. It is fast because the hardware only need be initialised occasionally and the software need only count lines and look for the presence of groups of digitisings without detailed analysis. Once a track has been found, the "following" method is used. This is slower but more precise because the software uses the edge and width count information for each digitising.

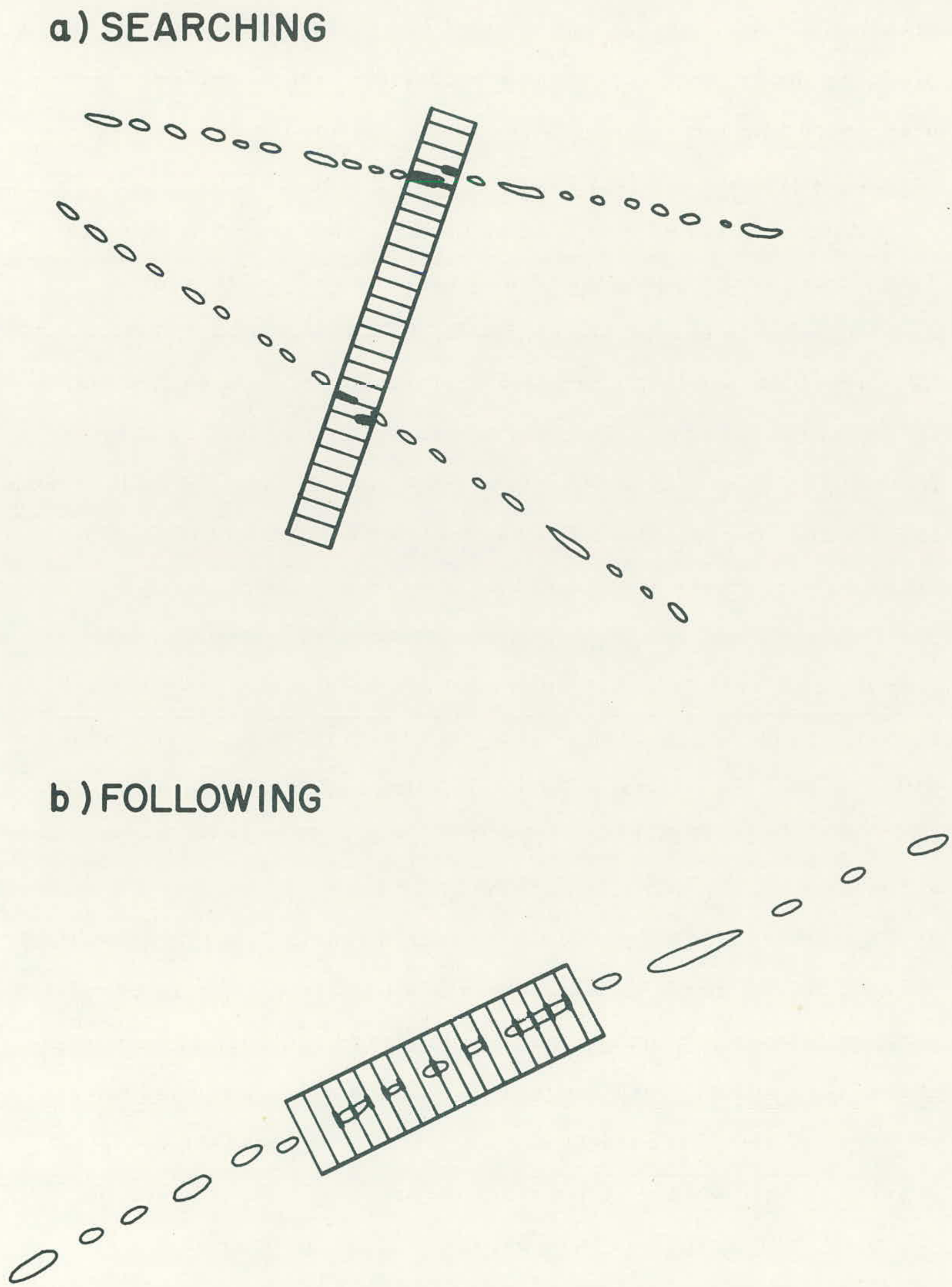


Fig. 5. (a) The use of long narrow digitising area for finding tracks or fiducial arms.

(b) The use of short digitising area for precision measurement of tracks or fiducial arms.

The whole operation may be separated into three phases -- scanning, fiducial measuring and event measuring. The scanning phase produces one or more approximate production vertex positions. For prescanned film the scanning phase is omitted and the approximate vertex position and topology are read from a tape.

Automatic scanning is done on the first view only. The program does a track search along the leading edge of the fiducial volume. It then follows each beam track candidate, first upstream out of the fiducial volume and then downstream until it loses it or reaches the far end of the chamber. The program saves time by leaping down the track digitising only 25 to 50% of the track, taking about 150 milliseconds to reach the far end. Beam tracks closer than 250 microns on film and off momentum tracks are ignored -- care being taken to cover the possibility of a forward high momentum secondary from an interaction near the edge of the fiducial volume. Next the program searches for secondaries issuing from the end of each beam track that terminated within or near the fiducial volume. It follows each of these for a short way, ignoring those that pass right through the expected vertex region, and looks for those that intersect the given beam track within 500 microns of one another or at the end of the measured beam. When this condition is satisfied, the rough vertex position is stored away. There are also problem cases which include suspected forward secondaries mentioned above, zero prongs, very small angle elastics and other cases where the required secondary tracks are not found. For these the operator is asked to point out the vertex using the track ball, or to press the "abandon" button, if there is no event or it is to be ignored (zero prongs and small angle elastics in our case). About 15-20% of the events are fixed up in this way -- it takes the operator 1 - 2 seconds to make the decision in most cases. The total path length of good beam tracks within the fiducial volume is calculated. A running total is available for cross section purposes.

A map of ten fiducials with their arm lengths and angles is fed into the program as data. A searching slice scan is used as shown in Fig. 5(a) to find the fiducial and then each of the four arms is measured with a single slice scan as in Fig. 5(b). As many as 8 and at least 5 fiducials are measured on each view. The film is accurately positioned by measuring two fiducials employing a more extensive search pattern.

The track measuring sequence consists of the following operations:

i) Search for tracks along the sides of an octagon centered on the approximate vertex position (see Fig. 5a).

ii) Follow each track found outwards and then back towards the vertex (provided the track has not already been measured and that it passes close enough to the vertex region).

iii) Identify tracks passing right through the vertex region and those measured twice by mistake.

iv) Attempt to calculate a vertex position by considering the intersections of all pairs of tracks. If successful, eliminate tracks that do not belong.

v) Decide whether to return to (i) to look for more tracks.

The complete sequence is looped through for up to seven different track searching octagons. Their radii are 6000, 3000, 2000, 1000, 300, 9000, and 15000 microns. If after any octagon the vertex has been found and all the tracks of the topology have been measured, the sequence is terminated. If the topology is not known, all octagons are completed. Thirty to forty percent of the views in the $\bar{p}$ experiment only required the first octagon.

The next stage is a meticulous checkout of the tracks as follows:

Beam track identification;

Charge balance;

Agreement with topology (as found on view 1 or as given);

Existence of well-defined vertex point;

Stopping points measured as such.

Other problems requiring operator attention are also found:

Tracks which, though consistent with passing through the vertex, do not extrapolate to within 100 microns of it;

Tracks with fewer than 8 points;

Tracks which end in "confused" regions;

Tracks which might be stopping (on newly scanned events).

If there are no such problems, the program flashes the completed measurement on the screen for the operator's benefit and continues to the next view.

If there are problems, the program displays its measured tracks and its list of problems to the operator and switches to State I (display mode) to allow the operator to study the situation. If the trouble is with the vertex definition, the program goes into State II inviting the operator to give a precision vertex measurement. As soon as the operator has pressed a button, the program performs the indicated tasks and starts the checkout process over again. Every time the operator adds or deletes tracks the vertex position is recalculated. The program will not continue until all the "fatal" errors are removed.

4. THROUGHPUT

Fig. 6 shows the weekly average measurement rates for various experiments over the period April-December 1969. The first four experiments were prescanned for the following topologies (the letter s refers to stubs).

5.5 GeV/c K^-d	3, 3(s), 4 prongs as well as
	1, 1(s), 3, 3(s) prongs + V^o
1.5 GeV/c π^+d	3(s), 4 prongs
5.5 GeV/c $\bar{p}d$	1(s), 2 prongs
5.5 GeV/c K^-p	2 prongs

The 2.3 GeV/c $\bar{p}p$ experiment was automatically scanned for 2, 4, 6 and 8 prongs, elastics with recoils less than 1.5 cm being excluded.

The throughput is particularly sensitive to low track densities (as in the case of the π^+d experiment), poor contrast fiducials and features presently requiring operator assistance (such as vee vertices and stubs). It is relatively insensitive to the number of beam tracks provided these are well spaced (10 - 12 per picture), and to whether the film is prescanned or not.

Fig. 7 shows a breakdown of where the time goes when POLLY is scanning and measuring at peak rate. These numbers vary widely

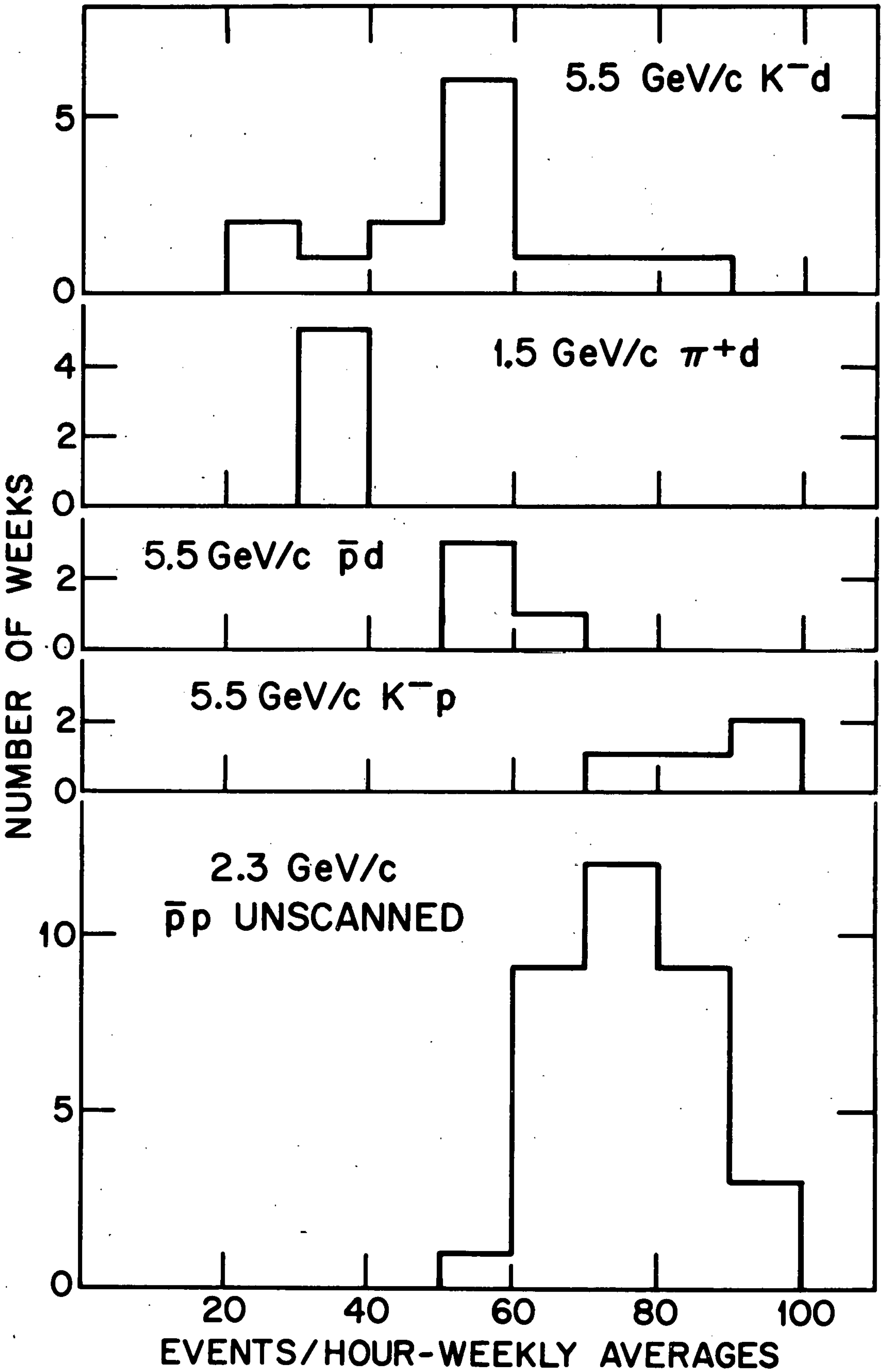


Fig. 6. Measurement rates for various experiments over a nine-month period.

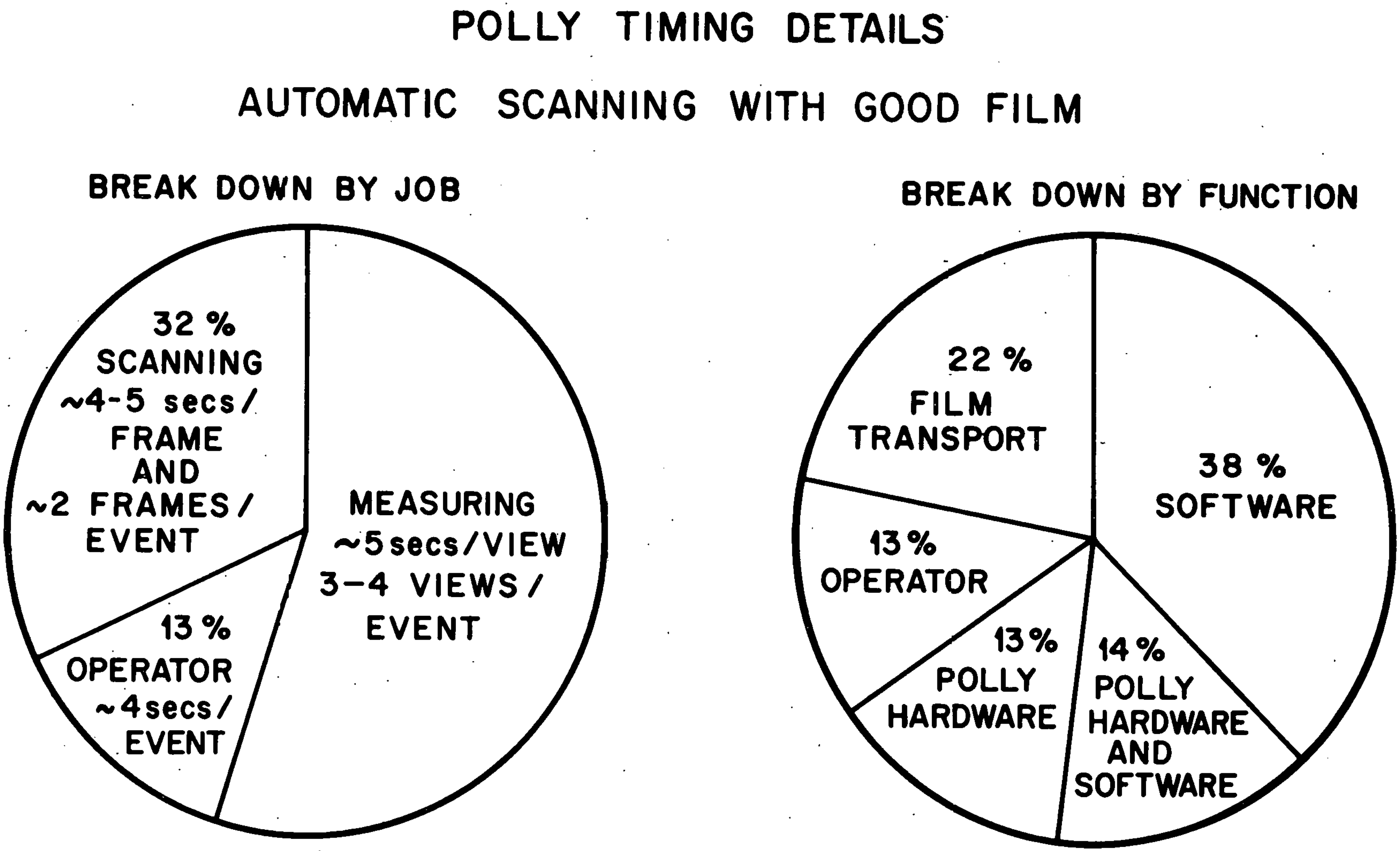


Fig. 7. Pie diagrams showing how POLLY spends its time.

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for different topologies and film qualities. Under more adverse conditions, the program "tries harder" but also requests operator assistance more often during the measuring phase -- the operator time might rise as high as 30%. The CPU is busy about 75% of the time.

During actual track following the CPU is busy 95% of the time and the POLLY hardware is being used 55-60% of the time. Tracks are followed at 60 milliseconds per centimetre on film.

5. ACCURACY

The calibration program, an overlay of the standard POLLY measuring program, measures 90 to 100 points on a standard grid. From these it evaluates misadjustments of the ramp speeds and delay circuits and checks the stability of the deflection and hysteresis effects in the coils. Finally, using average values for the grid point coordinates measured many times over in a pseudo-random order, it calculates a correction polynomial. The following residuals are typical (in units of 1 micron on film ≡ 2 microns on the tube face):

	RMS
A grid point measured many times in the same way.	0.5 - 1.0 μ
A grid point measured many times but with different widths and orientations of slice scans.	0.7 - 1.5 μ
A grid point measured many times but with all the other points being measured in random order inbetween.	1.5 - 2.0 μ
100 grid points fitted to best rectangular grid.	10 - 20 μ
100 grid points fitted to a third order polynomial.	1.5 - 2.0 μ
100 grid points fitted to a fifth order polynomial.	1.0 - 1.5 μ
100 grid points compared with the previous day's fifth order polynomial.	1.5 - 2.5 μ

We have observed drift effects associated with tube warmup which disappear after the tube has been on for a few minutes. We conclude that the deflection is stable and reproducible to better than 2.5 microns over the field of view (diameter 64,000 microns).

Fig. 8 shows the helix fit residuals for track reconstruction as given by the geometry program TVGP. The distribution peaks near 5 microns and does not have a long tail. Events fitting the final state $\bar{p}p \rightarrow \pi^+\pi^-\pi^+\pi^-\pi^o$ at 2.3 GeV/c with a single constraint show a strong signal due to ω^o in the $(\pi^+\pi^-\pi^o)$ invariant mass histogram. Fig. 9 shows this histogram in 2 MeV bins. The experimental full width of the signal is 22 MeV to be compared with the natural width of 13 MeV. Much better resolution is of course obtained for variables not involving a missing neutral or annihilation.

6. SCANNING AND MEASURING EFFICIENCIES

We have evaluated the efficiency of automatic scanning in two ways. In the first we rescanned several rolls of film by hand. A total of 9% (March 1970) to 15% (November 1969) extra events were found which POLLY had missed[3]. The exact definition of the fiducial volume and scanning rules (beam tracks less than 250 μ apart excluded) gave rise to borderline cases which have been included to derive an upper limit for the scanning loss. We have found that POLLY fails to find and follow from 4 to 7% of all beam tracks and consequently misses from 4 to 7% of all events in an unbiased way. Discounting this loss, we conclude that the scanning efficiency is better than 90%.

In a second evaluation we compared the total number of events found by POLLY (including 0-prongs and short elastics not normally

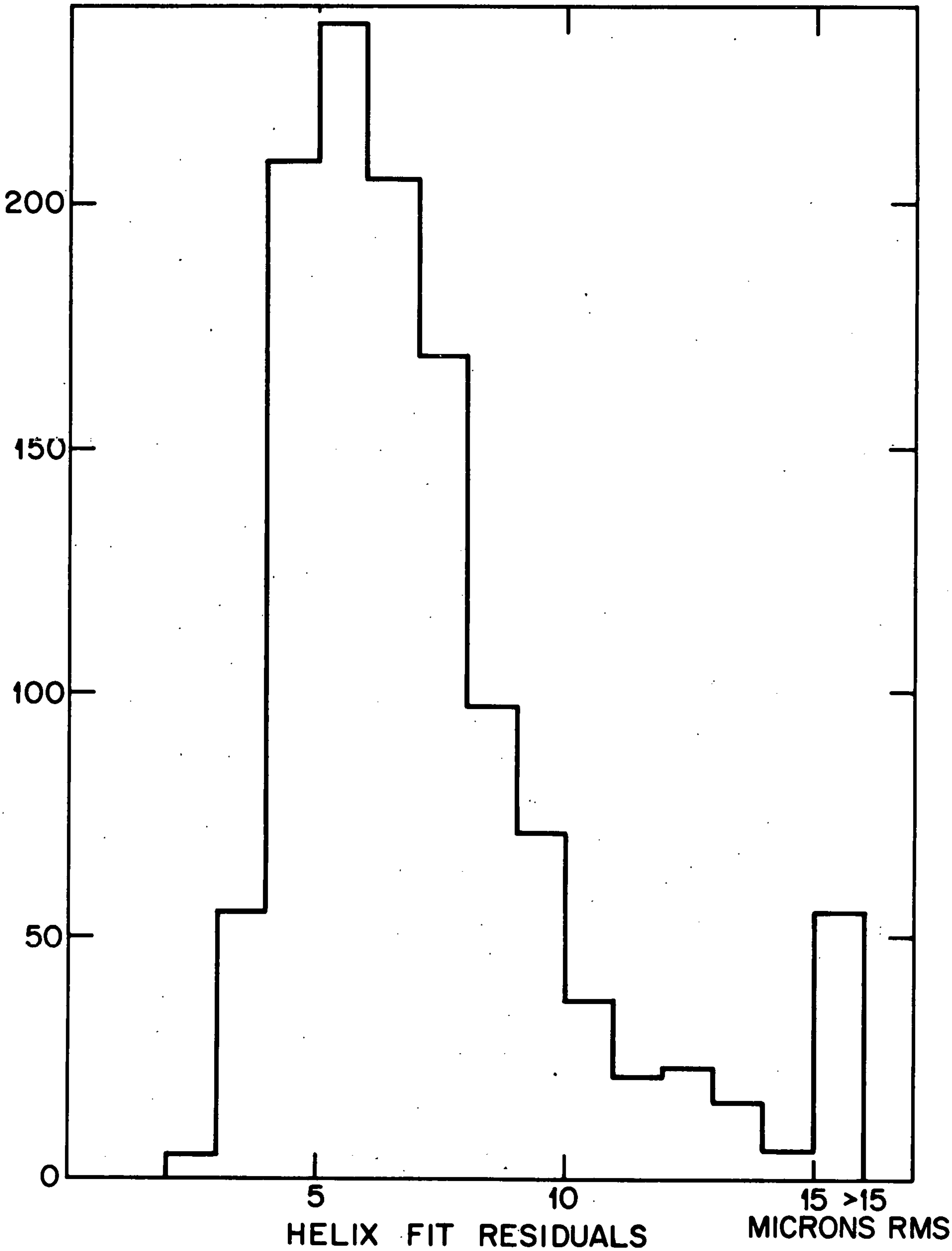

Fig. 8. TVGP helix fit residuals from a typical sample of tracks (8-10 points per track).

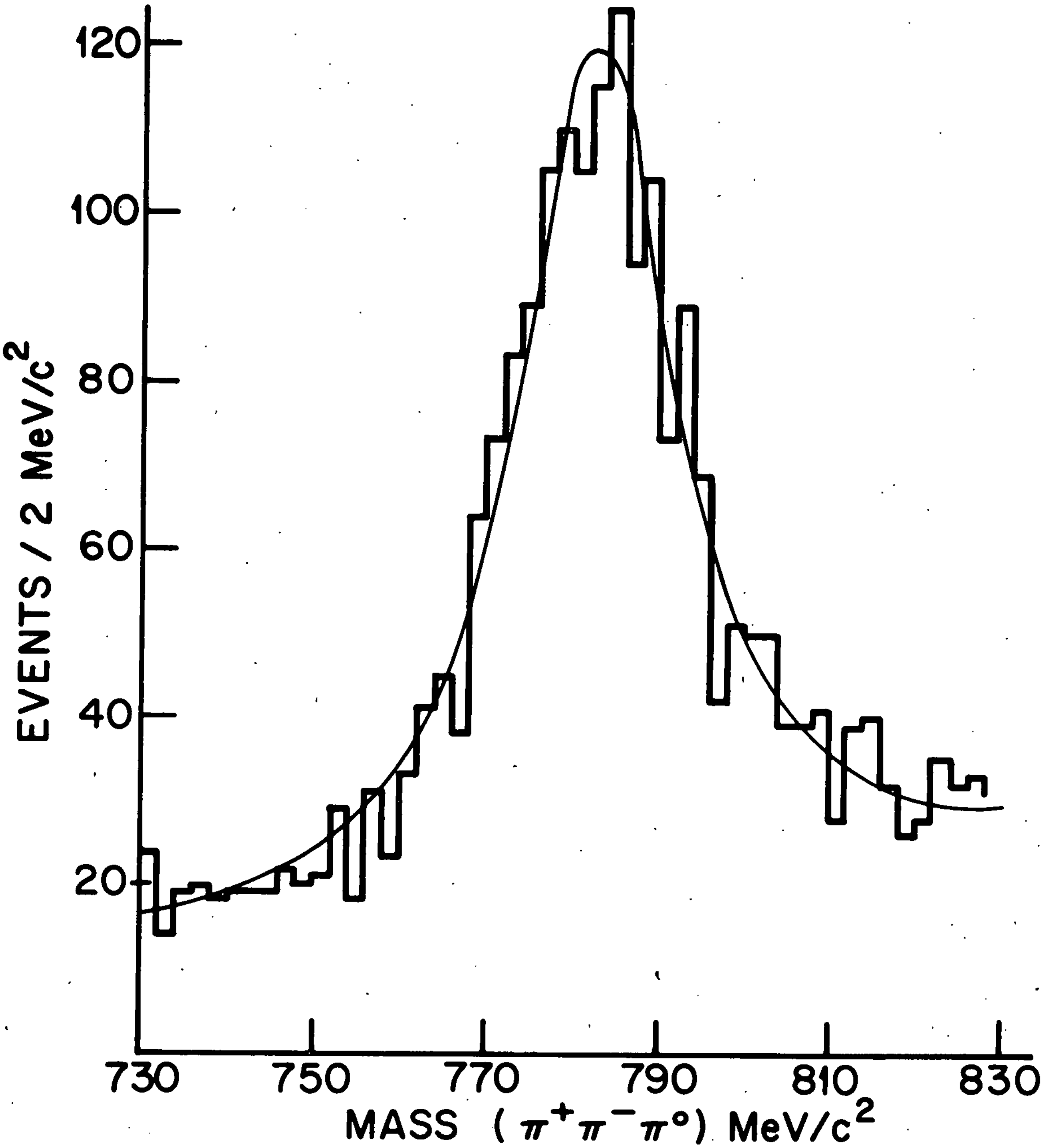

Fig. 9. The ω^o peak from the reaction $\bar{p}p \rightarrow 2\pi^+ 2\pi^- \pi^o$ at 2.3 GeV/c. The curve is a Breit-Wigner of width 22 MeV/c^2.

measured), the total beam track length recorded by POLLY as scanned and the known total cross section for $\bar{p}p$ at 2.3 GeV/c which is 84 mb[4]. In this exposure a Cerenkov counter in the separated $\bar{p}$ beam vetoed the camera flash whenever any beam contamination was detected. We therefore assume that the path length is all $\bar{p}$. In a sample of 8007 metres of track POLLY found 2349 events indicating an observed cross section of 81 ($\pm$2) mb (March 1970). We applied the same check on a roll of 5.5 GeV/c K^-p film using the same rather large fiducial volume. A total of 272 events were found in 3143 metres of track. This represents 24 ($\pm$ 2) mb to be compared with the known cross section of 24.3 ($\pm$ 0.8) mb[5]. However these figures are not uniquely related to the scanning efficiency since there is the possibility of bias in the recording of scanned beam track length as well as the loss of events.

The manual rescan showed that 1/2 - 1% of events scanned and measured was measured with the wrong topology, measured twice in error or simply did not exist. In many cases such errors would be the fault of the operator.

From 2 - 3% of events were abandoned during the measurement process. Frequently this was due to off-momentum beams or totally obscured tracks. We estimate that a third or less of these might have been usefully measured on a conventional machine while they could not be measured on POLLY.

The failure rate in geometry has shown continued improvement:

Run	2 Prong	4 Prong	6 prong	Total	No. of Events
I	11.6%	16.4%	26.9%	14.5%	70639
II	6.0%	12.6%	20.5%	9.6%	66997
part III	4.6%	10.8%	19.9%	8.0%	4740

So far only a sample of the failures have been remeasured on POLLY. Of this sample two-thirds passed geometry the second time.

7. IONISATION

As POLLY follows a track, it records the number of hits (H) and misses (M) and the average track width ($\bar{W}$). The projected ionisation is given by[6)]

$$I_{proj.} = \frac{k_i}{\bar{W}} \ln\left(\frac{H+M}{M}\right)$$

where k_i is a constant to be determined for each view. There are no problems associated with geometrical factors since the track is always digitised by sweeping the spot at right angles to the track. To reduce systematic effects, ionisation information is ignored at the ends of the track and near the edge of the chamber. During the measurement of a view the program holds the discriminator level constant. If it is changed by the operator, then $\bar{W}$ compensates for the effect of change in the apparent bubble size.

After kinematic fitting to a given hypothesis, the projected ionisation is computed for each track on each view. In comparing these values with the measurements, the following assumptions are made:

i) The error on the measured ionisation is taken as 1.2 times the statistical error, as determined from the width of the ionisation stretch function.

ii) Different views of a given track yield independent ionisation estimates.

iii) Tracks with ionisations greater than 3.0 are treated specially to avoid problems due to "saturation".

Tracks which are found to have large dips are excluded from ionisation analysis. A total χ^2 is calculated by summing over all track/views and minimised with respect to the three-view normalisation parameters, k_i.

Fig. 10(a) shows the χ^2 probability for some 200 fits of 4, 6 and 8 prong events to four constraint hypotheses. We attribute the peaking to the second assumption made above, which is unsatisfactory when the problem is no longer dominated by systematic errors. Four events had ionisation probabilities below 0.1%.

Fig. 10(b) shows the measured ionisation for beam tracks from a general sample of events. The factors k_i have been calculated by fitting the ionisation of the secondary tracks. The curve is a Gaussian (σ = 0.17).

Fig. 10(c) is a histogram of measured errors for secondaries with ionisations less than 1.9 -- it is compatible with the width of the Gaussian in Fig. 10(b).

Fig. 11 is a plot of the measured ionisation projected back onto the track and averaged over the 3 views against fitted momentum. The K tracks are taken from a much larger sample of film than the π and proton tracks.

8. CONCLUSION

This report has discussed the performance of POLLY during 1969. Since then further work has brought new improvements. More than 21000 events were measured in two weeks in March 1970 at over 100

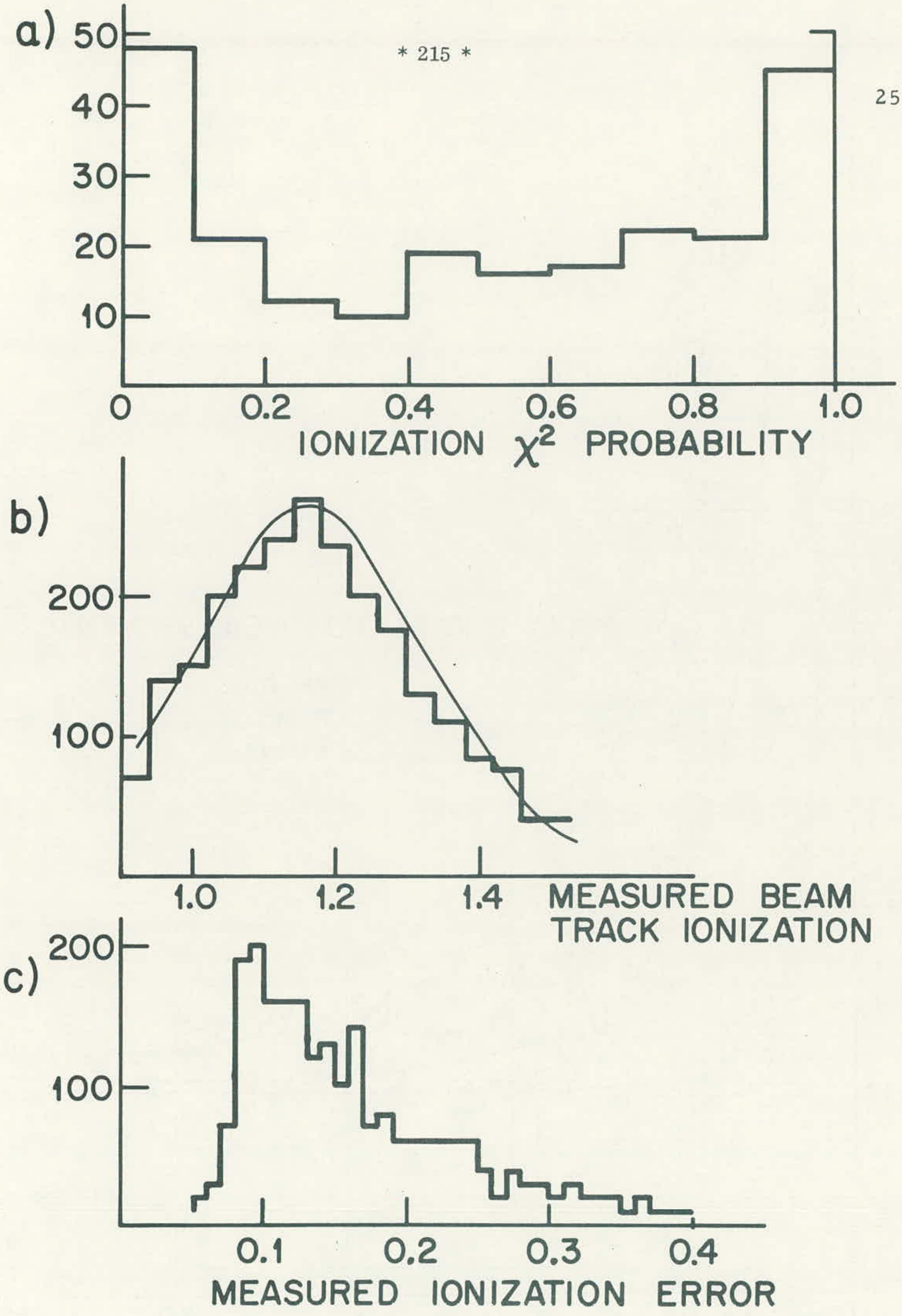

Fig. 10. (a) The ionisation χ^2 probability for 200 4- and 6-prong events with 4 constraint kinematic fits.

(b) The measured beam track ionisation from a general sample of fits, normalised to the secondary tracks. The curve is a Gaussian (σ = 0.17).

(c) The measured ionisation error for tracks with ionisation less than 1.9.

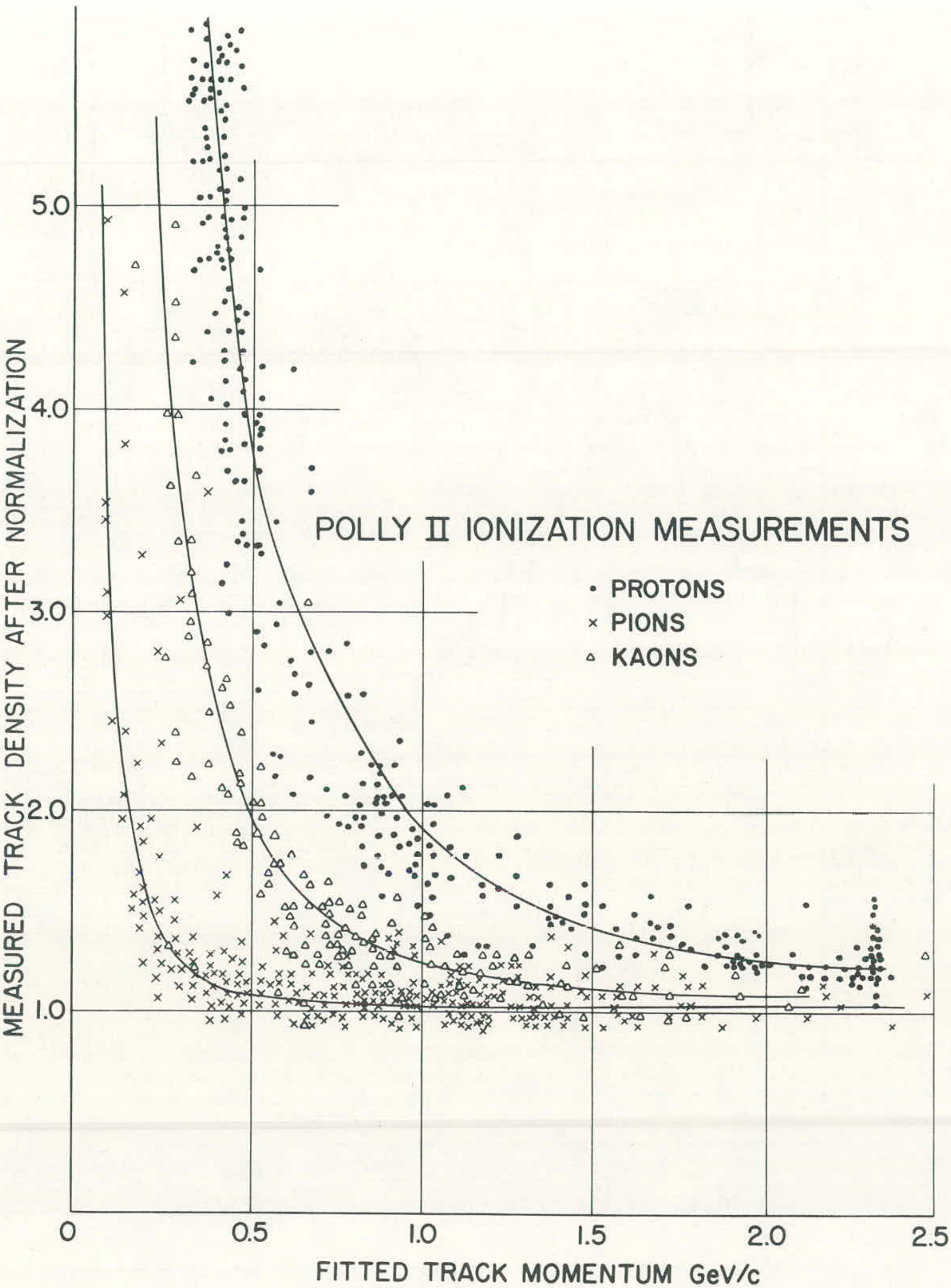

Fig. 11. Plot of fitted momentum against measured track density in space (averaged over views). The curves are the $1/\beta^2$ expectations for K, π and nucleon masses.

events per hour. The present software is set up to scan N-prongs and measure either N-prongs or N-prongs with a single V^o. The extension of the program to handle multiple V^o events with or without prescanning is envisaged as the next stage of development. Problems associated with large chambers, "coat hangers" and scotchlite illumination have been studied, and tracks have been followed successfully on film from the Argonne 12' chamber, the BNL 80-inch chamber and the CERN 1-metre model.

A new device, POLLY III, is nearing completion of the design stage at ANL (March 1970) and four manufacturers have shown interest in tendering for its production. POLLY III will have four film transports, an indexed mirror and one precision CRT. Standard slice scan analysis will be hardware-wired. We anticipate 15 to 20% improvement in overall speed on line to the same XDS Sigma 7 with similar film.

In summary, by pursuing a philosophy of sophisticated operator interaction and simply programmed logic, we have learned how to implement progressively more sophisticated algorithms to reduce the use of the operator. The close liaison between production and development has been another factor in the development of the present system which can process events completely at more than 100 events per hour so that most frames of the experiment are never seen by the human eye.

ACKNOWLEDGEMENTS

The success of this project would have been impossible without the enthusiastic cooperation of a large number of people. Special thanks are due to Bob Zieman and Gary Thelen on the engineering side and to our team of operators for their fine work; lastly to P. McDonald for her care in typing this as well as earlier POLLY reports.

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3) The sensitivity of the program to small angle kinks was tightened at the end of November 1969. Prior to that events with high momentum secondaries within 2 or 3 degrees of the forward direction and whose vertex was near the edge of the fiducial volume tended to get lost.

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6) R.C. Strand, Bubble Density Measurement with the HPD, BCHP-03-O-G (1963) (unpublished).
See also the discussion of bubble density measurement in Proc. of the Intl. Conf. on Advanced Data Processing for Bubble and Spark Chambers, Argonne National Laboratory, ANL 7515 (1968) and references quoted there.

BUDDE: (CERN) You said that you missed about 10% of the events in the scanning stage, have you looked at those 10% of events to find your scanning biases? Which kinds of events have a tendency to fail?

ALLISON: (ANL) I showed you an example in one of those earlier slides of an event that it nearly missed because it overshot the vertex by 6 or 7 mm. This is the kind of problem that occurs in that it just manages to make the far side of the fiducial volume by taking in a few mm. of some secondary track. Now we've made the program aware of this so it effectively tries to work with a larger fiducial volume, but mainly the problems are those of forward secondaries near the edges of the fiducial volume. But when we look at elastics making some reasonable momentum transfer cut we don't see any problem, although we are having problems with elastics further on in the system. So we think we've seen the major aspects of this problem and cured them, but I am sure there are still some left.

6 May 1970 11:00 a.m.

Session VI

Chairman: H. D. Taft (Yale)

OXFORD PEPR SYSTEM

J.P. Berge, J.F. Harris and J.G. Loken
Nuclear Physics Laboratory, Oxford University.
(presented by J.F. Harris)

1 INTRODUCTION

The purpose of this paper is to report the performance of our PEPR system in measuring some 13,000 $\pi^- p$ 2 prong events. The exposures were at 690 and 740 Mev/c in the Saclay 80cm chamber.

Working with zone guidance of 4mm x 4 mm on a single view the system measured events at an average of 150 events/hour including on-line operator helping. The pass rate through Match and Geometry was 87%.

Working with the vertex predigitized to 1mm on a single view, and anti-selecting at the scan table events with a confused beam, the current system measures at 400 events/hour without operator assistance. In this mode the pass rate through Match/Geometry was 91%.

2 GENERAL SYSTEMS DESCRIPTION

The flow of information from the scan table to PEPR and hence to Match and Geometry is shown in Fig. 1.

The zone guidance information for about 5,000 events is loaded onto an IBM 2311 disk pack. As PEPR measures the views in succession the view measurement data is merged into the file.

One's knowledge of the vertex position improves as we go from view to view and this is utilised in the event recognition strategy.

The MATCH program developed at Oxford by Peter Berge is a vital part of our system. It enables the PEPR automatic measuring software to put out extra tracks in cases of ambiguity, and also it can salvage events with one or more event tracks missing in a single view.

Fig. 2 illustrates the hardware facilities used by the system. If one is running in the 'help' mode an operator can assist the auto system with difficult events via display, lightpen and keyboard.

The current production load runs in 30K including a 4K bank for display storage. It is written entirely in Fortran IV except for the routines for basic scanning and measuring, film transport and displays.

Fig. 3 illustrates the Saclay frame format with the reference fiducials and databox at the left of the image.

3 PEPR SOFTWARE

3.1 General

The first production system for measuring with the current hardware, christened PEPHLP, ran from June 1969 to September 1969. It was developed as a stepping stone toward the goal of an automatic zone guidance system. The event recognition in this system was provided by the operator identifying the vertex and one point on each track of the event. This was accomplished by pointing with a lightpen at a display of data obtained by scanning the 10 x 10 mm region around the vertex with a spot.

As well as providing a basic framework for future development this system checked out the data flow through PEPR from the scan table to Geometry. During its lifetime the system measured 7,000 events at a rate of between 20 and 50 events/hour depending on film quality and the operator.

To step from PEPHLP to the current system, (whose general flow is shown in Fig. 4), the following fundamental developments were made:-

(i) New fast basic scanning and measuring routines

(ii) New fast track follower

(iii) Vertex oriented event recognition strategy

Developments (i) and (ii) were entirely 'home-grown', but development (iii) was based on the proven strategy developed by the POLLY group at Argonne.

3.2 Scanning and Measuring routines

These basic routines utilise the 1mm line element to scan for databox lines, fiducials and tracks, and where appropriate to measure them. Both routines provide software selection of narrow and broad pulses. Currently a pulse is classified as narrow if it is less than 60μ wide at ½ height. Between 60μ and 120μ a pulse is classified as broad. This facility is very important when track following; broad data is treated as 'noise' and appropriate logic is entered.

The operation of the scanning routine SCAN and the measuring routine MSCAN is illustrated in Fig. 5.

SCAN input defines scan co-ordinates (a,b), an angle range m_1 to m_2, and an angle increment n. It scans at the addressed point from m_1 to m_2 every n degrees. The area covered on film is approximately 1mm x 1mm. As data is gathered it is histogrammed into bins 48μ wide by planting the current angle into the bin defined by the data interpolation count. If the bin is already occupied the data is ignored. This technique has given a very fast basic element recognition.

MSCAN is used essentially in the measure mode for fiducials and tracks once SCAN has located the fiducial roughly or located a starting point on a track. The normal mode of operation is to scan at a single angle at the addressed point with gates of $\pm 50\mu$. It may also be used to measure angle as well as position by scanning through a range of angles, and defining the angle of the data by the centre of the angle range over which hits are obtained.

3.3 Track Following

TRACK is given a starting element (A, B, ϕ) by SCAN and starts tracking with steps of ½mm using linear prediction. When 2mm have been covered it uses a three point circle extrapolation predicting ahead ¼ of the current prediction chord. It continues in this mode with the step increasing in size until it reaches an allowable maximum of about 4mm. Then the last 16mm of track are used for the prediction, the 16mm sliding along with the track. Another cut-off for the prediction arc is 10^{o} of track. For curvy tracks this cut-off is reached before the chord length cut-off and the maximum step is equivalent to about 2^{o} of turning angle.

The gates for a scan are computed as a function of the step DL and the prediction arc length L. The minimum gates allowed are $\pm 50\mu$. MSCAN is called to scan at a single angle with 'narrow' pulse selection.

When either a gap or 'noise' is obtained when tracking it attempts to back-up first closer to the last point. If this is unsuccessful an attempt is made to bridge the confused region by predicting past it up to a maximum step of $\frac{3L}{4}$ where the prediction errors are about $\pm 120\mu$.

Tracking occurs in three phases - first towards the vertex, then away from it, and finally, if necessary, a retry towards the vertex. On reaching the tentative vertex region an event association check is made by seeing if the track passes through the 'error box' associated with the vertex. If it does not then tracking is terminated.

All tracking is performed in uncalibrated deflection co-ordinates. With the current parameter settings for track following this has proved entirely satisfactory. Track follower typically provides about twenty points/track. At the moment these are filtered to ten calibrated points. Kink detection is performed on the curve defined by the ten calibrated points. The algorithm requires the track to fit a smooth circle. The tolerance is about 20μ for beam tracks but gets much larger for lower momentum. A kink on a beam will usually be detected if the scatter is greater than 1^{o}.

3.4 Some basic measurement times

Typical measurement times for databox, fiducials and tracks are given in Table 1 below.

Table 1

Typical measurement times (for Saclay $\pi^{-}p$)

Function	Time on PDP6			Estimates for Processor = 2 x PDP6
	Total Time	Hardware Time	% Hardware time Unoverlapped	Total Time
Databox reading	100ms	10ms	100%	55ms
Measure 5 fiducials	150ms	10ms	100%	80ms
Track beam across chamber in 'tentative' mode	70ms	6ms	50%	36ms
Track beam across chamber in 'beam follow' mode	40ms	3ms	100%	21ms
Digitise 30mm of track at 20μ spacing	120ms	35ms	2%	60ms

These times are largely limited by the speed of the PDP6 processor, and thus an improvement of 2 in the speed of the processor would give very nearly a factor of 2 in speed.

The quoted track following times are for following a beam across the Saclay film format, a distance of about 40mm. Following a longer distance, as on the format for the CERN 2 metre chamber costs an extra 0.7ms/mm. However, when dealing with the CERN 2 metre format film, one need only follow about 20mm of track in the event search mode. Once the event has been found the extra length of track can be followed - for a 4 prong this would take an extra 100ms to follow an average of an extra 30mm on each track.

3.5 Event strategy

Tracks are searched for approximately radiating from a crude vertex (1mm to 2mm on film), and are followed as they are found. The search pattern is a variable number of circles centred on the vertex as shown in figure 6. Through tracks are linked and deleted, and an accurate vertex determination is attempted after each circle is computed. If a good vertex is found with all the correct tracks passing through it, the event is considered measured; if not, the search continues with another circle, or until hope is abandoned. In addition, for the present experiment it was found necessary to initiate an extra beam search at large radii for difficult events. If these efforts fail, at this point an operator may help the system when running in the "HELP" mode.

Since the fundamental principles have been described in talks about POLLY it is not necessary to describe them further here. However, it should be pointed out that the routines had to be considerably developed and modified for the following reasons:

1) The PDP6 computer is slower that the Σ-7

2) The PEPR hardware is <u>much</u> faster than POLLY

3) The PEPR line has useful advantages

Using points 2) and 3) to full advantage has enabled an extremely fast system to be developed in spite of point 1).

3.6 Operator help facilities

These were used in 2 modes for the $\pi^- p$ production.

a) To indicate the vertex with the light-pen on the first view. The operator need only indicate the vertex to an accuracy of better than 1mm. He is presented with a display of the 7 x 7mm area centred on the uncertainty box provided by the scan zone information. This is illustrated in Fig. 7.

The overhead in this operation is about 2 secs/frame.

b) When the automatic event strategy failed to identify the event unambiguously then the operator was given the opportunity to assist the system by a required combination of vertex identification, track addition and track deletion. The operator is given an appropriate message such as 'TWO FEW TRACKS', 'NO BEAM' or 'MEASURE VERTEX'. An example is shown in Fig. 8.

Typical reasons for operation intervention were

(i) Confused beams - in this case the operator makes no attempt to assist since the automatic system has already tried special beam search logic.

(ii) Very weak track due to poor illumination in chamber. In this case the operator tries to identify the track, and an attempt is made to follow the track - if necessary the threshold is dropped to a very low level, up to 5 tries being made at successively lower thresholds.

(iii) Production track goes through very confused region. The operator attempts to select a clear point with the light pen.

(iv) Illegal beam- a check is made for the beam momentum. If this is outside the allowed error limits then the system allows the operator to check the event. In many cases the cause is a 'rogue' beam which also causes problems in succeeding views since in all probability it is outside the normal spread in z of beams. Thus the extrapolation from the 1st view to the 2nd does not reflect the correct error on the vertex position.

(v) Scanner error - an event was passed through with a very short stopping secondary which was impossible to measure with the line. This was outside the terms of reference of PEPR measurement, and so no attempt to help was made.

By far the largest category was (i).

4 PEPR PERFORMANCE

A summary of PEPR measuring performance over a 10,000 event batch is given in Table 2 below.

Table 2

Week 9.2.70 - 13.2.70

Experiment 17 (740 MeV/c, $\pi^- p$ R.D. Film, 2-prongs)

Elapsed time (Hrs)	PDP6 Time (Hrs)	Lost time* (Hrs)
80	64.6	15.4

*Time-sharing of processor, operator training + breaks, film changing.

Auto events	Helped events	Total events
8,728	1,370	10,098

Total events/PDP6 time : 156 events/hour

Helped events/total events: 13.5%

The history of these events through Match, Geometry and Kinematics is shown in the first entry in Table 3 below

Table 3

Event History

	Fail Match/Geom	Fail Kinematics	Fit Kinematics	Multi-neutral or doubtful
All events	13%	1.5%	78.5%	7%
Selected scan sample	5%	2%	84%	9%

Helix Fit Statistics are shown for PEPR measurements on Fig. 9. They peak at 7μ and have a 1% tail beyond 25μ. All PEPR measurements were done using a 6μ least count.

For comparison a sample of 170 events were measured on a manual machine with a least count of 2μ. The resulting helix fit statistics are shown on Fig. 10. Even with the limited statistics it can be seen that the peak is shifted to 11μ.

The contribution to the helix fit errors purely from uncertainties in the chamber constants has been estimated as about 5 - 6μ.

4.1 Match/Geometry reject reasons

13% of all measured events fail in MATCH or GEOMETRY

	Percentage of all events	Percentage of reject events	
1)	2%	15%	Gross failure
			1/3 events cannot be found
			1/2 too few tracks are found on 2 or more views
			1/6 fiducials can not be measured
2)	5.1%	39%	VERTEX LOCATION PROBLEMS
			1/3 tracks in some view fail to intersect
			2/3 Vertex points in the several views are not corresponding points
3)	3%	23%	FAIL IN MATCH
4)	2%	15%	2 View measurement failures and troubles
			1/5 2 view measurement fails MATCH
			4/5 poor stereo on some track measured in 2 views

5) 0.2% 2% Assorted Measurement Failures and Program Shortcomings

(Charge balance failure, no beam track, etc.)

6) 0.6% 6% GEOMETRY FAILURES

1/3 MATCH passes marginal unassociated track images

2/3 probable Geometry arithmetic troubles

4.2 Performance Summary

A reasonable measure of efficiency of performance by a measuring system is the percentage of all events that have to be reinspected and remeasured after the first measured attempt. At our momentum, less than 1% of all two prongs are from events with two or more neutrals; all the rest are from elastic scatters or single pion production. Thus, to a good approximation, we should expect all events to fit some production hypothesis. We have found that measurements of similar events at close beam momenta with our image plane digitizers have to be repeated between 25% and 30% of the time. In the case of the PEPR measurements, the unsatisfactory results are between 15% and 20%. Thus, PEPR is now, on its first production measurement output a better system than our manual measuring system, even ignoring the superior quality of the PEPR measurement themselves.

5 POST MORTEM DEVELOPMENT AND SYSTEM EVALUATION

A brief study was made of the effects of imposing more severe scanning criteria in selecting our data sample. Three rolls containing 659 events were studied, of which 62 failed in MATCH.

Events were excluded where the event:-

(i) was incorrectly zoned on the scanning list.

(ii) did not have the beam track clear by at least 100μ on film for at least 5 mm in at least 2 views

(iii) did not have all production prongs longer than 3mm in all 3 views

(iv) was not the correct topology (e.g. a Dalitz pair or a 4 prong)

This anti-selection reduced the sample to 545 events, a reduction of 17%. Almost all discarded events were for reason (ii).

The performance of the 545 event sample is given in the second entry in Table 3. It was clear that the easiest way to significantly improve our performance was to be slightly more selective in scanning.

5.1 <u>Automatic running with pre-digitised vertex</u>

As a result of our study on our test sample of 659 events we decided to evaluate the performance of the system running completely automatically with the vertex digitised to 1mm accuracy on the first view.

A summary of the criteria for the run and the resulting performance are given in Table 4 below

Table 4

Scan criteria for automatic run on $\pi^- p$ (no operator help)

1 No track image shorter than 3mm in any view

2 Beam must be clear of other beams in at least 2 views (by 100μ for at least 5mm)

Selected Sample (2 rolls) 315 events

PEPR measuring rate 380 events/hour

MATCH pass percentage 91%

Thus this run gave a satisfactory performance through Match + Geometry at a greatly enhanced measuring rate. The percentage of events rejected due to the scan criteria was about 15%, mostly for criteria 2.

5.2 Time Breakdown for Software

A breakdown of time utilisation is given in Table 5 below.

Table 5

Time breakdown for $\pi^- p$

Running with pre-digitised vertex on one view (to 1mm accuracy)

Activity	% Total time
Film transport	40
Track following	22
Track search + kink detection	17
Databox + fiducials	8
Storing + calibrating track data	4
Various book keeping + I/O	4
Threshold setting	2
Vertex check	2
Track linking	1

Measuring rate: 380 events/hour

with ionisation: ≃340 events/hour

It is obvious from this table that the easiest way to speed up the throughput (from the software point of view!) is to have a faster film transport. For the $\pi^- p$ experiment there was, on average, an event every four frames, the average event to event film transport time being 1.2 seconds.

Another way to speed up the measuring rate is to code the vital parts of TRACK in assembly code, while still retaining the logical framework of TRACK in Fortran IV. This will be done in the near future.

5.3 Some 'Wishful Thinking'

It is interesting to predict the measuring performance of the current software + hardware system if it were driven by a computer whose CPU performance is twice that of the PDP6. Another desirable feature would be an event to event film transport average time of ½ second, this being accomplished by a faster film transport and an average, say, of 1 event every two frames.

Such an estimate is given in Table 6 below.

Table 6

Measuring System

Current Oxford hardware + software

Computer X (≡ 2 x PDP6)

Fast film transport

Input

2, 4, 6 prongs pre-digitised to 1mm in one view (vertex only)

-confused beams antiselected

-frequency ≃ one event/2 frames

Performance Estimate

Event recognition and measurement	1.0sec
Ionisation (30mm for 5 tracks)	0.3sec
Film transport time	0.5sec
Average time/frame	1.8sec

Measuring rate = 675 events/hr (?!)

6 CURRENT DEVELOPMENTS AND A FORWARD LOOK

All scanning and measuring for the $\pi^- p$ experiment was performed with the 1mm line element. The basic spot scanning and measuring routine has been written, and is currently being used to gather data for our ionisation algorithms which are in the process of development. It will be also used to find and measure short tracks (less than 1mm) which cannot be found with the 1mm line. The basic spot routine SPOTTY will also be used for measuring end points on tracks, and following very curly tracks which, with the 1mm line, give broad pulses. The lower limit for the 1mm line is about 3mm radius of curvature on films.

The next experiment for our current system is a 3.6 GeV/c $K^- p$ exposure in the CERN 2 metre chamber. Both normal and reverse developed film will be processed, a total of 100,000 events. It is hoped to go into production on this experiment by May.

Once the $K^- p$ experiment is solidly in production development work will begin on developing automatic scanning techniques.

7 ACKNOWLEDGEMENT

We would like to acknowledge the contribution of Dr. C.A. Wilkinson to the system. Dr. Wilkinson implemented our disk handling software, and was responsible for the original versions of the databox and fiducial routines from which the existing routines have developed.

DATA FLOW FOR OXFORD P.E.P.R. GRID GUIDANCE

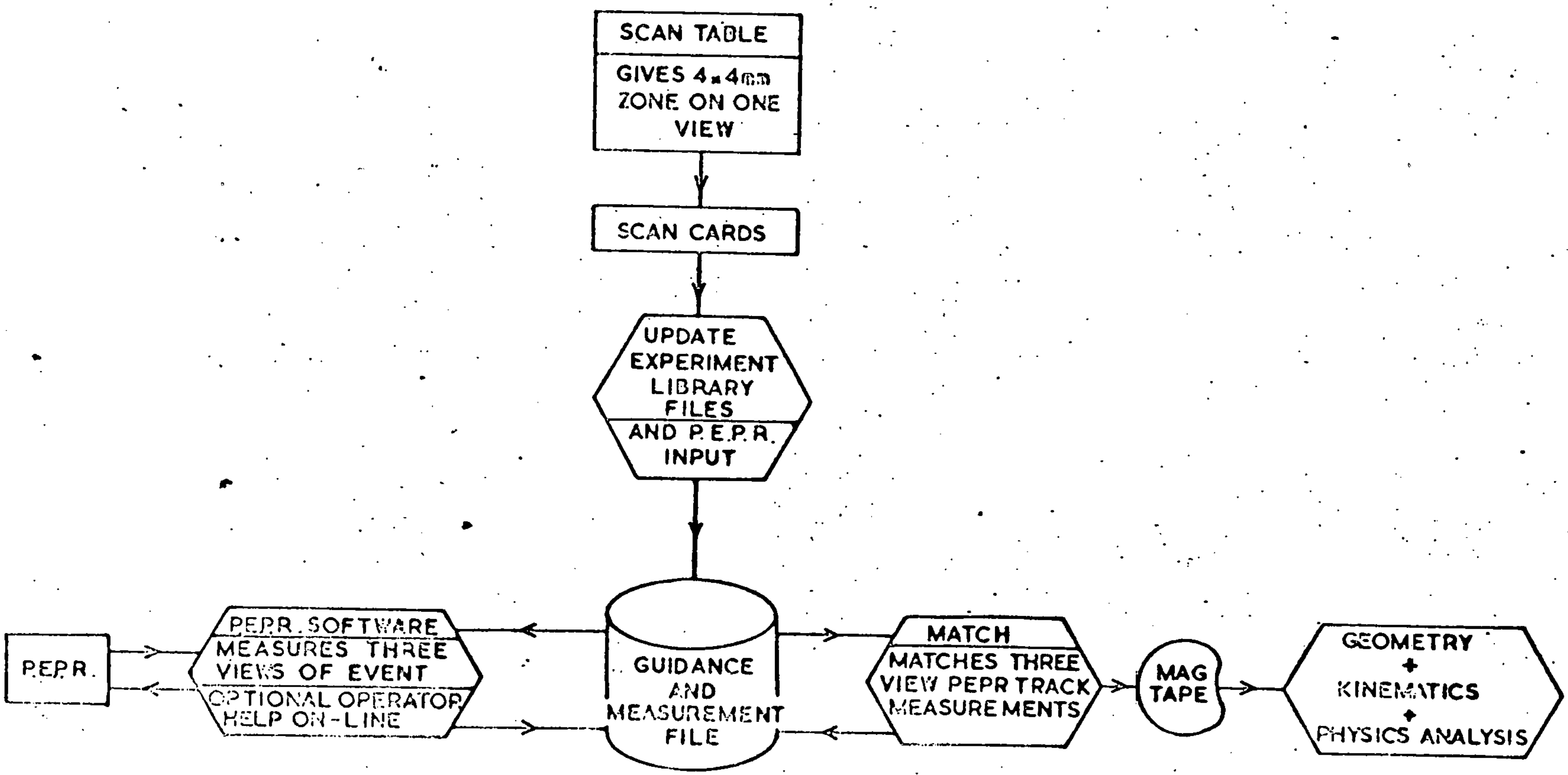


FIG. 1

HARDWARE SYSTEM SCHEMATIC

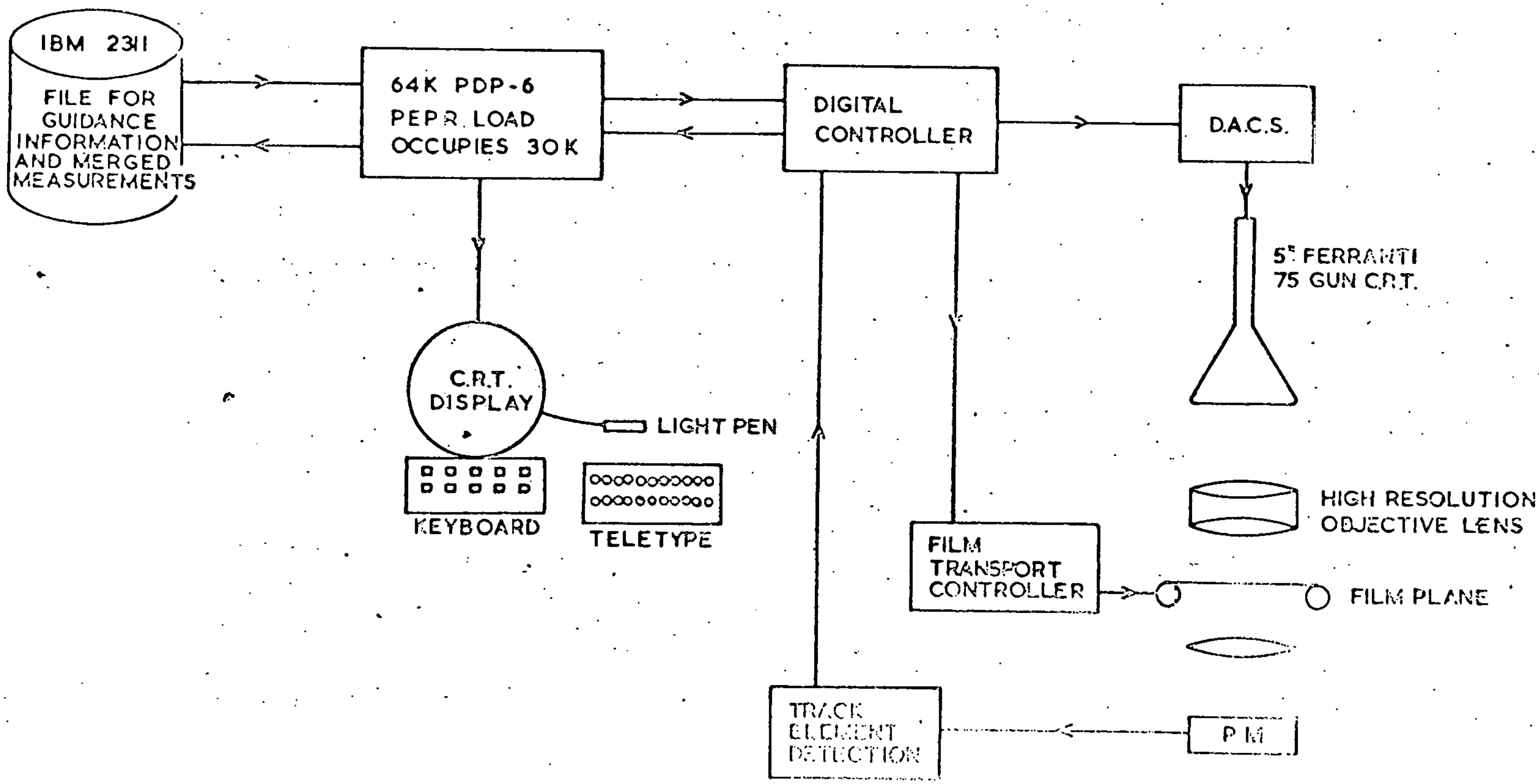
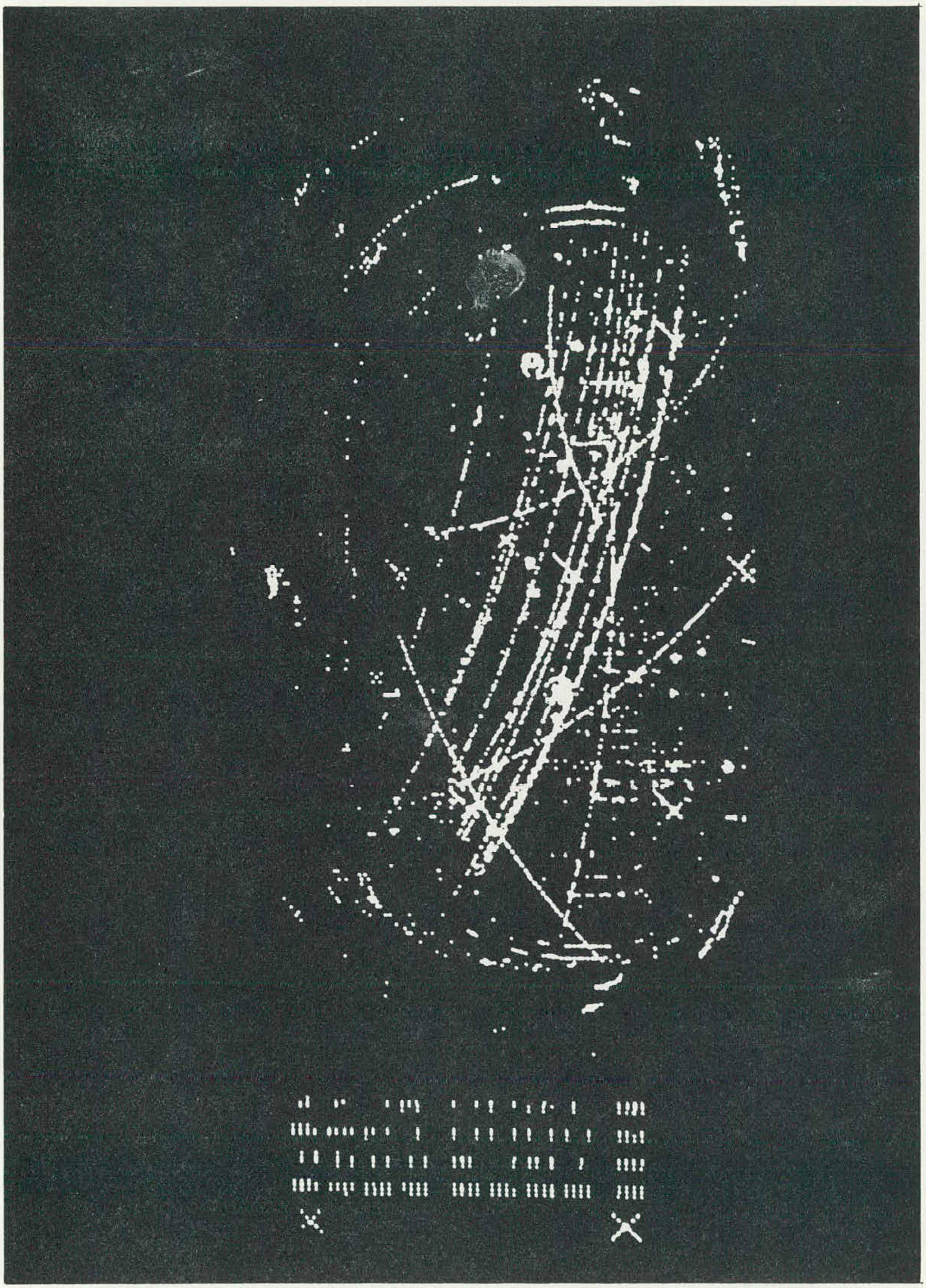


FIG. 2



OXFORD PEPR

EVENT MEASUREMENT - GENERAL FLOW

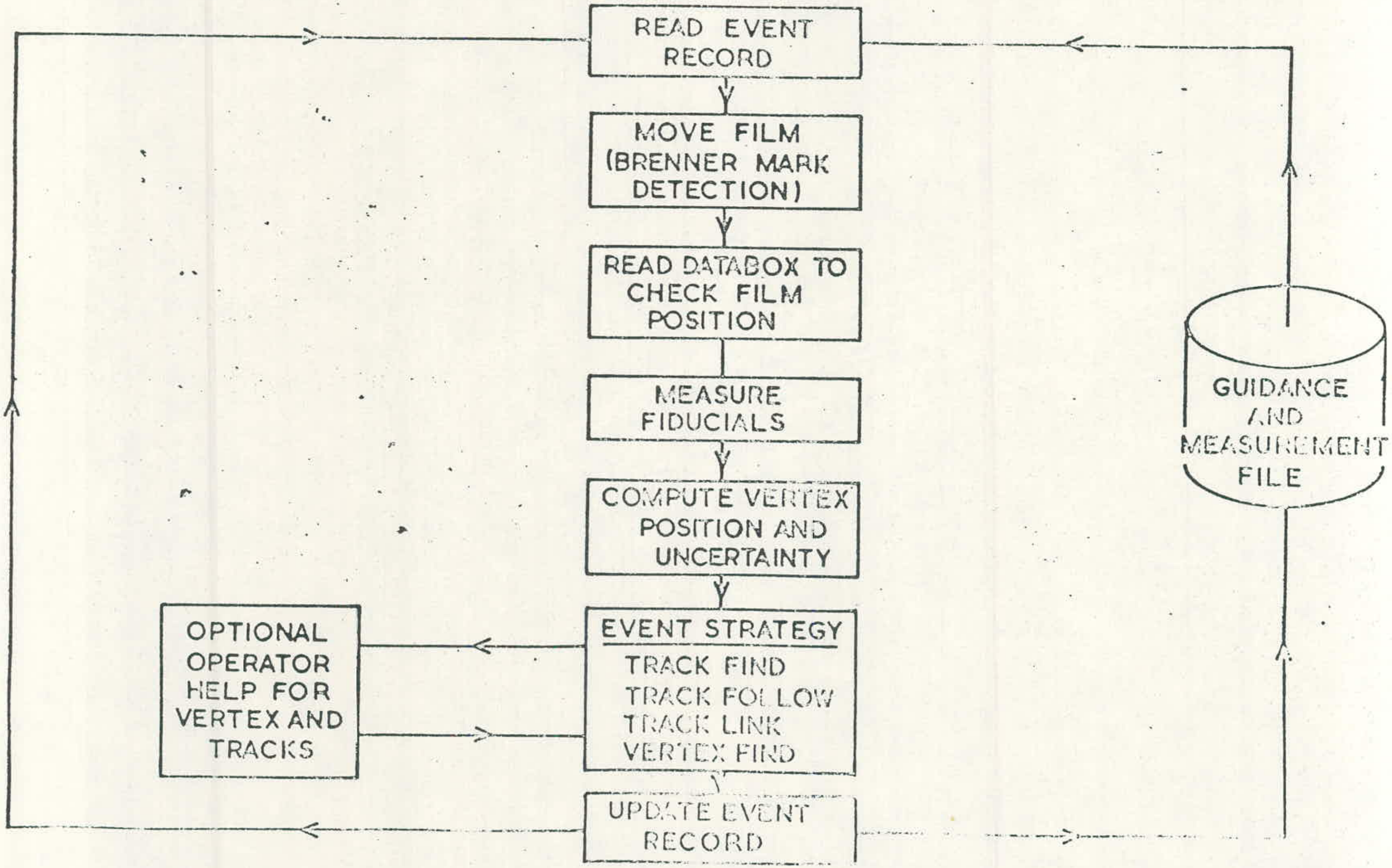
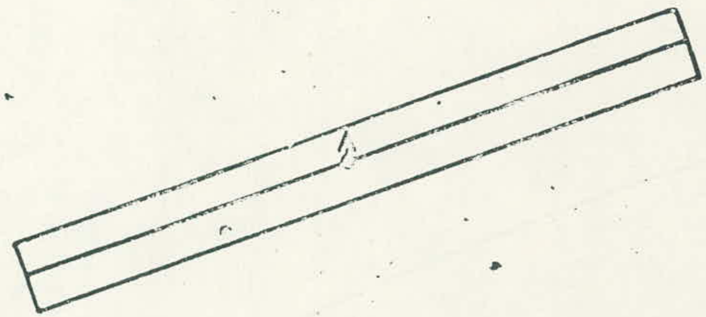


FIG. 4

BASIC SCANNING ROUTINES

"MSCAN"

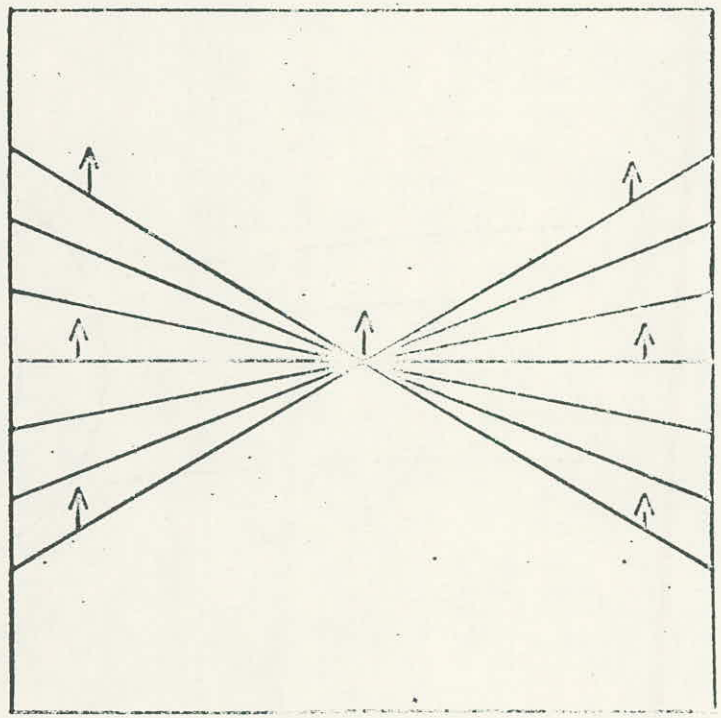


TYPICAL USAGE

1mm LINE x 100μ SWEEP

SINGLE ANGLE

"SCAN"



1mm LINE x 1mm SWEEP EVERY

4 DEGREES OVER RANGE

FIG.5 SCAN GEOMETRY FOR BASIC SCAN AND MEASURE ROUTINES

TRACK SEARCH STRATEGY

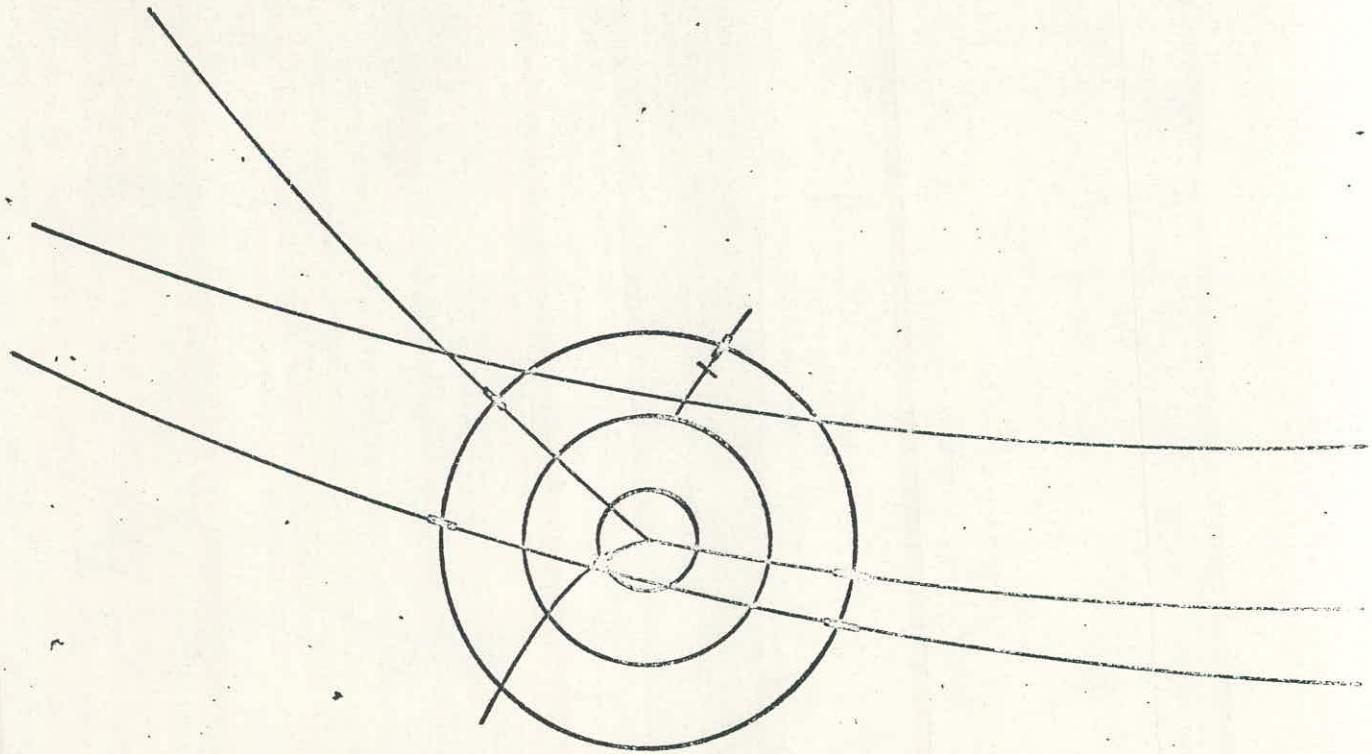
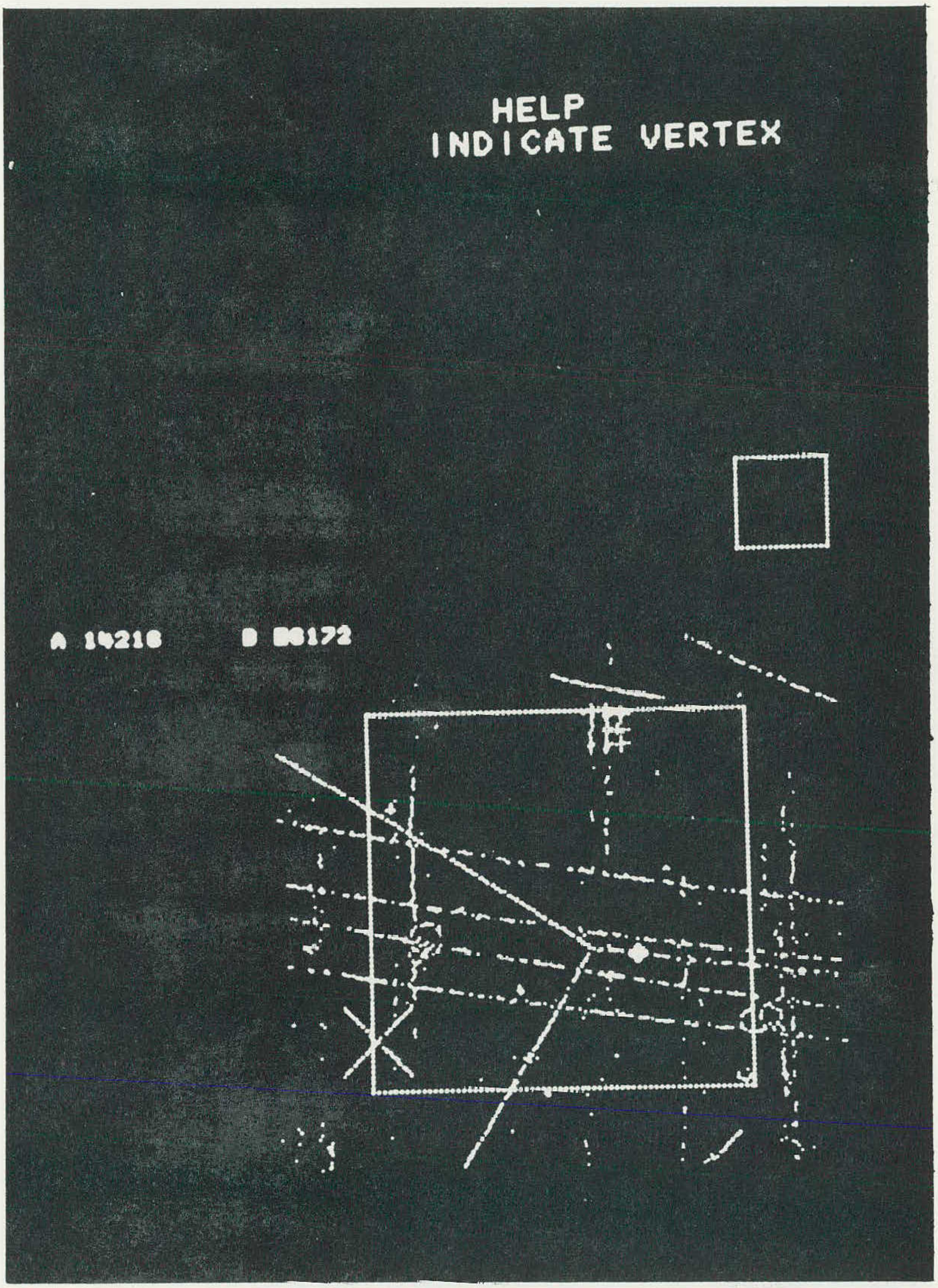
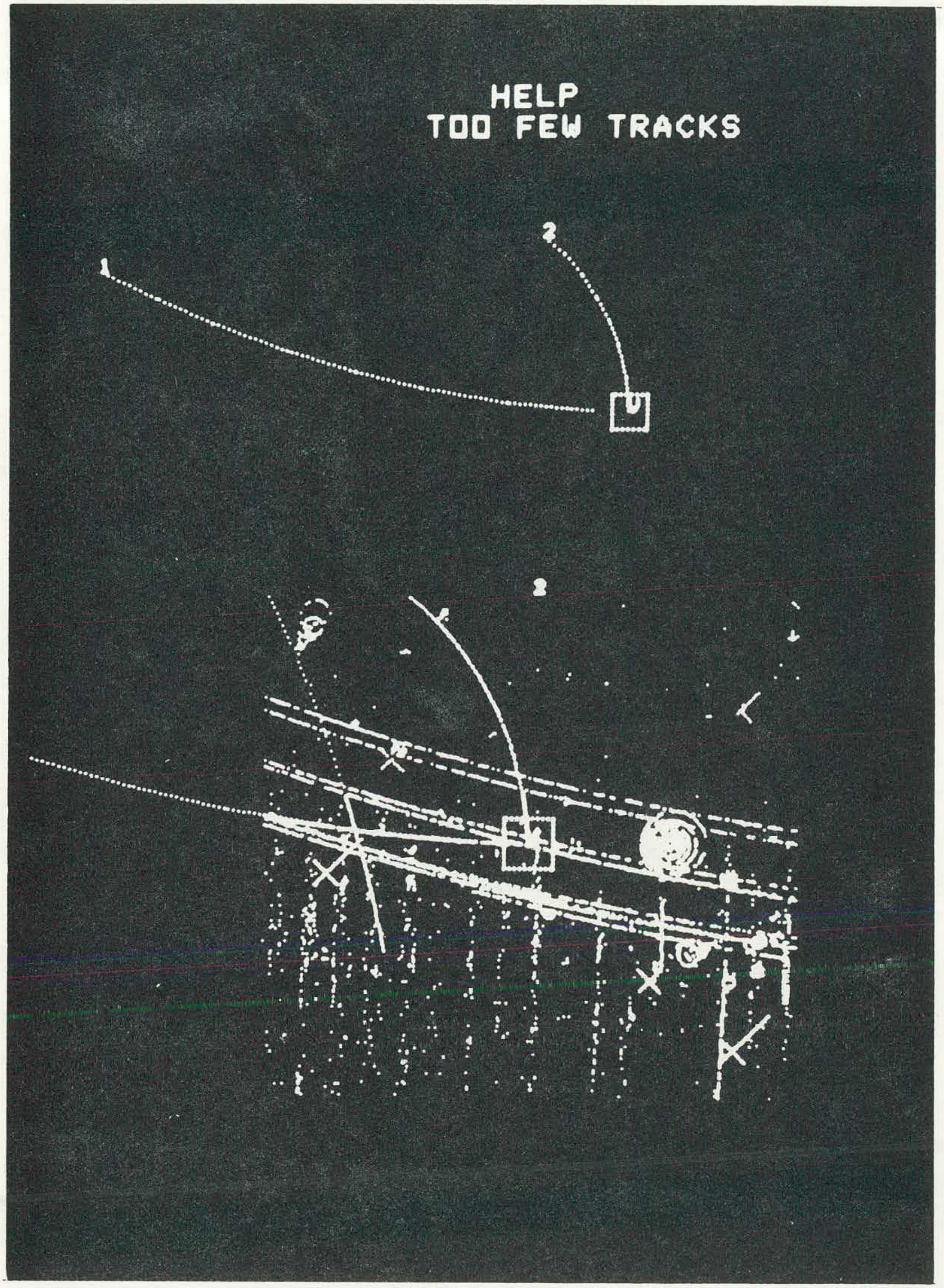


FIG 6 ILLUSTRATION OF SEARCH PATTERN FOR START POINT ON TRACKS

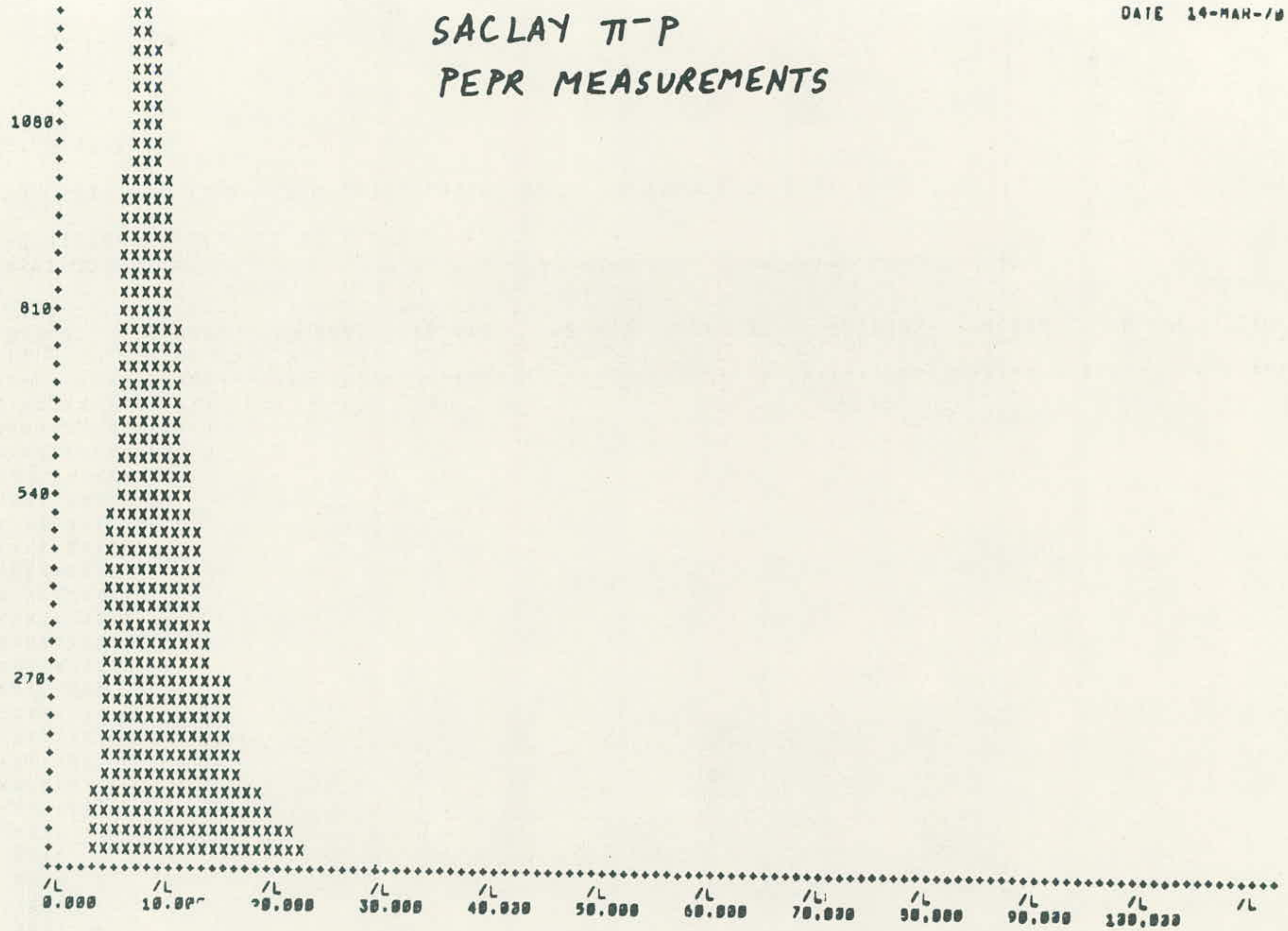

HELP
INDICATE VERTEX


HELP
TOO FEW TRACKS

SACLAY $\pi^- p$

PEPR MEASUREMENTS

DATE 14-MAR-78

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         150221086532211111
         1125481020588711075422211
         5280379890398497318403686677787488561345 234542121   121 1 1 111  21  2   21   111     11  2    2
```

THE PLOT CONTAINS 10135 EVENTS, X= 27 EVENTS, IMAX= 46, 0 EVENTS OUTSIDE PLOT

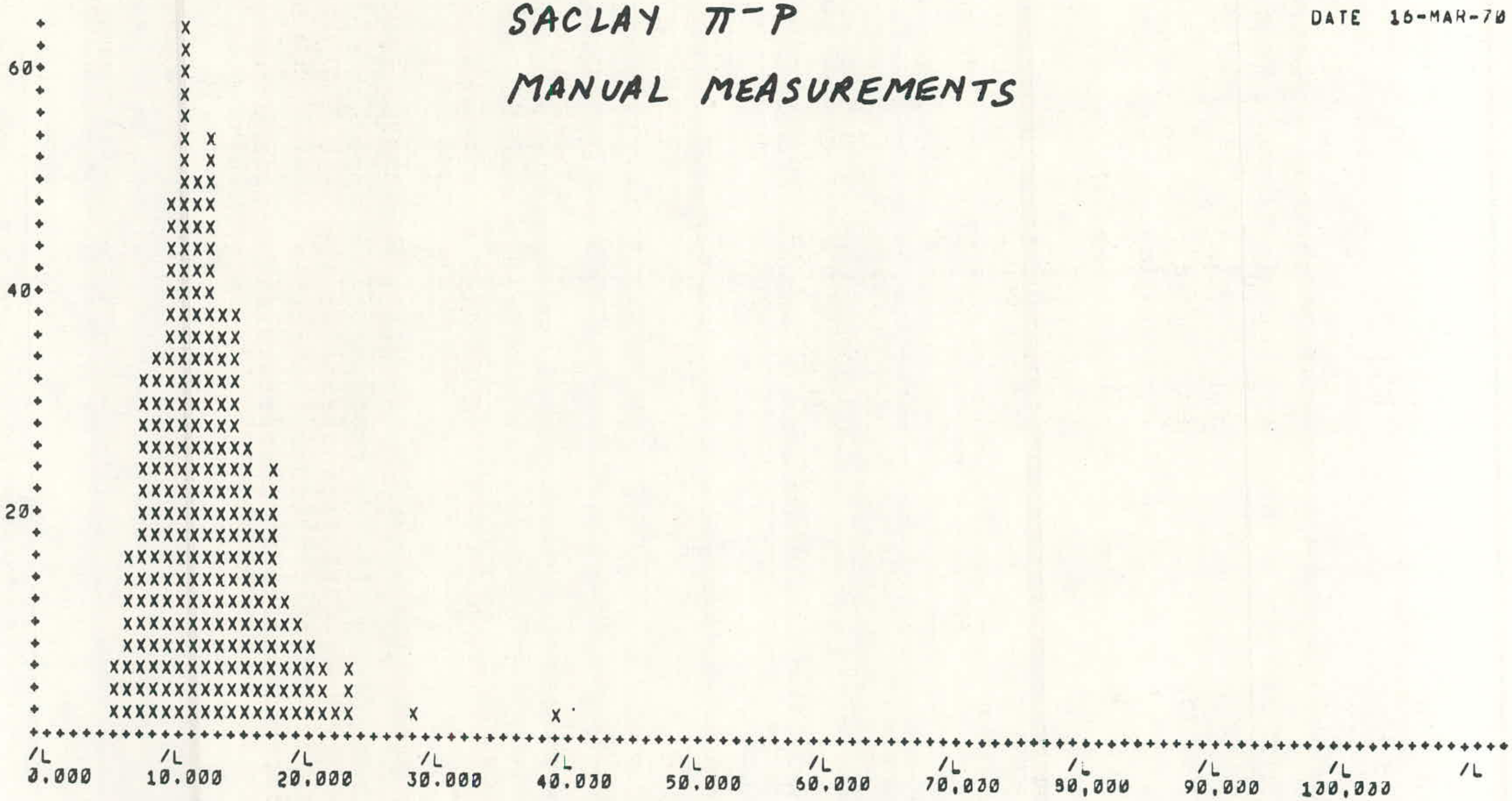
P-CODE 1/ 1

HELIX FIT STATISTICS

SACLAY $\pi^- p$

MANUAL MEASUREMENTS

DATE 16-MAR-70




```
     13346553322211
    1673585149870420962 61  12 1  1 1 1 2  1                1
```

HE PLOT CONTAINS   515 EVENTS,X= 2 EVENTS, IMAX= 32,   0 EVENTS OUTSIDE PLOT   P=CODE 1/ 1

HELIX FIT STATISTICS

DISCUSSION

TAFT: (Yale) Are there comments or questions on Frank Harris' talk other than ionization on which we'll have a few remarks after Barney Brooks.

ALLISON: (ANL) That was very impressive, I am glad I'm coming to Oxford. I would just like to point out one thing, could you tell us what percentage of a given track, going say half way across the chamber, you in fact digitize. Except when one is scanning beam tracks we digitize densely, that is to say we digitize all the track we can, partly because we are trying to pick up ionization information but also we are trying to get good fits, but you seem to have those anyway, so maybe this isn't doing us any good, so I'd like to hear.

HARRIS: (Oxford) Well, we're putting our 10 master points, and in fact, I shouldn't even say 10 master points. When we track follow we may pick up as many as 20 or 25 points and we just pick 10 points equally spaced. We don't, in fact, do any averaging, which again would give us in principle increased accuracy.

ALLISON: (ANL) That wasn't quite what I meant, I mean as you are going along the track. To begin with when you don't know quite which way the track is going, you have to just move slowly.

HARRIS: (Oxford) At the begining, the track follow step is 1/2 mm till it gets up to 2 mm. That's a parameter in the program. Then the prediction step gradually expands. From prediction step of L we predict L/4 ahead; L expands up to a limit and there are two cutoffs on the limit, 10 degrees of turning angle or 16 mm. So when it really gets going we're predicting from 16 mm, jumping 4 mm ahead.

ALLISON: (ANL) At the end of the track, how much of it have you done?

HARRIS: (Oxford) The average track length is, say 20 mm, at the end we've got spacing of 4 mm, I don't know how you define what fraction of track that is.

ALLISON: (ANL) O.K. so steps of 2 mm half way down.

WATTS: (MIT) May I just add a comment to that, you're sweeping with a 1 mm line segment?

HARRIS: (Oxford) 1 mm

WATTS: (MIT) The line segment averages over 1 mm so if the average step is 2 mm, you digitize perhaps half of the track.

HARRIS: (Oxford) Can I make another comment about that, for simplicity we decided not to play around with varying length of the line segment. If we were to go to higher energies we'd make the line longer, because we use this narrow TED selection. With the 2 mm line length, at TED widths of 60 microns or less the 2 mm line will only get hit at plus or minus 1 degree which gives us a very fine probe when we're scanning at a single angle.

VAN de WALLE: (Nijmegen) I also have a question about your sweep, do you sweep in only one mode?

HARRIS: (Oxford) Yes, I should have said all of this is being done in the PR mode with a 6 micron least count and only one sweep range except that we gate it. It is always physically sweeping one millimeter except that we are gating it when we're track following or measuring fiducials.

VAN de WALLE: (Nijmegen) And you slowed it down to 60 microsecs.

HARRIS: (Oxford) We've slowed it down to 60 microsecs, ie a factor of 5. PR and PE both sweep at physically the same speed. In fact, my feeling is we could slow it down by a factor of 3 again, and it wouldn't make any darn difference. We just don't need to run that fast.

ALLISON: (ANL) I'd just like to make a comment on that final thing that we speeded up POLLY by the hardware sweep speed by a factor of 2 keeping all other parameters constant and you had to know the program and things very well to see that anything had changed whatever, either in the quality or in the speed of throughput.

WATTS: (MIT) You sweep with one angle when you are track following?

HARRIS: (Oxford) After the first 2 points.

WATTS: (MIT) Yes, right, and so you put on an N versus phi display and said it was very good, I wonder if you know exactly how good it is.

HARRIS: (Oxford) We aren't using the spot at all so you can have no pinwheel but you can still have N versus phi effects. We've got no pinwheel effect in our system. We seem to have done a very good job of lining up the tube. I've looked at the maximum pinwheel right at the edges. It is something like 15 microns, so we don't have to worry about that effect.

The Oxford PEPR Hardware

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Contents

1 Introduction

During the commissioning of the Oxford PEPR I a number of significant hardware modifications have been made. These have resulted in an improved performance and they suggest changes to the PEPR II system, now under construction.

It is the object of this report to outline the PEPR I modifications and to describe the essential features of PEPR II.

2 PEPR I Hardware

The basic hardware configuration of PEPR I has remained unaltered. However, the following changes and additions have been made:

(a) Ferranti Cathode Ray tube (with Back Focus coil)

(b) EHT Shunt Regulator

(c) Light servo

(d) TV system

(e) Analogue dynamic focus

2.1 Improvements in CRT performance

The CRT that we have been using for the last twelve months in our production PEPR is a Ferranti 5G 75 Q4 MA. We find it a worthwhile improvement over the Dumont KC 2411: its performance is summarised in Table 1 below. It gives the same light output from a 14-19μm line as the Dumont tube gives from a 26μm line.

We use exactly the same line-up of CELCO magnetic components as at MIT, except for one addition: a high-quality focus coil mounted over the electron gun[1]. This extra component makes the spot three or four times brighter than it is on a standard Ferranti CRT of this type, and it completely removes a rather bright "halo" (due to secondary electrons struck off G_2) which otherwise appears round the spot-or line - when the CRT is driven beyond some 17v. (0.17 of cutoff).

The reason for this (Fig. 1) is that the Ferranti type 71 and 75 electron guns (unlike the Dumont one) use an electrostatic lens formed by two tubes, at final anode potential and G_2 potential respectively, to form a second crossover in the electron beam. The main focus coil acts as a lens whose "object" is this second crossover and whose "image" is the final spot on the phosphor. The advantage of using a second crossover is that it can be made smaller than the first crossover formed by the emission triode. In turn, this makes the final spot smaller. How much smaller the second crossover becomes is determined not only by the demagnification produced by the electrostatic lens, but also by the aberrations introduced by it. We found that the theoretical demagnification m_1, of the 75 gun was 1/12, but the second crossover was not less than 1/5 the size of the first. The difference is due to the aberrations.

Fig. 1 shows how we have introduced an extra focus coil to form the second crossover. The advantage is that now the demagnification is under the users control, and few aberrations are introduced because of the much higher quality of the magnetic lens. The electrostatic lens is still there, of course, but since we form the second crossover actually within its gap it is now acting as a "field lens" rather than as an "objective" so that its aberrations have only a minimal effect on the image quality.

For the magnetic lens, we are using a CELCO B-1613-1 with 1.2A in one gap only. This gives an overall demagnification m_2 of about 5 to 1. The size of the second crossover is roughly the same as in the un-modified gun. But as in light optics, the intensity of this crossover is proportional to the square of the magnification: thus we get an increase of intensity (beam current/unit solid angle) of $(\frac{12}{5})^2$. Taking into account the increased distance 1 between crossover and limiting aperture, we find the final increase in the beam current due to adding the coil is:

$$\frac{I_{b2}}{I_{b1}} = \frac{\omega_2}{\omega_1} \left(\frac{m_2}{m_1}\right)^2 = \left(\frac{1_1}{1_2}\right)^2 \left(\frac{m_2}{m_1}\right)^2 = 3.2$$

which agrees well with what we observe.

A minor bonus is that the beam leaving the limiting aperture fills a smaller solid angle - this reduces the effect of aberrations in the main focus coil and yoke. A more important bonus is that the extra focus coil acts as a crude spectrometer, which filters out of the beam any slow secondaries originating from G_2 (see Fig. 2). This allows us to use grid drives up to 40 volts - 0.4 of cutoff - without any visible secondary-electron halo. At such a drive the maximum light output that is safe for the phosphor can be got out of a 1mm or 2mm long line; i.e. at least 7 times more than that obtained from an unmodified 75 gun tube driven to 17 volts, where secondary- electron halo just begins to appear.

At present we use a final anode current of 160μA when forming a 1mm line, and the transient performance of the EHT supply - Walden 545A - is inadequate.

To overcome this deficiency a shunt regulator has been incorporated. This circuit consists of a high voltage triode (connected as a shunt load to the power supply) and an operational amplifier.

The amplifier monitors the total current drawn from the supply, and adjusts the triode grid drive to maintain the load as a constant value.

Without the regulator, switching from spot to line resulted in a EHT voltage change of 140 volts with a time constant of 20m sec. The regulator reduced this error to less than 500mV peak to peak.

2.2 The light servo

The light servo was first implemented to compensate for variations in the CRT grid base. These changes were attributed to thermal effects resulting in small distortions of the gun structure and they produced changes in the light output from the spot of 2:1 at a fixed grid drive.

The servo (see Fig. 3) consists of the three standard monitor photomultipliers, a summing amplifier and servo amplifier. The monitored signal is compared against a reference input and the error provides the necessary grid drive to the CRT.

The servo response time is less than 0.5 microseconds and consequently it can compensate* for phosphor variation which have wave lengths similar to tracks. This operation eliminates the need for making the film channel gain proportional to light output and allows the gain function to be placed under software control.

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* At the current scanning speed of 33 microns/microsecond

2.3 TV System

As an aid to hardware and software development an analogue TV system (see Fig. 4) has been built. This uses the normal CRT and photomultiplier to provide grey level pictures.

The scan can cover the total format at a rate of one frame (1000 lines) per second or 1 cm square (500 lines) 20 times per second. The line scan is triangular which minimizes the required deflection amplifier bandwidth. However, this necessitates a stepping frame scan to maintain parallel sweeps.

The centre of the picture cell is determined by the current values of the A and B registers. With the aid of a speed ball and simple MACRO program the A/B values may be changed dynamically providing easy access to all parts of the format.

A system of comparators detect when the analogue sweeps pass through zero and display a cross to indicate this point. To determine the co-ordinate of a feature on the film, the centre of the scanned area is changed (using the speed ball) until the point of interest coincides with the cross. This position must correspond to the new A/B values (as the analogue sweeps make no contribution at this point) and can be read by software.

As a consequence of installing the TV system we have to provide an analogue dynamic focus supply. This unit monitors the total deflection current and with the aid of a quarter square multiplier provides an $X^2 + Y^2$ function to drive the standard CELCO power amplifier.

3 Performance of PEPR II

The PEPR II system currently under construction at Oxford will contain the following new components:

(a) A Ferranti 9 inch, 75 gun, Q4 CRT

(b) A 1.5:1 objective lens designed by Dr. C. G. Wynne* (Imperial College)

(c) A new signal processor

3.1 9 inch CRT and optics

The CRT and magnetic components have already been intensively studied and the results have been reported by Willder[2]. Table 1 reproduces the essential line width values‡, with the equivalent performance of the PEPR I system for comparison.

Table 1 CRT line width performance

	CRT line width		Image Plane line width	
	Centre	Edge ‡	Centre	Edge
PEPR I 5" CRT 1:1 optics	14.6μm	19μm	15.6μm	22μm
PEPR II 9"CRT 1.5:1 optics	15μm	20μm	13.4μm	17.4μm

Line widths measured at 60% intensity.

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* Currently under construction by Wray Optical (Rank) Ltd.

‡ The image plane line widths relating to PEPR I are measured values and for PEPR II they are predicted assuming the theoretical OTF of the lens.

‡ 5" CRT 50mm radius, 9" CRT 94mm radius.

The above results were obtained using standard CELCO components and with the application of a Back Focus coil and dynamic astigmatism correction.

The lens has been designed for the convolved (Q4) phosphor - (S11) photocathode spectrum* and has a front focal distance of 41.92 cm, a throw of 97.74 cm and an aperture (at infinity) of f/2.8.

The predicted OTF gives a modulation of 50% at 40 cycles/mm (in the image plane) and an acceptance test demonstrating not less than 40%‡ has been specified.

With this combination of CRT and lens it will be possible to cover two views of the CERN 2 metre chamber, accessing the third view via a moveable platen. Recent results[3] have indicated that a reduction of 50% in the working light level would not be detrimental to the system performance. Therefore, we are considering the introduction of a pellicle, producing two image planes each capable of covering two views of the 2 metre chamber.

Although no definite decision has been made, with respect to the optics for looking at film from the Big European Bubble Chamber (BEBC). The most likely solution will be a 2:1 demagnification from the 9 inch tube which will cover the BEBC format including the proposed data box. Extrapolating from our 1.5:1 lens it appears reasonable to expect an image plane line width not more than 15 microns, and with an adequate signal/noise ratio the detection of 10 micron tracks appears quite feasible.

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* Spectrum peak at 0.410 microns and 10% points at 0.386 microns and 0.473 microns

‡ The corresponding value for the 5 inch 1:1 lens is 25% at 40 cycles/mm.

3.2 The Signal Processor

In view of the large dynamic range of the data from BEBC* film it is considered essential to produce a signal processor with the maximum flexibility. This will ensure an optimum performance under varying conditions and in the limit allow the weakest track to be detected.

To ensure an adequate S/N ratio an averaging technique has been employed. This allows the scanning time to be optimised as a function of the film quality. Fig. 5 shows a simplified block diagram.

3.2.1 General modes of operation

There will be three basic scanning modes and the essential characteristics are given in table 2.

Table 2 PEPR II Signal Processor

Scanning mode	Sweep Time‡ μsec	Sweep distance in microns	Resolution in microns	Mode of Operation		
				Normal mode S/N	Averaging mode	
					Range	max S/N
FIND	50	2000	20	N	1-64	$\sqrt{64}$ N
TRACK	50	400	4	$\sqrt{5}$ N	1-10	$\sqrt{50}$ N
SUPER TRACK	50	100	1	$\sqrt{20}$ N	1-10	$\sqrt{200}$ N

The FIND mode, which is used to locate starting points for the Track Follower must sacrifice time to improve the S/N ratio. To ensure maximum

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* Track contrast, track width, and background transmission

‡ Sweep time to be multiplied by number of sweeps averaged in "Averaging mode".

flexibility, an analogue hit detector is followed by digital averaging which takes the form of a 6 bit 100 word summing store allowing an averaging range of 1 to 64 times.

Track and Super Track which provide adequate sweep distances while track following, give an improved S/N ratio without a time penalty. However, they can be used in an averaging mode, limited to a range of 1:10. In this mode of operation the pedestal-free signal is digitized with a resolution of 1/100 of the sweep distance and the numbers resulting from successive sweeps are summed by the summing store.

3.2.2 First stage analogue processing

The first stage of analogue processing consists of format selection (light or dark field), gain adjustment, data overflow detection and matched filtering.

The data overflow detector is a simple comparator which sets a flag if the signal has exceeded a prescribed level. This will allow the software to adjust the gain and hence eliminate errors due to saturation.

The filters will have a conventional sine-squared impulse response and be selected to match the scanning speed.

3.2.3 Background Transmission

To overcome the problem of rapid variations in the background a detailed record of the local area is obtained during a dummy sweep using a defocused spot. The enlarged spot acts as a low-pass spatial -frequency filter, recording background changes which have wave lengths substantially longer than the tracks under consideration. The resulting signal is digitized with a resolution of 1/100 of the total sweep distance and placed in a 6 bit 100 word recirculating background store.

Subsequent data sweeps in the same local area use a reconstituted background to provide a pedestal signal, which can be subtracted from the analogue input, resulting in a pedestal-free track signal.

3.2.4 Width information

When using a scan line of variable orientation, track-signal width discrimination is essential. However, a sudden change in the track width may indicate the presence of a second track or artifact. To allow the software to make the maximum use of this information the track width will be recorded and can, if required, be read into the computer along with the track interpolation data.

3.2.5 Density Measurement

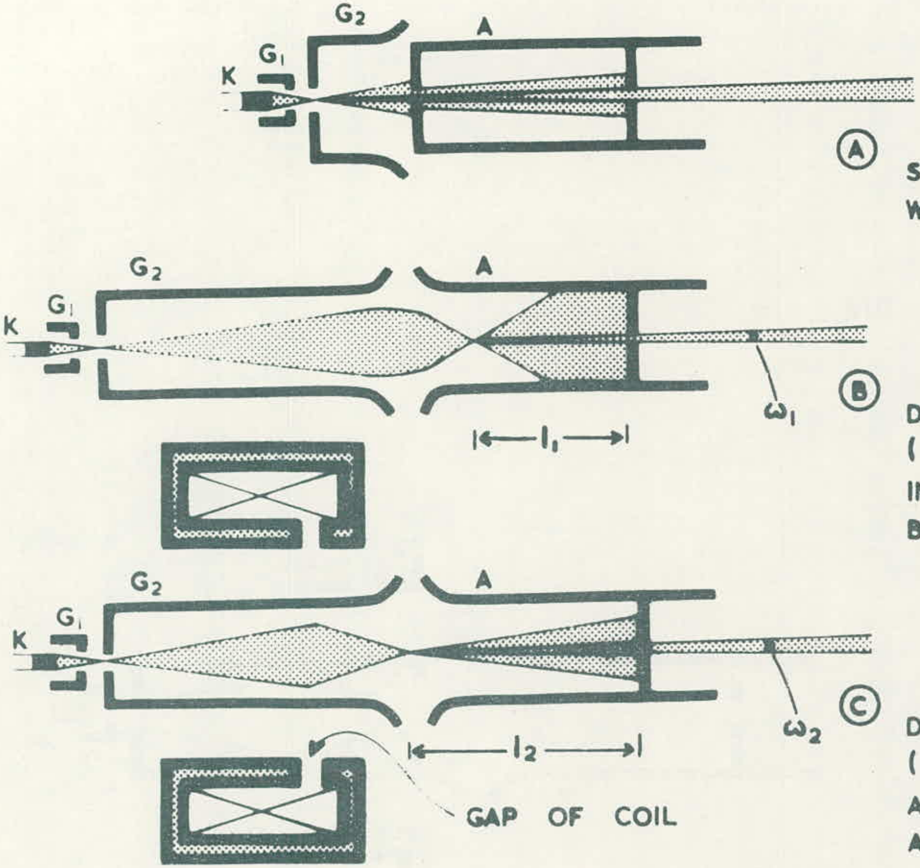
With a pellicle providing two image planes it becomes practical to make accurate measurements of film density. Fig. 6 shows the basic scheme in which hybrid analogue/digital integrators measure the photon flux in the reference and film channels. The film channel scaler is preset to a total photon count which will ensure the required measurement accuracy. The operation starts with turning on the CRT and the scaler is counted down to zero at which time the CRT is turned off. The count then contained in the reference channel is directly related to the film transmission.

References

1 C. B. Brooks, P. G. Davey and S. S. Willder, An improved micro-spot cathode ray tube. British J. Scientific Instruments, May 1970.

2 S. S. Willder. The correction of deflection astigmatism in micro-spot cathode ray tubes. Submitted to British J. Applied Physics.

3 S. S. Willder, C. B. Brooks. PEPR for BEBC? Data Handling Systems in High Energy Physics. Cambridge Conference, March 1970.

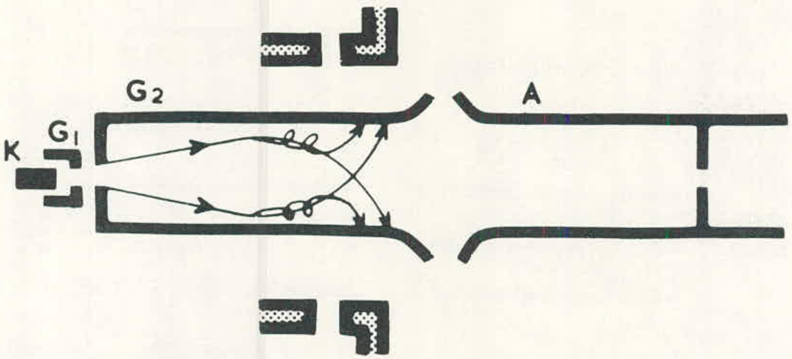


Ⓐ STANDARD SINGLE – CROSSOVER GUN, WITH LIMITED APERTURE.

Ⓑ DOUBLE – CROSSOVER GUN (2ND. CROSSOVER SMALLER, BUT MUCH LESS INTENSE : DEMAGNIFICATION IS PARTLY WASTED BY GROWTH DUE TO ABERRATIONS)

Ⓒ DOUBLE – CROSSOVER GUN USING BACK FOCUS COIL (LESS DEMAGNIFICATION IS NEEDED BECAUSE ABERRATIONS ARE MINIMAL. 2ND. CROSSOVER IS AS SMALL AS IN Ⓑ BUT MUCH MORE INTENSE)

FIG. 1



EFFECT OF BACK FOCUS COIL IN TRAPPING SLOW SECONDARY ELECTRONS IN G_2

FIG. 2

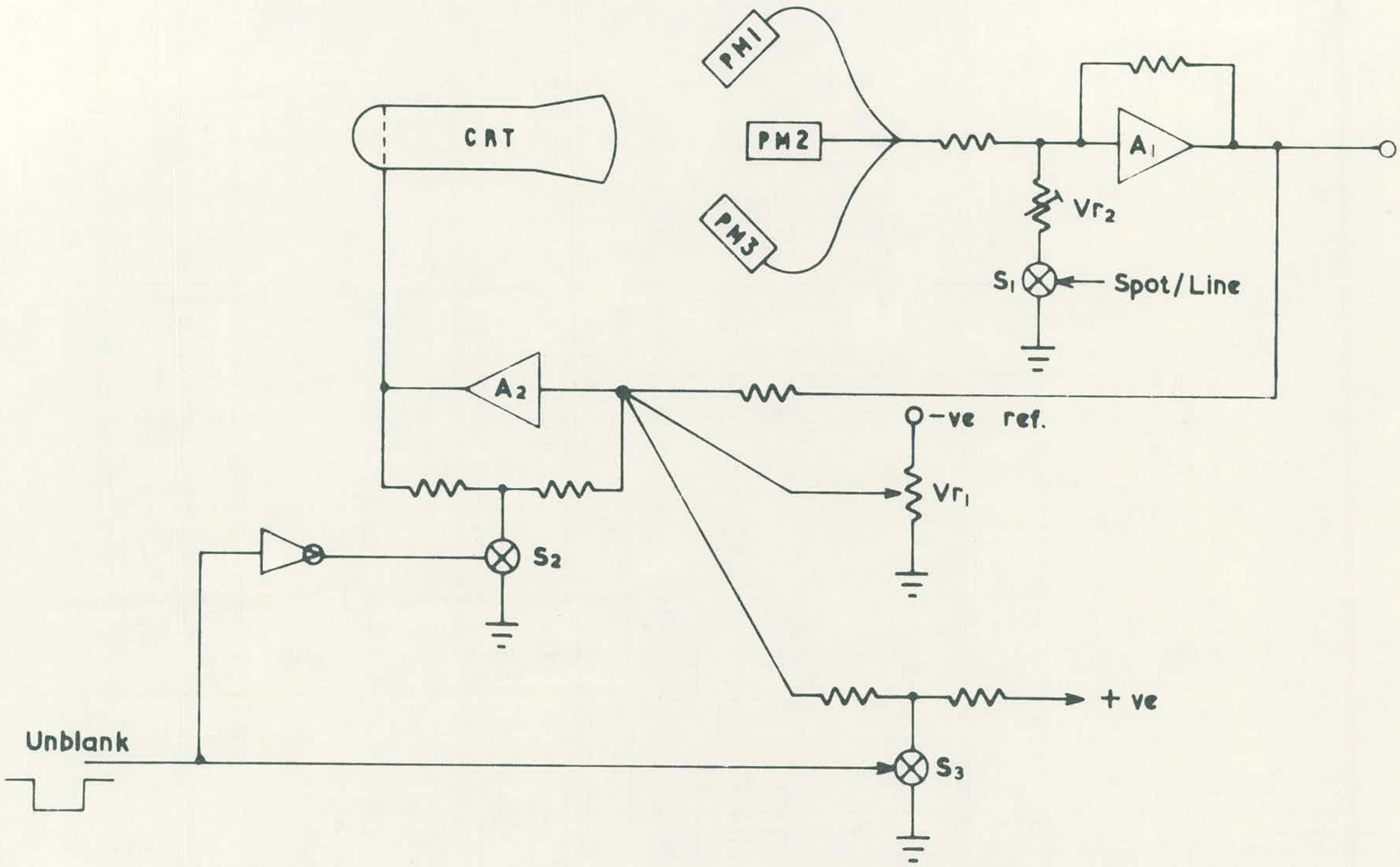


FIG. 3 PEPR LIGHT SERVO

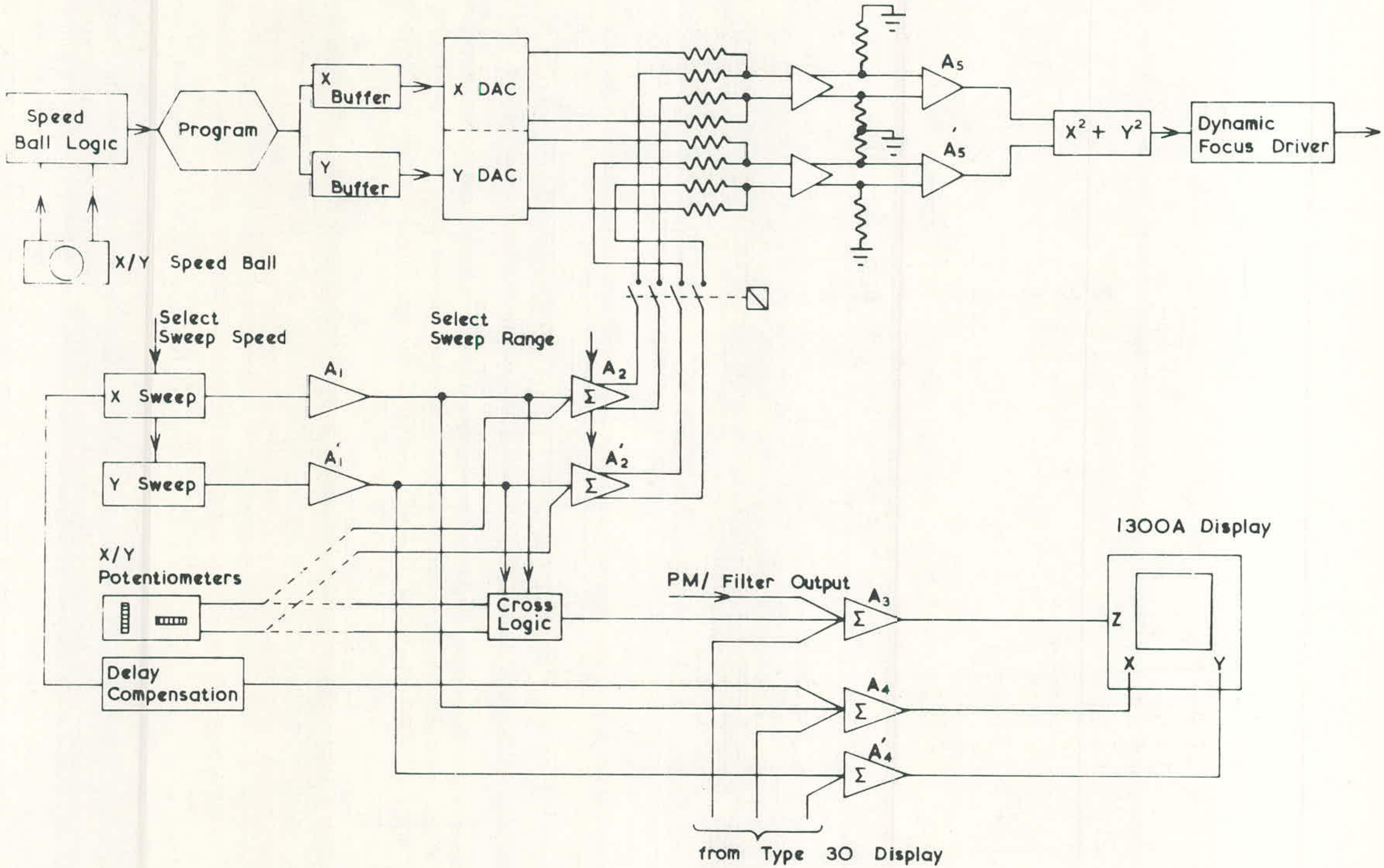


FIG. 4 OXFORD PEPR TV SYSTEM

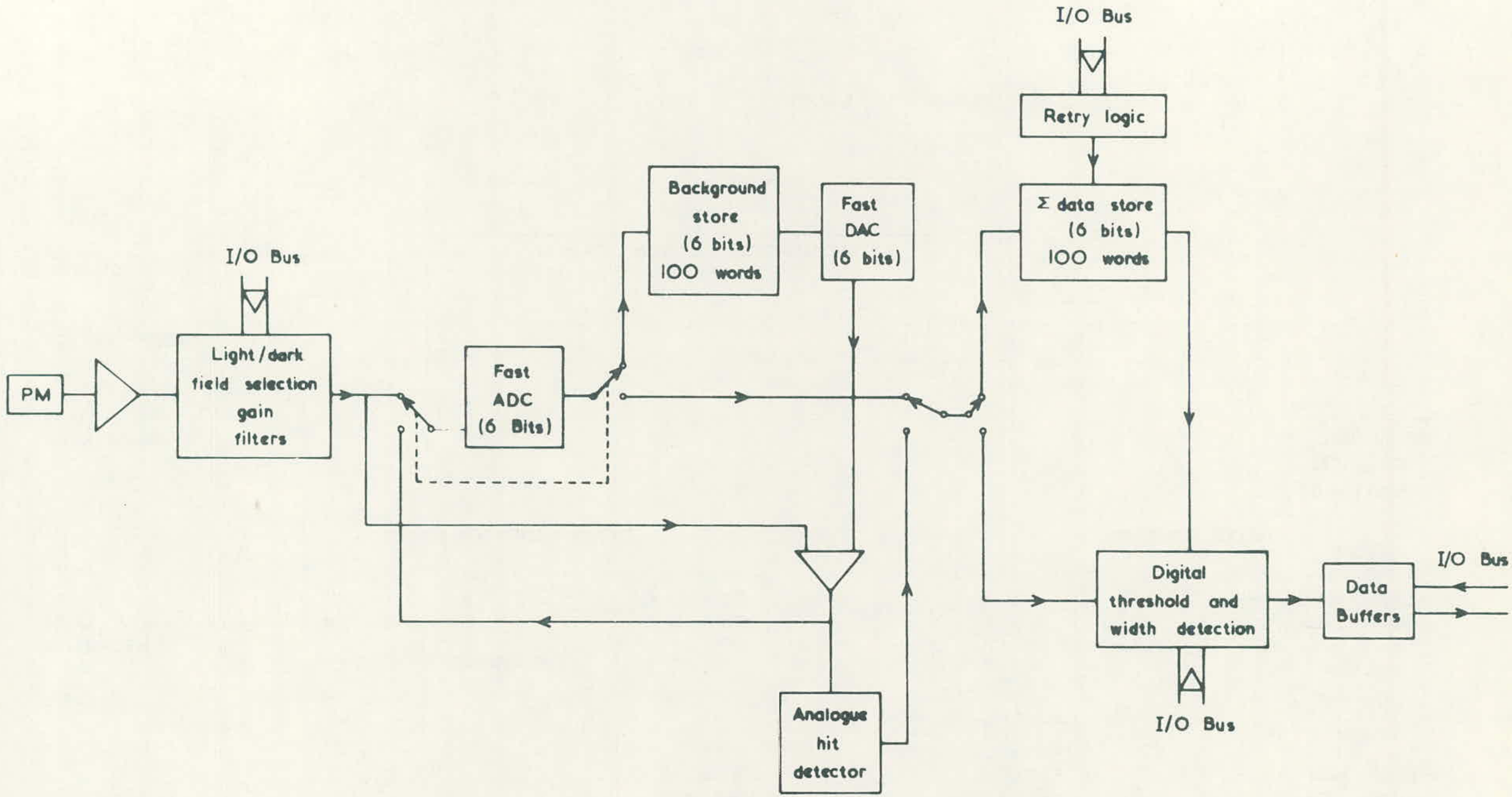


FIG. 5 PEPR II SIGNAL PROCESSOR

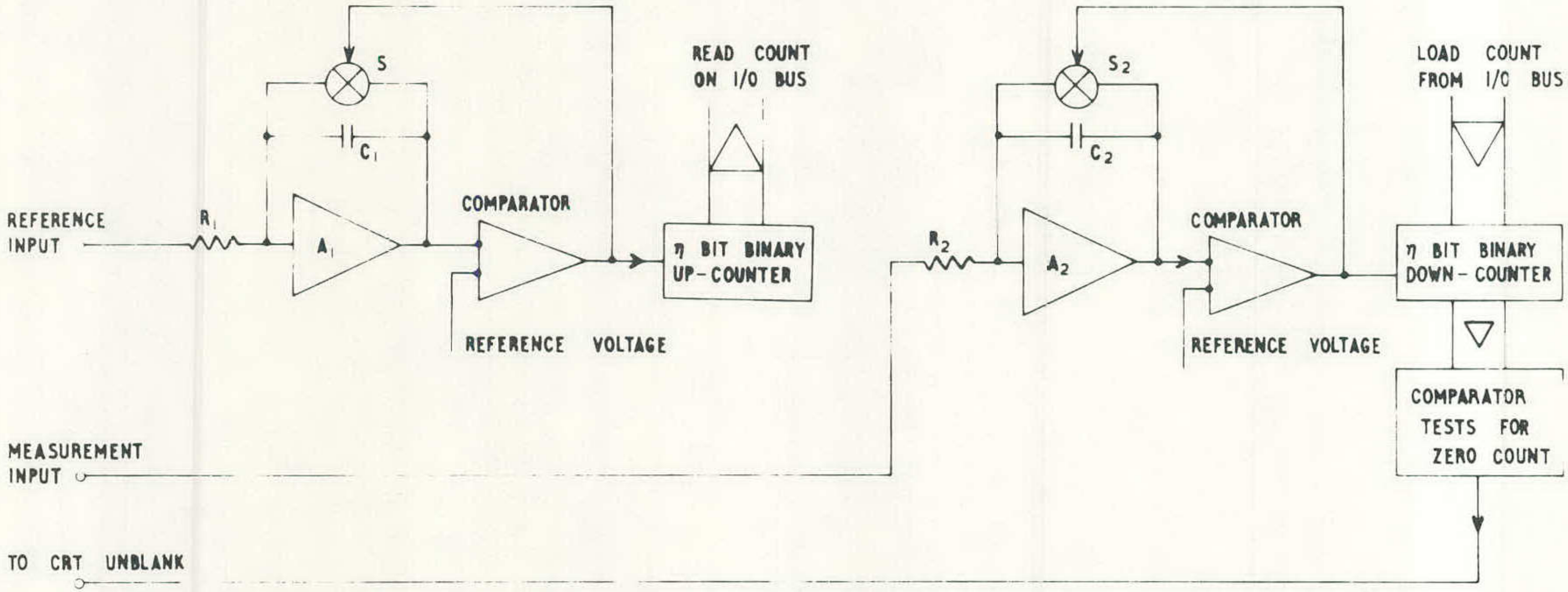


FIG. 6 DENSITY MEASUREMENTS

DISCUSSION

KENYON:(Washington) Why are you using Q 4 phosphor on your new system?

BROOKS: (Oxford) I don't know of a better phosphor.

KENYON: (Washington) I see, not a slower one that would have less........

BROOKS: (Oxford) The situation is this, I am not saying that Q 4 is the optimum phosphor. It is a very difficult and complex problem, you can't just talk about light output or spot size, or sweep speed, you have to take all of them together in some rather complex way, you've got to consider if you like, how many events I can measure with this tube in one year and you could, theoretically, derive an expression which would tell you which was the optimum phosphor. I am not prepared to spend a year to do that.

KENYON: (Washington) No, I was curious, I thought perhaps P 24 would have been better as far as your lens design was concerned. Another question, what are you doing about drift in your photomultipliers if you make density measurements.

BROOKS: (Oxford) Yes, I knew you were going to ask that question. Well, if you remember this is not bubble chamber film so I am inventing the problem. One can arrange to have whatever you're looking at to have a clear region, or you even take the film out altogether and at regular intervals you turn it on for some fixed period of time and both systems are supposed to be identical, both channels are identical and you let them run, you make a measurement and find out the difference, because the photomultipliers are the open loop part of that system and then you can either just remember the difference and take note of it, or you can correct the gain and have it automatically happening at some suitable interval.

VAN de WALLE: (Nijmegen) On that phosphor problem, I mean the slowing down factor that you are considering is typically from 5 to 10. What I think I recall, and maybe you can say whether it is right or not, is that the P 24 has a decay time which would imply a slowing down of more than 10, I think it is more like 10 to 15.

BROOKS: (Oxford) I think the P 24 is perhaps one microsecond. Well, the whole point is, for a long time we said we had signal to noise problems, we are now wearing a different hat, we now say we don't have signal to noise problems and to a large extent one of the big problems with changing phosphors is the cost and the time it takes to have lenses designed, you commit yourself to a lens and you've got to live with it.

ALLISON: (ANL) You remarked that the vertex errors dropped when you reduced the light you were putting through the system. What happened to the spot size when you reduced the light?

BROOKS: (Oxford) It doesn't change, believe it or believe it not, it doesn't change. As I pointed out, the line gets smaller than the spot and of course we are pushing much more current through the whole tube at that point.

ALLISON: (ANL) I see, and the relationship between the line and the spot remains the same?

BROOKS: (Oxford) We've run both the line and the spot at all levels of intensity which we feel are safe and we can't detect any difference in the spot size or line size.

ALLISON: (ANL) I'd just like to make another comment. Since on POLLY we have a system where we are trying to do production, we haven't been able to do or even think about a lot of things that you've been able to do, but some things have happened very often by accident. For instance, after a couple of weeks when we've measured 21000 events on un-scanned film, I wake up in the middle of the night wondering whether the spot was still focused and when we went and looked at it, it turned out that we had a spot about 50 microns across; this had been doing wonderful physics for us, and it didn't seem to matter and it seemed not to make any difference when we sharpened the thing up, we couldn't see any effect on the throughput.

BROOKS: (Oxford) Well, I would make two comments on that.

ALLISON: (ANL) That was with very good film, of course.

BROOKS: (Oxford) Well, that was the comment, you beat me to it. I think the big problem with spot sizes has really nothing to do with sort of fundamental things and noise ratio, but it's essentially the resolving of very closely spaced beam tracks which you don't have. The other point is that you were saying because it was a production system that you don't have the opportunity. It's, in fact, only because we have a production system that one can do these kinds of statistics. I would never have considered measuring 200 events 10 times at different light levels if we didn't have a very fast production system.

TAFT: (Yale) It seems to me that this is a philosophical argument, but I would like by way of leading into something which we may have a couple of minutes to spend on, to ask Allison: When you were running with a 50 micron spot, was your ionization still satisfactory?

ALLISON: (ANL) To see the effect of a 50 micron spot we have to go all through the loops of kinematics and come all the way back. We don't know when the spot, when the focus degraded; we had an emergency power shut down a couple of weeks before, so we may not have been running like this for very long. But the effect of it would be to make the chi square probability distribution even more peaking at low and high values because each sweep of the track would again, for the same reason, not be an independent estimate of the ionization, so that's what I would expect to happen, it may be that what we were seeing in the ionization was as much a fact of that as the fact that we were treating the three views as independent.

TAFT: (Yale) We have about 10 minutes, lunch has been set for 12:50 p.m., and there have been a number of comments about ionization. In order to try to finish off the subject I'd like to ask Peter Lucas to make a comment on some preliminary work we've done at Yale and then perhaps anyone else that wants to comment on the ionization should feel free to do so.

LUCAS: (Yale) Before commenting on the ionization I'd like to just comment on the effects of de-focusing. When we started our measuring at Yale there was a programming bug which computed the focus current as the strongest when it should have been the weakest and the weakest when it should have been the strongest. PEPR ran for a month like this and was much faster this way than when it was corrected.

The ionization work that we did is indeed preliminary, we've only done it on two different tracks. The two different tracks were, as a matter of fact, separated by 2 1/2 years, so that it's preliminary indeed. The chief statement of the results, however, could be the following: We didn't actually try to control it with digitized threshold gains, etc., but on the other hand we did control it with knobs and we've come to the conclusion that the track can be made either totally white or totally black by twiddling a knob only a small fraction of the amount with which we normally twiddle it just in going from one roll to another so that the affects of this are incredibly sensitive. The scheme we used was similar to the one described by Terry Watts. We attempted to simply scan along a track and come up with either hits or misses. Using the spot, we moved one main deflection count insofar as possible along the direction in which the track was moving so that the hardware scheme would be very similar to that which Terry described. In connection with this, however, we were worried about one thing: Since we are using exposures which had a large number of tracks in the chamber and the beam was not parallel, we were very worried about biasing the results with crossing tracks; the main programming effort went into resolving problems about crossing tracks. A crossing track can give a hit were it doesn't belong, it is less likely to give a miss where it doesn't belong, so that it would certainly bias the results. One thing we found by pursuing this was that in attempting to decide whether there was a crossing track we used our programs in a slightly different manner; in particular, the timing of the rate at which we referenced the film was considerably different in this mode than in the usual measuring mode and the results were that it was very irreproducible; namely, when a hit was obtained using an area scan or track follow mode we used a slightly different mode to try to just reproduce it and make sure we had the track whose ionization we were attempting to measure. It would often go away or become doubled, while in the usual mode it was not doubled. This suggests that there were indeed time constants of rather long duration which existed through many parts of the hardware, and this indeed turned out to be the case. If, however, we could work our way around this on at least our one track, the results were that if we could find level settings at which we got what looked like reasonable ionizations on any one track, that they were indeed reproducible; namely hits, misses, and regions of confusion all seemed to be reproducible, but the overall problem seemed great enough that we decided not to pursue it further.

One last comment is that in doing Dixon Bogert's sigma minus decay experiment in order to identify leptons, the ionization was actually determined by Horace Taft using bubble counting through a digitized microscope. And the comment as to the result of that is that the structure and shape of the bubbles were such that had PEPR tried to do it, the results would almost certainly have not been of any value.

TAFT: (Yale) That was for the Brookhaven 30" chamber under very poor conditions. Are there any other comments on ionization anyone would like to make, or questions to ask?

ALLISON: (ANL) I'd just like to make one or two comments because, in particular on ionization, the POLLY situation is different because, you know, even when one is just measuring, the ionization comes out very naturally, and so we have it all the time, and we actually use it during the track following and pattern recognition process. If you have a couple of tracks with a one or two degree intersection, then there is an area where it is only by means of looking at the ionization that POLLY can distinguish that there are indeed two tracks there and not one; POLLY knows it was following one track and the ionization in this area has suddenly become higher and it interprets that there is a crossing track. Now I am not certain to what extent the PEPR angular spread here will in fact tell you the same thing so that you don't need this information. The second thing is, during ionization, when doing it on the fly like this, one has to be very careful as to what to do with the discriminator level because, of course, if you change the discriminator level to get through a bad region you'll also muck up the ionization. Now, in fact, one can do something about this. It was shown some time ago by Strand at Brookhaven, that if you divide the log of the lacunarity by the average track width, I mean width in a POLLY sense, where this is presumed constant if you have constant discriminator level, this in fact, will give you a much better, will give you a good representation of the ionization when you vary the discriminator level a bit, or as they put it when the bubble size changes a bit, since one is only talking about effective bubble size, whether the bubble looks larger because it is larger or whether the spot is larger doesn't, I think, matter.

6 May 1970 2 p.m.

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Session VII

Round Table Discussion of the Problems of High Statistics Bubble Chamber Experiments.

Moderator:	I. A. Pless, MIT
Panel Members:	J. Ballam, Stanford Linear Accelerator Center
	T. H. Fields, Argonne National Laboratory
	C. H. Peyrou, CERN
	J. Sanford, National Accelerator Laboratory
	A. M. Thorndike, Brookhaven National Laboratory

PLESS This afternoon we will have a round table discussion among the people who have the responsibility for the broad programs of the major laboratories that supply bubble chamber film. As starting points for the panel, three subjects have been suggested that are of considerable interest to an audience which consists of the people who are going to analyze this film. The first subject is the impact of the measuring capabilities of the PEPR groups on bubble chamber physics. The second subject is the expected impact of the large bubble chambers on the way we think of bubble chamber physics. Explicitly, as stated in the letter which I sent, the questions are: will the bigger bubble chambers replace the smaller bubble chambers and will the amount of physics (which I defined as the amount of film that would be available to the general user) go up or down or stay the same? The third topic, which I think is probably the most delicate but therefore probably the most interesting, is whether the men on the panel can give us any idea of how they see the future going with respect to bubble chamber physics for the next three to five years. If possible, maybe they could give us some idea of how the physics, the amount of film, the crews, the beams and anything else pertinent are related.

One thing that was very clear from the talk among the PEPR groups was that the quality of the film was important. Either the film was very good and things went well or it was very poor and then things didn't go so well. Therefore another question that could be addressed is what further or future developments could be suggested to make the quality of the film better suited for automatic reduction and whether these could be implemented at the labs. This would result in more physics per unit time and, presumably therefore at lower cost, since time in fact means money.

Let me now introduce the members of the panel: Joe Ballam from SLAC, Charles Peyrou from CERN, Tom Fields from Argonne, Alan Thorndike from BNL and Jim Sanford from NAL. I think they represent the major labs in the Western world that produce lots of bubble chamber film. The ground rules are that each of these men will have fifteen minutes in which to discuss any subjects they choose. They are not required to talk about the topics previously mentioned. If any member of the panel wishes to interrupt another briefly, this is permitted. I will try to keep things on schedule so that we can have three speakers before the coffee break and two after. That will leave adequate time for discussion with the audience about any particular points of concern. There is a time buffer between the end of the panel and the begining of cocktails which we can use and if people are really excited we could even use part of the cocktail hour, but I don't think that will be necessary.

As an introduction I have written a table on the blackboard which condenses information on ultimate performance received from nine PEPR groups. I will not say which nine and I will not break the information down by individual groups.

	Total Events	$	Ph. D.	Total	Years
Upper	1000K	650K	10	55	4
Average	500K	400K	6	30	2.5
Lower	160K	300K	3	13	0

When these nine groups are fully operational they intend to analyze a total of four and a half million pictures per year. Their combined budgets will be $3,600,000 and the work will involve about 270 people of whom 54 will be Ph.D. physicists. It will take an average length of time of 2.5 years to reach this plateau of operation. The center row of the table corresponds to these figures and gives the average value for total events, annual budget, Ph.D. physicists in the group, total manpower including the Ph.D.'s and years to reach the plateau for one group. The upper row gives the largest figures received for each quantity, and the lower row, the smallest. Neither the upper row nor the lower row represents a particular installation, they just indicate the spread in the values received. The spread in dollars, from $300,000 to $650,000, is quite small while the spread in the amount of events is quite large, 160K to 1000K. The average values really represent most PEPR installations quite accurately. Although the numbers are not necessarily accurate, they represent the best estimates of nine different institutions presently owning PEPR.

(Question from audience): What is the average number of pictures per event?

PLESS: I would say that the number is about two pictures per event. Charles is disagreeing, he probably has a better number.

PEYROU: I think that it has to be one picture per event if you want to have that many events.

PLESS: All right, it has to be one. This topic may be included in your discussion.

I think we should start at one side of the table and go around in order. So the first speaker will be Joe Ballam.

BALLAM: I thought that I would first give you some idea of what bubble chamber picture production might ultimately be expected at SLAC in terms of both accelerator and bubble chamber performance as well as what the financing might be. I will give a realistic estimate of possible performance but what may prove to be an optimistic estimate of financing. Let me show you a little calculation of picture rates which I made. I used the actual numbers for the period of operation July '68 to July '69

SLAC POTENTIAL
PICTURE RATES

Based on fiscal year 1968

1) Average expansion rate 1.5 expansions/sec
2) Accelerator efficiency = 0.55 actual pulse/scheduled pulse
3) Beam and chamber efficiency = 0.45 pictures/actual pulse
4) Overall efficiency = 0.25 pictures/scheduled pulse
5) The number of pictures/chamber/year = 1.5 (expansion/sec) x 0.25 (efficiency) x 3×10^9 (seconds/year) x 0.6 (fractional time scheduled) = 6.7×10^6 pictures/chamber/year.
6) If two chambers operate and each is scheduled 80% of the time that the accelerator is scheduled then:
the number of pictures/year = 10.7×10^6 pictures/year

We ran at an average rate of 1.5 expansions per second for both chambers. The accelerator efficiency is the actual number of delivered accelerator pulses divided by the number of accelerator pulses that were scheduled over this period and the result is about 55%. This is a different kind of efficiency from the kind implied when you say the machine was operating 90% of the time. As you know, SLAC runs at 360 pulses per second and so this efficiency figure does not necessarily represent the efficiency of the accelerator for all experiments but refers only to those pulses scheduled for bubble chamber experiments. The next factor depends on the bubble chamber beam lines and the chambers themselves. We divided the number of pictures that we actually took by the number of good accelerator pulses and arrived at an efficiency of 45%. Our overall efficiency is therefore 25% It was also about 25% over the years of bubble chamber operation at LRL and so 25% seems to be a universal number. Now we calculate what we expect of our chamber. We take the expansion rate times the efficiency times the number of seconds in a year and multiply by the fraction of time the accelerator was scheduled. The number used here is 60%. This depends on the budget and it is unlikely that in the future we are going to do much better than that at SLAC. The result is 6.7×10^6 pictures in a calendar year. We have two chambers at SLAC and we can assume that we could schedule experiments in each for about 80% of the time that the accelerator is scheduled. You probably can't do much better than that because of the logistics of beams and incompatible experiments. So in the end we arrive at 10.7 million pictures per year. I think that is a realistic number for SLAC in terms of what we have already done. We can keep the chambers scheduled 80% of the time the accelerator is scheduled because the accelerator is only scheduled 60% of the time. This leaves 40% of the time to make modifications, clean the glass, and do all the various things that we and the experimenters want done with the chambers. Therefore if we had enough people to run both chambers at the same time, it could be something that we in fact could do. There is also the possibility of increasing this number. I think that with some hard work we could get the average cycling rate up to twice a second and, of course, the machine scheduling could go up from 60% to 80%. So the number 10.7 million pictures per year could actually be increased by some factor.

FIELDS: Could you compare the number of pictures you actually took during that period with your number, 10.7 million?

BALLAM: I was going to. At SLAC we are financed so as to limit the number of pictures taken. Also, we are at present financed so that we do not have enough people to run both chambers at the same time. One chamber can be kept full of hydrogen on a stand-by basis, while the other chamber is operated. That requires fewer people since you don't have to fill the cooling tanks and change cameras as often.

Running only one chamber at a time we took some five million pictures during that year.

So I think the number of pictures we took is commensurate with the estimate of a capacity for 10.7 million. The fact that we did not take more has to do with tight money. We hope that we can improve the situation some by changing film format and making the operation of the chambers more efficient than it is now. I think that with the money we used to obtain 5 million pictures last year, we could get 6 million pictures this year. There is some agitation in the laboratory to try, in fact, to get the number of pictures up to 7 million next year. That would be our capacity.

The following is a chart of particle beams available for bubble chamber experiments. You can see that all energies are covered up to the highest energy, if you include the new r-f beam which is under design for the 80" chamber. Perhaps Thorndike will comment as to whether it is approved for construction. At SLAC a beam has been approved for the 40" chamber which will take care of π^+ and π^- between 2 and 15 GeV and an r-f beam is proposed that will provide K mesons at discrete energies around 8 and 14 GeV. Then there is a neutral beam that provides K^o's from 2 to 10 GeV. By changing the angle of production you can change the peak of the distribution of K^o's from 2 GeV up to nearly 6 GeV. The r-f beam for the 82" chamber provides $\pi^\pm$ and K's at discrete energies and protons at 9 GeV. Finally, there is a laser photon beam.

Looking at this beam chart, it seems to me that the bulk of the pictures we are talking about would have to be taken with π beams except for the photon beams which are available at SLAC. If you look at the compendium of bubble chamber pictures prepared by Irwin Pless for this conference, you can see that between zero and 8 GeV there has been a fairly good spread of π^+ - p exposures in the United States. However, my contention is that there have been only three experiments done so far with the kind of statistics that people are now interested in; one with something like 450K pictures, one with 500K at 4.5 GeV and the big one at Berkeley at 7 GeV. For π^+ - d, practically no high statistics experiments have been done. So, in terms of high statistics experiments, by which I mean between 500K and 1,000K pictures, we want to fill up the range between zero and 16 GeV, and, at Brookhaven, even beyond. There appear to me to be at least 10 or 12 energies that should be done providing there are people who have the capacity for getting the physics out of the exposures.

If you look at the π^- - p and π^- - d exposures, I don't think the situation is very much different. There is only one decent exposure at 4.5 GeV with hydrogen and nothing with high statistics in deuterium. With these needs, I would conclude that SLAC could do some 14 or so exposures of 500K to 750K pictures with π^+ and π^- at any energy you want between 4 and 16 GeV and that this could be done in a period of something like two or three years. By that time it is possible that this kind of physics, as analyzed by bubble chambers, would be exhausted. Otherwise you would have to think of experiments with an order of magnitude more statistics and if you talk about 10 million pictures per year for a group, or experiments with ten million events, it doesn't match with anything, either the capabilities of the PEPR groups, or the capabilities of the bubble chambers. When I look at the physics that comes from high-exposure experiments look at the 700K exposure run by LRL at SLAC in which they found the non-splitting of the A_2, I find that to do a better experiment, you would have to increase their statistics by at least an order of magnitude.

There are still some K^- experiments that we can do at SLAC but only at discrete energies. Beyond that, I can say that for high statistics experiments that can be handled very well by automatic devices, that is a very heavy, solid, three year program. After that I can't see much further. It could turn out that there will be still more exciting things to do in this program, but I do not know.

Another thing is the possibility of using counters to trigger the taking of pictures in bubble chambers. There is an experiment at SLAC which has been approved for 16 million expansions with only 800K pictures expected. The lights and camera will be triggered by a fast going particle that goes out through a thin window in the back of the chamber. With spark chambers downstream of the exit window you determine the momentum and angle of the fast forward-going particles and with the bubble chamber you look at what has been left behind in the hydrogen. This is an approach which has not yet been tried at SLAC, but we are going to see if it makes any sense from the point of view of the physics that you can get out of it.

Another thing I would like to mention is that there are a lot of plans to use visible hydrogen targets inside of bubble chambers where the target is surrounded by track-sensitive neon. In that case you get into the business of looking at electron pairs in a heavy liquid. In order to determine which π^o an electron pair comes from, it is nice to measure the energy of the pairs at least to a precision of 15 - 20% as well as their direction. I do not know what this means in terms of automatic measuring because the tracks of the electron pair do not go very far before they suffer scattering or bremsstrahlung. The angle between the electron tracks at the production point is small, and determining this angle and the location of the vertex by extrapolating easily-scattered tracks back to the vertex will be hard. Because of this and the increased number of vertices that have to be included in a fit, the programming would be a different kettle of fish than the normal work.

The last thing, just to finish up, is that we are building a rapid cycling chamber which is to be used as a target as well as a chamber. It is 15" in diameter and 4 to 5 inches deep with a thin window all around. The particles leaving through the 360° window will be detected in trigger counters that will control the lights and camera advance for the chamber. This kind of chamber is also going to be used in a hybrid system with spark chambers. The whole problem of matching events measured in a target chamber with taped output from wire chambers is another problem that I think still has to be solved.

That is all. I would be willing to answer questions.

PLESS: At the end of each speaker's presentation let's have perhaps two really burning questions. Is there anyone who has a burning question. (no response) If not, then why don't we go to Charles Peyrou and see what happens there.

PEYROU: I think I will start with a question to Irwin Pless' table. I am slightly puzzled by the number of Ph.D.'s you have to get your pictures. By European standards your number is very small. What I mean is that in Europe the number of people who are involved in bubble chamber physics publications is too large, but that here in your chart it seems to me that it would be too small. The "average" group plans to take 500K pictures with about 5 channels, each about 100K. One physicist to think about 100K pictures and to extract the physics out of it seems too small. When I was doing physics myself, the usual thing was to have at least one other physicist vis-a-vis to discuss the problems to see if you were saying something stupid or not. These guys are probably also worrying whether or not PEPR is giving the right results on top of trying to understand the physics. What are they going to do in the afternoon?

PLESS: I think that is a very appropriate question for the audience and since I stated very explicitly that I am not going to identify any group on the chart with respect to the sizes shown, I will not direct questions to specific groups, but during the discussion part of the program I think we could nail that point down.

PEYROU: (contd.) Now the situation at CERN is as follows. For the time being we have two hydrogen chambers operating. They are the 2m and the old 80 cm of SACLAY. The 1.2m chamber is finishing its program this year and will be replaced by Gargamelle. The 80 cm is scheduled to be stopped for many reasons that are obvious. It is therefore now just producing the pictures that you would need in the next years and that no other chamber can give. These are slow or stopping K's and anti-protons. What we are doing is to use a chamber which is finishing up, in the territory where it is best and where it is bad to use the 2 m chamber because it would just be a bad 80 cm chamber when used for stopping anti-protons. The program is to complete three million pictures of low energy anti-protons and K^- in two years. Then it will be stopped.

The 2 m has taken 3.3 million pictures last year which is about the maximum that you can expect without double pulsing. We tested and ran at least two or three weeks of double pulsing last year and now the normal mode of the 2 m is double pulsing, provided it does not take more than fifty per cent of the profits. If we want to run 12 GeV anti-protons which requires the whole machine, clearly we cannot double pulse, but even if we were only taking half of the machine for one pulse, we would not be allowed to double pulse. We would love to triple pulse, but since we use fast extraction for the bubble chamber, we take out a piece of the flat top. The fact that we are doing double pulsing does not mean that we will take 6.6 million pictures, because of efficiencies, but if everything goes as fine as last year we will take 5 million pictures a year. We will probably not make 5 million this year because we are starting very late and we have a big overhaul to do, but we should plan on the basis of 5 million pictures a year with the scheduling committee. We have taken 10 million pictures since the chamber started.

By early 1971 we will have the Big European Bubble Chamber called BEBC for short, which is 3.70 m in diameter. While BEBC is very similar to the Brookhaven 4 m design more than to the Argonne 12', it has a field of 35 Kilogauss, or even 33.333 Kilogauss because in the approximation where the velocity of light is 3×10^8 m/sec, it makes the curvature 1 m/GeV/c, so we will stop the field before it gets to the maximum value. In talking about this chamber I am, in a sense, going to the second or third topic before finishing the first. We do not expect the chamber to give many pictures at the start because we want to use it to try new tricks, which is the third topic. We do not want to just use it as a larger 2 m. We want to try gamma-ray detection, for example. Whatever, we do, though, I hope that in 1974 it will be in full production. At that time we will have 5 million pictures per year from the 2 m and 3 million pictures from BEBC because I do not want to promise double pulsing with BEBC. It is not expected that it will be able to pulse every 100 msec. If it could be double pulsed after 200 msec, we could do that if the flat top is going down then. We should devise a good way to superpose a bubble chamber pulse in the middle of the flat top. We have schemes for it, but they do not work with r-f separators since they have a spill which is too long. I believe there are some ideas at Brookhaven and maybe Thorndike will comment on that. If some ideas work out, we will go to double pulsing, but that is for later. Our plans are actually to have around 1974, 8 million pictures: 5 million for the 2 m, 3 million for BEBC. I would like to have these pictures produce 8 million events. It is clear that in the high statistics business you are going to process every single encounter, even including elastic scattering. There is absolutely no reason that in a chamber of the size of the 2 m and especially the size of BEBC, you should not have one event per picture. If you have one every two pictures, that is toobad. I donot see any reason not to have that kind of yield. Of course that will yield much too much for certain channels and not enough for others, but that I cannot arrange; I cannot arrange cross-sections. I can produce many miracles, but not that one.

I believe that Europe processed 4 million events last year with the measuring machines they have already, which are mostly not PEPR's. So all we can expect in Europe is 8 million events. I think it is much better to process very fast events on a fast machine to get a sort of general view of what you are doing, than to assemble events of a mammoth collaboration of 10 labs, each having produced 50K measurements.

To come to your two topics, I am much more optimistic on bubble chambers than I used to be two years ago, I think. I maintain that practically nothing is done in strong interaction physics but with bubble chambers. There are experiments from elastic scattering from polarized targets and charge exchange, there is a missing mass experiment. Of course, I know strong interactions is boring because if you do strong interactions, you have to understand the Veneziano model. Strong interactions is done essentially all by bubble chambers, period. Even the best data on elastic scattering at high energies, comes, as far as I know, from bubble chambers. Weak interactions is a completely different problem, they do fine work. Strong interaction physics will be done with bubble chambers or possibly bubble chambers plus the CERN OMEGA project, although I consider the philosophy of the CERN OMEGA project not too different from bubble chambers. In terms of philosophy, I do not see how it is going to be done for a long while other than with bubble chambers. So I think we have a bright future.

The only thing which is a little disquieting is what happens as we approach what some people call Asymptopia; I call it the Desert of Pomeranchuk. If, as certain people argue, you have to do experiments that progress geometrically in energy, then even with Batavia, you are not going to go very far with bubble chambers. You will end soon. It will go 16, 32, 64, 128, 256 GeV, and then that is it. One million pictures at each energy and done. If on the contrary, the physics effects vary arithmetically with energy, then you are still in business for 100 years with no problems. I do believe it will be interesting.

There is one thing which is not on the agenda on which I have some ideas. It is the use of bubble chambers with big accelerators, a topic which is unfortunately not my concern, but is yours. I have. heard that wise gentlemen assembled in Aspen and decided that the use of bubble chambers with big accelerators is not quite good except for neutrino physics. But I do not know of anything at all that they propose to replace the bubble chamber, just like Churchill said about democracy: it is the worst system of government except all others. With a 500 GeV accelerator, a bubble chamber is the worst instrument except all others. I do believe, personally, at the start-up of a big accelerator, that the best instrument is a bubble chamber. Whether giant chamber or a smaller one with a very high field, this I have not decided yet. Of course, you will not measure the very high energy stuff, but everybody knows the best physics done with an accelerator of 25 GeV is at 5 GeV, so at the begining of the 500 GeV you are going to do 50 GeV experiments. If you can do good 16 GeV experiments with a conventional chamber now, you can then with BEBC or a high field chamber have the same precision.

There is another thing you can do at high energy; the very high energy stuff is going to go forwards, on the average, so you can put a window in your chamber, put a spark chamber downstream and then measure the curvature on a 60 m lever arm.

About the big chambers and the physics from them: Personally I do not think we have built them just to operate on neutrino experiments. At the begining, for example in the Brookhaven report on the 25' chamber, plates and other devices have been omitted. Everyone who has built big bubble chambers says he is going to do neutrino physics. I think all these schemes, the hybrid scheme, or the plates game, have to be tried. That's why I do not see any great production with BEBC at the start if I can help it, despite the pressure that there will be. We have to try gamma-ray detection. Furthermore there is one thing I believe very much, but which will need even higher statistics; that is systematic use of secondary interactions. I have read many reports which say that the poor BEBC has only 35 Kilogauss and since 20% of the tracks are going to be deflected by secondary interactions, they are in bad shape to measure events with sufficient accuracy. But I do believe that if you use the secondary interactions, you are in better shape. You can make identifications of particles, you can get rid of ambiguities, etc. You must remember that since our primaries at CERN are not going to be larger than 15 GeV, the secondaries are not very energetic. Therefore the chances that the secondary interactions will satisfy a 4C fit are large. This of course makes life wonderful for programmers: super-Grind with multi-vertex fits will be fascinating. You have to allow for a growth in ability to use the new chambers in good ways. Just think, we are still improving the optical constants of the 2 m chamber and the 80". I think really nobody knows what the big chambers are going to do. The real basis for building the big chambers is the following: let's try it and see. Let us try to get precision. This seems to be reasonably sure now. The problem of thermal distortion seems less serious then we were afraid of, judging from the trials which have been done in the U.S. and the work we did with our model at CERN. Let us also try various ways of using them. If the worst comes to the worst, they are super 2 m, super classical chamber, and maybe better. But if we can do something with n eutron detection and gamma-ray detection then we will make a jump forward which of course, may mean a drawback in statistics at first. In the begining you will have so many vertices of gamma-rays and neutron scatters that you will not be able to use PEPR. You will have to use a poor girl with a light pen, very slowly pointing to all possible secondary vertices. It will be awful, but of course we will catch up and learn how to do it right. That's about all I have to say.

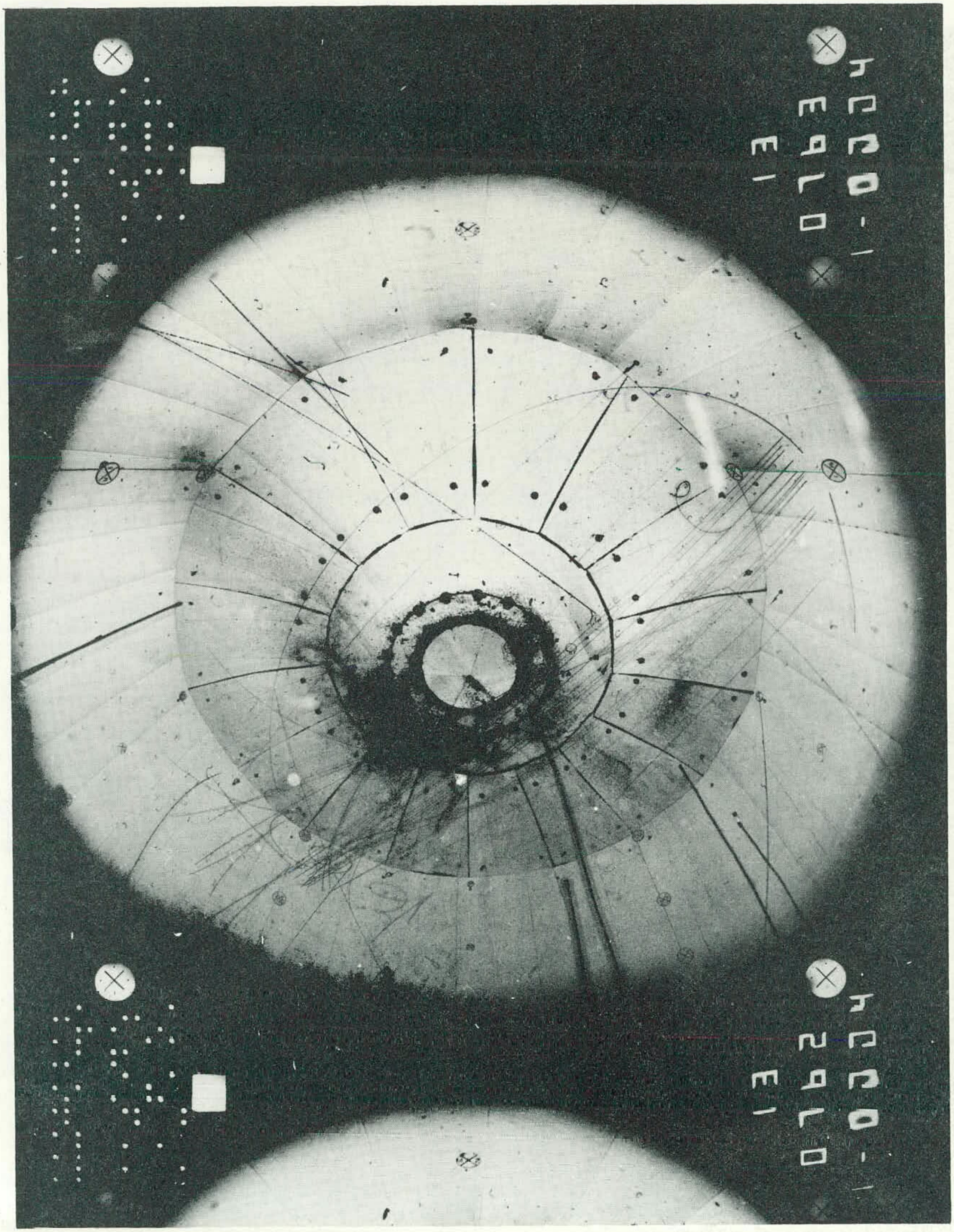
THORNDIKE: Is there any estimate of the degree to which the big CERN chamber will be committed to neutrino experiments during its first few years of operation?

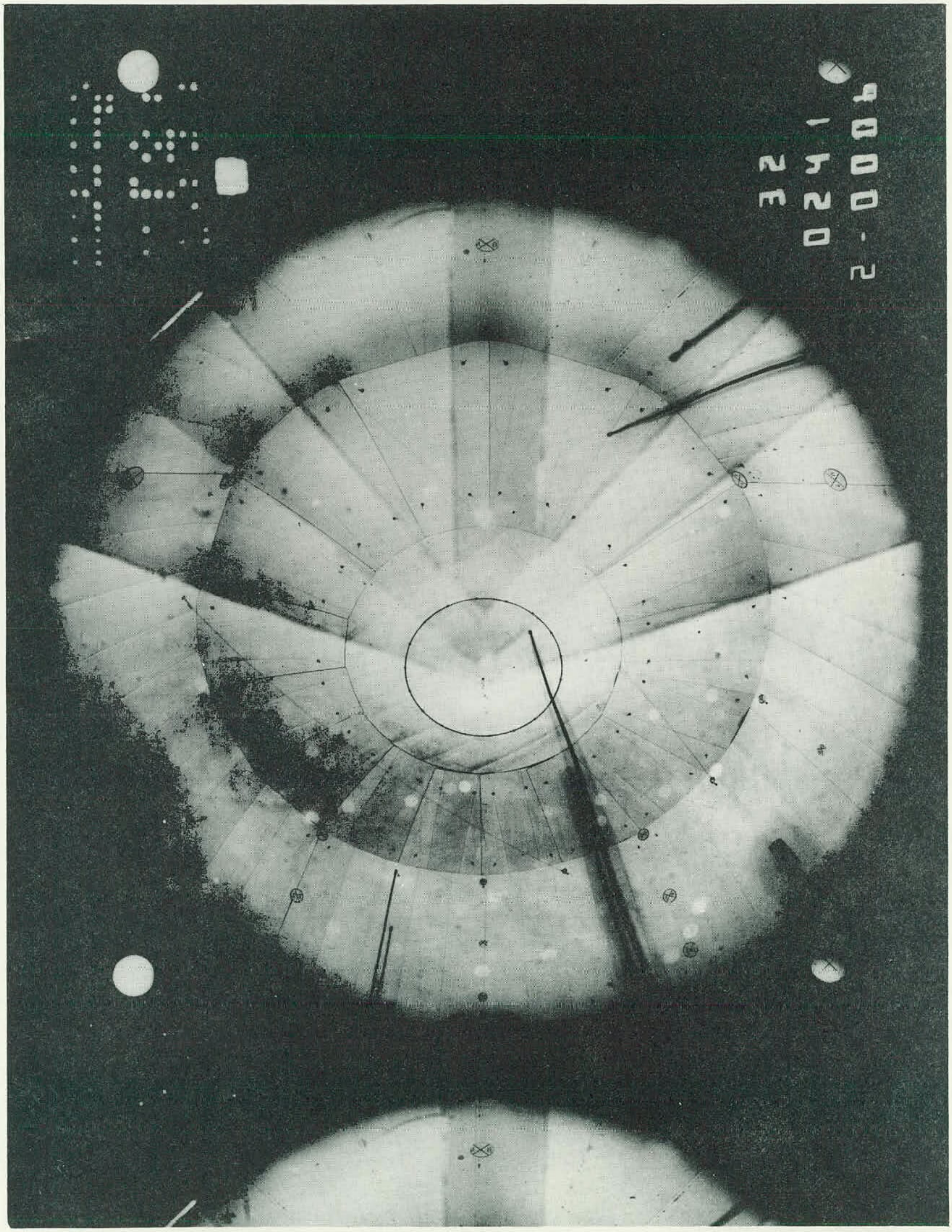
PEYROU: There will be a Gargamelle experiment which however will not be in hydrogen. Batavia will have an experiment with neutrinos of 100 GeV or 50. We might have one experiment.

BALLAM: Are there any attempts to marry PEPR devices and the big chambers?

PLESS: Mulvey's group at Oxford said they were going in that direction, this morning.

FIELDS: I would like to comment briefly on three matters. One is bubble chamber picture production and its outlook at Argonne, second is





to say a few words about the status of the 12' chamber and third, to make a few general comments about the future of this business.

On the first subject, there are two operational chambers at Argonne. The 30" hydrogen chamber has been running on about a 50% duty cycle for the past year or so, and alternates with the other active chamber, the 40" Michigan-Argonne heavy liquid chamber. The 30" does multiple pulsing: double and triple pulsing are its usual operational modes. I would say that, within the next year, the 30" chammber will be run continuously and the 40" heavy liquid chamber may close down. This has not been determined definitely, and the physics community can have a major effect on such decisions. It would be interesting to move the 40" HLBC to another separated beam (1-2 GeV/c), but it probably makes more sense to build that same separated beam for the 12' chamber, operating in a hydrogen-neon mode. So that is the situation as far as production in existing chambers goes.

The 30" chamber now has a thin exit window and a fast neutron and K_2 downstream spark chamber detector. Mistretta et al from Wisconsin have used this system to tag pictures which contain events with fast, forward neutral secondaries, as well as to measure the direction of the neutral particle. They seem to be having reasonable success with that and have been running for the past month or two. There is certainly more that can and should be done along such lines.

We expect the 12' chamber to yield physics pictures in the near future. The rough schedule is that there is hope of taking pictures of 12 GeV protons and perhaps of neutrinos on hydrogen this summer. There is a planned K_2 decay experiment, using a beam of mono-energetic K_2, which is liable to occur within the next year. Another factor in the 12' bubble chamber time scale is the r-f separated hadron beam, which is probably another good year away. It will give separated K beams up to about 8 GeV/c, and π beams to 10 GeV/c.

As far as the 12' chamber itself goes, all of the major systems on it seem to work satisfactorily. On matters of turbulence and track visibility, it looks quite encouraging. We have taken some test pictures recently, and the details of the pictures are relevant here.

Fig. 1 shows a recent picture, with about ten incident tracks of 12 GeV protons. Several cosmic ray tracks are also visible. There are two wires of 400 μ micron diameter stretched at the bottom of the chamber. These provide a good monitor of turbulence and general picture quality. At the bottom of the chamber there is some boiling, but most of the lines are seams between Scotchlite panels. There are vapor pressure thermometers hanging down into the chamber.

There is an intensity variation across the picture arising from the relative refractive index of the Scotchlite and the hydrogen. One of the things we have planned is to put in a variable density filter near the focal plane of the camera to even out the intensity variations. The first crude try has been made by just cutting out some neutral density filters and installing them. Fig. 2 shows the result. With a more refined arrangement of this sort, we expect to be able to achieve good contrast throughout the picture. Nonetheless, these pictures will offer a challenge to automatic measuring machines.

My last comment involves some speculations about the future. In the first place, with various automatic measuring machines becoming operational over the next few years, one can anticipate a factor of two or perhaps three in the number of pictures needed by bubble chamber physics groups in the United States. Secondly, harsh reality is that the number of bubble chamber pictures being taken in the U.S. is not increasing. According to the 1969 HEPAP report, U.S. bubble chamber picture production in CY 1967 was 15 million and in CY 1968, 19 million. I understand that the figure for CY 1969 was about 13 million. These numbers represent perhaps half of the maximum theoretical capacity. Thus, a purely statistical look tells us that a severe mismatch between analysis power and total picture production is developing.

Coffee break.

PLESS: Could you give us similar numbers with respect to potential production and hopes for actual production of bubble chamber pictures at Argonne, similar to what Ballam and Peyrou gave for their laboratories.

FIELDS: The plans are sufficiently indefinite, as I mentioned that it is difficult to give actual production goals. Maybe what I can do instead is to indicate realistic upper limits on what the chambers can do.

The two relevant chambers are the 30" hydrogen chamber and the 12' hydrogen chamber. The 30" hydrogen chamber was taking pictures for all but two months of 1968. In that year there were two changes of plates in the chamber. It took 3.3 million pictures. I think it is safe to assume that something like 4 million or so is a realistic upper bound for the 30". In 1968, of those 3.3 million, 400 thousand were taken in the single pulse mode, all of the others were taken in double or triple pulse. The basic numbers are that when running simultaneously with counters, the accelerator has a three and a half second repetition rate. On special occasions, with no flat top, the ZGS runs with a 2 second repetition rate.

For the 12' I would hope that 2 million pictures per year would be achievable in the near future. From the tests on the 12' chamber, there is no reason to believe that it will have great difficulty multi-pulsing. Probably we will be able to double pulse.

PLESS: Alan Thorndike

THORNDIKE: Let us just give a quick look at what BNL has for bubble chamber equipment at the present time. There are 30" and 31" chambers for small ones, with one crew, so we operate one or the other of them, but not both. In the most recent operation of the 31" it was running well in a double pulse mode. We are supposed to be able to do the same thing for the 30" but this has not actually been done with an experimental run yet. The 80" chamber is available with a crew and eventually it should be possible for it to run in a double pulse mode. We have had some references to what we call track sensitive target, a hydrogen vessel inside the overall chamber which might have a hydrogen neon combination to give improved detection of gamma rays, neutrons, and K_o's. The track sensitive target is something which we are working on actively. I think it is very important to the future. We don't have one to put into the 80" chamber at this moment but we have on that's drawn up. We are making it, and we think it is going to work sometime. There is also the 7' chamber in experimental operation. It has been having extensive revision of its expansion system, and if all goes well, we will be cooling down for the next trial in about a month. Where does this fit into the overall high energy physics picture?

Brookhaven is coming along fairly well on the conversion program, which is aimed at giving a substantial increase in beam intensity which is most of interest to the counter and spark chamber experimenters, but of some help in bubble chambers. In particular, it makes double pulsed operation more feasible. It does, potentially, give something like a factor to two increase in beams available for spark chamber experiments so it can mean an overall increase in the program. However, at this time, our support in dollars for operation has been just about constant and since dollars buy less, that is a real reduction. We have less people to support what is potentially twice as big a program and that causes a lot of difficulty. In terms of general priority, I think the lab's position is to keep the AGS running full time as the highest priority of all, and we do have every intention that it will continue to run in three shifts around the year except when there is some reason to shut down for either repairs or completion of the improvement program, or some special reason. In fact, the present schedule is that it will run for about the next twelve months, then there will be the second and final shutdown for the AGS conversion, which is spoken of as being something like four months. To get back to a priority list, there is an activity, perhaps a bit remote from what we've been talking about, which normally comes second on the list, and it is to be kept in mind because it does compete for people and to some degree money, although not on a very large scale. This activity is the continued investigation of new accelerator developments and in particular the possibilities of cold magnet synchrotrons which might permit more economical higher energy machines. There is a limited program of development of that sort of thing going on at Brookhaven. At the moment however, there are no specific proposals for things that might be built at BNL.

Next on the list is larger bubble chamber construction. This has not proceeded as well as we had hoped. About five years ago Brookhaven's first proposal to build a large chamber was handed in, but we have not received any official support for doing that, so we haven't been able to build one. The 7' chamber's development was carried on out of the capital and operating funds of the Lab. This has been our best effort in that direction in the real world. Last fall there was a good deal of discussion concerning where we stand and what are our long term plans under these conditions. This led to what I jotted down as a "1969 plan" when I put some things on my sheet of paper for an outline. One main element in this is that in the fall of 1971 there would be a shift of the 80" bubble chamber into a new north experimental area. This is motivated by the desire to bring things together in a more rational fashion and to provide a single beam coming out of the AGS for bubble chambers, with some new and interesting secondary beams. The package was to have a high momentum separated beam that was being referred to as the new rf beam. We would try to have that ready as quickly as possible, which means the end of the calendar year 1971. At Brookhaven there is a great deal of interest in bubble chamber neutrino experiments and a neutrino beam would also be included which might be somewhat later, like the begining of calendar year, 1972. We also had in mind a lower momentum separated beam that would be third on the list with no very clear prediction as to how soon it would be available.

On chambers, our thinking is that it is realistic to provide operating crews for two chambers at Brookhaven. The main expense of running a bubble chamber is the salary of the people who keep the chamber working. The plan calls for two crews, one of them basically able to run the 80" chamber, and the other able to run either the 7' chamber or one of the smaller chambers, if desired. So the plan involves moving the 7' chamber to the same region, converted from being an experimental device that could be run for short periods with great labor to being a production facility with a complete refrigeration system. A regular operating crew for the 7' is not currently possible but with the concentrated facility, both the 7' and the 80" could be kept running. We would hope at the same time to increase the 7' chamber significantly in size to something that would be on the order of 12 or possibly 14' in overall dimension, but similar in construction to what it is now. It appears that the31" chamber would go into mothballs by this time, because it uses beams that are available in other places. We would keep the 30" with its low momentum beam, just as it stands for the time being, and perhaps move it eventually to the north area.

In terms of what we thought reasonable for rates, we seem to have a new constant of nature, which is that a good bubble chamber ought to take four or five million pictures a year. That was the number that we came up with for the 80", assuming that double pulsing works relatively well and can be done some reasonable fraction of the time. The best year we have had in the past gave something like 2.7 million so that very roughly applying a factor to 2 to what we did in the past seems reasonable. However, we have never done that well, so maybe that's being optimistic. In terms of the 7' we were asked to make an estimate for HEPAP and the numbers that we came up with were a million and a half pictures in calendar year '72 as the first year of operation and perhaps 4 million after a couple of years if everything went well. Looking some years ahead, maybe 5 million from the 80" and 4 million from the 7' plus possibly some pictures from the 30" if there was someone who had a good case that he wanted us to run it. But to run the 30" we would have to have an intermission on one of the other big chambers; we wouldn't have a separate crew available for the 30". We would hope on this time scale to have the capability of running a track sensitive target in either the 80" or 7' chamber and to have made the 7' bigger. I think this does reflect a feeling that one of the things bubble chambers may want to do in the future is to try and give as complete information as possible about neutral secondaries as well as better statistics. The track-sensitive target in a fairly large chamber, which gives you a chamber to detect and measure everything, is a step in that direction. It would present lots of problems for people interested in software.

Now, where do we actually stand at the moment? We are in a position of agonizing reappraisal, because the amount of money we think we are likely to have seems to gradually shrink a little bit each time the subject comes up again, and the cost and time involved in doing anything seems to get a little bit bigger each time it is considered. So it doesn't seem to us at all clear now that the plan I described is something that we can really do on a time schedule that would be satisfactory. In particular, the manpower to construct this new north area, with new beams in it, may really just not be available at Brookhaven. In any case it would certainly conflict strongly with other parts of the program that have considerable emphasis behind them, so that it might be very hard to move the 80" chamber into a new position during the 4 month shut down of the AGS. When we started it was felt that it could be done while the machine was off the air. Now it looks as though it would take probably at least twice that long. Also as we begin to get specific estimates on how much this would cost, it looks as if it might cost more than we have. So I think the decision of whether that plan is to be implemented in 1971 or possibly put off until a later date is a very open one at the moment. The question of how much is practical in terms of a modification or extension of the 7' chamber also depends primarily on how much money and manpower we have available, so it is all part of the same general uncertainty. It is clearly the direction that we want to move in, but it is not very clear how far and how fast we can accomplish it. That is what I know about it.

PLESS: If there are any questions from the floor I would entertain two of them.

(Question from audience): I heard you say that the first neutrino experiment with the 7' chamber will be at the begining of '72. I thought it was to be immediately after the AGS starts again.

THORNDIKE: We have hopes that when we turn on for the next month that the chamber will work beautifully. If all goes well, and if it works beautifully, we will see how soon we can arrange to head some neutrinos in its direction and how many of the currently approved 300K pictures (out of the million that were proposed) we can take before something breaks. Your guess is as good, if not better than anyone else's, as to what will happen.

PEYROU: If it is not going to make the neutrino pictures before '72 it might change my statement about BEBC. We might be interested in that case.

THORNDIKE: We are going to do our best, clearly. How well we will succeed we will know in another year.

(Question from audience): With regard to the new rf beam, if you don't move the 80" chamber into the new area for awhile will you try to move the new rf beam into the present location, or is that impossible, from a geographical point of view.

THORNDIKE: I think that is impossible because the distance is not long enough. At any rate if we don't make the move, then, in all probability, we will leave things the way they are. I might mention that at the moment, the backlog of requests for 80" bubble chamber pictures stands at 10 million. Since we only took 1.5 million during calendar 1969, we have a seven year backlog at that rate. I hope we can do a lot better than the '69 rate. Since there is a very extensive backlog for things the chamber could do right where it is, it is not clear that it is desirable to move it to a new place where it can do other things and not finish the backlog.

PLESS: Now, Jim Sanford will talk about NAL's plans.

SANFORD: At NAL, led by Bill Fowler and our new bubble chamber group, we have started plans which will lead to the rapid construction of a fifteen foot long cryogenic bubble chamber. This is patterned very closely after the bubble chamber design that is being developed at Brookhaven, earlier versions of which led to the 14' diameter chamber and then later to the proposal to build a 25' chamber for NAL. When it was apparent that the 25' chamber proposition would not be included in the President's budget, those of us at NAL decided that the way to proceed would be to build a bubble chamber out of the available equipment and construction funds that the lab already had authorized or had at least a good expectation of getting. The present plan is for a 30 thousand liter cryogenic chamber estimated to cost 5 million plus manpower for design and construction. We will split the money between equipment funds and construction funds and intend to have a bubble chamber ready to go in the high energy area in 1973. Now this is a fast schedule but then the whole project is moving very rapidly these days. We can only meet this schedule by using design features that have been incorporated into chambers that you have heard about today, or plans that have been made for chamber modifications. There will be six cameras using 70 mm film, of which 3 will be used, at a given time. The possibility exists for interleaving of pulses where three cameras might be used to take neutrino pictures and the other three cameras might be used to take pictures of strong interaction particles in the same machine pulse. Whether that is feasible or not I think depends upon whether the liquid configuration inside the chamber is suitable for neutrino experiments and for strong interaction physics at the same time. We hope to use this 70 mm film in such a way that groups can handle this film without developing an entirely new analysis system. In terms of the possible output of such a device, I, like the others on the panel, put some numbers together. Of course, it is very difficult to predict on a new accelerator exactly what year might achieve a certain level of operation but then looking ahead to 1974, which should be the first stabilized year, assuming we start running for high energy physics with a chamber like this in early 1973, by 1974 we would be up to something like 4 million pictures a year. Now since the pictures would very likely be split between neutrino physics and strong interaction physics I estimate that there would probably be two million available for hadron beams. Let me mention something about the beams that we are hoping to have since some of this might not be familiar to you. Needless to say, with a high energy accelerator we are emphasising the high energy beams. It is of course true that there is a very large flux of lower energy particles from such an accelerator but I think that would be an inappropriate use of NAL. We have 3 beam possibilities in mind. The dominant one in the experimental area is the neutrino beam, since that sets the overall configuration for the experimental area. This area is about a thousand meters long containing a decay tunnel 600 m long followed by an iron muon filter of 300 m. Therefore as far as the bubble chamber is concerned, the flux of neutrinos is between 3 and 40 GeV even though there is a long tail up to high energies that undoubtedly will be exploited in a number of counter experiments. I think the bubble chamber will work in the region around 10 GeV where there is a substantial amount of flux. Paralleling that beam there will be an rf separated beam which I quite honestly believe may in the begining not be separated. It's a matter of how rapidly the equipment can come together. If it is run unseparated in the begining, it will be able to transport particles up to a secondary momentum of 100 GeV/c. Of course, that would include protons, and π^- and π^+ up to that energy. By the use of S-band deflectors which are the familiar ones, we would be able to separate K's between 30 and 50 GeV. With higher frequency rf separators, such as X band, we would be able to go from 30 to 90 with virtually no regions that are not covered. There will be adequate flux for bubble chambers. Since the upper limit on unseparated beams seems to be about 90 GeV we are not going to fix that beam such that it can go much higher than that. We have another possibility, at the end of the K pipe, to get out protons or π^-'s up to the full energy of the accelerator so that they could be brought into the bubble chamber.

FIELDS: What does that mean, full energy?

SANFORD: This experimental area will be capable of operating at 500 GeV. That is to say 500 GeV protons. Whether it is important to have 500 GeV protons for the bubble chamber in the begining is something we have not sat down and worked out. We have worked out the design for 200 GeV protons and π into the chamber.

I have mentioned three beams: the neutrino beam, a separated beam which will go up to 100 GeV, and an unseparated beam which at the present time is designed up to 200 GeV, but I believe will be extended higher for use in the bubble chamber. I'm not going to speak very much about other chamber possibilities at NAL, except that it is a subject which is under continuing discussion. Proposals will be coming in rather soon from experimental groups and we at NAL will be very much influenced by the nature of these proposals with respect to the need for the second chamber. Desire has been expressed for it but I think the proof of that is in the proposals. If there are available dollars and a chamber suitable for bringing to NAL we will find a place to put it.

Of the new developments with respect to chambers I think the strongest one is the hybrid system where one will use a bubble chamber to determine the vertex of the interaction point, but spark chambers and magnets downstream from such a device for analyzing the high energy particles. We have not embarked on a plan for building such a device, but there has been strong sentiment expressed in our summer studies for doing such a thing. That's a device the bubble chamber people look at and see a bubble chamber, while counter people look at it and see a hydrogen target.

Leaving the machanical problems for a moment I want to say that I think that bubble chambers are in a defensive position at the present time in terms of support. I have sensed this from a number of discussions that have been held on program committees and elsewhere, and I think if bubble chambers are going to be able to supply the film that is needed for the analysis you are working on, more support and help needs to be given to bubble chamber operations and to the people who are responsible for providing them. It is just not feasible nowadays for bubble chamber staffs to be increased. New ideas, techniques, and methods to improve the picture taking capabilities of these devices is extremely important. Hopefully, one can bring down the cost of taking the pictures. I think that is something that one tends to push off in a corner and assume that it will just be taken care of automatically. In fact as we see, the output from chambers has not been going up and help is needed there. I also hope that more people can turn their attention to the need for justifying the use of chambers as a means of doing physics, both at the new accelerators and at the old accelerators. We must be a little more articulate in this regard because nothing is automatic any longer in terms of getting a new chamber or even of improving an existing one. Arguments are faced on all fronts, so in order to run well and avoid a mismatch between the chamber outputs and the capabilities of our analysis systems, I think we need to clarify the justification and the arguments for chamber operations as satisfactorily as possible. I think that's all.

PLESS: Let's play the game according to the same rules. Any explicit questions to Sanford about NAL?

BUDDE: (CERN) What kind of magnet do you plan for the new chamber. Maybe you said it but I missed it.

SANFORD: Excuse me, I did not say it. A super-conducting magnet with a field of 30 kilogauss.

PLESS: There's one more.

VAN de WALLE: (Nijmegen) If you need a second chamber, what would prevent you from moving the 12' Argonne chamber to NAL?

SANFORD: There are two considerations that have been looked into, quantitatively. The first is that it is an important part of Argonne's program, as we have heard here, and the country is going to lose that capability by moving it. The other point is that the cost for moving it has been estimated several different ways and all of them come out to be about 6 million dollars. It's an unbelievable number when you first hear it, but the bulk of it has to do with the fact that one of the main features of the chamber is that it requires a large building. Pieces of the magnet require a one hundred ton crane and it is built in a style that anchors it to that particular part of Illinois where it is at present. When we considered these two things together, it seemed that with some of the new ideas that exist, the intelligent thing to do would be to build another chamber for approximately that amount of money.

PLESS: I think that answers that.

BALLAM: I was going to make up a very small chart for the panel, that would show how many pictures are planned for calendar 1971.

PLESS: Why don't we start with Peyrou first, then go around the table. He wants to know how many pictures you actually plan to take in calendar 1971.

PEYROU: I plan to take 5 million with the 2 m. I mean with the help of God.

PLESS: Of course.

PEYROU: 1.5 in Gargamelle and 1.5 in the 80 cm.

PLESS: Tom you're next.

FIELDS: I didn't get a chance to think, but I won't let that stop me; I said 3 million with the 30" and 2 million with the 12'.

PLESS: You want to look into the crystal ball, Alan?

THORNDIKE: Well let us see. The figures I had noted down carefully avoided 1971 because that's a bad year during which the AGS won't be running for a fair fraction of the time. Taking that into account I guess one would say maybe three million from the 80" and one half million from the 7', and probably two million from the 30" and 31". I have no idea in what proportions.

BALLAM: And at SLAC the 40" would be, say 2 1/2 million and the 82" would be four million.

COMMENT: --- seems like a price fixing conference ---

BALLAM: The reason I did this was for the immediate future for people who had PEPR's and so forth. I didn't add it up, but I guess anybody could. That makes 8 million in Europe, which doesn't affect too many people here and 17 million in the States. This assumes that the chambers will work quite well, and so forth.

PLESS: I think the question assumed a reasonable probability that there is money for it and that acts of God are not unfavorable.

BALLAM: I think the labs know their budgets pretty well, at least for fiscal 1972, so we can regard this as a reasonable upper limit; you have to apply a factor of less than one to get reality.

PLESS: I would like to break that factor into two parts, one is acts of God, and one is money. Let me start with you, Tom. In terms of money, what kind of factor would you say?

FIELDS: This is what we have budgeted to do by various means, by mothballing the 40" chamber and so forth.

PLESS: An excellent answer. Do we get the same kind of answer from you, Joe?

BALLAM: As far as SLAC is concerned, that we have been thinking very hard about trying to finance and schedule 6 1/2 million pictures. I think it is a realistic number barring any national sledgehammer on us.

PLESS: If they come around halfway through and say you are going to lose 30% of your money, well that's not good. You have to realize that this program represents essentially closing three chambers, the LRL 25" chamber, the heavy liquid chamber at Argonne, and effectively one of the small chambers at Brookhaven. The Brookhaven number includes some 7' running but a very small amount.

There is a disparity between the 13.3 million that was run in 1969 and the number predicted but you have to remember that during that period the 72" chamber was down and being transferred to SLAC in '68. Between '68 and '69 we dropped from 19 to 13.3 million.

FIELDS: There were a number of complications in the statistics and the biggest one is that the PPA chamber was included for an actual 4.6 million in '68 and an actual 0 and anticipated 0 in calendar '69. If you exclude that, then the actual picture production was constant from '68 to '69.

PLESS: I think that brings out that picture taking has been semi-constant for the last couple of years. The number of pictures being more or less realistically proposed for calendar '71 is an increase over calendar '69 which is a good sign rather than a bad sign. However, let me turn to Charles, he was the next one to have a question he would like to ask.

PEYROU: Well there is a comment I would like to make. There is one thing which is never realized in the progress of bubble chamber physics. It is that many experiments we have done in the past required a lot of pictures and a lot of scanning to study a particular reaction, but lots of pictures were not analyzed because of lack of measuring power. Now with PEPR's we can do much more physics with the same amount of film. The other question is to Sanford. My question is just a little bit nasty, but here it is. I can understand how it would cost 6 million dollars to move the 12' chamber from ANL to NAL, but I do not understand how you can build a 14' chamber at a new accelerator for only $5.5 million.

SANFORD: With respect to the cost of constructing it, I think that the estimate is based very much upon the cost of building the 7' at Brookhaven and extrapolated from that. Now the 7' hasn't been working yet, so we'll have to take that into account.

PLESS: To get the topic back on to what I think is the main statement of this discussion, namely the interaction between labs and the users. Are we or are we not matched? I think this is a good time to come back to the original question that Charles asked about the table where the average number of physicists was quoted as 6 per group. Obviously the labs are going to feel very uncomfortable if they are turning out lots of pictures to a group that is inadequately staffed to handle it and I've asked a good friend of mine from Oxford, Professor Mulvey, to answer that question if he would.

MULVEY: (Oxford) My impression on looking at the numbers, is that it means that about one physicist per 100K pictures, one Ph. D., that is. Of course, we know there are also graduate students who usually make the most important contribution, so at least there is one extra graduate student per Ph. D. My feeling is that it is not an unreasonable number in the present context of the physics that might be done by the groups that we are discussing here. This selection of nine groups doesn't give you the sort of sample that you would get if you took all the groups in Europe at present. There, a lot of groups with a number of physicists using a small number of machines each measure relatively small number of events each year. So if you take an overall sample, the numbers come out to be more physicists per 100K events. My impression is that with the sort of physics experiments that will be done by the groups that we are now discussing, and others like them, one Ph. D. per 100K pictures doesn't seem an unreasonable number. Somebody commented on the number of authors on present-day papers. These always contain a large number of graduate students as well. Also sometimes, since the experiments in the past have always taken a long time, it is not unusual for some of the authors to have been in on the begining of the experiment and then to have left, but their names are still there. So there are several factors which mean that the number which Charles commented on is perhaps reasonable. The sort of experiments that one is looking forward to doing are experiments in which the physics demands large statistics. We know a number of groups over the world that will be able to handle these experiments better than before. The worry is that some of the smaller groups are going to find life rather difficult.

In a certain sense the numbers that have been mentioned as the number of events to be measured are rather modest. It is clear that people are taking the number of hours in a day times the number of events per hour and then multiplying by conservative factors. We noticed this at CERN a couple of years ago when we made a survey of the various groups and asked them how many events they had measured in the year that had just finished and how many they expected to measure when they got all their things working. Very few groups made theoretical calculations. They made guesses on the numbers and events they thought that could be handled by the physicists. They recognized another limitation that everybody realizes to some extent, namely, the availability of film.

Perhaps another limitation which I feel to be serious is the amount of computer time that we shall have for the post-measurement physics analysis. With these big experiments being more and more sophisticated, one takes a lot of computer time to get the interesting physics answers out of a large sample of data.

Since I've got the microphone here, I am tempted to go on and make some other comments. One is that I very much agree with the suggestion that one should look more at triggered experiments to make more efficient use of the time and to reduce the somewhat ridiculous cost of film. Another remark is that there is going to be a high field rapid cycling chamber which perhaps has some possibility of being used as a hybrid chamber with thin windows. I hope that it can be used with external counters. This thing could be built at the Rutherford Lab for use possibly at CERN.

PLESS: Would anyone in the audience like to say something?

ALLISON: (ANL) I've got a whole list of non-burning questions, but I hope that they might inflame some discussion. With the 12' chamber coming up we've been thinking about things like automatic scanning and big chambers probably as much as anybody, although we haven't done anything in practice, of course. One advantage of considering something like automatic scanning in a big chamber is that you have a demagnification of 50 to 1 on film and so when you search along your beam track in the film plane you are looking for a lot of cross section and automatic scanning should pay off. You will find events very often. There is another problem of how many beam tracks per picture one could stand on a frame. That has come up in the discussion and it seems to me that the whole problem is proportional to the number of beam tracks you can scan in one picture. We have them very nicely defocussed and parallel, spread out in a thirty inch chamber at the present time. The automatic scanning went sufficiently well that in the final run we ran with more beam tracks, and we asked for more beam tracks per picture. The problem with something like the 12' chamber is to spread them out over 12' which is pretty wide aperture for the beam. And this brings up the whole problem of stepping magnets and things again. And I am not a beam expert, but it does seem to me that stepping magnets are a good thing. We can put more and more into a bubble chamber with the beam tracks separated, and thus can do more and more physics per square centimeter of emulsion and per expansion and so on. I would also like to suggest that with automatic machines fisheye optics presents no real problems because it is a function of position on the film and can be mapped out. Also, automatic machines are going to have an additional advantage over human operators in the problem of increasing chamber to film demagnification for economic reasons. A lot of us have been forced to consider taking 35 mm film where we previously had 70 mm film. There is an enormous amount of money to be saved here. I have strong prejudices against it, from the point of view of our particular machine but I have difficulty in justifying those biases in terms of actual numbers. My interest is in terms of what other people think about this. Should one consider using a tube with a 20 micron spot looking at 35 mm film with 1 to 1 magnification or **is** this sort of thing not going to make for good physics. My biases tell me that one would not do so badly on precision but one would have a lot of trouble on recognition, for example in recognizing short tracks and problems like that. It will be here that I expect the trouble to come, and it will need more fix up time and more help in such situations but I guess this is particularly addressed to the PEPR groups as to what their feelings are on this.

PLESS: Does anybody want to make any comments? I think there were several interesting points brought up by that list of non-burning questions.

PEYROU: On the question of many primaries, one should save Government's money by using as many as you can. The question of having them spread with a stepping magnet is a complicated business. But what you can always do is to use a sweeping magnet, that is you can just increase the spread by using sweeping magnet. If you use a sweeping magnet you don't need the stepping magnet any more because your separation is then so large that overlapping tracks would be very rare. But you cannot use it with r-f separators, remember that. You'll have to sweep in nanoseconds. It can be done, but it is a little more tricky. There was another question. What was the last one?

ALLISON: (ANL) The problem of how much demagnification one has. Is this particularly a problem for CRT's?

PEYROU: This is an unpredictable business, in the following sense. Everyone worked with the four classical chambers and took years to get the proper optical constants. They they thought that the big chambers would have serious distortion from theoretical turbulence, but all of a sudden you get into business and you find the precision is better than you thought.

PLESS: I agree with that comment. The crucial thing, I believe are the optical constants of the chamber. The people who own the chambers and are responsible for them can help in getting those accurately. We can discuss that. Now are there any other comments or questions to the panel from the floor?

LUBATTI: (Washington) I would like to speak to the question raised by Peyrou having to do with the size of the group. I have had some experience working with large collaborations. In fact I have been on papers where we have had 25 people and I know that very often on some of these papers, perhaps 4 or 5 people actually contributed to the real work of seeing that particular piece of research through to the publication. And then I ask why it is that we ended up with so many people? It makes a lot of sense in the classical bubble chamber operation; when one is trying to handle 1/2 million pictures, one is forced into the situation of having many scanners and having a lot of people who are in fact doing lots of administrative tasks, but not so much physics. In Seattle, we are starting with 3 Ph. D.'s and we have essentially done all the material work necessary to bring PEPR into production. Once we are in production, some of us will have to start teaching so we will become 1/2 or 1/3 time physicists. I hope to add two post-Ph. D.'s to the staff, and then stop there. At the 5 Ph. D. level, having about one student per man, I think that we can easily handle 300 to 500 thousand events per year. We must bear in mind that as you increase the statistics in any one channel the details of the analysis don't change too much. One still has to fit the same curves, one has more statistics of course, one has to pay more attention to detail but the work does not go up linearly, at least not in my experience.

BALLAM: I would really like to disagree with that because the minute you get into a high statistics experiment the physics changes. The minute the physics changes you need some people sitting around thinking about how you are going to handle the physics -- what interpretation you are going to give to it and how you are going to re-analyze and re-do the data in different models. You have to talk to different theorists and so forth and so on. So, it seems to me that the need is not for more clerical people because it's true that once you have a system going it grinds along pretty well because you understand the systematics of your scanning girls or what have you and you understand how to handle the data. That is fine, but you need twice as many people to sit down and think about what to do with it all. I found that to be true the minute we got into some high statistics experiments at SLAC; the quality of the physics just changed and I think that's very important.

PEYROU: Well, I agree completely with you, but on top of it, even if you forget about the difference between low statistics and high statistics, one of the best periods of my life and one of the most productive was when I was in America. I ended the year with two papers, one based on 20 events and one based on 5 events but thoroughly understood, and it was signed by 4 people, one Professor, two young Ph. D.'s and one graduate student. I always considered this is the ideal sort of publication.

The point I am making here is that when you have 500K events you have not 500K events of one type of physics like we had 20 lambda's. You have a lot of junk, and you want to use all that junk because you are not likely to get any more film. I make the very optimistic assumption that there will be five interesting topics each with 100, 000K events which is, of course, an ideal situation and I was saying you need to have free three full-time physicists to analyze that stuff. It is the ideal constituency of people who will try to discuss and meet each other and finally write the paper without too much ado. I agree that if you write a paper with 20 people that this is a most time consuming affair. Mine is just an ideal model. I simply don't believe that you are going to do much physics with fewer people. I do not believe a man can look at 100,000 events, to to the computer, come back and sit down and get done good physics without talking to someone about it. If you believe that the other fellows are going to hear your problems when they also have their 100,000 events to worry about you are a fool. This is my point. That is why I do believe it is all right already with the two Mulvey suggested, the graduate student and the boss, providing the boss has a lot of time to think and not only to make administration. Of course I know there are many other people involved, like programmers and sometimes programmers have good ideas about the physics, but that was my comment.

LUBATTI:(Washington) Let me just add, I think we are all in agreement with essentially what I said, that is, that 5 Ph. D. 's and roughly one student per Ph. D. is about where the cutoff is.

AUDIENCE: Per topic?

LUBATTI:(Washington) Yes, per topic, O.K. that's right, one has to choose his topics.

PLESS: Is there anyone else that would like to make a comment? John Mulvey back there, and I think there is another hand up after him.

MULVEY: (Oxford) I just wanted to ask a question that hasn't been raised and it is irrelevant and nothing to do with the panel, but one thing that we might begin to think about. It relates to some extent to the size of the groups. Some people involved in this total effort, we, for example, might begin to think about other applications of the systems, hardware and software that we have built up to handle bubble chamber film. We will not be running our actual measuring systems 100% of the time. We have brought a number of brilliant engineers and other people together, who are always improving the systems, and while it is necessary to look at the big bubble chambers and many other problems that are still left, I was wondering if anybody in the audience has any comments about what they are doing or what they might think of doing in using these systems for applications other than bubble chamber physics. I know POLLY and it's predecessor at Argonne has already done work of this kind. I don't know whether, Mr. Chairman, you think we have 5 minutes for people just to mention various ideas that they have had or heard of.

PLESS: I think certainly that we can spend a few minutes on that, does anyone want to make a comment, does anyone actually have active plans at this instant? There's a hand back there, that's Ray Kenyon.

KENYON: (Washington) I have worked in CRT scanning for a few years. One application that I have worked on was the CRT scanner for Don Glaser at the University of California. That scanner was used in the field of molecular biology which is a perfect place for a CRT scanner because many of the things in molecular biology and micro biology require counting cells or bacterial colonies. In the medical field we thought of pattern recognition in the field of pathology and in addition, I have done work myself on the si de in hematology, actually, blood typing.

PLESS: O.K. Tom Fields would like to make a comment to that also.

FIELDS: There has been a CRT machine at Argonne for some time (CHLOE), operated as a computer center facility, which has been used for two purposes in high energy physics, namely spark chamber film and oscilloscope film. It was also used by biologists and other people for automatically scanning their film. It is now retired and is being replaced by ALICE. This is a CRT machine in the applied math division with a broad range of application, which should be operational within the next few months.

I would like to ask Joe Ballam or anyone else to comment on their experience in using these bubble chamber machines for streamer chamber film.

BALLAM: Well, John Brown of SLAC will discuss the plans to use a HUMMINGBIRD device for streamer chamber pictures. The LRL people are also using a Spiral Reader for it and have been successful in reconstructing streamer chamber pictures with rather a small amount of difficulty. I do not think in general that streamer chamber pictures will represent an insurmountable kind of trouble for any of you who have been involved in this business. There is the problem of flares in streamer chambers which may require different kinds of programming. There will be some different kinds of obstacles than you have in bubble chamber pictures.

As long as I've got the microphone, I want to ask a question of the audience in general. I have tried to help you by squeezing out from myself and other people in the large laboratories the numbers of pictures that we think we can take, but I don't have a corresponding number from you besides the chart. I don't really know how many events you expect to analyze in calendar 1971. I do not mean future eventualities, but I would like to know what is the match between the 19 million pictures multiplied by some number which is less than one and the actual number of events that's going to be analyzed. The biggest effort I have seen so far in terms of starting an experiment, doing the work, and finishing it up, was somc 1.3 million pictures that group A of LRL analyzed. There were 13 GeV K^+ and 7 GeV π^+ reactions. It took them esentially two and a half years with 2 Spiral Readers and some pretty savvy people in the business to do this experiment. I am not at all sure that this experiment is finished yet; I don't know how many events they've actually analyzed. I know that they have presented charts of pictures involving 3000 or 30,000 events in a given channel, and that is real channels, like the 4C channel $K^+ \pi^- \pi^+$ p or something like that. I would like to hear from people what the match is in calendar 1971.

DAY: (Maryland) I like to speak to the point of other applications before we get on to this. There is an application which probably some of you who are regularly exposed to astronomers will recognize, and that is that there are an awful lot of star plates around, some of which have never been looked at. There are very interesting problems which are hung up on making absolute measurements on small things like absolute motions of galaxies and clusters and variable star times and things like that so, in fact, if you ever mention in passing to astronomers that you have a device like PEPR, they really start turning different colors.

PLESS: O.K., if you have any spare PEPR time you know where to get customers. Let me try to answer for Joe, and then we'll ask for others. I think it is fair for the chairman to answer that question first for lots of reasons. We are talking about calendar 1971. I personally feel very committed to do 300,000 events for calendar 1971. Now whether we do it or not, we will know in about a year and a half, but that certainly is what we are committed to do.

LUBATTI: (Washington) I like to speak to this comment about LRL because I happen to have some information. I remember a year and a half ago at the New York meeting something like 5 or 6 months after they had finished that horrendous K^+ run, Stan Flatte presented a scatter plot with some 20,000 four prongs in it, so they did get a fair sample out fast. I also happen to know that soon after that, Stan Flatte got involved in another experiment at SLAC and Art Rosenfeld is involved in the laser beam. He is also involved, of course, in the π^+ run, Lina Barbaro-Galtieri for example, is involved with Bob Tripp's big exposure and I could just go on down the line. I think it is unfair to point to the LRL people as a typical example of a bubble chamber group.

That is really quite a huge enterprise and a lot of these people have taken on tremendous interests: all of them are doing several experiments. I could even name other people who are working half time with Luis. Bob Watt and other people are working and solving problems for Luis in positioning his new spark chambers. I know all these people well because I've worked with them in the past and I always like to see what they are doing because they always do interesting things. The only point that I want to make is that they are all marvelous physicists and just like all of us they have tremendous interests and so they do lots of things. I think it is a little unfair to use them as an example of someone who has dragged their feet, they just have not been uni-directional.

BALLAM: I was using them as an example as people who didn't drag their feet; I thought they did a fantastic job.

LUBATTI: (Washington) I think they could have done it even faster if they weren't doing ten other things, and I think when Irwin says he is going to get 300K out I believe him, because he is not going to do anything else, he's going to push on it.

PLESS: Is there anyone else that wants to stand up and be counted? You are not required to, but is there anyone that wants to stand up and be counted? After that there is one other topic that we should address ourselves to and then we are going to close this panel. The topic that we want to address ourselves to is the pessimism, if I may use that word, of Jim Sanford regarding the bubble group and the rest of the world, if I can use that phrase. First, Dixon.

BOGERT: (Yale) I am not quite sure we'll phrase it in the form of calendar 1971, but I will say that in the next 12 months starting, say June 1, we will process about 150 to 160,000 events from 200,000 pictures.

PLESS: Good. Anyone else want to make a simple clear statement like that?

THORNDIKE: Well, I could make a comment about Brookhaven's data processing which is not by PEPR, but is a significant part of the overall use of pictures.

PLESS: That's clear.

THORNDIKE: For 1971 I think a fair number of events processed would be about 500,000 for the two bubble chamber groups at Brookhaven.

PLESS: 250,000 each?

THORNDIKE: It probably would not divide precisely equally, but the total is just based on a very modest extrapolation of the present production rate. Maybe while I have the microphone, I can introduce one other question. When we totalled up the picture taking capabilities, we left out one big accelerator, namely the accelerator at Serpukhov, and I wonder if anyone has any comments as to whether it will contribute anything during this period, say the next 5 years, to the group of people here.

PLESS: Joe, would you know?

BALLAM: No, I think Charles would know that better than anybody else.

PEYROU: Seven second repetition period, that is the only answer I know.

PLESS: O.K. the answer seems to be unclear, but it is down by at least a factor of 2 from any other accelerator. There is another hand.

McILWAIN: (Purdue) This is not based on a PEPR but on 5 SMP's. We measured 180,000 events in calendar 1968 on a fairly steeply rising production curve. We have levelled off this year, so the yearly rate is around 200K to 250K. To do that we have to be able to keep the girls we have and it is not at all clear that we will be able to do that.

ALLISON: (ANL) I am probably going to disagree with some numbers that Tom has given, but in the last year we measured 250K events. I think we start running into real trouble with our Sigma 7 if we were going to measure more than 500K events, so we'll certainly be somewhere in that region. I hope towards the upper end of it. That probably depends on our ability to finance the processing of data further on in the part of the system that has nothing to do with the measuring machines.

PLESS: Excellent, O.K., then let me get to what is the last comment. It is a question which I think is very important. It is addressed to the pessimism about the bubble chamber group's ability to withstand the criticism or to get the funds and support relative to the counter groups. Incidentally that pessimism clearly rings through to me when discussing the bubble chamber program at Batavia where what is projected is 4 million in pictures total with half of that being neutrino work. There is no indication, at least to me, that we could improve that number very much with just one large chamber there. So I would like to first ask Sanford, if I may, an unloaded question. As you see it from the National Lab. at Batavia, considering the various pressures from counter people, from bubble chamber people, and the young guys, from the old guys and the people who were young bubble chamber people and now are old counter people (I do not know of any of the reverse -- people who were young counter people who are now old bubble chamber people but I know certainly a lot of the other): How does the situation appear to you as sort of an overall impression? Is there any new blood that you see coming into the field, beating on NAL's doors saying that we want more bubble chamber pictures for our universities?

SANFORD: That's an unloaded question?

PLESS: That's an unloaded question.

SANFORD: Before I answer that unloaded question let me say something about that 4 million pictures that you may have missed. I did not count the potential for multiple pulsing in that estimate. I do not think multiple pulsing has really yet produced a significant amount of additional pictures, so I have not wanted to count that in at this moment. If one does take that into account, it would of course not help the neutrino situation particularly, but it could very significantly improve the number of strong interaction physics exposures.

PLESS: Could you make a comment about the necessary targeting and how much you steal from the flat top to achieve multiple pulsing at this machine as say compared to the PS at CERN.

SANFORD: It is the same story really. It is the tug of war between the flat top users -- the counter program -- and the bubble chamber groups. I would say that everything that Dr. Peyrou said is going to be applicable at NAL as it was at Brookhaven in the years of experience that I had there. NAL has one external proton beam. One can bring that beam out and split it according to program choice, not according to radiation damage within the accelerator or distributing the protons between internal targets and external targets. That is a more efficient way to make use of the protons, that is, as an external beam. I think we will gain some by that. As to the mechanism of doing this, it will be quite possible to have bunches of protons coming out every hundred milliseconds over a period of a second or so. So, in principle, one should be able to take 10 pulses over that second. I think that at the end of the 10 pulses the chamber just sort of gasps and so Fowler tells me maybe we will only do three pulses or something like that. I did not count that in the 4 million. After all, one of the big troubles with doing multiple pulses is that the film still gets used up and that is a significant cost item. I do not know whether we can afford the finances to do the multiple pulsing. One may do multiple pulsing for a short period of time and then do other things with the accelerator. To the question of competition between counter groups and bubble chamber groups, I did not mean to be pessimistic in raising it. I wanted rather to bridge the gap a little bit between those of you who are designing the systems to use the pictures and those of us who are trying to build the accelerators to build the detectors. We cannot take anything for granted in this business; we see bubble chambers that are being turned off, like the 25" and one of the Brookhaven smaller chambers, and maybe others yet to come and I think that one needs to make one's feelings known as to what the implications of that will be upon the physics output. Also, because we have such an investment in the analysis systems that are underway don't hang back if you have good ideas to express. I believe that it can help us when we are trying to seek approval for operating funds to keep chambers going or authorization for new chambers. These things are not happening automatically, by any means.

On the question of the balance between bubble chambers and counters, we all know a great many people who have in the past done bubble chamber work and who have changed over to counters. These people, having worked with bubble chambers in the past and now working with counters are older individuals and are now sometimes on program committees or on other groups and they spread the word as to why they changed. I think that new groups and younger groups have not really been heard from very much yet, in terms of their need for pictures. This is why I was pleased to be able to come here today to see some of the numbers that are being talked about. At program committee meetings it is very easy to talk persuasively about a very specific counter experiment, but the arguments for the bubble chamber experiments are much broader unless one picks out just a particular reaction and speaks only of that. To get high statistics, one needs a great many pictures and I think that the base for bubble chambers is in the fact that there is so much in those pictures, more than just what you are using at that given moment. I think that one has to emphasize that one will use those pictures for all these different purposes rather than just the one that has been proposed. In this way we can show that we are squeezing out of this as much physics as possible. I do not want to be interpreted as being pessimistic at all about this. I think it is possible to be articulate and persuasive about the need for bubble chambers but I think that more people must speak up nowadays, to keep this field at the productivity of which it is capable, and to keep the output from the accelerators going strong, and to match the output of the data to the capacity of the measuring systems.

PLESS: Would you like to make a comment Charlie?

PEYROU: Yes, this is a favorite subject of mine. I thought the fact that bubble chambers are on the defensive was only an illness of Europe. We all the time say, well, look we are so modest, we take only 20% of the machine intensity, all the rest is for you. Of course, money, money we cost. The true practice is the following, we are all suffering from the Rutherford-Faraday complex: we like nice experiments done by one man. The bubble chamber is not like that. The only man who was intelligent, I am sorry, I apologize, who was intelligent in bubble chambers was Don Glaser and after that Alvarez and all the rest is nothing. But the fact is, strong energy physics is done by us, not by them, and I defy anybody to prove me the contrary and I believe that the strong interaction physics is the greatest mystery. After all, weak interaction works more or less, O.K. nobody knows how it works at very high q and I apologize to the electromagnetic people, but the mystery is strong interaction.

There is also no lack, as far as imagination is concerned. Imagination comes into the PEPR's and the HPD's and the Spiral readers and the programs, but it is not seen. I think we should completely abandon that defensive complex. We are doing the best experiments; it cannot proved to the contrary. I bet I can walk on even the Brookhaven floor which I think is better than CERN's on certain points, and I can find an experiment and prove that one can do it cheaper and better with bubble chambers with less bias and more precision - any experiment I find on strong interactions.

PLESS: How would you like to come work for Batavia?

SANFORD: May I make a comment? One of the healthiest things I see coming along is some of work on ideas for hybrid systems which I think will help to bring some of these diverse interests together. I was not being entirely facetious when I said that the counter groups look upon this thing and see a hydrogen target in a hybrid system and and bubble chamber people look at it as a bubble chamber with some detectors downstream. It is quite possible to be doing physics with that device when you are not taking pictures, between the 100 milliseconds when you pulse the chamber. Even when it is a rapid cycling which can go faster than that, you can also be doing other physics. You will not see and you will not have a picture of the vertex, so you will not have some of the same physics insight into what is going on but it will be possible to also use that as a single arm spectrometer, for example, in a missing mass experiment, using the same device. I think that may be a very profitable multiple use of these pieces of expensive equipment. I am looking forward to some new development in that direction.

PLESS: Does anyone else like to make some comments from the floor about this problem and/or the NAL in particular? Lubatti from Washington.

LUBATTI: (Washington) Yes, in fact, I have felt this way for some time and I have mentioned this to Joe Ballam on occasion. In fact, Charles Peyrou was giving the opposition a little bit too much, when he said that they can do elastic scattering and charge exchange better. The reason I say that is that I remember just recently I was at Berkeley for a User's meeting and the people in the Powell-Birge group showed some results that they had. They had taken a bunch of low energy film from the 25" between 1 and 2 1/2 GeV, mainly to do an elastic phase-shift analysis. They took all of the 2 prongs and measured them all very fast. Then it turned out that in that sample they had a tremendous amount of elastic scattering at every energy that just came out on the DST. They plotted it out and looked at it. When they looked through the literature they found there were just two counter experiments in that energy range. One had 5 times less statistics, and one had 3 times less statistics. Recently I was at the Philadelphia Conference where they were discussing one of the great enthusiastic things in strong interactions these days -- the omege rho interference. I noticed that there was a counter group that started an experiment in 1965 and they have just now gotten results. They have roughly the same statistics on di-pion production as Wally Selove has from a bubble chamber run he took last year. I could find other examples, but I don't think that is the point, I think in general, when the counter people began telling us in 1964, '65 and '66 that they could beat the bubble chambers with statistics, people bought that argument. I have not yet seen an experiment where, by the time they get through folding in their inefficiences and their geometrical corrections, they have comparable statistics to a good typical bubble chamber experiment that can be run in a month at any accelerator and analyzed in about eight months.

PLESS: I think since the hour is late, let's take one more comment and I think then we'll call it quits. Allison?

ALLISON: (ANL) O.K. I would just like to sound another note of optimism. When I sat down to try and write a thesis about 2 years ago on a final state involving 5 or 6 particles, I found a complete absence of theoretical models of any kind beyond thermodynamic ones to compare with such data. It seems to me in the last few years there has been an enormous improvement in the theoretical models which compare with multi-body final states that have a large number of prongs, and this sort of stuff cannot possibly be done by any other technique than bubble chambers, at the present time. It involves detection simultaneously of a large number of particles. It seems to me that this development, if it goes on this way, is very good for bubble chamber techniques.

PLESS: O.K. Remy will be the last speaker, except for me, of course.

VAN de WALLE: (Nijmegen) I am not going to contribute anything new, I would just like to amplify that when we were doing an experiment at 5 GeV we were measuring a lot of two prongs and found that we had a lot of elastic scatterings which we did not care too much about, but when we started looking at them it turned out we had them down to a momentum transfer which was much lower than any counter experiment had done before, so we started to look at them in a neater way.

PLESS: O.K. I would like to thank the panel for their excellent presentations and their fancy foot work and close this discussion. Thank you.

-no

COMPILATION OF BUBBLE CHAMBER STATISTICS

Concerning the completeness and the precision of graphs:

1) The following data is plotted:

CERN and Saclay	1967	1968	1969	
BNL	1907	1068	1969	1970
ANL	1967	1968	1969	
LRL	1967	1968	1969	
SLAC		1968	1969	1970

The number of pictures on the graphs are not the actual experimental values. They are weighted by the size of an 80" bubble chamber: example, the actual 100,000 pictures in a 30" bubble chamber are reduced to (100,000 x 30/80) pictures.

The 1970 graphs represent in general the "accepted" number of pictures of a proposal. But, if an experiment already has completed a larger number of pictures, then we have used the completed number of pictures.

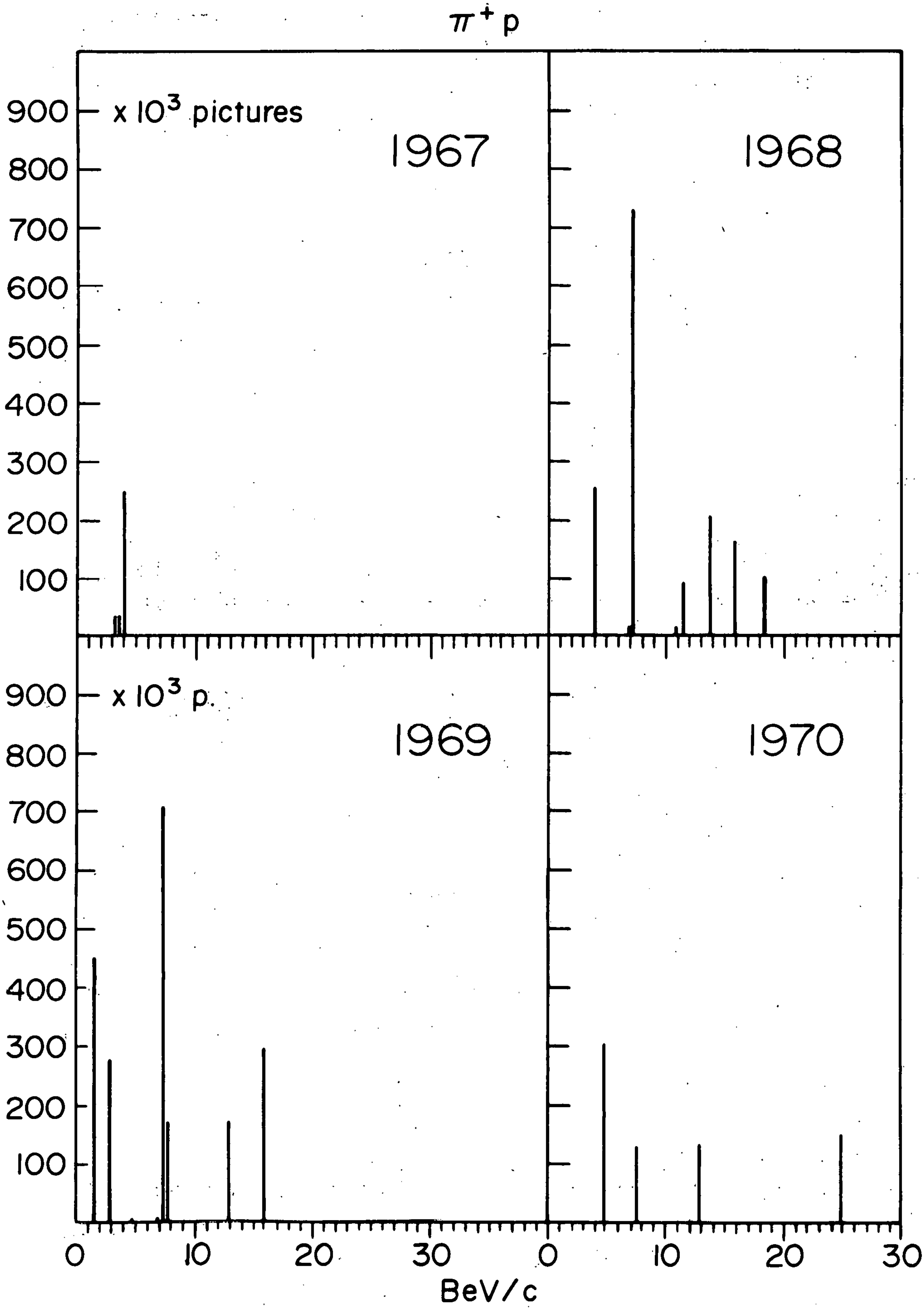
We have averaged the momentum of the incoming beam to its mean value, even when the momentum range is large.

When not enough information is available for combined experiments we have made assumptions.

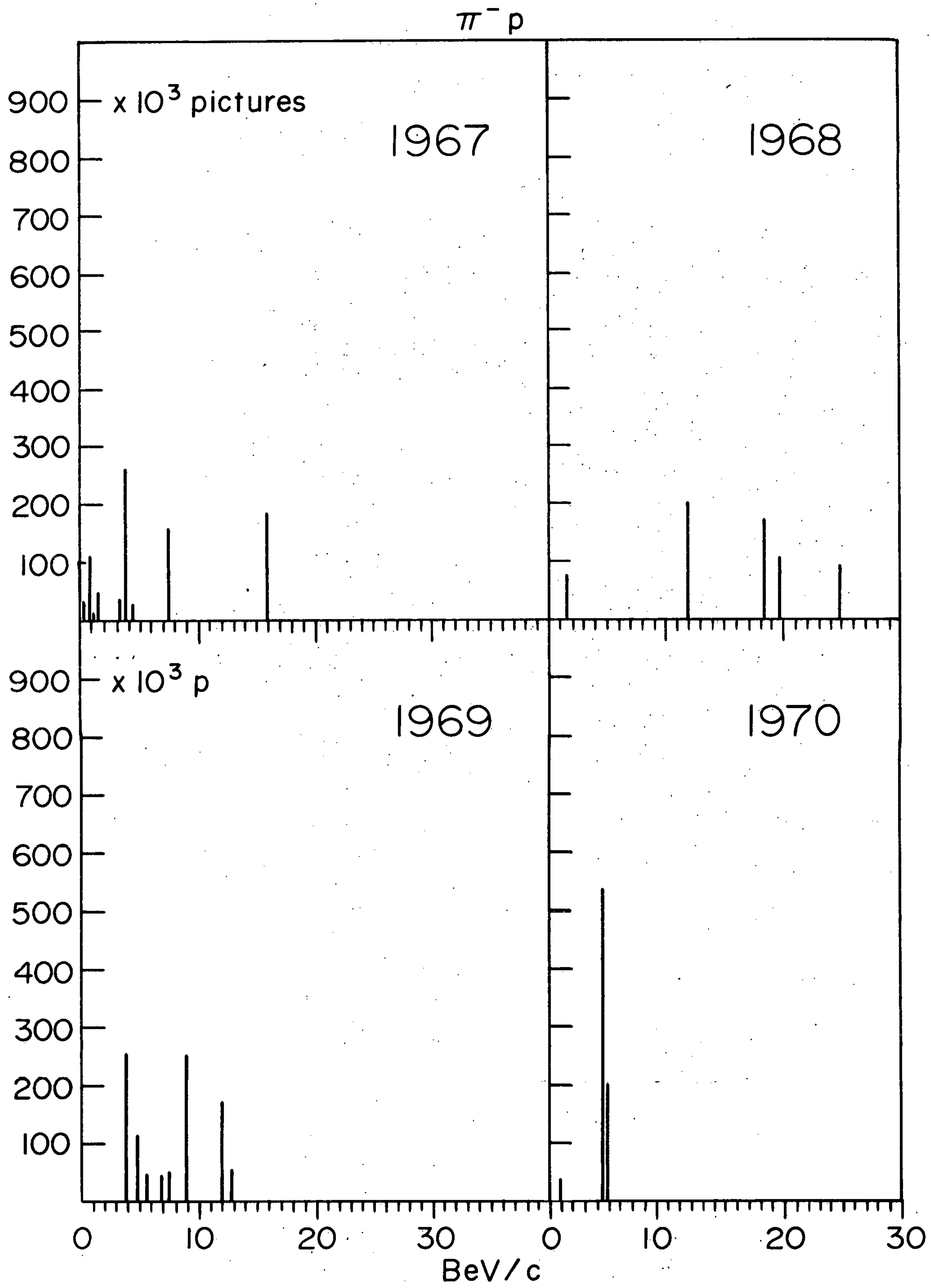
Example, if, for ($\pi^+ p$), ($\pi^- p$) interactions 100,000 pictures are taken altogether.

Then we assume $\pi^+ p \rightarrow$ 50,000 pictures
$\pi^- p \rightarrow$ 50,000 pictures

2) The classification of the data is done by histograming with a computer. The bin for the histograms are chosen to be 0.300 BeV/c. The lower bin limit is the plotted value on the graphs. Therefore, the maximum systematic beam momentum shift one can expect is -0.300 BeV/c.



π⁺p
x 10³ pictures
1967
1968
x 10³ p.
1969
1970
900
800
700
600
500
400
300
200
100
0
10
20
30
BeV/c


π⁻ p
x 10³ pictures
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x 10³ p
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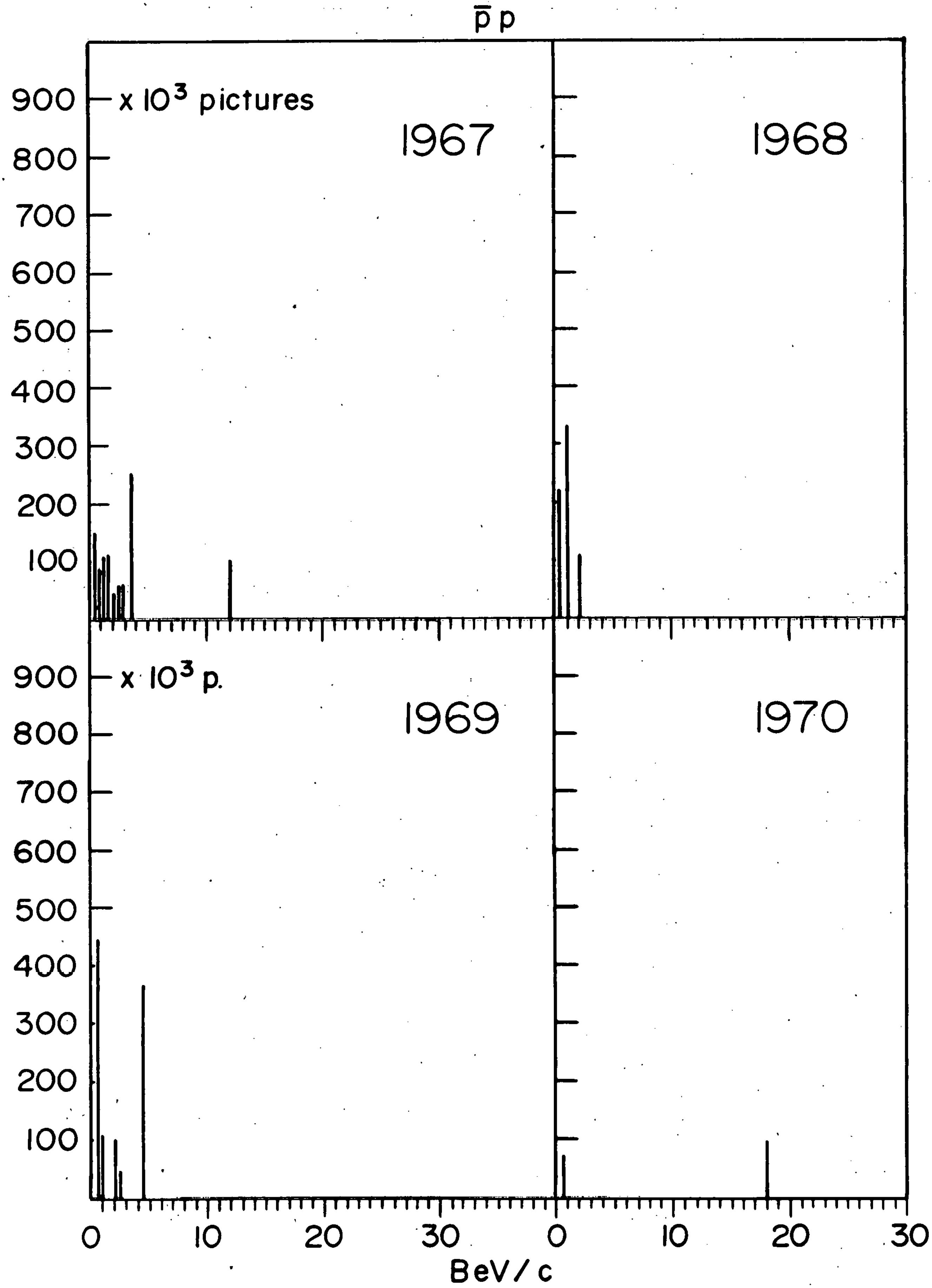
BeV/c



p̄ p
x 10³ pictures
1967
1968
x 10³ p.
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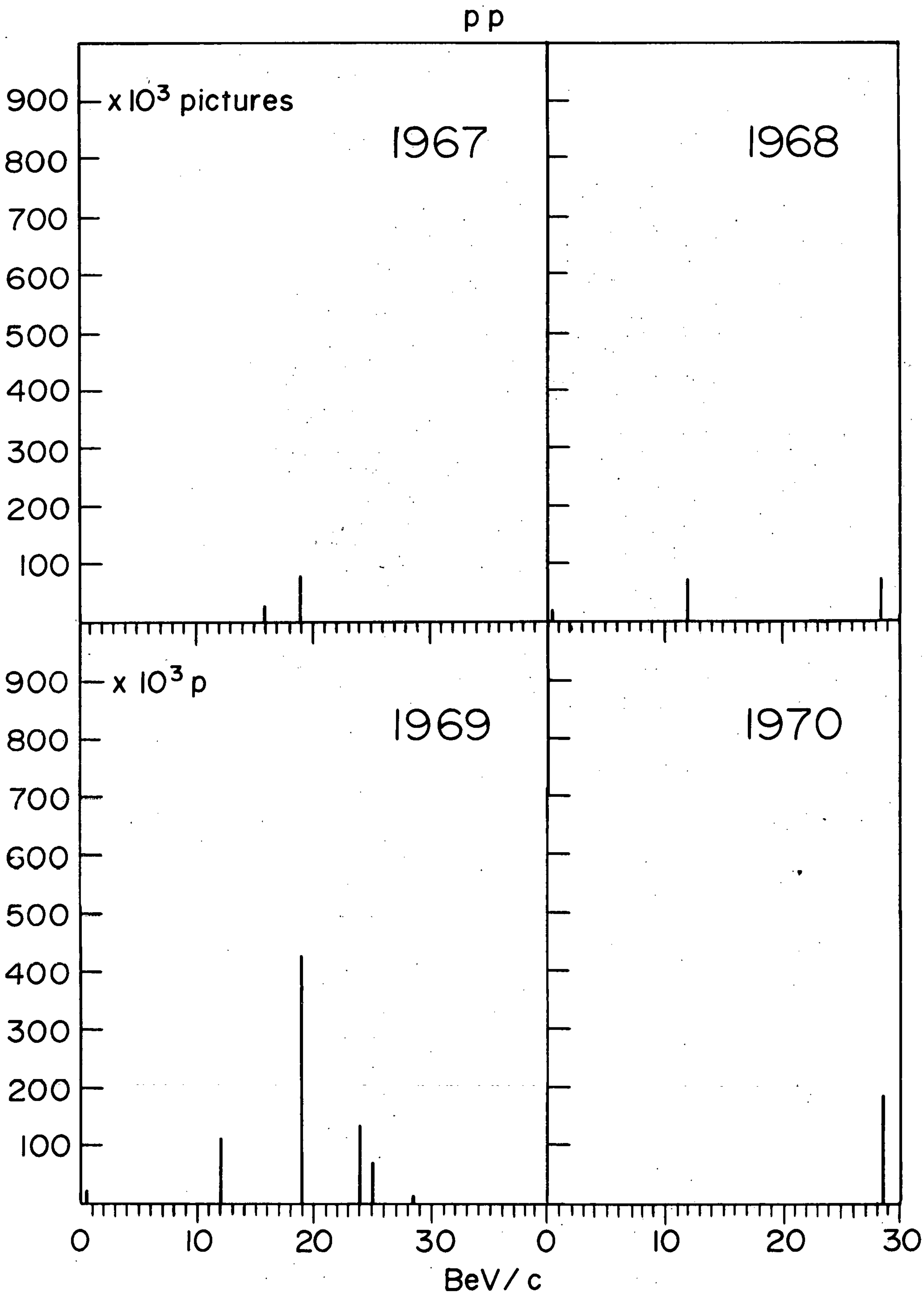
BeV / c


p p
x 10^3 pictures
1967
1968
x 10^3 p
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1970
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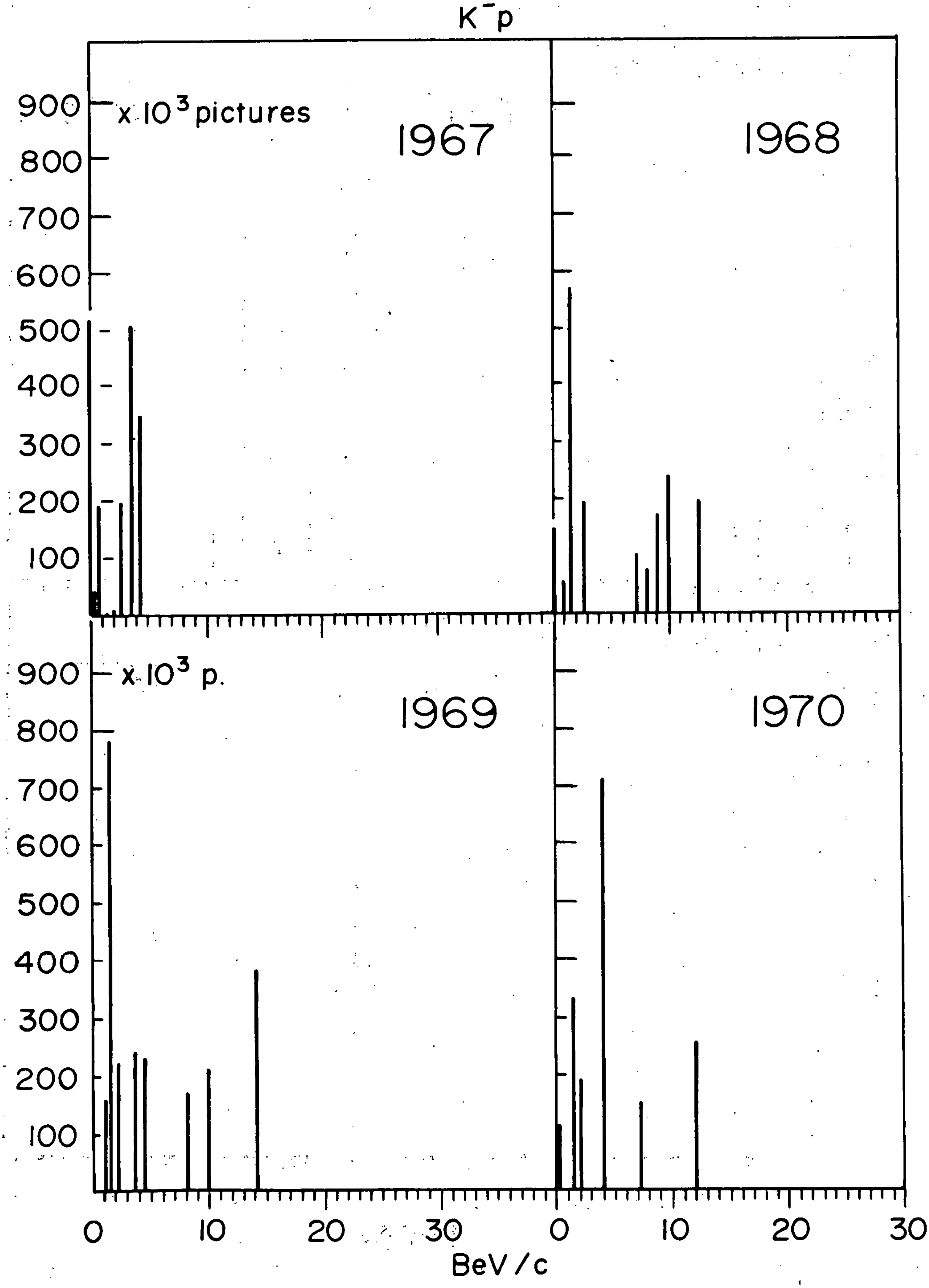
BeV/ c


K⁻p
x 10³ pictures
1967
1968
x 10³ p.
1969
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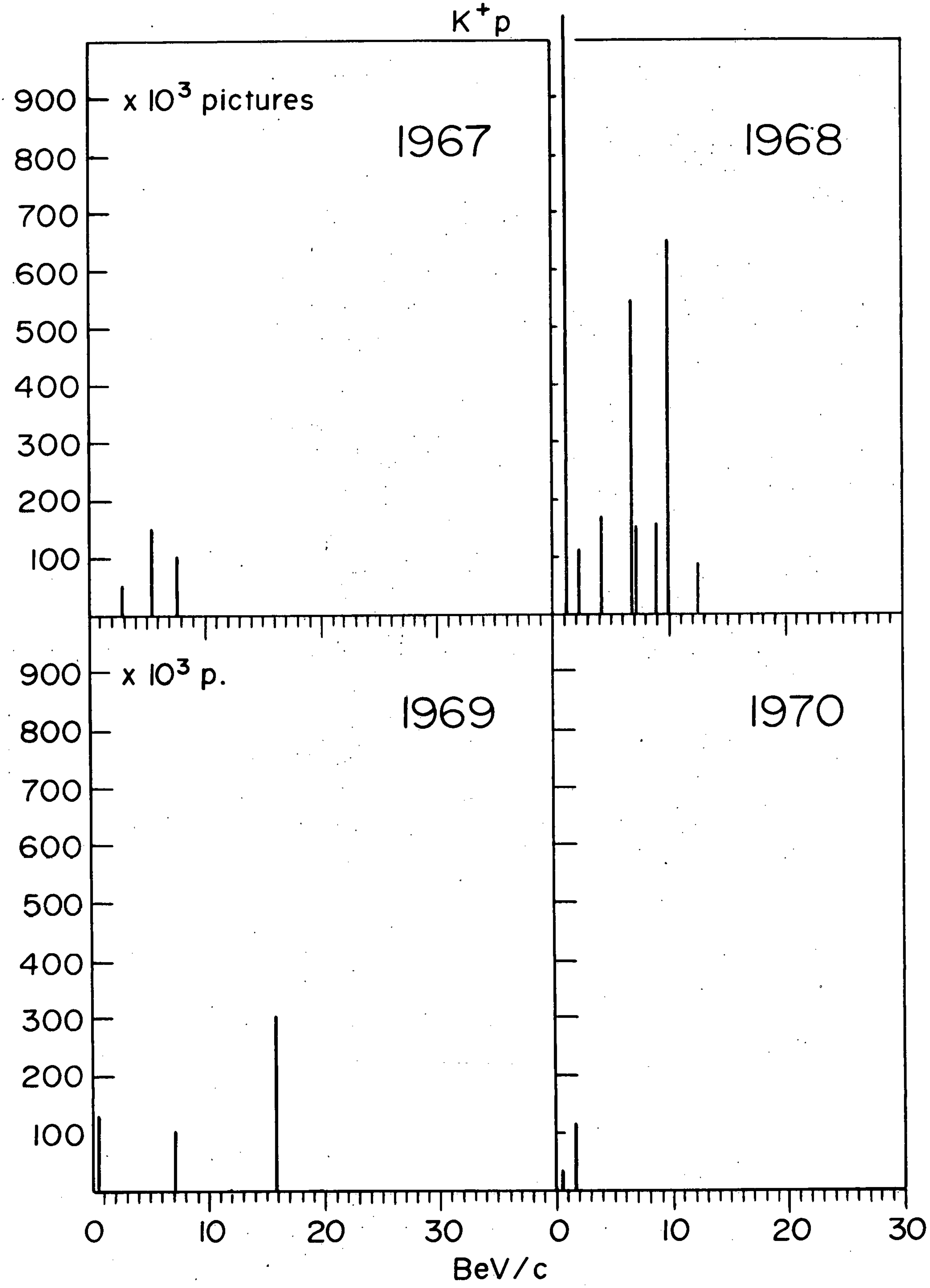
BeV / c


K+p
x 10^3 pictures
1967
1968
x 10^3 p.
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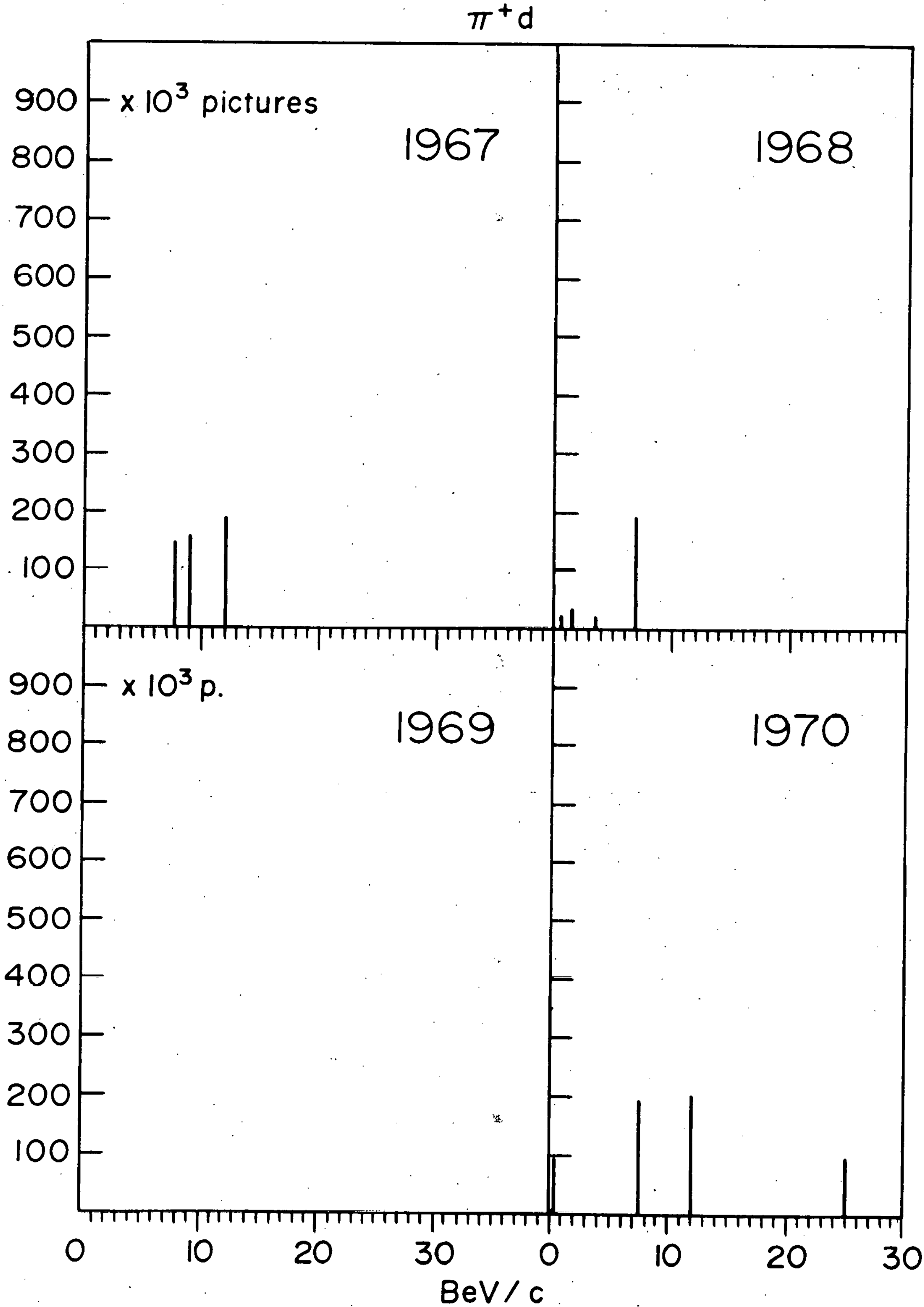
BeV/c



$\pi^+ d$
x 10³ pictures
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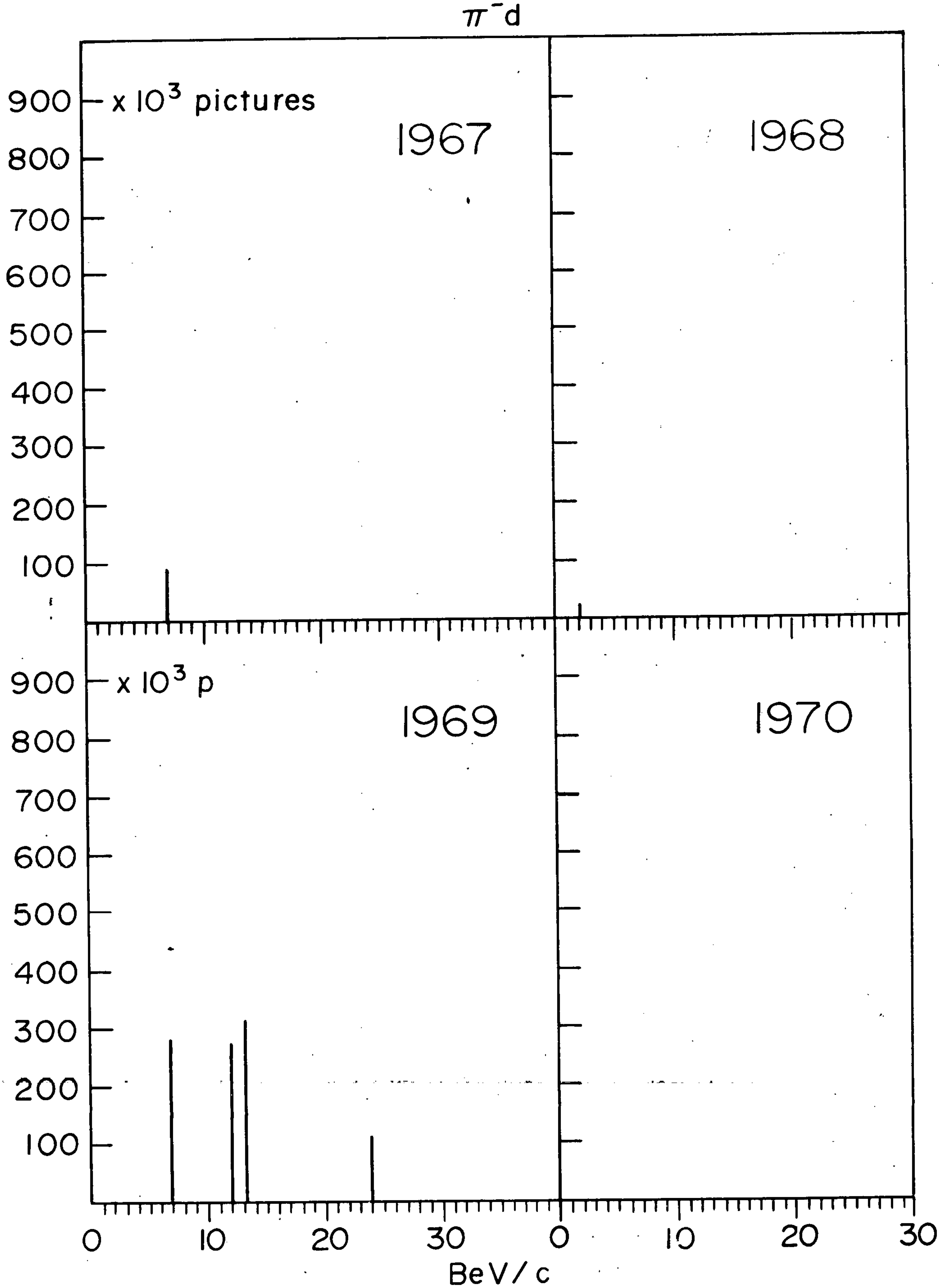

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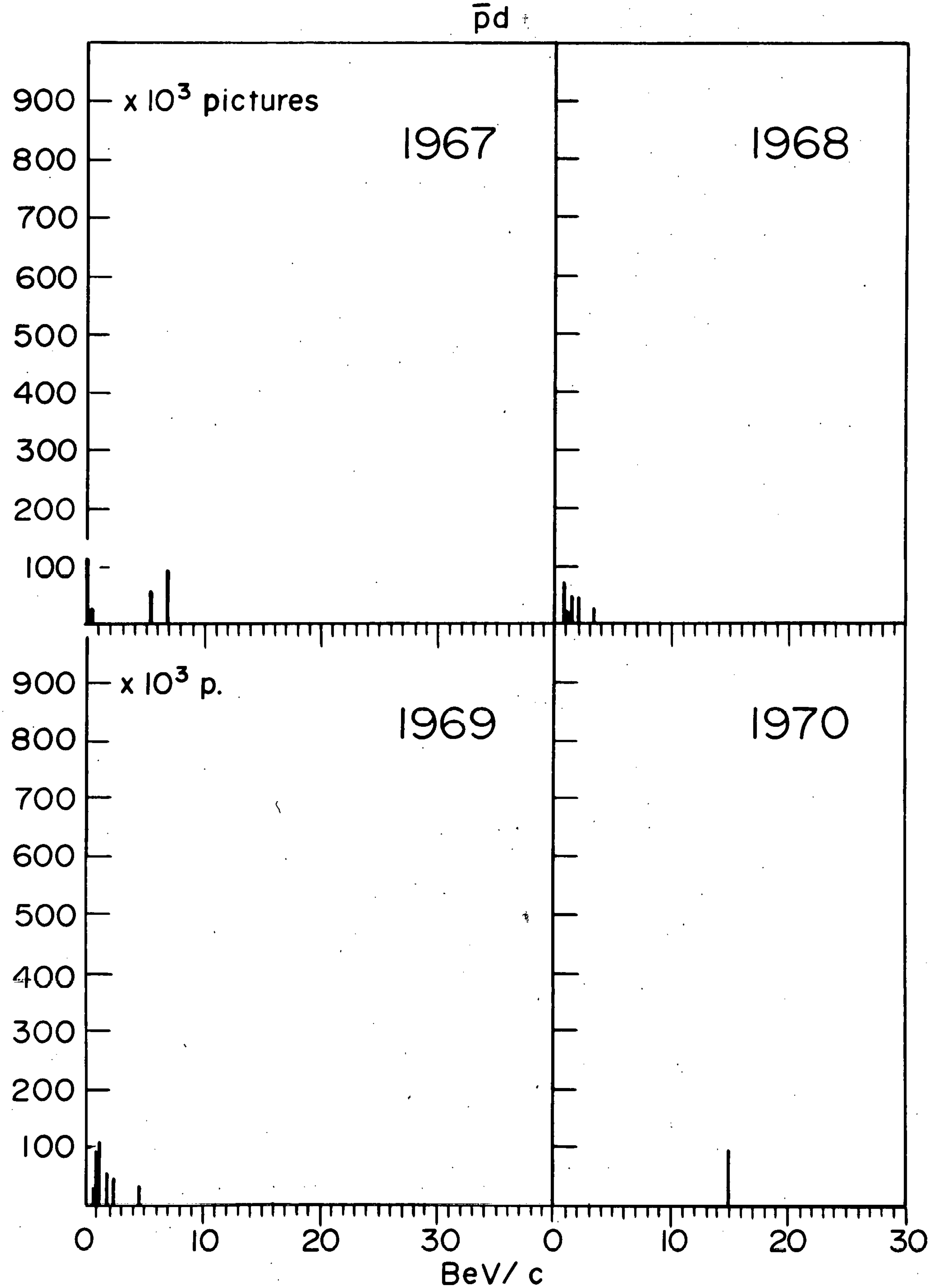

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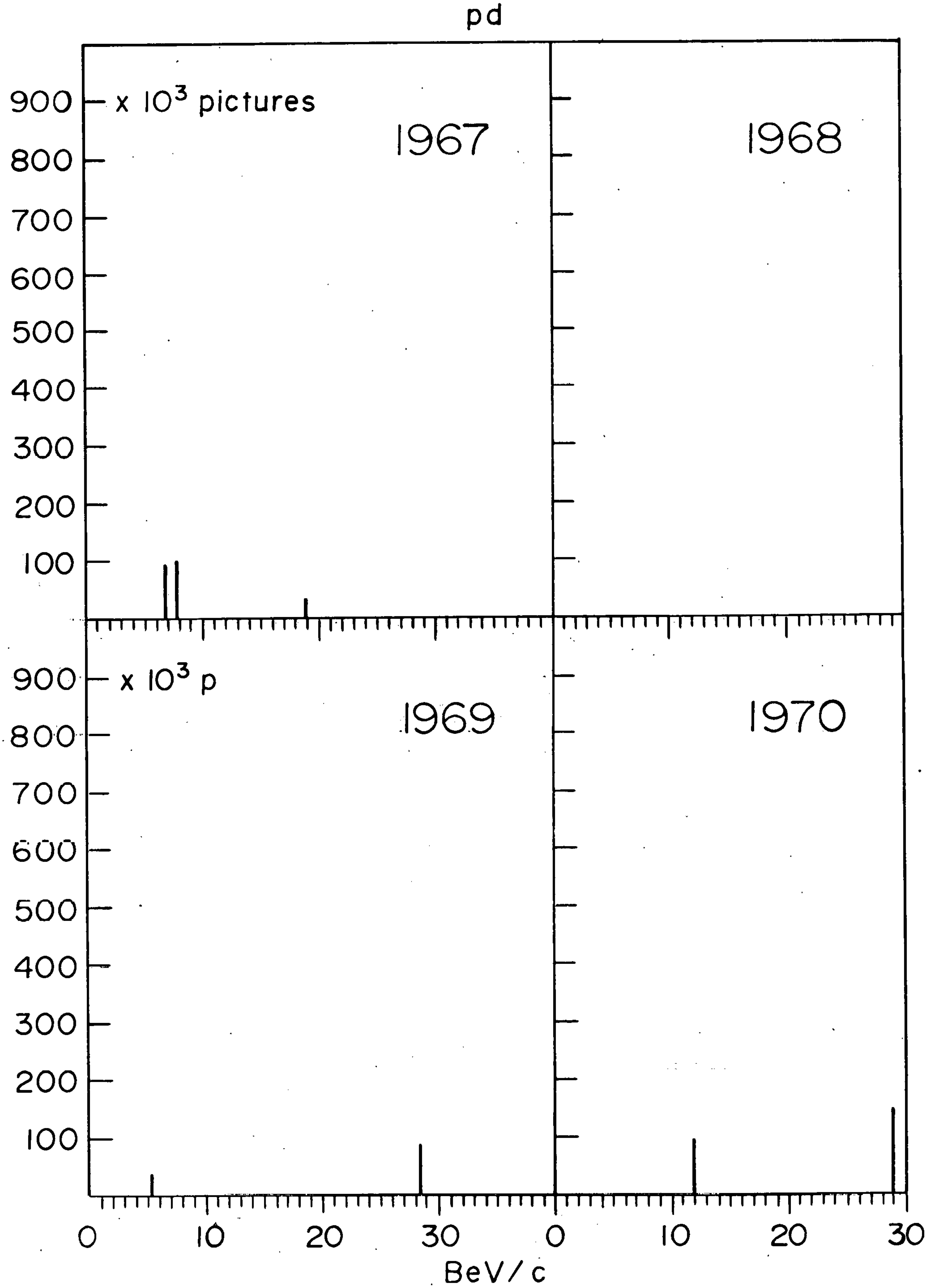

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x 10^3 pictures
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x 10^3 p
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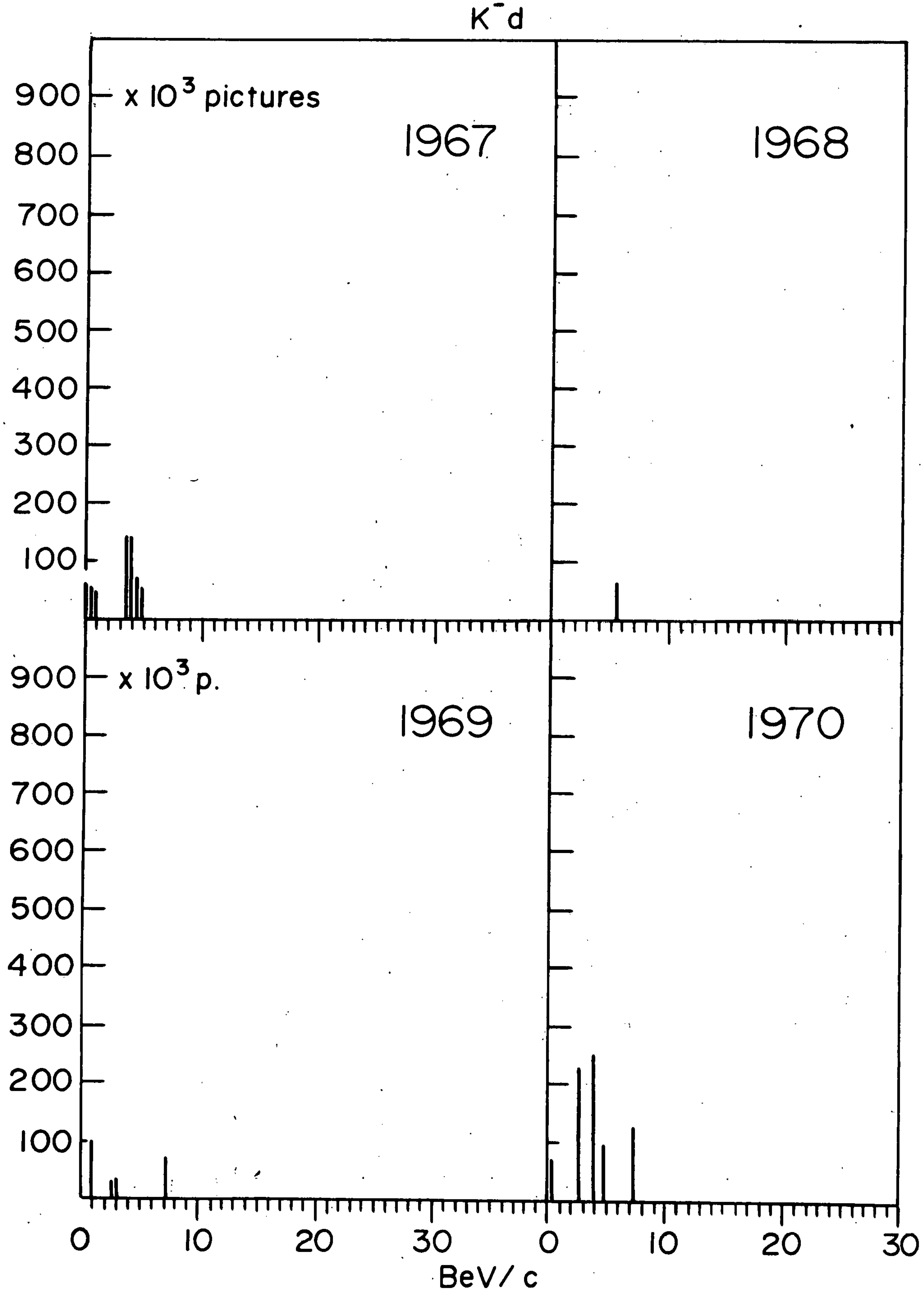
BeV/c



K⁻d
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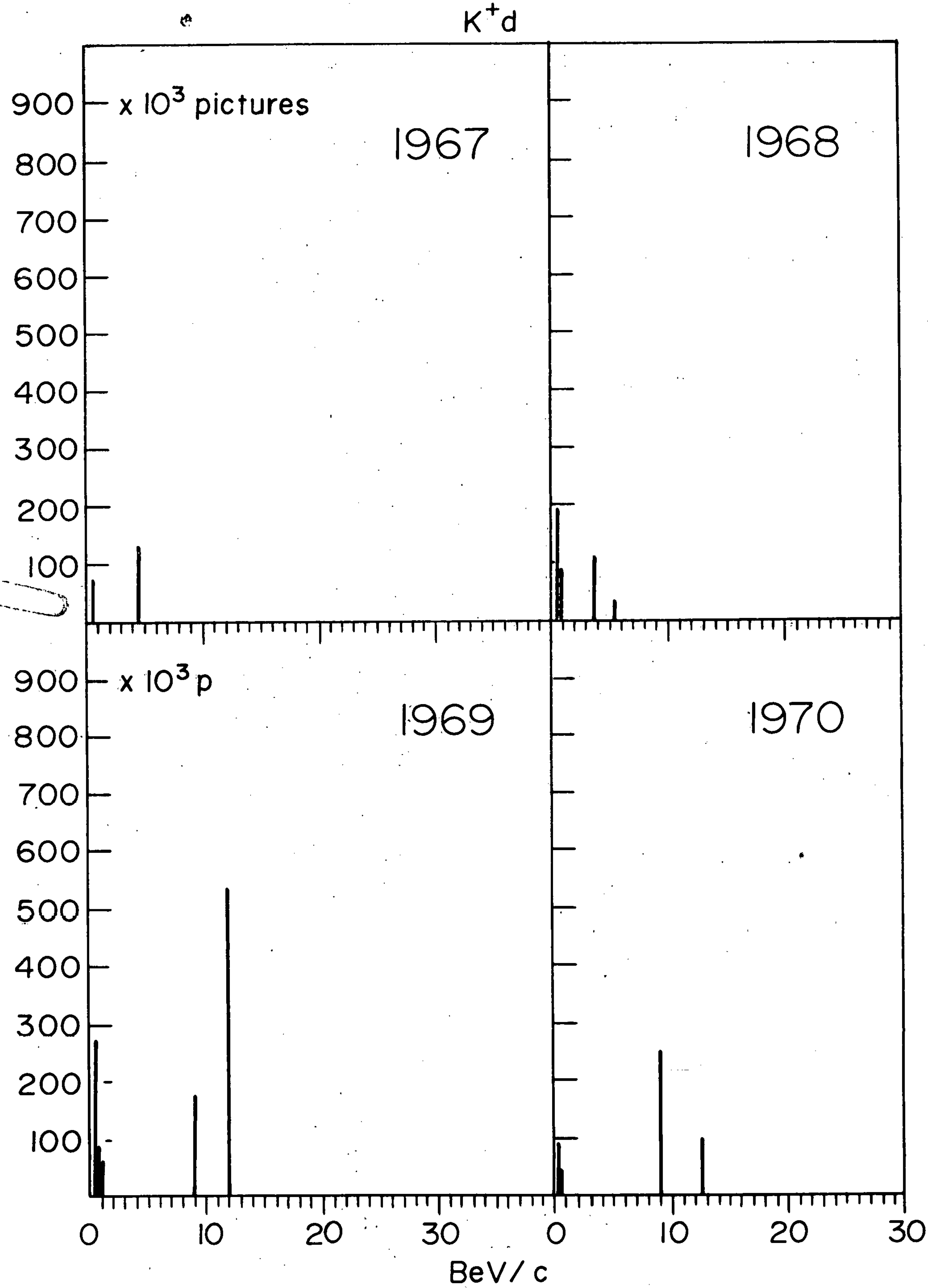
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BeV/ c

7 May 1970 9:30 a.m.

Session IX

Chairman: H. J. Lubatti (Washington)

STATUS OF HUMMINGBIRD FILM DIGITIZERS*

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In this report I would like to review briefly the status of CRT film digitizers at SLAC. I will start with a short description of the hardware, and then summarize our experience to date on three different experiments. I will omit any discussion of the Spiral Reader, although it is also a part of SLAC's automatic data analysis effort.

I. BRIEF DESCRIPTION OF HARDWARE AND COMPUTER CONFIGURATION

A. 360/91 Computer

Our CRT film digitizers are connected online to an IBM 360/91 multiprogrammed computer. The overall computer configuration is shown in Fig. 1. It is obviously a fairly complex system; most of it need not concern us, however, save for the 2250 display scope (which is used for online interaction), two disk drives (used for storage of programs and data) and a high-speed selector channel to which our hardware is connected via a 2701 parallel data adapter.

In its current configuration the 91 is a fairly powerful machine, with the throughput of roughly 2 CDC 6600's. It is a multiprogrammed machine, usually processing half a dozen jobs at once: a mixture of batch and express jobs, terminal programs and one or more "online" programs. Typically up to a thousand separate

*Work supported by the U. S. Atomic Energy Commission.

jobs are processed each day. The facility is open for normal operations from about 10:00 am to 3:30 am Monday morning to early Saturday morning.

A recent development in the operation of the facility, whose impact upon automatic data analysis has not yet been evaluated, is the rationing of computer use. For a number of months users have had an accounting of their running on the 91. Use is measured in so-called "computer units", determined by a fairly elaborate algorithm based on one's use of core, CPU cycles, I/O accesses, and so forth. One month ago users were restricted to using only a predetermined number of computer units per quarter. To the extent that demand exceeds supply, this is going to provide quite an impetus toward efficient programming.

B. Hummingbird II

The Hummingbird II is by now rather an elderly CRT flying spot digitizer. It uses a 7" Ferranti 7/29AO cathode ray tube to generate a 65 × 105 mm raster using essentially 1:1 optics. The deflection and focusing coils are manufactured by Celco, while the analog electronics are homebuilt. The digitial logic is based on DEC cards, although as we shall see later, it is being converted to IC logic. The film transport is rudimentary, using a stepping motor to drive 70 mm perforated single strip film.

The raster is composed of 4096 least counts. The least count on the film in the X direction (along the line) is $\sim 4.7\,\mu$, while in the Y direction it is 25μ. Only static pincushion correction is used, so the raster is fairly noticeably distorted. At the start of each line a y-coordinate is read out to the channel; if the spot crosses a black mark on the film, the x coordinate of the center is read out. The center coordinate is determined by delaying the PM pulse and detecting the crossing point, provided the signal exceeds a certain threshold. No pulse-height or width information is supplied. Thus the output to the computer consists of a string of two-byte words as follows:

$$\ldots O, Y_j, X_{j,1}, X_{j,2} \ldots X_{j,n}, O, Y_{j+1}, X_{j+1,1} \ldots X_{j+1,n}, O, Y_{j+2}, \ldots$$

The zeroes are used to identify the following half word as a y coordinate.

The scanner is capable of executing a fairly limited repertoire of commands from the computer:

(1) raster-scan an area beginning at Y_i and ending at Y_f, with the PM gated on between X_i and X_f;

(2) select a line density of every line, every other line, or every fourth line;

(3) set the PM threshold to one of sixteen values;

(4) advance (or back up) the film up to 20 frames, in increments of 1% of a full frame advance.

C. Hummingbird III and TV Display

Hummingbird III is very similar in overall design to HB II. It uses a 9" Litton L-4192 pentode CRT with a P24 phosphor to generate a spot which is imaged with custom made 1:1 Zeiss optics onto a film platen — field lens — photomultiplier unit. The film drive is designed to handle 3-strip perforated 35 mm film. It contains a pneumatically driven carriage assembly which moves up and down in a vertical plane to position the appropriate view (or a calibration pattern) over the fixed vacuum platen. Celco focusing and deflection coils are used, driven by electronics based on Beta Instrument Company circuits. Digital logic is made from IC's. The logic design is such that it will eventually link both HB's to the 91 channel.

HB3 was designed for use with bubble chamber or streamer chamber film, which can produce upwards of 50K digitizings per frame. Since the IBM 2250 display scope can only hold about 1300 points in its buffer, a different type of display would be desirable. We have constructed a digital TV display using a conventional industrial TV monitor with a 512 × 512 raster. The picture is stored on a fixed-head disk (manufactured by Data-Disc) which refreshes the interlaced image every thirtieth of a second, as a normal TV set does. A lightpen is attached which stores its recorded data on a separate set of tracks. Also available are a separate set of tracks for display of points with enhanced brightness. A program function keyboard is also included. This TV scope is capable of displaying 100K points without flicker. There is a fairly high software overhead in converting a FORTRAN array of points, vectors, or characters into the appropriate bit string for storage on the disk.

II. μ-p SPARK CHAMBER EXPERIMENT

Our first experiment, completed last December, was a spark chamber experiment designed to study μ-p elastic and inelastic scattering, to see if the muon and electron exhibited any differences in this respect. A sketch of the experimental layout is shown in Fig. 2. The muons, after scattering from the target, passed through two spark chambers, a 54" momentum-analyzing magnet, two more conventional chambers, and then four more chambers interspersed with absorber to distinguish muons from pions. 90° stereo views of all eight chambers were taken. In addition there was a proton recoil chamber mounted underneath the target which we did not use in our analysis. There were a total of 10 fiducial marks (each in the shape of a V), and a data box containing a BCD representation of the roll and frame number.

The overall program design changed somewhat as we moved from the interim 360/75 to our current 360/91 computer. In the final version the program was a single package occupying 300K bytes of core. The program drove the scanner in a buffered manner, i.e., while the current frame was being processed, the next frame was being digitized. Since only one frame in three contained a real event, the film had been rapidly prescanned by human operators. The first processing of a frame (typically containing 4000 digitizings or "hits") consisted of stringing "hits" together into small straight line segments (called "blobs"). The 400 or so "blobs" which resulted represented fragments of tracks, fiducial marks, data box bits, scratches, etc. The fiducial marks and data box were then found and checks made for fiducial separation, data box parity errors, etc. Next the remaining "blobs" were sorted into the expected chamber locations and connected where possible to form "segments," i.e., images of a single track in a particular view of a particular chamber. Then segments were joined to form complete trajectories, after making allowances for displacements and rotations caused by the spark chamber optical system.

If the program could not come up with a single unambiguous "goldplated" event, matching the description on the input scan card, then the program halted for operator intervention at the 2250 display scope; this was the case on 75% of the frames, so the data analysis was scarcely "automatic." The operator could link "blobs" into "segments" or "segments" into tracks using the light pen. With this manual intervention, the program could process events at the rate of 60-100 per hour.

In the course of processing some 125K frames on this experiment from March 1968 to December 1969, we came by several hard-learned lessons.

a. The program design, which started of course before the experiment was run, naively assumed that the pictures would be "perfect." We didn't allow for the fact that half the fiducial marks (made from electroluminescent strips) would burn out in the course of the experiment, as would a number of the bits in the data box. We didn't make allowances for "ghost" tracks caused by reflections in the Lucite walls of the chambers, and we had trouble as well with variation of spark intensity as a function of the number of tracks in a chamber. Occasional low chamber efficiency also caused us to miss tracks.

Some of these problems can be circumvented. For example, you can cover the Lucite walls with black paper to cut down reflections, but you have to think of it before you take the pictures, not afterwards. Some problems can be dealt with in the scanner hardware (e.g., better track-center circuits which work over a wider range of image contrast), and some can be overcome in software (e.g., better track-finding algorithms that don't assume effectively that every gap in every chamber will fire). It does seem to be a fact of life that you don't learn these lessons from reading about them, but only by having them happen to you.

b. A second problem which we generated for ourselves was to try to cover up failures in the scanner hardware with software "fixes." The particular problem we had was quite complex and difficult to explain, and would probably not be of general interest. It had to do with the way in which we calibrate the scanner, which is to scan a pattern of 54 crosses whose center positions are accurately known in a rectilinear coordinate system. A fifth-degree polynomial is used to transform HB coordinates into true film coordinates, with the transformation coefficients being determined from a scan of the cross pattern. What happened to us was that, due to gradual misalignment, the spot size in the corners of the raster got so large that the crosses in that area weren't properly digitized. The calibration routines then omitted them from the fitting procedure used to find the transformation coefficients. If the various distortions (such as pincushion) are large, however, (and they are since we don't use dynamic distortion corrections) then the transformation coefficients are very sensitive to the presence or absence of these corner crosses. In our output this would manifest itself as small shifts in angles, for example, compared to hand measurements, and these shifts would vary with time, depending upon how many crosses in the calibration pattern had been well digitized. The proper solution, which we finally adopted was to stop and realign and tune up the scanner, rather than try futilely to remedy the problem with software changes.

c. A third lesson we learned, as has everyone else before us, is the importance of having physics analysis programs ready before vast numbers of measurements are accumulated. In our case this meant having a well understood and debugged geometry program for fitting an overconstrained trajectory through the magnet, based on track measurements in the various chambers. Such a program wasn't available for us until we had measured the majority of the film, at which time it uncovered the problems referred to in the previous section.

As a result of all these problems, our first experiment was a mixed success. Production went in fits and starts as problems were uncovered. In the end it turned out that most (80-85%) of the events in the experiment were not muons scattered from the target, and HB measurements were trusted when they indicated this. If the event appeared to come from the target, however, it was remeasured by hand, since that was considerably more straightforward than trying to understand the milliradian systematic errors present in HB output.

III. COSMIC RAY SPARK CHAMBER EXPERIMENT

A second experiment in which we are currently involved is a cosmic ray spark chamber experiment. This is a collaborative effort between SLAC and LRL. It is designed to measure the momentum and angular spectrum of cosmic ray muons at sea level, and in particular to check the zenith angle distribution of the highest energy muons. About 1.6×10^6 pictures have been taken. A rapid hand scan is being done to pick out the highest energy muons for subsequent hand measurement with the greatest possible precision. The other 98% of the data is to be analyzed on the Hummingbird, where a slightly lower precision is acceptable.

The experimental layout is shown in Fig. 3. The apparatus is by and large the same as was used in the μ-p experiment, but slightly rearranged. There are three chambers for determining the trajectory of the muon before the momentum analyzing magnet, and three after. There is also one chamber inside the magnet. All but one of the chambers have a 90° stereo view. There are 20 V-shaped fiducials and a BCD roll-frame data box. Counter information was recorded at the time of the experiment by a PDP-8 and this data is available for merging with Hummingbird output.

The overall program design is similar to the μ-p experiment. The main difference is that the track-finding algorithms are more global, and don't depend so much on precisely what is happening in a given chamber. There is also a second pass feature in the program whereby if an event isn't found using the "blobs" then one can go back to the original digitizings to see if an event can be found.

The film is much "cleaner" than the μ-p film was, largely as a result of the lessons learned in the latter experiment. (The duty cycle is also better: 100% instead of 0.1%). Consequently our track-finding efficiencies are better. Currently we correctly resolve about 85-90% of the frames. About two-thirds of the remainder have no real events in them at all, while the other third (about 3-5% of the total) contain events of varying degrees of complexity (e.g., showers). The goal in this experiment is to do without manual intervention for event finding, and we are fairly close to achieving this. The processing rate is about 700 frames per hour, limited essentially by the rate at which the Hummingbird can scan and move film. A factor which may limit our overall production rate, however, is the rationing of computer units referred to earlier. Our allocation is such that we may be limited to about 8 hours a day of production rather than the potential 16. An interesting sidelight to this accounting and budgeting problem is that the computer costs for a single frame of cosmic ray film are currently about eight cents.

The biggest single problem remaining in the cosmic ray experiment is the question of the unresolved events. In an experiment of this magnitude and potential statistical precision, the fraction of unresolved events should ideally be about 2%, instead of the current 10-15%. Since the film has not been completely prescanned, the problem is as much one of deciding there is no event as of finding one which is there. It is not clear what strategies will be used to solve this problem.

The second problem is to keep a close watch on potential small systematic distortions caused by the Hummingbird hardware. Since we do have the most important spatial reconstruction programs online, we can monitor our accuracy much better than we could in the μ-p experiment.

IV. STREAMER CHAMBER K_2^O DECAY EXPERIMENT

This experiment, on which we are starting some shakedown runs, is our most ambitious to date. This experiment, a collaboration between SLAC and BNL, is designed to study leptonic K_2^O decays using a streamer chamber as detector. Absorbing plates are put in the chamber to help separate pions, muons and electrons. A sample picture is shown in Fig. 4. This film (3 strip 35 mm perforated) will be digitized on HB III. A small amount of film has been taken on this experiment already, but the bulk of the data will not be collected until June.

The nucleus of the streamer chamber software is CERN's Minimum Guidance program. The film will be prescanned (since only one frame in 5 contains an event) and a rough vertex position recorded on a scan card. The Hummingbird will then scan all three views with a "normal" scan, and an orthogonal scan if so indicated on the scan card. A vertex-finding program then uses the rough vertex position to find a precision (100 μ on the film) vertex which serves as input to the Minimum Guidance program. The MG program then follows the decay products emanating from the vertex (into the orthogonal scan if necessary) until the tracks reach the absorbing plates. A "pseudo-vertex" is then constructed on the exit side of the plates to follow the tracks as they emerge (if they do) from the other side. The pieces of track found by the MG program in the three views are then edited, labelled, and checked for topological consistency. If it appears that an event matching the description on the scan card has been found, then the measurements will be given to SYBIL, a three-view geometry program similar in purpose to TVGP but adapted to the peculiarities of the streamer chamber.

If at any point along the chain the program experiences difficulty then manual intervention from the TV scope is called for. A sample display (used for debugging, not production) is shown in Fig. 5. The operator can then erase irrelevant digitizings, link track segments, indicate vertices, etc., using the light pen and program function keyboard.

The overall program is designed to function asynchronously, i.e., the scanner fills up disk storage with digitized frames for processing, the vertex finding routine accumulates vertices, the MG program finds tracks, and messages and pictures for display are stored, all more or less independently. The idea is to avoid a strict sequential "bucket brigade" operation, and rather to have all pieces of the program working on their own input queues. Of course eventually some one part (e.g., the scope operator) becomes a bottleneck, but this asynchronous design keeps him continuously busy.

We have so little experience to date that there is nothing but bad news to report.

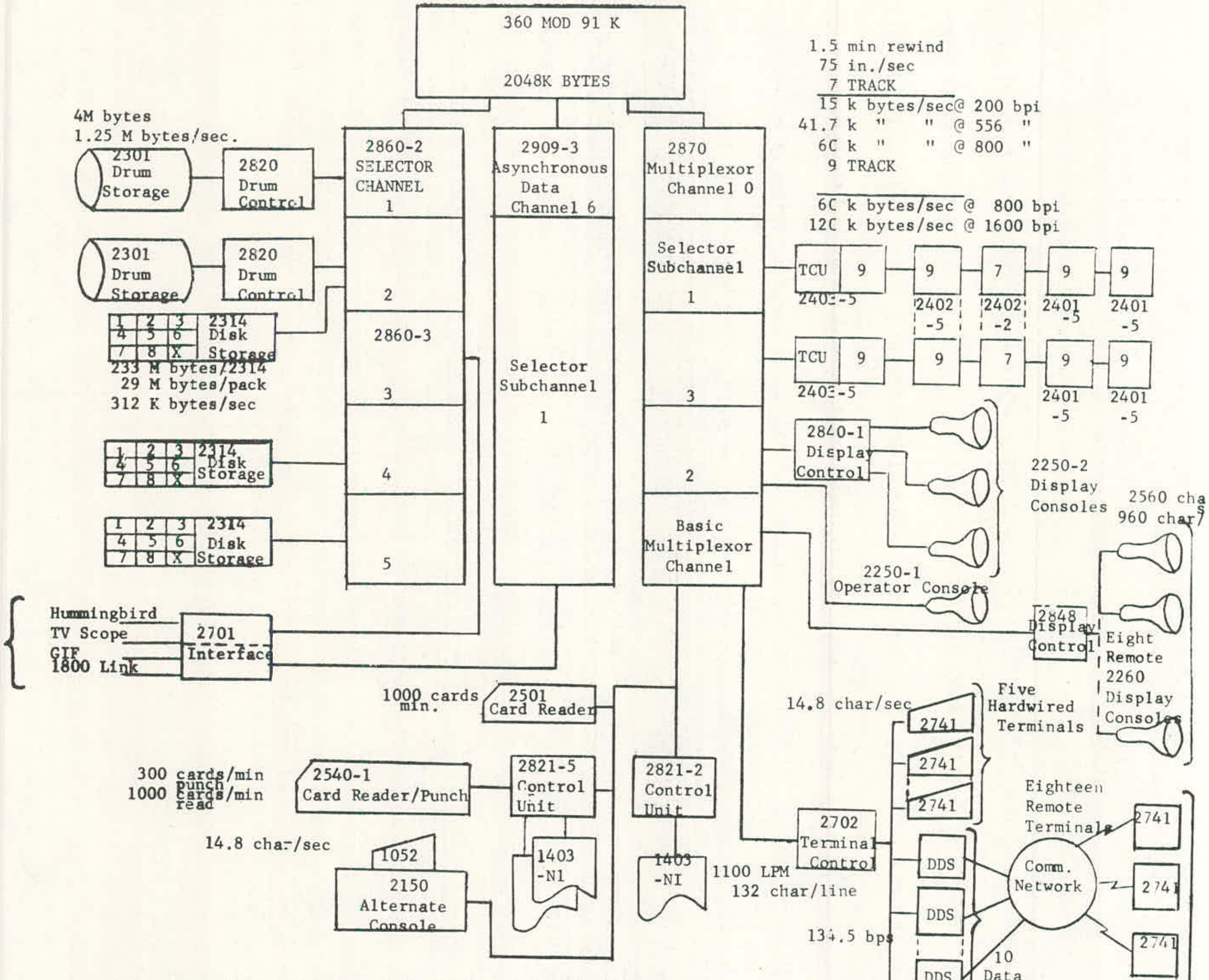
A. We are experiencing a great deal of difficulty with HB3 hardware. Two critical problems are noise in the IC digital logic, and stability in the focusing and deflection circuits. The track center circuit is also undergoing considerable rework. Although HB3 is similar to HB2 in overall specifications and capabilities, the actual detail design is almost completely different, so there remains considerable debugging to be done.

B. The film we have, while better than early streamer chamber film, still is far from optimum. There is still great variation in track contrast and width, as well as large flares which can obscure considerable portions of an event. It is not clear at present whether streamer chamber film in its present state is actually amenable to automatic data analysis.

C. The list of software problems is almost endless. The total program is very large, involving some 200 subroutines occupying more than on megabyte of storage. Since we are only allowed 300K of core storage, this means a great deal of overlaying both of instructions and data. Consequently there is a great deal of channel activity between disk and core, and we are currently trying to sort out this channel traffic. While the vertex finding program works well (97%) on a small sample of events, we will undoubtedly run into problems when we try to go to a larger less selected sample. The same is true of the MG program; it finds about two-thirds of the tracks in a small sample but there still remains considerable tuning of program parameters. The editing program has been only partially checked out, and no events have yet been input to SYBIL. Thus, while there has been reasonable success with individual program components, overall system checkout has not been attempted and considerable problems can be expected.

V. FUTURE PLANS

Perhaps the most accurate statement is that we are so busy with present problems we haven't had any time to work on future plans. At the moment there are no intentions of expanding or improving our hardware capabilities in any significant way. There is a specific proposal for a spark chamber experiment on electroproduction of hadrons to take place in about a year, but no serious programming work has yet been done. A rather massive streamer chamber exposure to a high energy K^- beam is also planned for the beginning of 1971, and if we have reasonable success on the K_2 experiment we can expect a large amount of work in analyzing this next experiment. And, of course as an ex-bubble-chamber-physicist, I have a personal interest in adapting our streamer chamber program to bubble chamber experiments. Unfortunately, in my current role as a bureaucrat faced with what he considers an inadequate budget, my main problem at the moment is to figure out how to do more work than we can handle with fewer resources than we need.



(Revised 12/10/69) SLAC CENTRAL COMPUTER, IBM 360/91 - DECEMBER 1969

Fig. 1

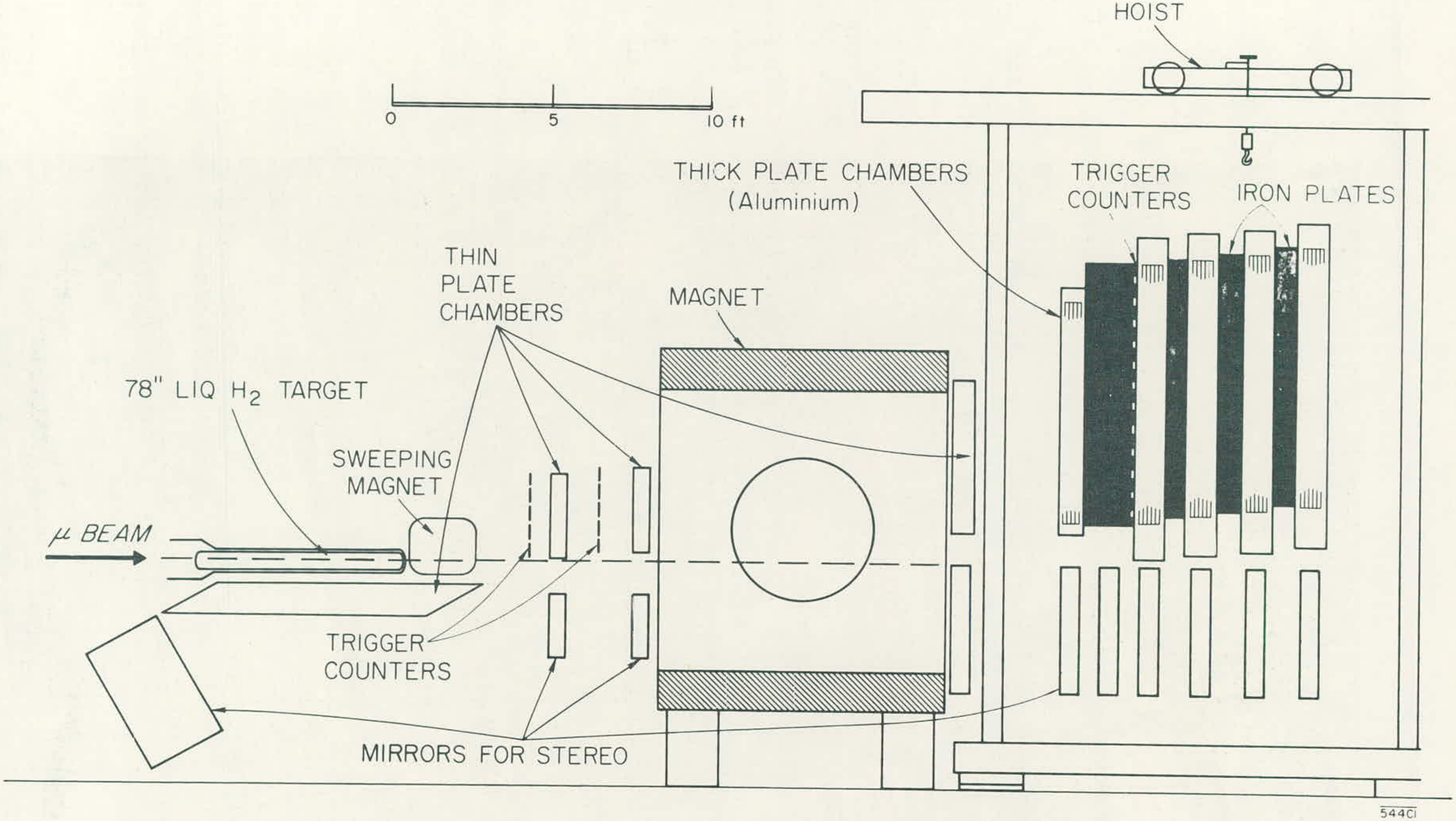

HOIST
0
5
10 ft
THICK PLATE CHAMBERS
(Aluminium)
TRIGGER
COUNTERS
IRON PLATES
THIN
PLATE
CHAMBERS
MAGNET
78" LIQ H2 TARGET
SWEEPING
MAGNET
μ BEAM
TRIGGER
COUNTERS
MIRRORS FOR STEREO
544C1

Fig. 2

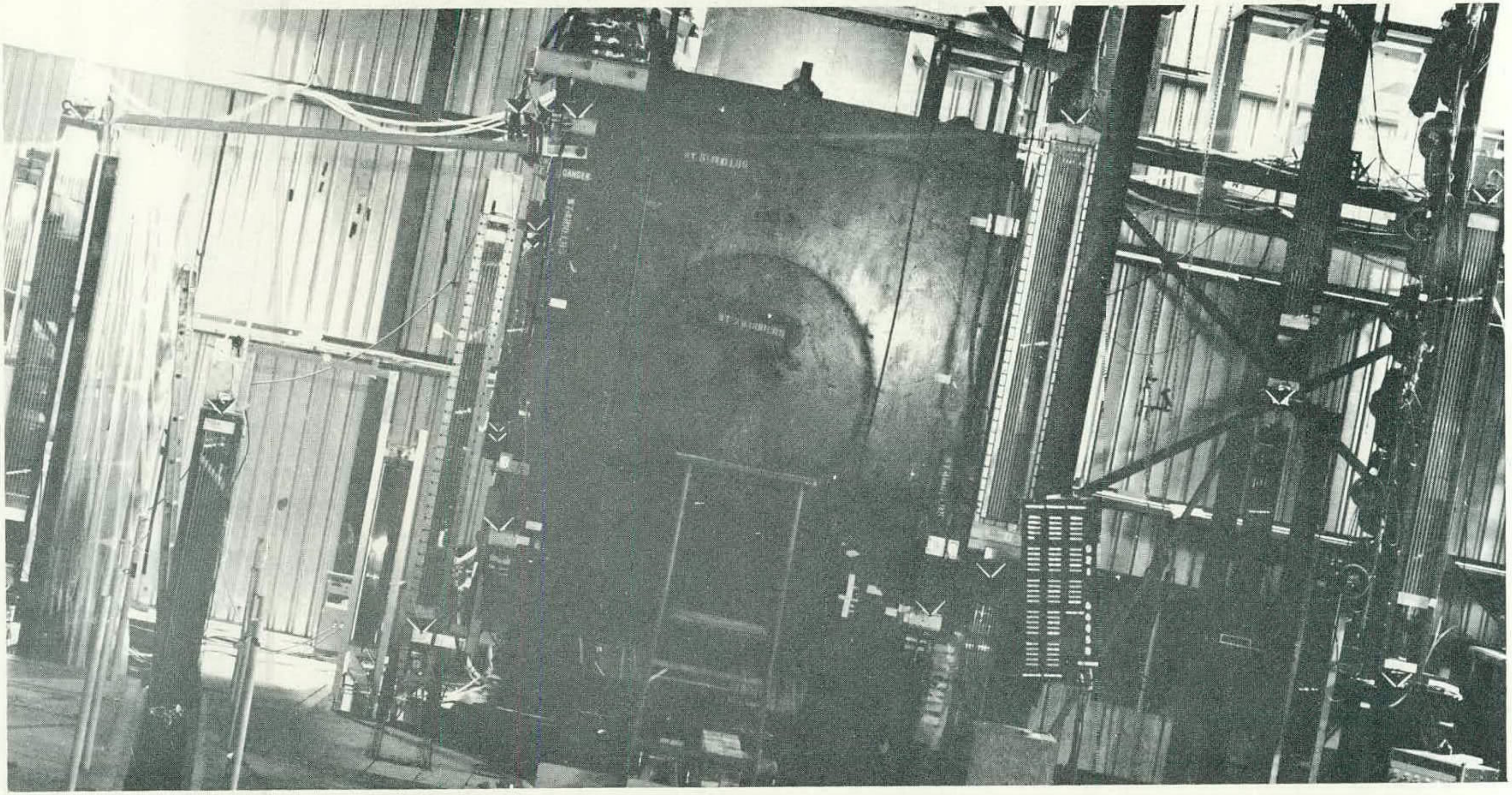


Fig. 3

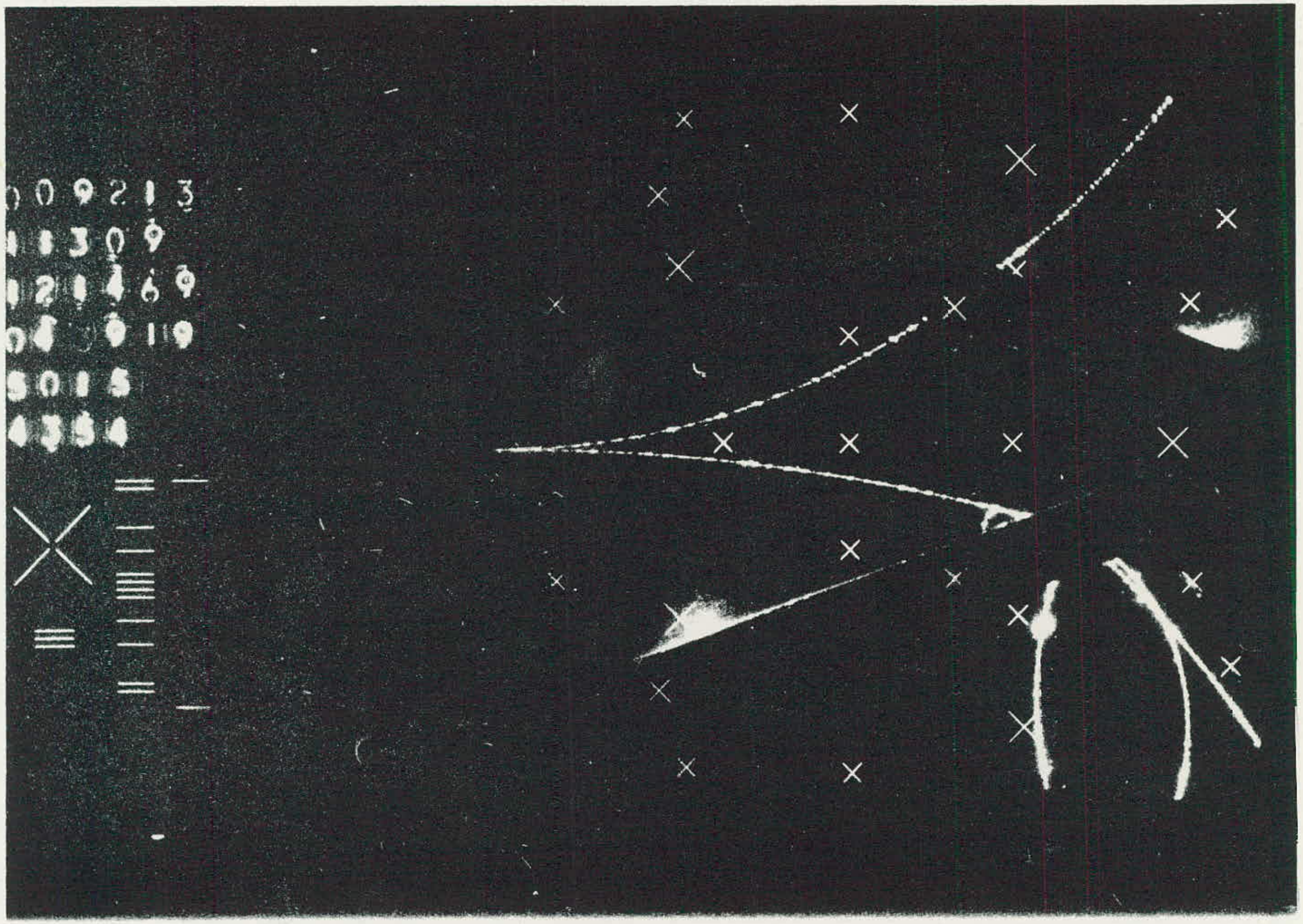
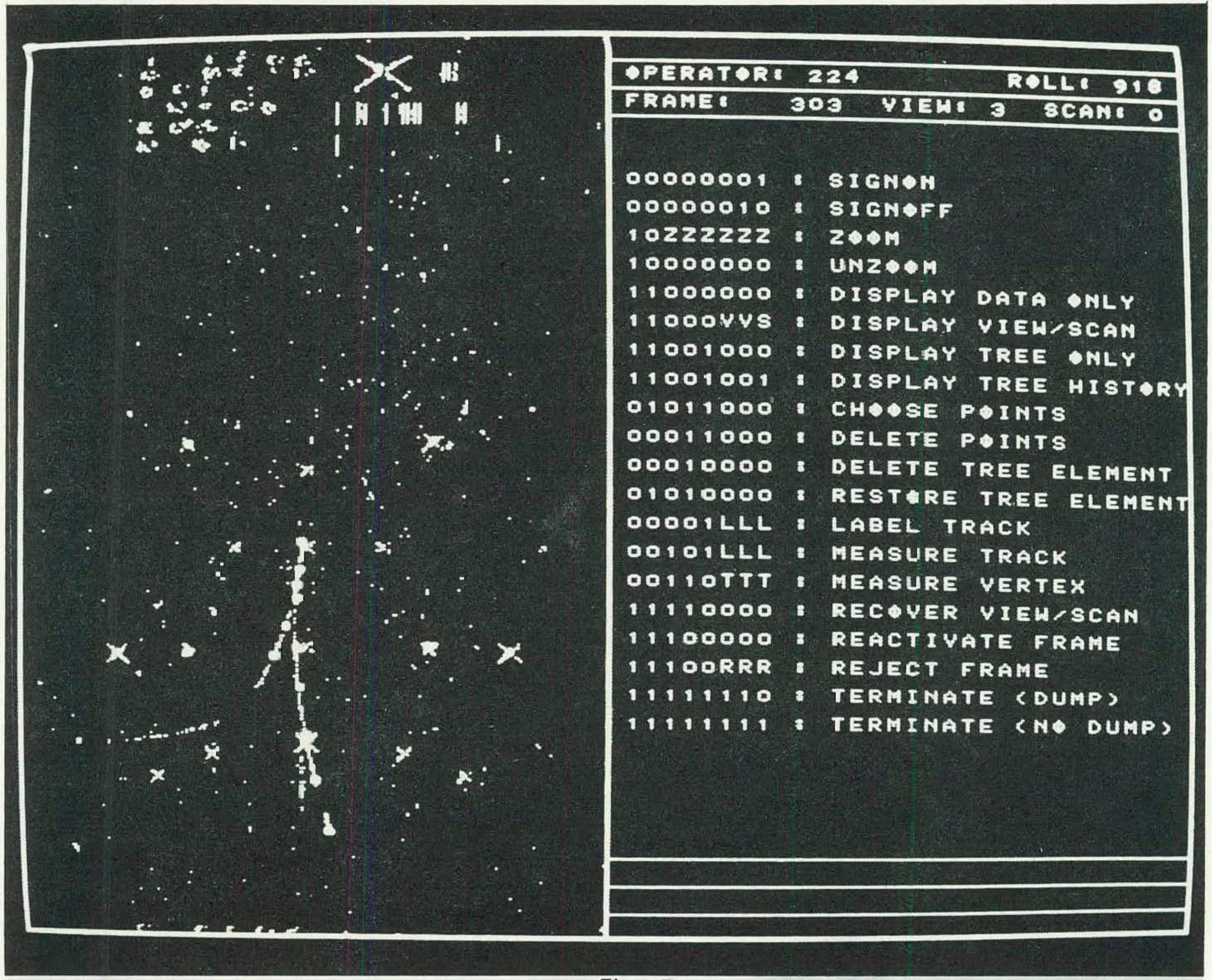


Fig. 4



OPERATOR: 224
ROLL: 918
FRAME: 303 VIEW: 3 SCAN: 0
00000001 : SIGNON
00000010 : SIGNOFF
10ZZZZZZ : ZOOM
10000000 : UNZOOM
11000000 : DISPLAY DATA ONLY
11000VVS : DISPLAY VIEW/SCAN
11001000 : DISPLAY TREE ONLY
11001001 : DISPLAY TREE HISTORY
01011000 : CHOOSE POINTS
00011000 : DELETE POINTS
00010000 : DELETE TREE ELEMENT
01010000 : RESTORE TREE ELEMENT
00001LLL : LABEL TRACK
00101LLL : MEASURE TRACK
00110TTT : MEASURE VERTEX
11110000 : RECOVER VIEW/SCAN
11100000 : REACTIVATE FRAME
11100RRR : REJECT FRAME
11111110 : TERMINATE (DUMP)
11111111 : TERMINATE (NO DUMP)

Fig. 5

DISCUSSION

BASTIEN: (Washington) I want to point out that if you apply the global method that I described the other day to the film that you showed, it's really very easy to get the tracks.

BROWN: (SLAC) Yes, your global method depends on a couple of things which are I think of the streamer chamber film are more likely true in fact than a bubble chamber. An important one is the tracks are circular and the ionization loss in the streamer chamber is not what you'd call overwhelming.

HULSIZER: (MIT) I wonder if you'd care to comment on the perils, problems, advantages and disadvantages of trying to a) develop and b) operate on line to a large computer with a device that requires a fairly frequent access.

BROWN: (SLAC) I think we've probably had somewhat better experience than some of the other people who are here in working on a large multi-program machine such as this. There is no question that if I had my choice I'd rather have my own machine myself, but, I think it's also true that it would probably cost more than taking some chunk of the 91. The 91 is a really fairly inexpensive machine and as I mentioned earlier, the sort of computer costs for the cosmic ray film where we actually do a fair amount of arithmetic is 8 cents a frame, or another way of putting it, we probably spend nearly 60 or 70 dollars an hour for computer time on the 91, that's a fair bargain. You can even put it another way if you want to sign up for computer time on the 91 on the weekend it'll only cost like about $30/hour. So the 91 is a fair bargain and if you can sort of back through the bureaucratic problems of working on a machine with a whole lot of other people and making sure the system doesn't change underneath you, it's not too bad. I agree, if I had my way I'd rather have my own, but I find this not an intolerable situation.

HULSIZER: (MIT) One of the questions I wonder about is, are you in core all the time so that you can have immediate access to your program?

BROWN: (SLAC) That's right. While we're running, we're core resident all the time, and we use about 20% of the total core that's available to the user. Because of the balance of jobs inside the machine we manage with between 5 and 10% of the CPU cycles while we're in there. Again because we sort of sit there all day long, we have been barred from using things like tape drives and line printers, but we do have two disk packs in which we can store our answers and our and our programs and our data and so on. If you invest a certain amount of work in this, you can save yourself, since you expect to have a program which runs all the time and you can check-point yourself so that when the system crashes, which a multi-program machine will do, you can pick up from where you left off with essentially no loss.

HULSIZER: (MIT) When you talk about dollar figures, like $30/hour is that for your partition.

BROWN: (SLAC) That's simply what it costs SLAC to run the 91 in a rather low key operation over the weekend. Perhaps a more realistic number is the operating costs of the machine alone, not the costs of paying for the buying of the computer. They are about $100,000/month for the whole computer which typically runs about 500 hours in a month or a little bit more. So the whole 91 is probably $200/hour and it usually runs 5 or 6 jobs and we use between 10 and 20% of that.

A Branch Off The PEPR Tree

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1. INTRODUCTION

A new automatic measuring device for bubble chamber film has been developed which incorporates features of the PEPR[1,2] and POLLY[3] systems. This device, called RIPPLE, is built around a precision cathode ray tube (CRT) and uses a portion of the standard PEPR package developed at MIT and manufactured by Astrodata, Inc. The line element generation portion of the package has been eliminated from this system; however, and the RIPPLE uses a flying spot as its digitizing element. The digital controller for this device has been newly developed and combines some of the best features of the previous devices. A block diagram of the system is shown in Figure 1.

Before beginning a description of the RIPPLE system, it might be useful to define the functions that an "ideal" controller might perform. It is important in this respect to recognize the practical limitations of available computers, in particular their calculation times (5 to 10 μs per arithmetic operation). An "ideal" controller would obviously remove as much routine calculational burden as possible from the computer. The flying line element of the PEPR is such a feature; it recognizes line elements without the computer having to handle each individual bubble many times in different combinations. However, the line element is not an "ideal" precision encoding element. It is basically a summing device for bubbles along its length; and as such it can be confused in regions of crossing tracks, nearby tracks,

Work supported by the National Science Foundation, the U. S. Atomic Energy Commission, and Duke University.

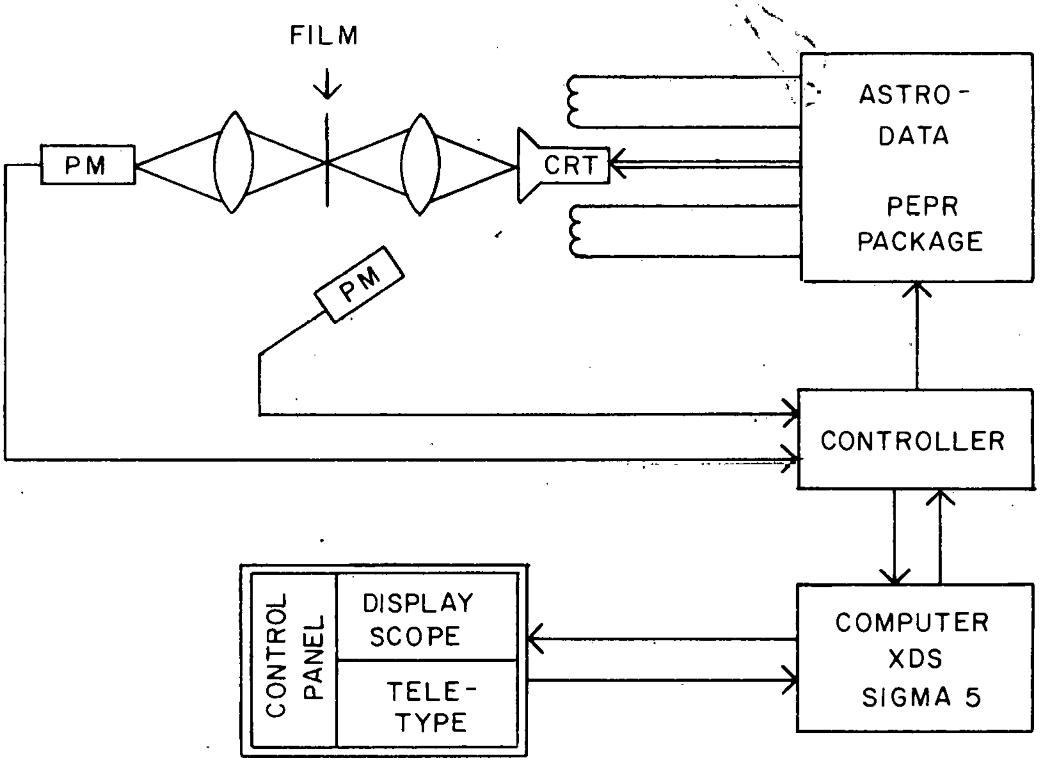


Fig. 1

Block Diagram of the RIPPLE system

etc., since the sum can then contain a few incorrect bubbles. Therefore, the "ideal" device would first detect a particularly outstanding sum composed of many bubbles at some position and angle but would then transfer to the computer the coordinates of each bubble used to obtain the sum. The computer could then use more elaborate methods to eliminate the few offending bubbles and base its resulting master point calculations only on the best information. If such a device were built around a flying spot sweep it would certainly require significantly more controller logic than is currently used.

The RIPPLE was developed with this ideal in mind, and represents a small step in that direction.

2. SWEEP

The primary task of any automatic bubble chamber film measuring device is track following. Currently most methods of scanning for events also involve following beam tracks until a vertex is found. In many ways, the ideal coordinate system for this task is a polar coordinate system centered on the track being followed. In the RIPPLE this polar system is generated by sweeping the CRT spot in a series of small concentric circles of decreasing radii. The circles have a maximum diameter of about 6 mm and can be centered anywhere on a 32000 x 32000 point grid on the CRT face. The coordinate system formed by the sweep is of particular importance in the hardware line element detection system discussed in Section 3.

A comparison of this polar sweep and the raster sweep of the POLLY is shown in Figure 2. If the polar sweep is centered at the intersection point it will "see" each crossing track as a group of bubbles near a particular angle. The corresponding property of the raster sweep is the ability to see parallel tracks when oriented with its sweep lines perpendicular to the tracks.

From this it is clear that the coordinate system formed by the raster sweep is optimal for displaying and finding beam tracks, where the angle of the track is known, but the lateral position is not. The polar scan, on the other hand, is optimal at a vertex where the position is known but the angles of the outgoing tracks are not. Although less distinct, the polar scan is better for detecting narrow angle crossing tracks or two super imposed tracks which split apart. If the polar scan is centered on a track, its sweep line will cross the track at approximately 90°. This is an important point with regard to the hit width which will be discussed in Section 4; a crossing angle of 60°, for example produces hits up to 15 percent wider, depending on the bubble distribution along the track.

Figure 3 shows the details of the sweep pattern generated by the RIPPLE controller. The computer obtains the particular pattern desired

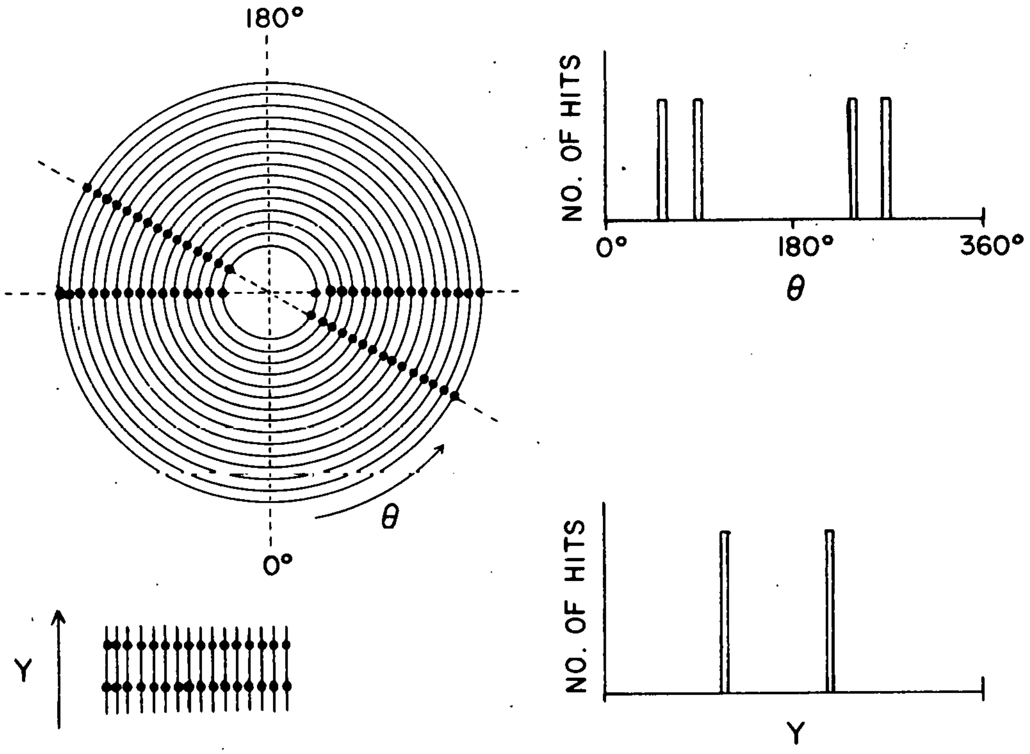


Fig. 2

Comparison of circular sweep of RIPPLE versus the raster sweep of POLLY. With the circles centered on a vertex or interaction, all hits on a track occur near a particular angle, θ, and can therefore be histogrammed on the θ axis to find tracks. The raster sweep has similar properties for parallel tracks.

by presetting: the maximum radius, the minimum radius, the step size between radii; the gate A and gate B opening angles, the common gate width, and the X, Y center of the pattern. The gates, which may be opened to cover a full 360°, only determine which hits will be transferred to the computer. The hit detection logic continues to function over the full 360° enabling the controller to "see" the film over the full sweep pattern. During track following narrow gates would normally be used, and, under these conditions, only hits on or near the track being followed are available to the computer. This, of course, reduces considerably the

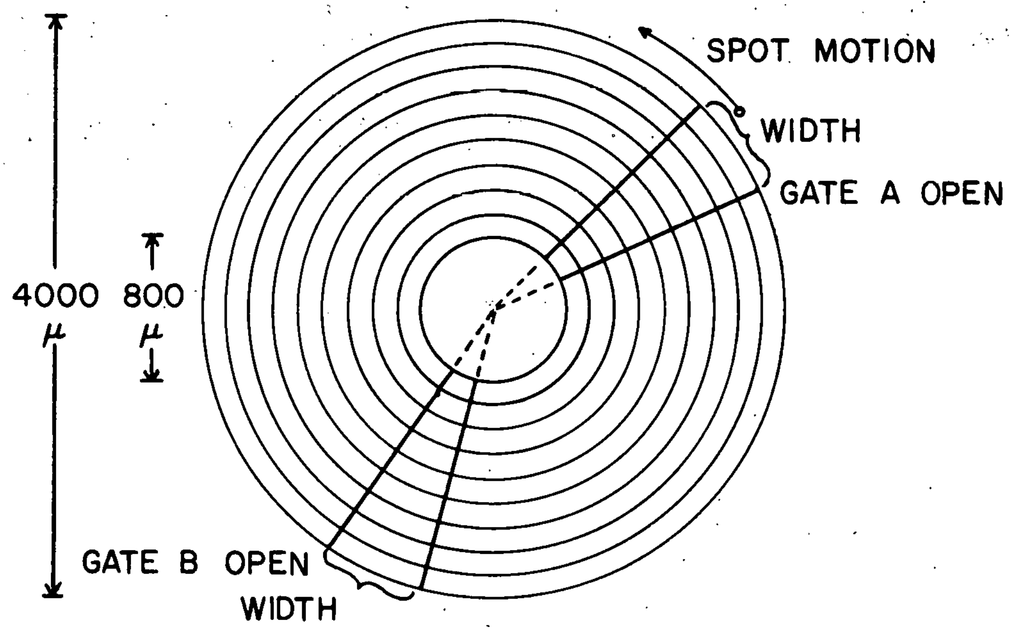


Fig. 3

Sweep pattern of spot around a main deflection point. Dimensions shown are on film after a 3:2 lens reduction. The r, θ position of a hit is transferred to the computer only if the hit occurs within one of the separately adjustable gates shown.

number of computer operations necessary to process the sweep.

The schematic diagram of the sweep generation electronics shown in Figure 4 indicates another pleasant feature of the polar sweep. It may be generated very simply by filtering square waves to obtain only their fundamental frequency. The digital square wave signals are automatically phase locked to the master clock which forms the time base for the hit digitizing system. The sweep signal thus formed is stable and relatively insensitive to fast noise signals.

Using a 10 MHz oscillator to drive the master clock, the angular

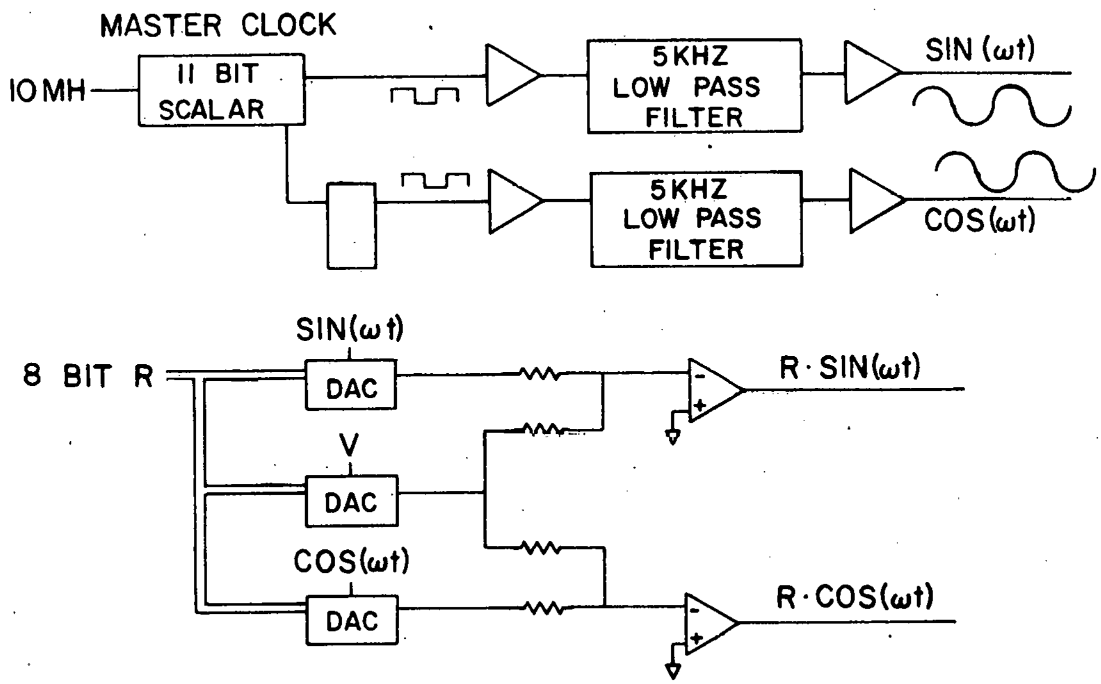


Fig. 4

Schematic diagram of electronics for circular sweep generation. The low pass filters transmit only the fundamental component of the square wave signals. The digital to analog converters multiply the SIN and COS functions by a digital circle radius, R.

least count of the sweep is 100 ns corresponding to 0.176°. A circle radius on the CRT of 2000μ thus requires a spot velocity of $60\mu/\mu s$. We have no difficulty digitizing the film at this velocity, nor do we have difficulty with the four times slower spot velocity needed for the smallest radius. This angular least count of 0.176° corresponds to a 6μ least count at 2000μ radius, 3μ at 1000μ radius, etc.

3. HISTOGRAM FEATURE

In an effort to regain some of the track element recognition features given up when the PEPR flying line was eliminated, an automatic histogramming feature has been incorporated into the controller logic. The feature lacks the versatility of the flying line but appears to have very useful properties. The digital logic involved is capable of accumulating hits in 64 angle bins as shown in the top part of Figure 2. The 64 bins normally span the full 360^{o} as shown and operate independently of the hit gates. The controller accumulates hits over a full sweep pattern, normally composed of several concentric circles. At the end of the sweep pattern the controller scans the histogram bins sequentially and obtains the sum of each pair of adjacent bins. If one of these sums exceeds a value preset by the computer the bin number and bin count of both components of the sum are transferred to the computer. The logic is thus able to count all the hits of a given track, even if the hits are distributed into two adjacent bins. With this feature the computer need handle only a few numbers in order to roughly scan the full 360^{o} pattern.

The digital logic used to accomplish this operation is sketched in Figure 5. The central item is an integrated circuit scratch pad memory unit available with address logic on a standard T-series XDS logic module. The read and write times for the memory are 110 ns and 165 ns respectively. The counting of hits is accomplished by reading the appropriate byte, adding one and restoring the byte. The memory unit is cleared preceding the sweep pattern and read, compared, and transferred to the computer following the sweep pattern.

Examples of the operation of this feature are shown in Figure 6. This six prong event is in a frame of 82" bubble chamber film. Each part of the figure is a single sweep composed of 46 concentric circles with radii varying from 1500μ to 400μ in 24μ steps on the film. The single dot near the center of each sweep is the center of the circles. Each sweep has been positioned manually by observation of the display shown here. Below each sweep is the complete angle histogram generated by that sweep pattern. The

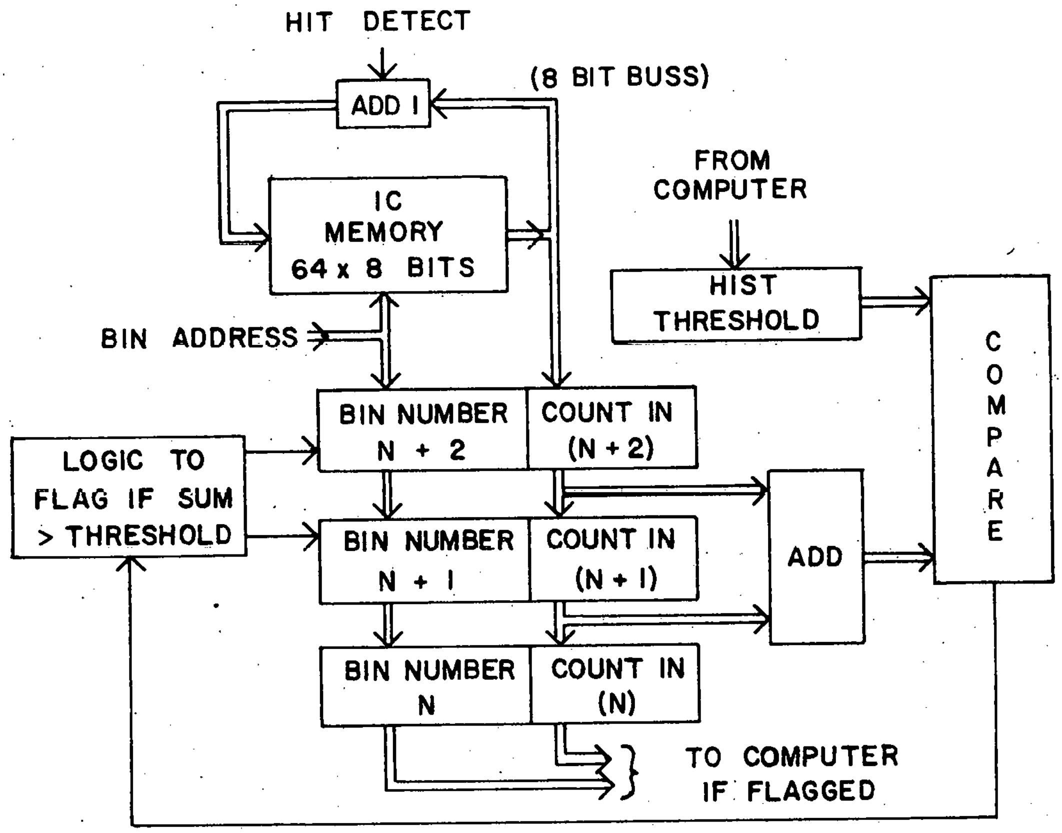
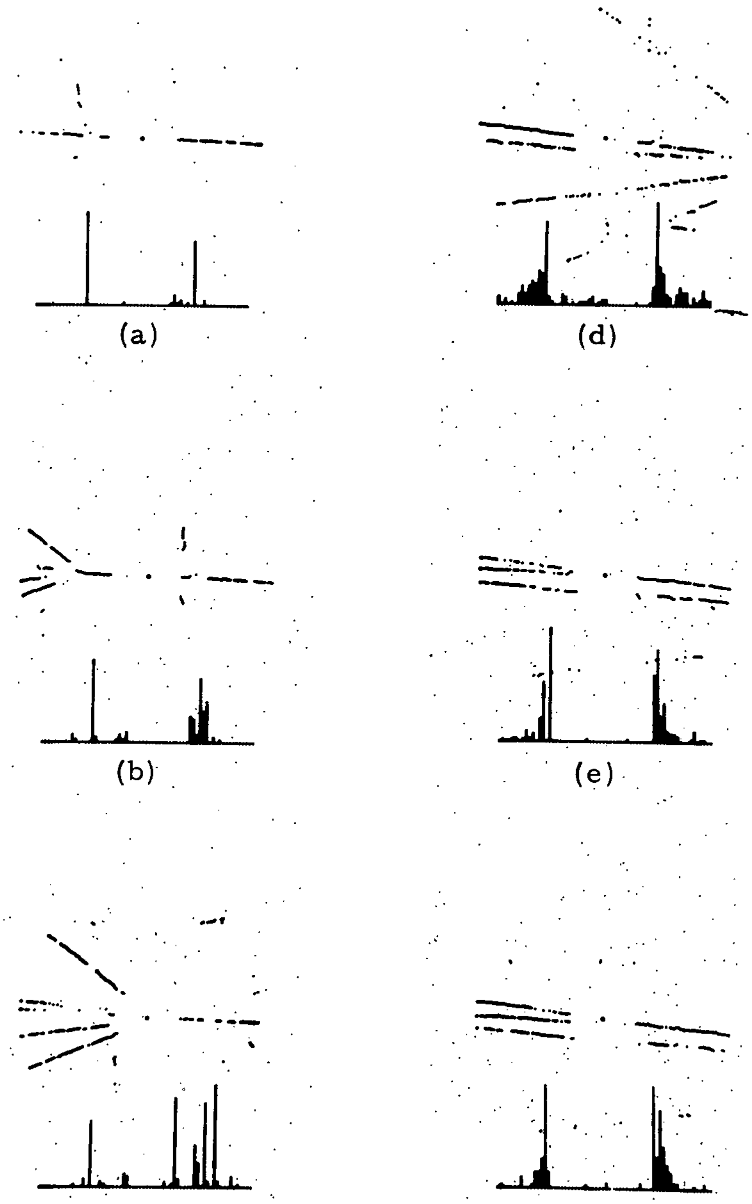


Fig. 5

Block diagram of electronic logic used to count, store, and retrieve the angle histogram information. The memory is automatically scanned following the sweep; whenever the contents of two adjacent bins combine to exceed a computer preset threshold, the bin numbers and contents of both bins are transferred to the computer.

histogram scale runs from 0° to 360° in 64 steps with 0° being toward the bottom of the picture.

Figure 6a shows the beam track for this six prong event. Track elements are clearly seen at angles near 90° (backward direction) and 270° (forward direction). In Figure 6b the histogram indicates several hits at approximately 270° which are caused by the six prong vertex. When positioned near the vertex in Figure 6c the histogram clearly indicates at



(a) (d)

(b) (e)

(c) (f)

Fig. 6

Sample sweeps on high energy π^+p film from the 82" bubble chamber. The pictures show a sequence of sweeps on the beam track of a six prong event and on an outgoing doubled track which splits apart some distance from the production vertex. Each sweep pattern is centered at the isolated dot near the middle of each picture. The histograms below each sweep show the number of hits as a function of angle, θ, measured counter clockwise from a 6 o'clock position. The scale of each sweep is approximately 3 mm on the film.

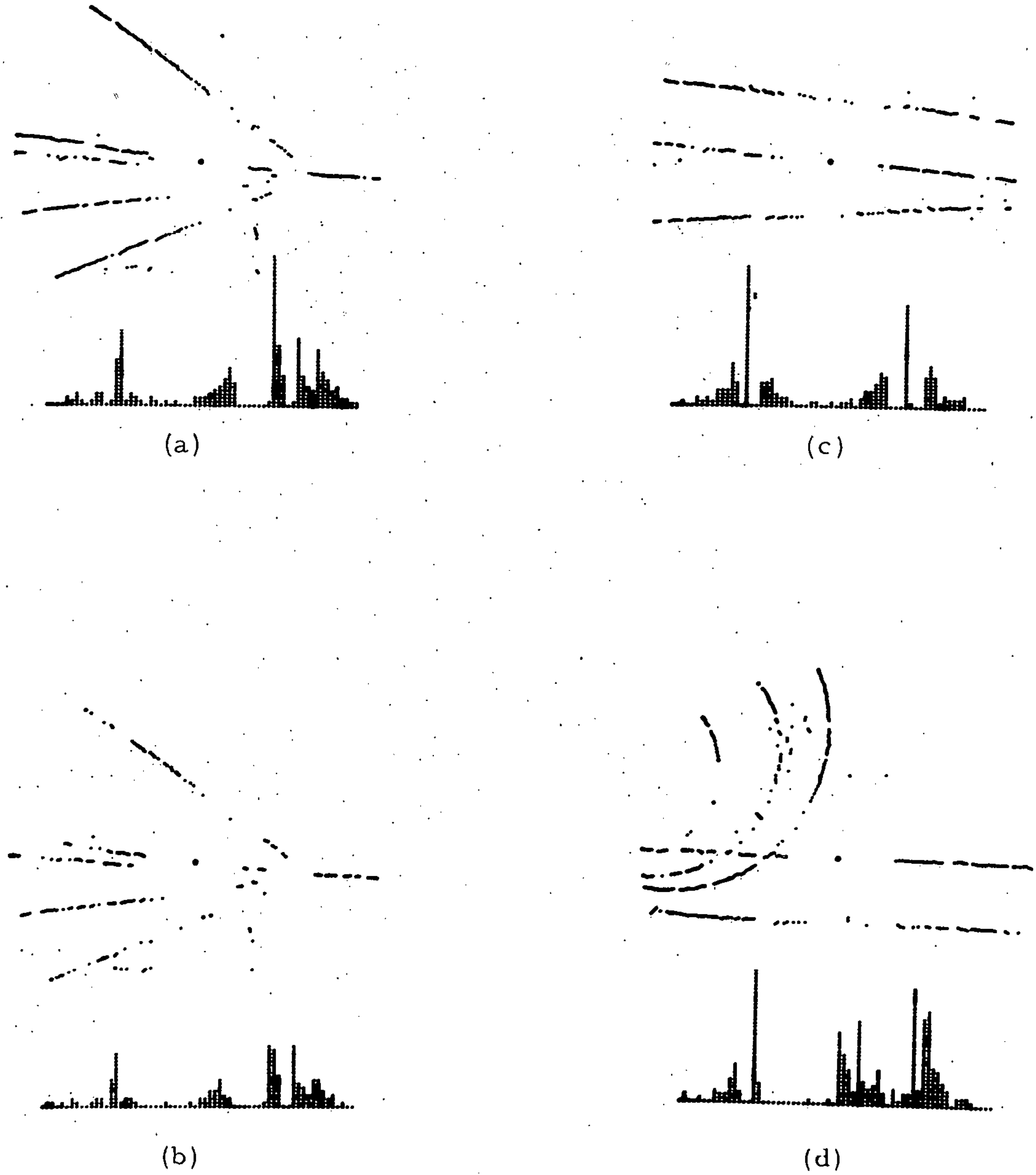


Fig. 7

More sample sweeps of the same frame. Sweeps (a) and (b) show hits on the doubled outgoing track (second from the top counter clockwise) with a wide and narrow hit width restriction. Sweeps (c) and (d) show the complex histogram pattern obtained in regions with many nearby tracks.

least four separate tracks. Observation of the actual hits shows five tracks, the sixth is superposed on the second track counter clockwise from the top.

In Figure 6d the sweep has been positioned as though the doubled track were being measured. In can be seen that this track appears to be heavily ionizing due to it being a double track. At this point the software would have no reason to suspect a double track, however. In Figure 6e the histogram information might indicate a problem in the forward direction for this track. And in Figure 6f, with the sweep positioned just beyond the place where the tracks split apart, the histogram clearly shows a new track just below the one being followed and all six prongs of the event have been found. The parallel track just below the one being followed confuses the histogram display considerably, but the separation of the doubled track still stands out clearly in Figure 6f. It should be noted that when following such a track the gates would be quite narrow such that the computer would not have individual hit data available for all the hits plotted on this display.

Figure 7c shows the histogram pattern for a sweep centered on the middle track between a parallel track and a track which crosses somewhat to the right of the sweep. It can be seen that the histogram contains useful information about the nearby tracks even though the polar coordinate system is not optimal. Figure 7d shows a large delta ray making many crossings of the track being followed. Note that the sweep does not digitize the delta ray well when the sweep and delta ray track are nearly parallel.

4. HIT DETECTION

The hit detection part of the RIPPLE system includes features found in PEPR, POLLY, and FSD and includes one idea from the SATR system at Wisconsin. The signal obtained from the photomultiplier (PM) viewing the film is normalized by the signal from three monitor PM's in a PEPR-like automatic gain control circuit. The output of this circuit has a base line and hit pulse height which are essentially independent of the CRT light level. After filtering (the time constant is a function of the circle radius), the leading and trailing edges of hits are detected by a threshold comparator circuit. The threshold is formed by a product of a computer preset hit threshold and an averaged base line signal level held in an up/down counter. This latter signal corresponds to the pedestal signal of PEPR and POLLY but is simpler to obtain here since the sweep pattern is continuous.

When the leading edge of a hit is detected the contents of the master clock are transferred to another scalar, called the slow clock. The slow clock is then counted at half speed until the trailing edge of the hit is detected. When this occurs, the slow clock is stopped and used to form the hit data for the computer and/or the histogram counter.

A hit width feature has also been included (as in POLLY) which has several desirable properties. At the time the leading edge of a hit is detected an analog ramp generator is allowed to start charging; the rate of charge is proportional to the instantaneous circle radius. In order for the trailing edge of the hit to generate a digital hit signal it must occur between two computer preset ramp voltages. Thus a PM pulse which is too narrow or too wide does not generate a digital hit signal. This technique differs from POLLY in that the decision to pass the hit is made in the controller rather than in the computer.

A very important consequence of this feature is sketched in Figure 8. In the region of crossing, or close parallel tracks, two bubbles can merge to generate a wide PM pulse. Without the width circuit, the logic described here may digitize midway between the two bubbles as shown in Figure 8a. It is much better if the width circuit blocks the digitization to yield the result shown in Figure 8b.

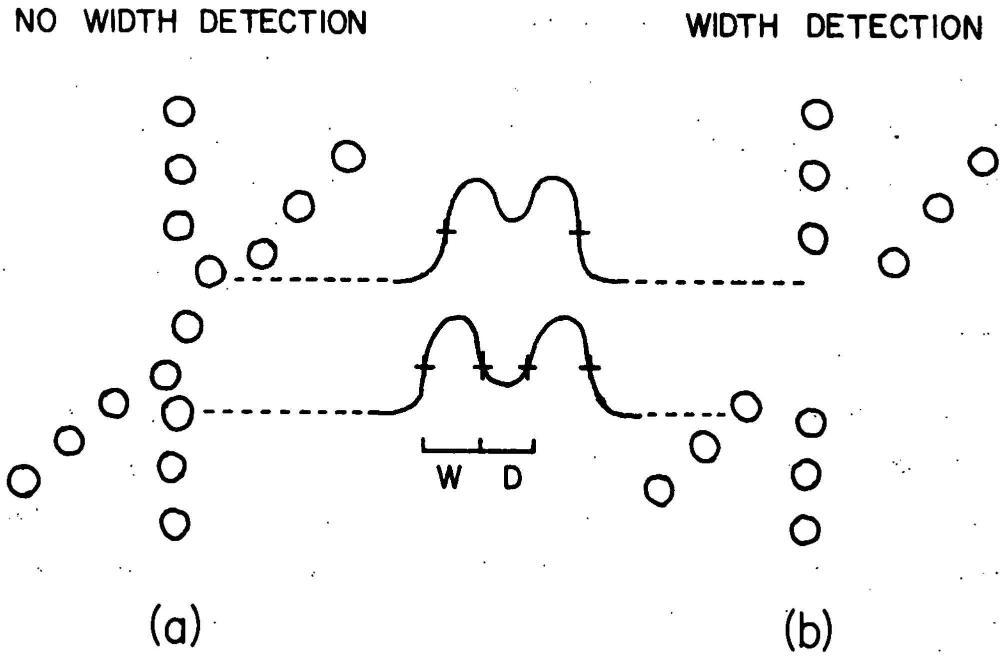


Fig. 8

Representative sketch of hits from two crossing tracks showing the removal of erroneous hits when hit width restriction is imposed. This is one of the major advantages possible with the flying spot sweep.

Another desirable consequence of this width circuit is shown in Figure 7a and 7b. In this event, also shown in Figure 6, the second track counter clockwise from the top is in fact doubled. In 7a the hit width is set very wide and this track digitizes much more often than the track just under it. With the width set more normally in 7b, many of the hits on the double track are eliminated, but the next lower track is unaffected. Note the loss of hits on some other tracks due to the scan lines not crossing the tracks at 90°. For multiprong events at high energies this width information can prove to be very useful indeed.

At high energies, it is particularly important that the hardware be able to successfully digitize and transmit hits on closely spaced forward tracks.

To match the fast hardware times to the relatively slow computer channel times, a feature common in the SATR system has been included. A hardware hit queue composed of seven 20 bit registers accepts hits within 200 ns of each other and transfers them to the computer whenever the channel is available. This system effectively eliminates one source of "dead time" between hits.

5. SOFTWARE CONSIDERATIONS

Two main features of the polar sweep require somewhat special software treatment. First, the circular sweep covers a lot of area, most of it useful only occasionally. Because of this the time for a sweep pattern is relatively long, typically 5 ms. This time makes it important that we take advantage of the Sigma-5 computer's I/O channel possibilities and overlap calculations with the sweep. A software development is in progress which will permit the RIPPLE to switch from one track to another rapidly thereby overlapping calculations for one track with sweeping for another. The scheme will also allow the RIPPLE to exhaust the automatic jobs it can perform while awaiting the results of a request for operator intervention. Details of this software will be reported at a later date.

Second, the hits are returned in a polar coordinate system r, θ, centered at a main delection X, Y. Assume for the present that it is unnecessary to correct each hit individually, rather that the hits can first be used to determine a master point and the master point can then be corrected. Since it is then desirable to remain in the polar system, simply to avoid the time consuming conversion to X, Y coordinates, a scheme has been developed to obtain a master point from hits expressed in polar coordinates.

Referring to the sketch in Figure 9, we define $\overline{\phi}$ as the estimator of the track angle and Δ as the perpendicular distance from the sweep center to the track. For each hit we write the approximate expression

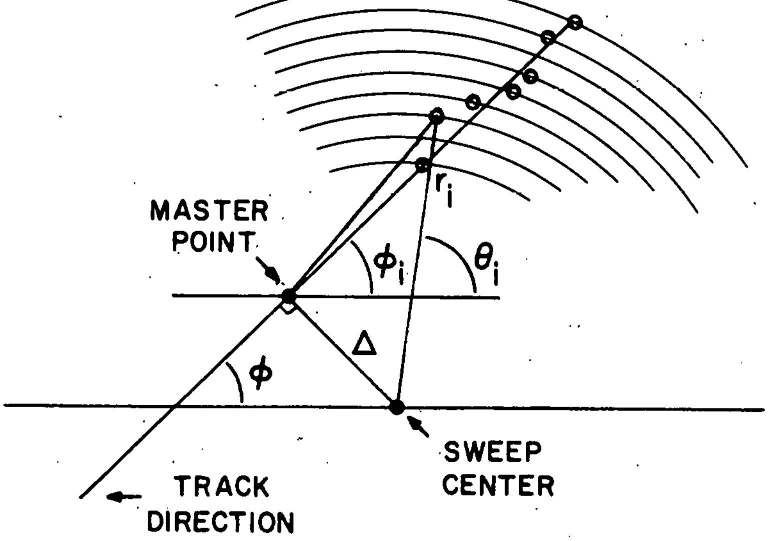


Fig. 9

Coordinate system used to determine a master point near the sweep center.

$$\phi_i = \theta_i - \frac{\Delta}{r_i} \tag{1}$$

where r_i and θ_i are the hit coordinates. The expression for the average track angle $\overline{\phi}$ based on n hits is

$$\overline{\phi} = \overline{\theta} - \frac{\Delta}{n} \sum_{i=1} \frac{1}{r_i} \quad , \tag{2}$$

where $\overline{\theta}$ is given by

$$\overline{\theta} = \frac{1}{n} \sum_{i=1} \theta_i \quad .$$

A simple chi squared for the angle ϕ would be

$$\chi^2 = \sum_{i=1} (\phi_i - \bar{\phi})^2 . \quad (3)$$

Substituting in (1) and (2) and minimizing this quantity with respect to yields

$$\Delta = \frac{\Sigma_i(\theta_i - \bar{\theta})(I_i - \bar{I})}{\Sigma_i(I_i - I)^2} , \quad (4)$$

where $I_i = \frac{1}{r}$ and $\bar{I} = \frac{1}{n} \sum_{i=1} \frac{1}{r_i}$.

This technique finds the point from which the track hits occupy the smallest angular spread. The point thus determined should require little additional correction for sweep distortion since it is quite near the center of the sweep. The master point so determined must be, of course, corrected for the CRT geometrical distortion, but this is one correction per sweep rather than one per hit.

6. CONCLUSION

The above discussion has described features of the RIPPLE system which are currently operational. We have not, as yet, tried track following, fiducial finding, vertex finding, or secondary track finding in an automatic and routine way. We do feel, however, that the RIPPLE system is well matched to these problems and will soon be in automatic operation.

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DISCUSSION

PLESS: (MIT) One comment about the line and the spot and width detection. With a PEPR you never sweep at an angle more than 45 degrees, therefore you can get about a factor of 1.5 in effective width of a track that you scanned depending upon its orientation. We have broad and narrow TEDs and these have each a dynamic range, which gives you a whole dynamic range of about 10 (9 is a better number). So if you have tracks such as the indication you show, the actual width of crossing tracks would be like a factor of two or somewhat larger when you cross it and you can see this with the broad TED and then the narrow TED will not fire. If you have wide tracks, you see just the track itself and then when you have a crossing situation you described, you don't get any hit at all, so in fact there is such information. It may not be quite as good as in your scheme but it's not zero.

FORTNEY: (Duke) I don't want to get into an argument but I don't want to give it up completely, because part of the trouble is just your good point and it's the bad point too. You have the long line element which means that you don't get this detailed stuff unless you are really parallel to the track. If you're even off by a degree that's I think 10 microns at one end versus the other end of 1000 microns line you tend to get a somehwat smeared thing laterally, compared to what we get with the single spot which is the only point. You can't have one without the other.

PLESS: (MIT) Let me see, I think you are concerned about one degree, not 45 degrees. Is that what you are referring to?

FORTNEY: (Duke) Yes, the line does not line up perfectly with the track. So with respect to the average signal you get, it's a little bit broader than it would be if we just looked at bubbles, so that your discrimination cannot be quite as good. I agree with you it's within the same range.

ALLISON: (ANL) I'd just like to ask first of all you're sweeping at a constant angular speed, is that right? So what sort of least count do you have at the edge and the middle.

FORTNEY: (Duke) We have 2048 angular least counts which corresponds to 3 microns at one millimeter and we get worse further out and better further in.

ALLISON: (ANL) And you're prepared to go out as far as 3 millimeters.

FORTNEY: (Duke) We go out as far as 3. I know I can detect hits at 3. We're going at very high velocity when we're out at 3. The thing is designed for the region around 1mm but it does detect hits out as far as 3 without any trouble.

ALLISON: (ANL) And there is another problem dealing with distances as long as that. Master point calculations which assumes the track is a straight line for that length begin to run into problems. Right? I'd just like to make one or two points, I guess we're all moving in the same direction, I'd just like to make one or two comments on POLLY III. This master point calculation, where we just calculate the center of gravity of these hits will be done in hardware, so we won't under normal circumstances do any bubble by bubble software processing and this will save some time. Also in doing this, the sorting of bubbles according to whether they're too wide or too narrow that was being discussed here, we will be doing by hardware and this will give us effectively a hardware digital filter which can be controlled by the program because, what is accepted as a bubble may be too wide because it looks like a double track or is too narrow and is interpreted as not a bubble at all but as some noise in the electronics. This will be a hardware register which is loaded by the program. So we hope to have a filter which is essentially under program control.

FORTNEY: (Duke) I'd just like to say that there are many points in this which one really has to make a decision. We knew the way you did it as far as hit widths when we started and decided to put it in the controller. You do lose something by putting it in the controller because when I get a too wide hit I don't get it and when you get a too wide hit you know you got a too wide hit. But you just can't have both all the information and keep the software fast.

ALLISON: (ANL) But, we are insisting in POLLY III that not only will we get out these summed averaged values which we normally use, but we will also get each line, piece of line data in core, although we will not normally use it for display purposes and so on, it will be there.

LUBATTI: (Washington) Are there any more questions? I have one and that is, do you have any feeling now what sort of time, what sort of measuring rates do you think this beam may lead to.

FORTNEY: (Duke) Well, I didn't give any times but if you looked at the slide carefully you saw 5 kilocycles there at one point. We take 200 microsecs to do a single circle and typically we could run with, our entire sweep pattern taking between 5 and maybe 8 milliseconds, something like that. It turns out, as evidenced from what the Oxford people have said that although we are a little bit more extreme, that the hardware speed is not the limiting factor as Irwin has said many times. I find exactly the same thing even at these somewhat slow speeds that I still spend a lot more time in software. Of course, I am just at the begining and we haven't speeded it up yet, but it doesn't seem to make too much difference..

LUBATTI: (Washington) That's really my question, when you do the software sorting out of the histogram and what not.

FORTNEY: (Duke) Well, that part is a gain not a loss because that's something you normally worry about. It's just when you're looking for the vertex or haven't found all the outgoing prongs so that's a plus and it's in there instead of something that would take even longer. But the general track following is just a problem of getting a master point from a lot of bubbles rather than simply looking at one hit and deciding that that's what you're interested in. There is a gain however in that, I think, we do have more information as a result. We can solve a crossing track problem closer to the vertex and this sort of thing, so you get something back but we don't have the potential of the PEPR in terms of speed. There is no question about that, but we expect to run at something the order of 60 to 100 events/hour.

7 May 1970 11:00 a.m.

Session X

Chairman: J. H. Mulvey (Oxford)

Bubble Chamber SPASM at Harvard*

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* Work supported by the A.E.C. under contract number AT(30-1)-2752

The device SPASM was originally conceived and designed to measure spark chamber film.[1] However, an extension of its capabilities to measure bubble chamber film required only a more appropriate film transport and optical system and suitable software --- the electronics package required no change. The additional cost for the new CRT system, optics and three view film transport was about $35K. We therefore concentrate on describing the new hardware reminding you only briefly of the way the digital and analogue circuitry used by SPASM works.

Perhaps the simplest way to do this is to say that the SPASM sweep is like a POLLY slice scan which contains only a single line. The more obvious difference between SPASM and POLLY is that SPASM can record up to 3 hits in a single sweep while POLLY can record only one, and that while the complete POLLY slice scan is hardware generated, a SPASM slice scan is done by software. We have available programmable sweep lengths ranging from 2mm down to 32 microns, but in practise only those longer than 250 microns are useful. Each sweep, regardless of length, is divided into 128 units so that our least counts range from 2 microns up to 16 microns. During the active part of the sweep which lasts for 12.8 microseconds a 10 MHz clock counts into a 7 bit scaler. When the discriminator output starts to show a film absorption, the contents of the scaler are transferred into a half speed scaler counting at 5 MHz. During the time that there is an absorption level a width scaler accumulates a number of counts at 10 MHz. When the spot has finally passed the absorption region the width and half speed scalers will stop. The width scaler will then register a number depending on the sweep velocity and width of the absorption on the film. The half speed scaler will register the center of this hit with respect to the starting position, measured in clock counts. For further details we refer you to previous descriptions, such as that given at ANL eighteen months ago.[2)]

The bubble chamber film measuring hardware is made up as follows. A 7" Ferranti 7/71/Q4 tube with deflection and focusing coils made by Celco. We hope to achieve a spot size of about 25μ in the center, and in a week or so we should know whether we are there or not.

We are using a 1 : 1.5 lens from Applied Optics and Mechanics. Our idea was to use the lens to demagnify the tube onto the film. At that time we were thinking in terms of 70mm film, so that successive views of the same event would have to be mechanically indexed into position in front of the CRT --- we weren't rich enough to consider buying three sets of tubes, coils and lenses. However, it became clear that a move back to 35mm film was taking place at Brookhaven, from where we expect to get all our film for the forseeable future. If we could get the three views sufficiently close together it seemed possible to fit all three into the field of the CRT simultaneously, a much simpler scheme. The longest frame, that from the 80" chamber, is about 3" long. Three views stacked side by side cover about 4". This nicely fills the demagnified 'scope face. We therefore had manufactured (by Bucone Corporation who built our previous single view film transports) the triplex film transport shown in Fig. 1.

The spacing between films is about 2mm; the registration accuracy is 0.007 inches --- achieved by separate registration pins. The mechanism which operates the registration pins also carries a spring loaded glass plate which presses tightly against the film to hold it flat in the measuring position, and withdraws to allow free motion when the film is advanced.

The advance distance is controlled by a group of four mechanical switches --- though these could be replaced by computer controlled flip flops -- which allow one to select any advance distance from 1 to 16 sprocket holes. Both forward and reverse motions are allowed. Spooling is controlled by spring loaded arms connected to potentiometers which operate the take-up motors. Advance speed is approximately one 16-sprocket frame per second.

The SPASM optical system is shown in Fig. 2. The reference photomultiplier is fed by a dichroic pellicle beam splitter --- we could probably do without this photomultiplier, at least for bright field film, but we prefer to remain conversative and keep it in for the time being. We use Fresnel condensing lenses to save space behind the film. A light source is located under the film transport; light from it passes through a red filter, is reflected in a dichroic beam splitter, passes through the film and is reflected in the dichroic pellicle beam splitter up through the projection lens. In addition to the dichroic mirrors we place blue filters in front of the photomultipliers to isolate them from the projection light as much as possible. If we can make this isolation sufficiently high, we will be able to have the optical image always available. However, we are not committed to this and I mention it only as a possibility.

The projected image of all three views will be approximately 2' by 18" on a vertical screen. It will be flanked on two orthogonal sides by microphones for a sonic pen[3)] which we use in a manner similar to the track-ball in POLLY. The sonic pen is essentially an acoustic spark chamber in which the spark is produced in a stylus wielded by the operator. The accuracy of the simplest version of the device is better than 1 part in 100, quite adequate for our purposes.

Each time a spark occurs while the stylus is between the microphones an interrupt to the computer occurs and the latter reads not only the spark position, to 9 bits of accuracy, but also the settings of 18 buttons in a control box. These buttons allow the operator to communicate with

with the computer and to control the performance of the program in certain well defined ways. Typically the program only reacts to the control buttons when it gets into trouble, otherwise it continues under its automatic flow.

For the computer to communicate with the operator we have a Tetronix 611 storage 'scope with a 6" x 8" tube face. On this is displayed a message to the operator and the results of an area scan of the film centered about the point at which the trouble occurred. While this display is stored the position of the sonic pen is continuously displayed in a non-store mode so that the operator can move the pen around without obliterating the data she is looking at. The coordinate systems of the sonic pen and the measuring CRT will be made to correspond as closely as possible so that the position of the pen displayed on the 611 'scope and the position of the pen on the optical display agree reasonably well. That then constitutes the hardware for bubble chamber SPASM.

Control of the device is through a PDP-1 computer which runs under a multi-programming system. The PDP-1 is in turn coupled through a high speed data channel to a Sigma 7 for geometrical reconstruction in real time. Our Sigma 7 currently has 44K words of memory, which should have risen to 52K by the end of this month. We also have 2 discs and 3 magnetic tape drives as well as card reader and printer.

The overall configuration is shown in Fig. 3. Our Sigma 7 is also used for batch processing all of the High Energy Physics work at Harvard so that it is not possible to control SPASM with it directly. Instead SPASM is controlled by our PDP-1, which is essentially available full-time, and we use the Sigma 7 only for on-line geometrical reconstruction.

The present software on the PDP-1 uses a clear point guidance scheme. This requires a point on each track --- nominally in an unconfused region --- and each vertex to be supplied as input. The program finds the track near the clear point and follows it back to the vertex and out until it leaves the fiducial region, or otherwise terminates. If trouble is encountered the operator's help is requested on the 611 display 'scope. Typically she can either give the program some additional data, abort the track or view or event or reassure it that it is really doing O.K. By additional data we mean either a totally new point from which to start measuring, or a point on a clear part of a track ahead of a confused region in which the track follower got stuck. This version of the software is fairly well debugged and is ready for production testing as soon as the new hardware is ready. With no operator intervention the system can measure two prong events at the rate of about 90/hr.

The PDP-1 program can send the measurements of the complete event to the Sigma 7 for geometrical reconstruction. At present we have no software facility for reacting to the results of the geometrical reconstruction; however the hardware and executive systems under which the computers operate do have the facility for returning data and we plan to take advantage of this as soon as the system has achieved an adequate level of production.

We have done some preliminary trials of the system using the single view spark chamber hardware to measure some sample tracks and fiducials. Repeated measurements of individual fiducials gave a spread of about $\pm$ 2 microns on the film. On THRESH reconstructions of tracks we have obtained residuals on the film of about 10 to 15 microns --- though we point out that we made no attempt to optimize these figures by careful calibration. Our calibration proceeds in two stages. First we calibrate the interpolation counts of individual sweeps in terms of main deflection units on the 'scope, then we tackle the pin-cushion distortion by measuring a grid and fitting a polynomial. We get residuals of about 5 microns for the grid intersection points when we do this. In any case, these numbers are only a rough guide to the performance as they were obtained with the old hardware.

Our first experiment will really try the system --- K^-p from about 500 MeV up to 1 GeV in the 30" chamber. Initially we will only try simple things like two prongs and τ decays, but we will work up our courage to tackle Σ decays and the like, as quickly as possible. We also have intentions of going to vertex guidance to cut down the scanner's load and we are just making a start on this by copying the now well tried POLLY scheme.

Sometime early this summer then, we hope to go into production testing and after a few months experience finding out how things go, we should have a viable system.

Acknowledgements

We wish to acknowledge the participation in the development of the SPASM software of C.J. Bordner, Jr. P. deBruyne is thanked for his valuable consultations, J. Blandino for the mechanical drawing and H. de la Rambelje for electronic technical aid are also thanked.

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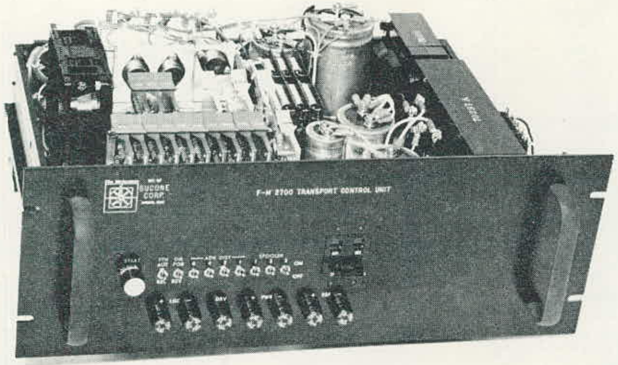

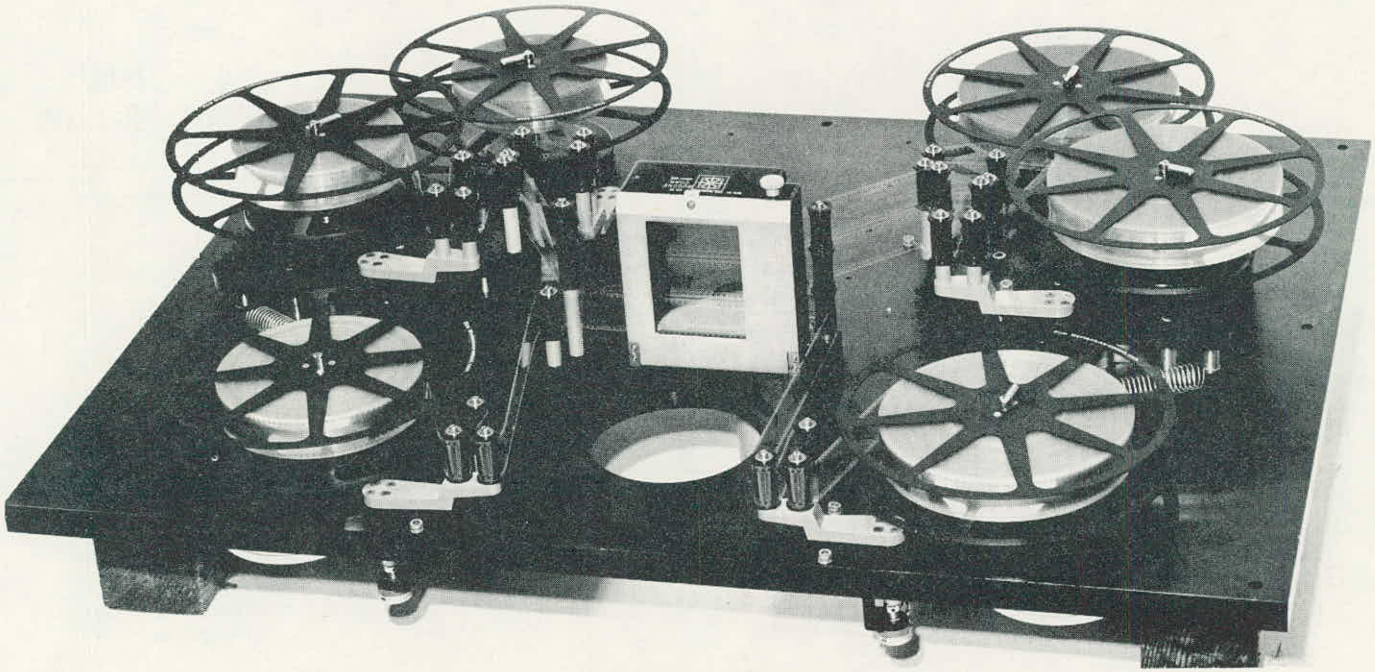

Fig. 1 Triplex 35mm Film Transport

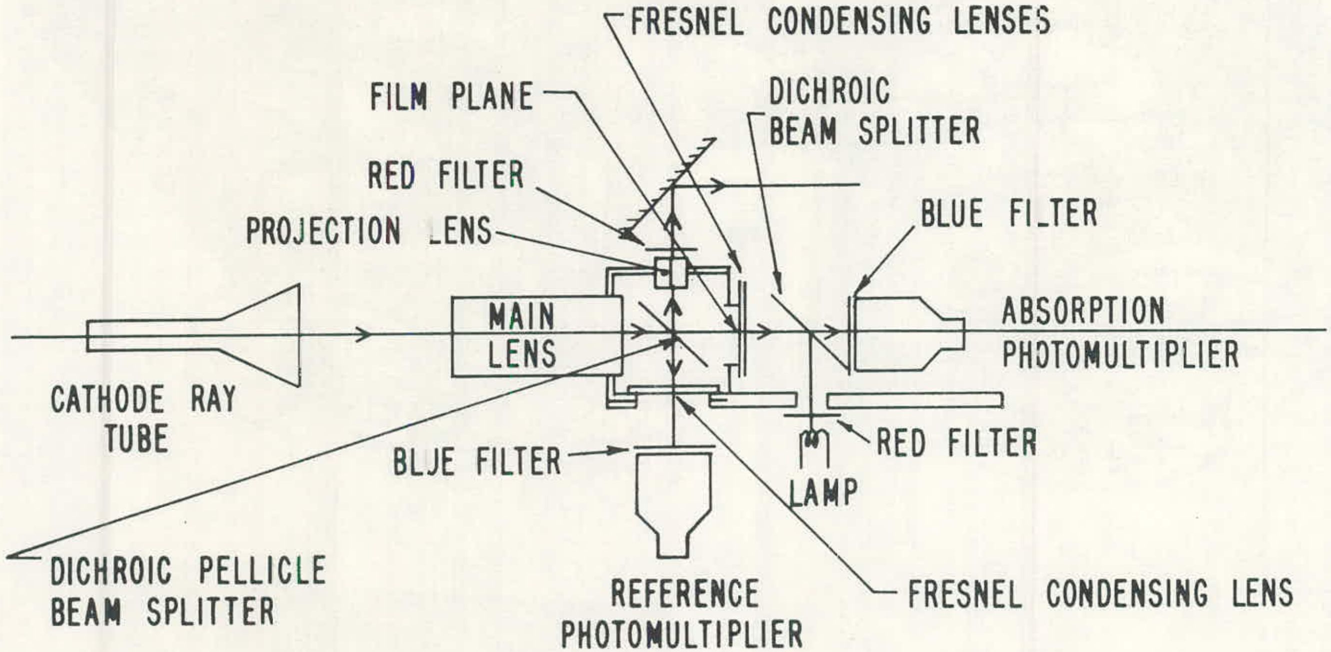

Fig. 2 SPASM Optical
System

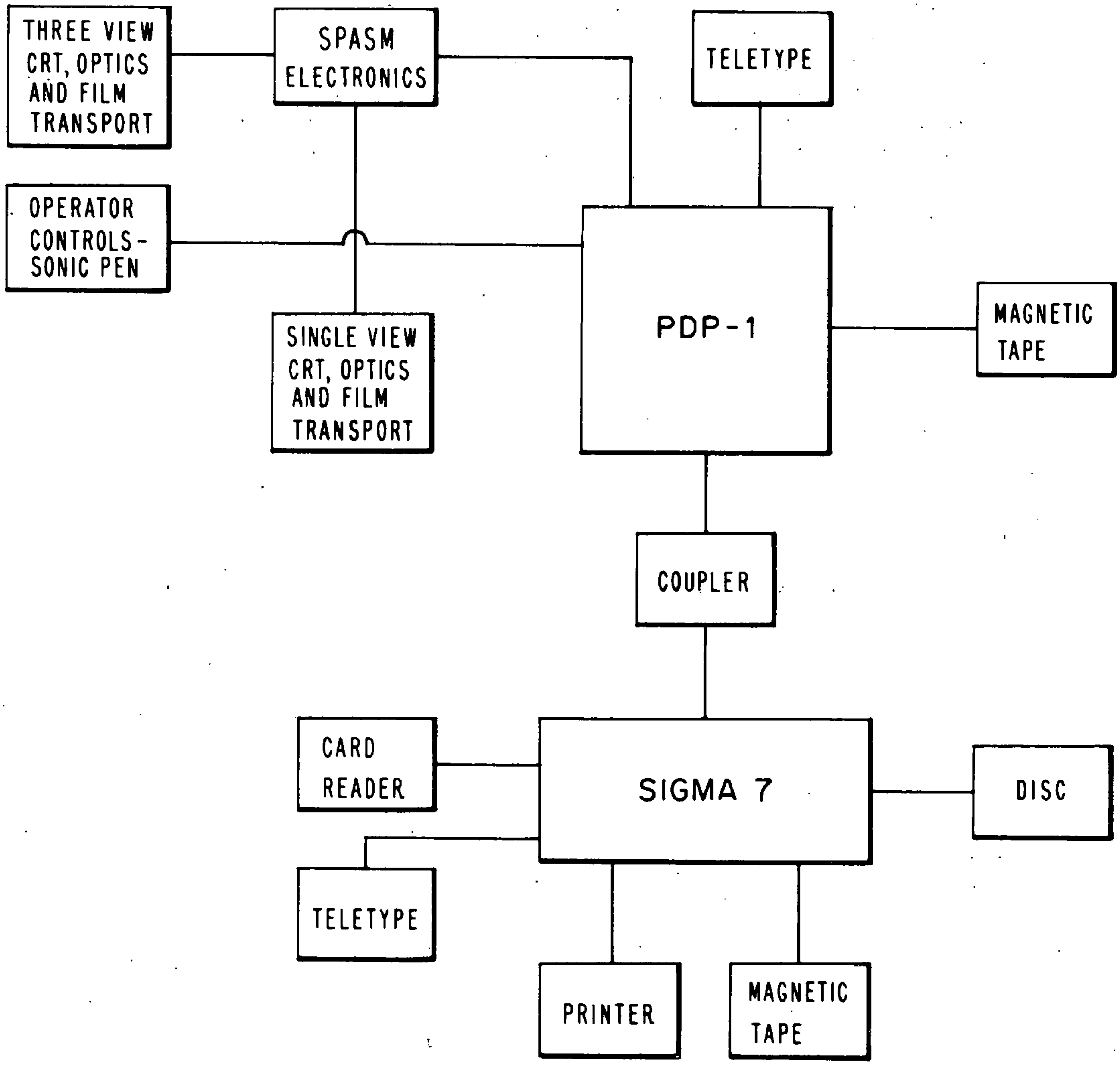

Fig. 3 SPASM and Computer Configuration

DISCUSSION

WATTS: (MIT) Did you say you had sent events to the Sigma 7 and reconstructed them there?

SISTERSON: (Harvard) The reconstructions we have done had been using a 360 to do our tape conversions. We have a tape conversion problem from the PDP-1 which is 200 BPI 7 track to Sigma 7 which is 800 BPI 9 track so, in fact, the reconstruction events have gone through the 360 but we have, in fact, also put some data through the linkage between the PDP-1 and Sigma 7 but it hasn't been data to reconstruct yet.

A PROGRAM TO MATCH BUBBLE CHAMBER TRACKS IN SPACE*

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INTRODUCTION

We have developed a new and very general computer program for matching bubble chamber tracks in space. Our approach is to use the simplest possible geometrical situation to get potential triples of track. Then, we rely on sophisticated logic to sort out the correct triples. The efficiency of our program is approximately 98% on any topology. The running time of the program is a little over two seconds per event on an IBM 360-65.

The basic problem of MATCH is: Given the tracks in three views, we want to label corresponding tracks so that the three view geometry program can reconstruct them in space. A typical set of tracks in three views is shown in Figure 1. We notice that some extra tracks may appear in one or two views. These are present when the more sophisticated pattern recognition programs for measurement of bubble chamber film are used. These extra tracks are eliminated by our MATCH. In other words, we solve the very general problem of an unequal number of tracks in the three views.

I. GEOMETRY

We set all the indices of refraction to 1. This leads to the geometry of Figure 2. It is trivial to show that the following relations hold, if the x-axis is along the projections of cameras 1 and 2.

$$x_1 - x_2 = \frac{Z_1 \, d_{12}}{Z_1 + h} \, , \qquad (1)$$

where Z_1 is the depth as measured from the front glass and h is the height of the cameras above the front glass. Then,

$$0 \leq x_1 - x_2 \leq \frac{d_{12}\, d}{h + d} \quad . \tag{2}$$

In general, if the x-axis is along the projections of cameras i and j, one has the following relation:

$$0 \leq x_i - x_j \leq \frac{d_{ij}\, d}{h + d} \quad . \tag{3}$$

When the measurements are rotated in a system where the x-axis is along the projections of cameras i and j, that system is defined as the preferred coordinate system {i,j}. For future reference we define:

$$\Delta_{\{i,j\}} \equiv \frac{x_i - x_j}{d_{ij}} \quad . \tag{4}$$

Where $\Delta_{\{i,j\}}$ is evaluated in each one of the preferred coordinate systems.

II. LOGIC

The logic can be divided into five distinct sections:

1. The pair test: All the track measurements of views 1 and 2 are rotated in the preferred coordinate system {1,2}. Similarly, all the tracks in views 2 and 3 and views 3 and 1 are rotated into the preferred coordinate systems {2,3} and {3,1} respectively. Consider a track in view 1 and a track in view 2 in the preferred coordinate system {1,2}: we want to know if these two projections (called a pair) can belong to the same track. It is clear that if relation (2) is not satis-

fied for any two corresponding points on this potential track, the track goes out the top or bottom of the chamber. This is what we define as the pair test. Corresponding points are calculated as shown on Figure 3. We should point out that since we know the curvatures of the tracks before we apply the pair test, we do not try to pair tracks with opposite curvatures. Furthermore, there is the complication of bad stereo. If two tracks are lined up with the stereo axis (more precisely, if the angle between the tangent to the track at all the master points and slave points makes an angle of less than 10° with the stereo axis), we compute the difference between the average of the y coordinates of the tracks. If this difference is greater than some parameter we reject the pair. Finally, since the indices of refraction are in fact not all 1.0 we use the following relation instead of relation (2):

$$-|\varepsilon_1| \le \frac{x_i - x_j}{d_{ij}} \le \frac{d}{d+h} + |\varepsilon_2| , \qquad (5)$$

where ε_1 and ε_2 are adjusted so that no good triples can be rejected.

2. Triples: Given a set of potential pairs in the preferred systems {1,2}, {2,3} and {3,1} one constructs a table of triples as shown on Figure 4.
3. Triple estimators: Given this potential set of triples

we are now in a position to find out how good a triple is by means of an estimator. The basis for our estimator is given in relation 4 where it is apparent that $\Delta_{\{1,2\}}$ is equal to $\Delta_{\{2,3\}}$ and to $\Delta_{\{3,1\}}$ for all points of three views of a real track in space. It is therefore a simple matter to compute the following quantity E for each triple of tracks:

$$E = \frac{1}{N} \sum_{i}^{n} (\Delta^{i}_{\{1,2\}} - \Delta^{i}_{\{2,3\}})^2 \qquad (6)$$

where N is the number of <u>corresponding triads of points</u> on the track.

It is clear that to first order optics (namely, setting all indices of refraction to 1) and assuming all measurement errors to be zero, E should be zero for a good triple. The corresponding points used in the estimator calculation are computed in a way similar to that of the pair test except that we use three views of a track instead of two.

Because of the real optical situation E_{max} (the parameter specifying the maximum value of E) cannot be set to a very small number. In fact, to include all good triples, it must be set to large value and some bad triples will usually come under the limit. In order to sort out the bad triples left over after the estimator test we must rely on sophisticated logic which we are now going to describe.

4. Equivalence classes: We arrange the triples in what we call equivalence classes. This is illustrated on Figure 5. Given the 10 triples shown on Figure 5, it is possible to separate them in three distinct equivalence classes. An equivalence class is defined as a set of triples which are related together because they have tracks in common in either one of the three views. For instance (Figure 5) in equivalence class number 2, track number 5 in view 1 appears in both triples. Similarly, in equivalence class number 1, track 1, view 1 appears in triples number 1, 2 and 4. Track 3, view 2 appears in triples 1 and 3, etc. In this way equivalence class 1 can be built up and is seen to ultimately consist of seven triples. Our problem is to find an independent set of triples.

Before we attack this problem, consider the following equivalence class:

1 1 5

2 1 6

6 5 6

This equivalence class could be called three views of one track, namely, 2, 1, 6 or three views of two tracks, namely 1, 1, 5 and 6, 5, 6. We adopt the position that the second interpretation is more likely than the first. This means that we must find maximal independent sets of triples.

5. Maximal Independent Sets: In order to find the maximal independent sets we use a procedure which is logically equivalent to the one we are about to describe. Consider the triples of equivalence class number 1 (Figure 5): triple 1 is directly related to triple number 2 via track 1, to triple number 3 via track 3 and to triple number 4 via track 1. We build up a table of all directly related triples in that particular equivalence class and we end up with the table shown on Figure 5. This table is equivalent to the graph shown at the bottom of Figure 5. We draw a vertex for each one of the seven triples and we join the directly related triples by lines. It is now clear that triples number 3, 2, and 6 are not related to each other, nor are triples 6, 1 and 5. These two sets constitute two maximal independent sets. In order to choose between two maximal independent sets with the same number of triples, we sum the estimators and choose the one which has the smallest sum.

This, then, summarizes the steps taken in this MATCH program.

III. RESULTS

We will now briefly describe the results that we have obtained from a sample of 30,000 events which have been processed through this program. We have tested MATCH on PEPR Point Guidance Output, which means that the number of tracks in each view is always the same. In other words, there are no extra tracks, even though the capability to handle those is in the program (and will very shortly be put to a severe test when our vertex guidance systems are operational). We have mentioned that the estimator has to be set high because of the optical approximations and this results in very large equivalence classes. Extraction of the maximal independent sets is very time consuming if the equivalence class is large, since the time involved goes exponentially. To get around this problem, we set the estimator low, take away all the good triples with a low low estimator, remove the corresponding tracks and re-enter MATCH with the estimator set much higher. This two pass process results in much smaller equivalence classes and speeds up the program considerably. Occasionally, one triple will remain after the two passes through MATCH: This last triple is assumed to represent the three views of a track.

The efficiency of the program is approximately 98% on all topologies. In fact, looking very carefully at 539 events, we concluded that only three of the failures were really attributable to MATCH. However, the failure rate was set at 2% because sometimes it is not clear whether an event fails because of bad track information(i.e. poor PEPR data) or a MATCH failure. However, our estimate of 2% is conservative.

The amount of time spent executing the program is two seconds in an IBM 360 model 65.

IV. ACKNOWLEDGEMENTS

We wish to thank Professor Irwin Pless for his continued encouragement for this project and Mrs. Wendy Chien for helping us in the testing of the program.

FIGURE CAPTIONS

Figure 1. This sketch represents the three views of a two prong event input to MATCH. Track 2 view 1 and track 2 view 2 are spurious and will be eliminated by MATCH.

Figure 2. Bubble chamber geometry when all the indices of refraction are set to 1.0. The x-y plane is the so called front glass system.

Figure 3. Two tracks in the preferred coordinate system {1,2}. One track is chosen as the MASTER track(on the basis of worst stereo), and from each point on the MASTER track a corresponding point on the SLAVE track is obtained by interpolation. For each MASTER point a quantity δ is thus obtained. If the following relation;

$$-|\varepsilon_1| \leq \delta \leq \frac{d_{12}\, d}{h + d} + |\varepsilon_2|$$

does not hold the two tracks cannot represent a pair.

Figure 4. From the pair table in preferred systems {1,2}, {2,3} and {3,1} one obtains potential triples of tracks.

Figure 5. Triples are arranged into Equivalence Classes. In in each Equivalence Class a given triple is not necessarily directly related to all other triples. The table in the upper right hand corner shows the directly

related triples. This table can be pictorially represented by the graph in the lower right hand corner. From this graph one immediately sees the sets of triples that are not directly related, and hence the maximal set (or sets) is obtained.

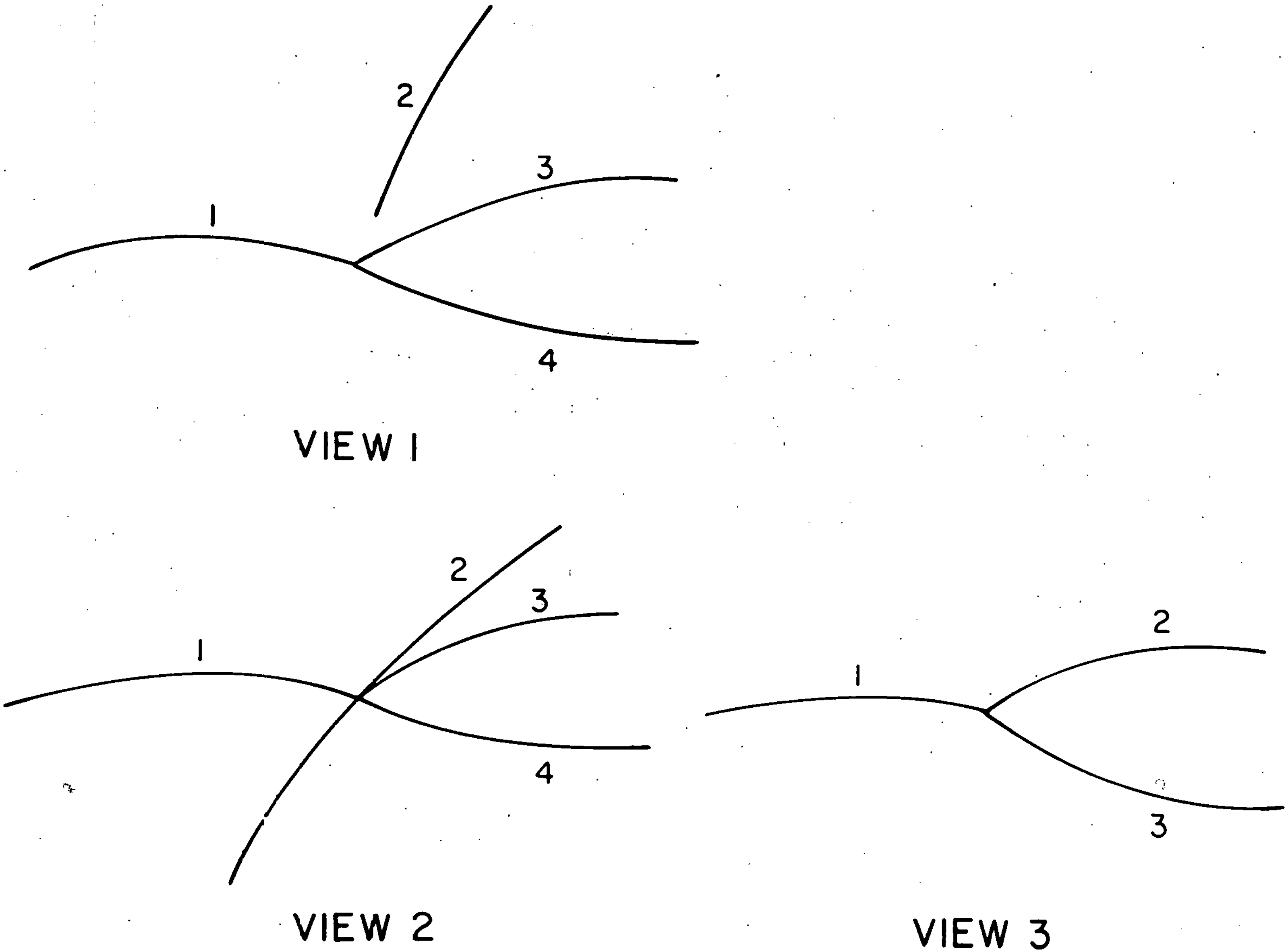

FIGURE 1

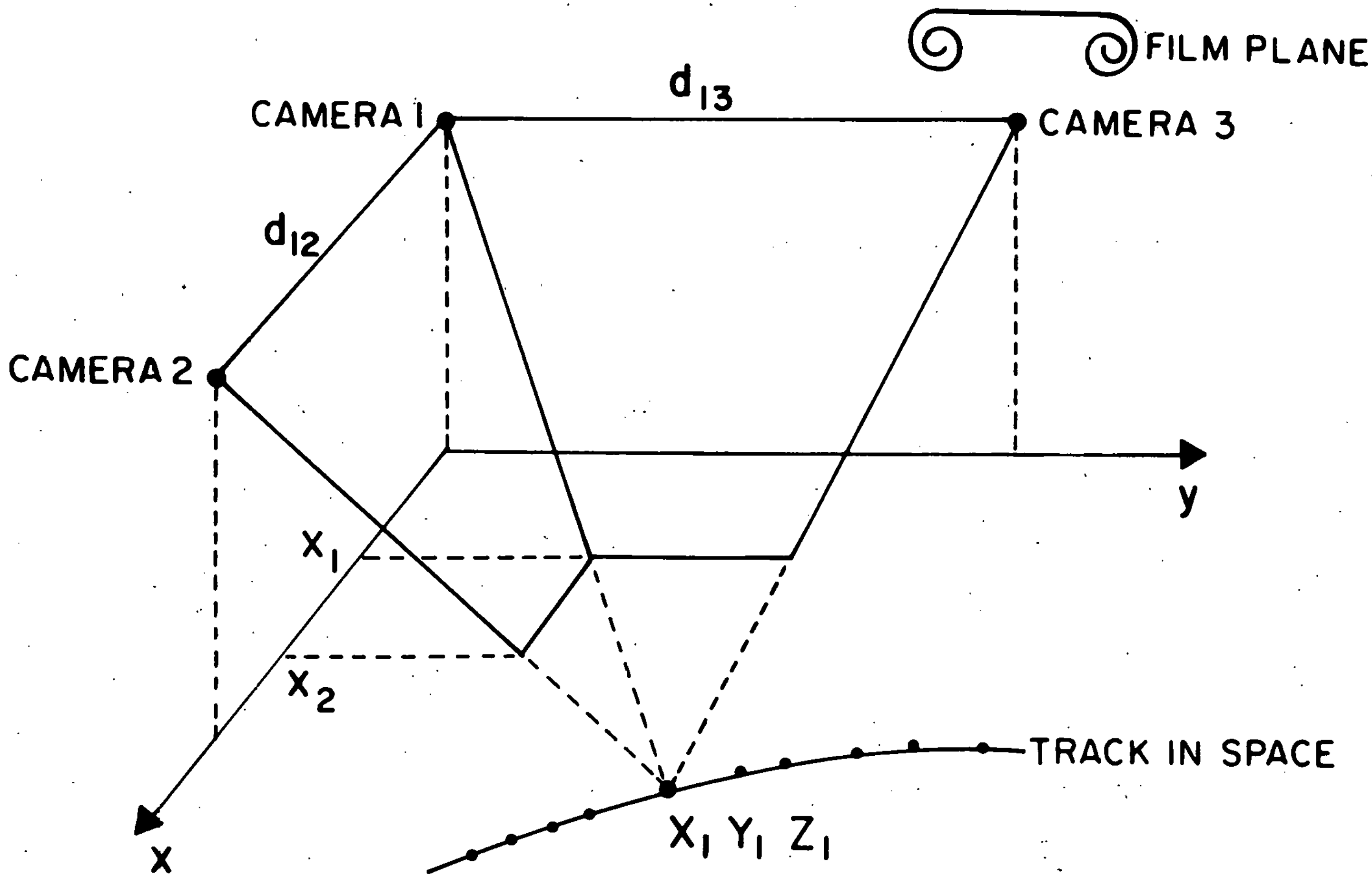
FILM PLANE
d_{13}
CAMERA 1
CAMERA 3
d_{12}
CAMERA 2
y
X_1
X_2
TRACK IN SPACE
X
$X_1\ Y_1\ Z_1$

FIGURE 2

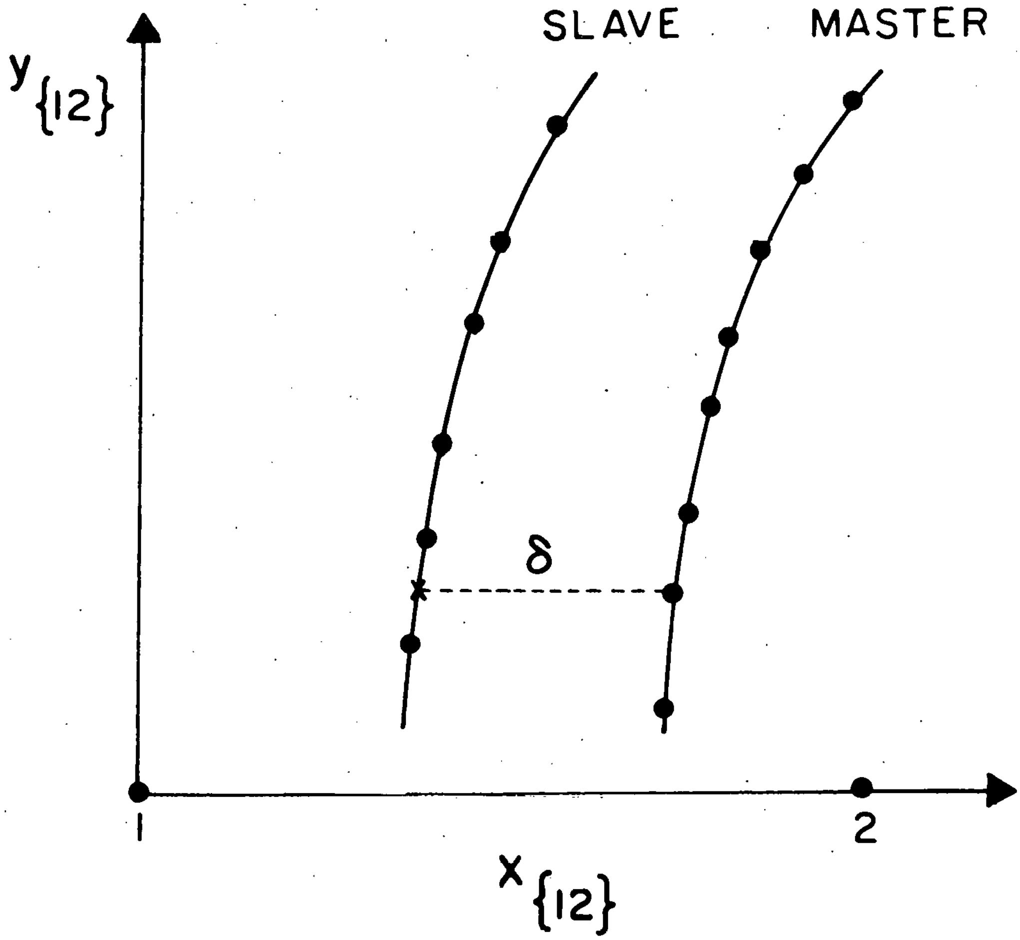

FIGURE 3

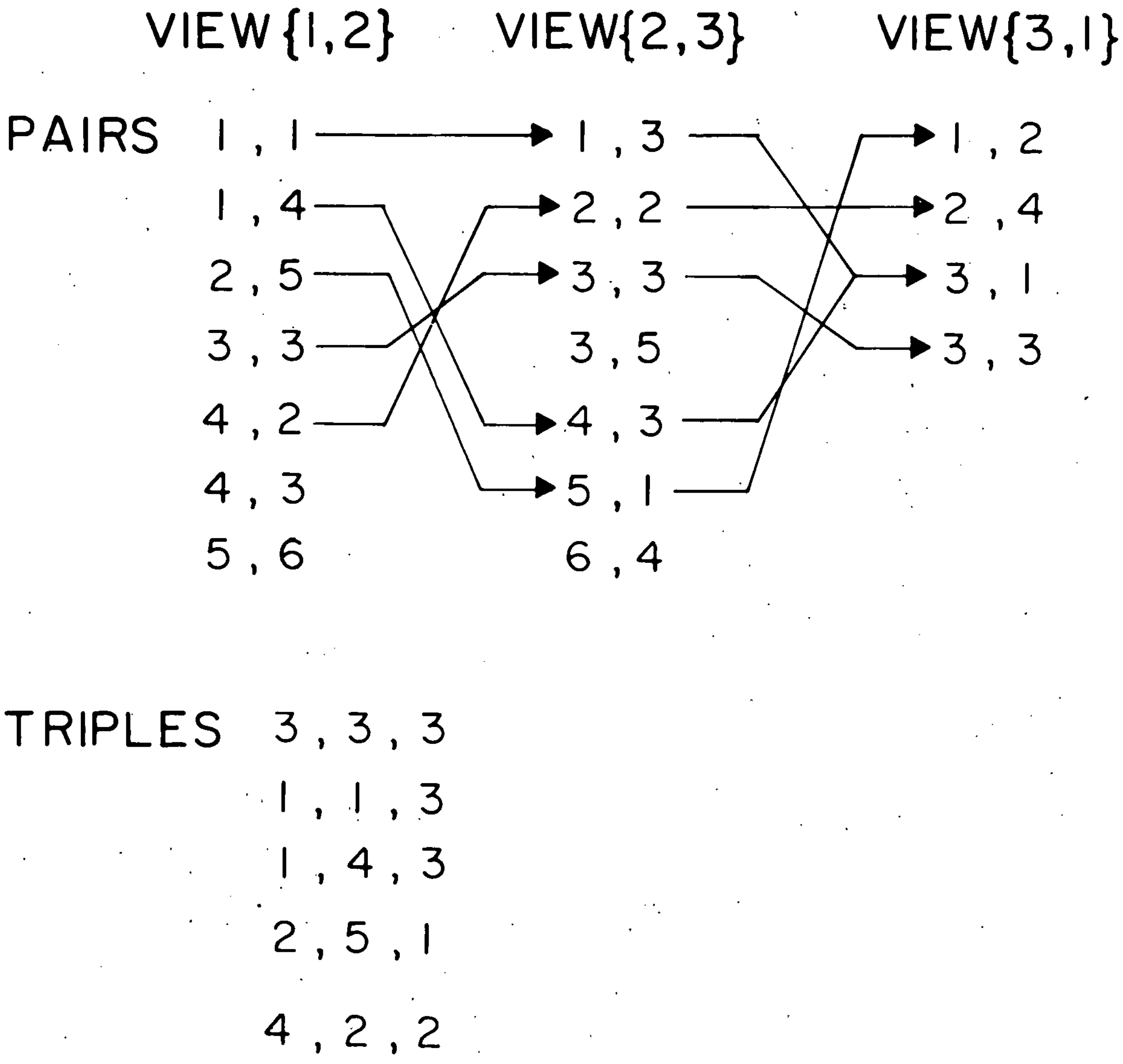
VIEW{1,2}
VIEW{2,3}
VIEW{3,1}
PAIRS
1 , 1
1 , 4
2 , 5
3 , 3
4 , 2
4 , 3
5 , 6
1 , 3
2 , 2
3 , 3
3 , 5
4 , 3
5 , 1
6 , 4
1 , 2
2 , 4
3 , 1
3 , 3
TRIPLES
3 , 3 , 3
1 , 1 , 3
1 , 4 , 3
2 , 5 , 1
4 , 2 , 2

FIGURE 4

	EQUIVALENCE CLASSES V1	V2	V3
EC1	1	3	6
	1	4	6
	2	3	7
	1	6	7
	2	4	7
	3	5	8
	3	4	9
EC2	5	1	2
	5	2	5
EC3	9	7	1

VERTEX	(Triple Numbers)
1	2 , 3 , 4
2	1 , 4 , 5 , 7
3	1 , 4 , 5
4	1 , 2 , 3 , 5
5	2 , 3 , 4 , 7
6	7
7	2 , 5 , 6

MAXIMAL SETS

6	1	5
6	3	2

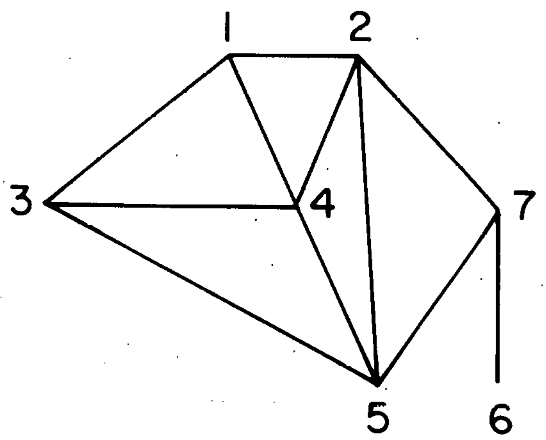

FIGURE 5

DISCUSSION

HULSIZER: (MIT) Pierre, I thought that at MIT there was one event that ran for 2 hours in MATCH.

BAUMEL: (MIT) There were events that were running in the order of over 30 minutes and one in particular that we frantically called Pierre about was 33 minutes and the result was that it did eventually fail, but that was because it had some charge error of some sort and I think that was probably due to the way it was IPD'd and not to MATCH; but we have had some long events that ran 20 minutes and 30 minutes and have converged to give correct results. The time required to Match all 3 views clearly depends on the size of the equivalent class.

BASTIEN: (Washington) This can be cut down by using instead of a 36 by 36 word matrix a 36 bit by 36 bit matrix and that will be much faster. (Editor's Remarks: → this refers to the Subroutine Maxset Track Matching Matrix).

CRESTI: (Padova) I am reasonably familiar with the MATCH they use for the Spiral Reader in Berkeley, you probably know the speed of it, how does it compare with your program MATCH?

BASTIEN: (Washington) I am sorry I don't know that.

CRESTI: (Padova) I have just received more information. About half of the time at the rate of between 10 and 15 events a minute on a 6600, is spent in MATCH which would make 3 seconds of 6600 for MATCH. So the time is probably in the same order as yours.

BASTIEN: (Washington) Yes, I would suspect that it's about the same order.

PLESS: (MIT) On the average event our MATCH takes less than a second.

MULVEY: (Oxford) Let me ask a question to clarify this, if you put in 100 4 prongs how long will it take on MATCH?

PLESS: (MIT) It'll take on the average less than a second for the four prong on the 360/65. Now there are some pathological cases, just to make this very clear because there is some confusion. You see, we thought MATCH was working great for months and all of a sudden there was an event that wouldn't go through and then another month and another event wouldn't go through and the reason it didn't go through was it ran longer than the hour that we said that we wanted this total run to go. We picked up these events, and there are probably a half a dozen in the couple of hundred thousand events we processed, to study the pathalogical cases, where some of them take 2 minutes, some of them take 33 minutes and then fail and some take 23 minutes and succeed. However, if you take the average event, the average event that we run takes less than 1 second on the 360/65, some may take one tenth of a second, some may take three-quarters of a second depending upon exactly the size of those equivalence classes. But the average, except for these 6 or so pathalogical cases, which even if we threw them in wouldn't change the average, take less than a second on a 360/65, certainly less than 3 seconds on the 6600. (Editor's note: Bastien and Chien (MIT) made a timing run in August 1969 through program TRIMERGE which includes MATCH. On a batch of 1025 events (2, 4, 6 prong events) the average time per event was 2.1 secs on the IBM 360/65. This time includes tape moving and the full processing performed by TRIMERGE. Actual MATCH time is of the order of 1 sec/event or less).

ALLISON: (ANL) I am not a mathematician, and I guess I feel a bit sort of emotional about classes going up factorially. I didn't do any of the programming but I believe we have, at POLLY, a MATCH system which employs these same things. Largely as a result of me going around and getting worried about it, we changed to a different system. As I like to think of it, a girl sits down and she's been measuring on a manual machine for years and is just told to measure the tracks in the same order, she doesn't go through these fantastic operations of sorting the tracks in order to measure tracks in the same order on the different views, so it must be possible to match most of the tracks by exceedingly simple methods and only to use anything as sophisticated as this on a small subset. Could you comment on the use of this on a small subset only of events, or does your method of triples really work exceedingly rapidly on 2 prongs as well as 4 prongs.

BASTIEN: (Washington) It certainly works exceedingly rapidly because what happens in most cases is that you get equivalence classes with esentially one member.

THORNDIKE: (BNL) Just one more question about timing, suppose you were to imagine a very complicated event, let's say a 10 prong event or something like that what would your estimate be of an average time per case there? Based onthe experience so far, and for the usual confusion that 10 prongs have, eg. all forward.

BASTIEN: (Washington) If you have a 10 prong at 25 GeV/c certainly it will build up a large equivalence class, the exact time I don't know.

PLESS: (MIT) We have had 8 prongs in our film, in antiproton film we've even had 10 prong events, and the answer is, it takes less than a second to do those on the average.

FORTNEY: (Duke) As I understand it, the information you have is angle and curvature, that's what you're really trying to match on. I'd like to know if anyone has considered the possibility of doing what people also do, of using any kind of bubble finger print? Has anyone found that to be absolutely unreasonable?

BASTIEN: (Washington) I don't know anything about such matters; off hand, I would say that's difficult and not really necessary.

FORTNEY: (Duke) Well, it seems if you had more information about the track you'd have smaller classes.

BASTIEN: (Washington) This is really very hard to do eg. because there are differences in magnification.

LUDLAM: (Yale) Somewhat in response to Wade Allison's question, our experience at Yale has been we have 12.6 GeV K^- events which have very forward tracks in the 80" bubble chamber. Since we're doing a large amount of pre-digitizing before the PEPR measurement I thought we'd try and write a MATCH program working with just the 3 points and using the same philosophy as Pierre has discussed. We found that in these events, if you just measure the tracks in say clockwise order, 85% of them will match.

We've also found that we get good results, I'd say at about the 96 to 97% level, with only the 3 point tracks. And most of the time, the equivalence classes contain only one or two tracks, even on the 10 prongs, because they are in fact better than the 2 prongs and 4 prongs because the tracks spread out quite a bit more.

In terms of timing, we're measuring all topologies and 100 events take one minute on the 7094.

CRESTI: (Padova) I am still trying to understand something about the time. You have point guidance on all 3 views which means you have the correct number of tracks in all three views.

BASTIEN: (Washington) That's correct, you have the correct number of tracks.

CRESTI: (Padova) O.K. that's probably the reason why your MATCH takes such a short time compared to what I was expecting.

BASTIEN: (Washington) If you have a vertex guidance system, you may get an extra track occasionally but this is not going to slow it down by much, not by any appreciable fraction, that's clear.

PLESS: (MIT) This information of how many tracks there are, is not fed into MATCH. Quite frequently, because PEPR is not a perfect machine, it'll miss a track on a certain view, so there are two extra tracks in that particular view or one track missing, depending upon how you want to discuss it. MATCH does not know how many tracks it is trying to find fortunately, otherwise we'd fail everything. If you're missing one track in one view and a different track in another view, in fact therefore there are two pairs only and you don't have a triple for two of the actual tracks. In favorable cases, not all the time, MATCH will even sort out this particular problem. So the particular MATCH that we have here, does not depend on knowing how many tracks there are in the event, it will do the best it can with whatever information you give it, and it still takes less than one second per event.

LUBATTI: (Washington) We plan to use MATCH at Washington, with the vertex guidance scatter system described by Bastien. MATCH will be extremely useful since we will be able to relax some of the criteria and accept spurious tracks into MATCH, to be rejected there. By coding in machine language, MATCH will be extremely fast, and an integral part of our vertex guidance system.

Philosophy of a Three-view PEPR

J. N. Snyder, University of Illinois

Unlike most of the participants I pursue this business as an interesting summer hobby. Thus I will be describing a program that we generated at M.I.T. last summer and about which I have not thought since August. When originally asked to speak, I was asked to speak on the philosophy of a three - view system and the programs for it; and as is befitting a philosopher, I therefore made no effort to prepare myself. Let us see where the M.I.T. PEPR project currently stands: it has two very very sound existing components, first the point guidance system which you heard Terry Watts describe yesterday, one view point guidance, secondly it has this MATCH program working excellently. The question was, starting with those two and in an evolutionary fashion rather than a revolutionary one, could we build up slowly so that one could test new components step by step, making sure each one is working correctly before proceeding to the next. Also could we build up in two directions, first by exploiting three views of a given event for which the hardware is being modified either by having three tubes each looking at one view or, as an interim measure the three views being looked at by one tube. From the point of view of the program this is logically equivalent, the only difference will be in the element recognition routine, which has to know whether it is switching between three tubes or switching between three different areas on one tube. Secondly we wanted to try to evolve possibly at the same time from where we now stand with a point guidance system, into a vertex guidance system, and finally into full pattern recognition. As I say, I have not had any contact with this since last August and hence I am not really aware of how the program I left behind has evolved. I understand it is proceeding slowly because of calibration troubles and the arrival of the PDP-10 which has given other things higher priority.

Basically, the idea is as follows. We have within the memory of the PDP-6 or 10 as the case may be, the complete point guidance one view PEPR system which you have already heard described. The master control program of this three view system first does point guidance one view PEPR on each frame, just like it does now. Only now, it may be days apart that it does the different views. In the three view system all three views of the film are available to scan and the data banks apertaining thereto are available within the machine. You apply MATCH to this data, and if everything matches you are done. I would suggest one apply MATCH with rather tight control parameters to make sure that one really has valid triples coming out. If everything doesn't match there will be tracks left over in each view, maybe the same number of tracks in each view, maybe not, because as you will see later, at this stage of the processing you may have arrived here via other routes than a point guidance system. MATCH has already indicated what pairs of tracks are possibly candidates from the same space track. The control program will take a possible pair and use the mid point on one of the tracks along with the other track, to compute a point in space. Hopefully this is what you might call a three dimensional clear point from which you could start tracking. It probably goes without saying that transformation problems here are a logical horror and everything in three views will probably run very slowly as a result. As you can see, we are going to try to track in space, so one has to transform up to the front glass coordinates and from there transform back to the optics on to the film in PEPR coordinates; this will be very time consuming.

The spatial tracking philosophy is extremely rudimentary from a geometrical point of view but is overlaid with logical horror. Let us assume, as I just stated, we have a clear point in space. We also have a track direction at that point, because of PEPR's fortuitous provision of angles, and then we simply make a small step in space along a straight line prediction to a new point on that track. We project this point back to where it should be on the three films and then look at those three places. Now many things could happen: you may get an element at this spot on all three films, on two, on one or on none;there is a different strategy to pursue in each case. If the number of elements is zero or one, that constitutes a gap and you try to step on till you pick up the track again. But suppose, for example, that you got an element on each of the three films. Taking each of those three hits in groups of three pairs, you can compute in space three different space points that, presumably, these three hits came from. They will not necessarily all be coincident, so there are various parameters by which if these three points are grouped in space sufficiently closely one assumes that they are the same point, and the average of them forms a new space point. Thus you track this way, and this naive philosophy is the one that is going to be tried first.

If the three points that you reconstruct in space this way, are spread out greater than appropriate parameter, it could be due to the fact that there is a nearby track in one view and you've hit it. It's a little difficult from the three points to figure out which view if bad, but you can, and you can reject this view and try to track on just the two views until you get by this spot. The problem of crossing tracks will hopefully be much simpler, by this type of strategy than it is when you are looking at just one view at a time, because it would be highly improbable that all three views of a given track you are trying to follow would be in trouble by crossing tracks in all three views at the same spot. Hopefully, the tracks that are left over from the first MATCH pass can be resolved in this fashion and you will pick up more valid triples, this time by actually tracking them in space. We have not yet provided the appropriate routines or even appropriate strategy for what happens if further you have single tracks left over in the various views.

The next thing I'd like to describe about this particular program, is the fact that it also has built in it a very rudimentary vertex guidance scheme which, following the evolutionary philosophy, does use essentially everything else that is already existing. Those who know one view point guidance PEPR reasonably well, know that there is a routine called the clear point locator in it which has the duty to use the very rough digitized coordinate of a point on the track to find an actual point on the track. This track point is then handed to the track following routine which proceeds to follow the track. This is done for each and every track. In the program I am describing we would inject only a vertex coordinate from the IPD and at the same time provide the scanning technician with a mechanism so she can label each event that it is to be processed using point guidance, using vertex guidance or using no guidance. Suppose she has indicated that the event is to be subjected to vertex guidance. In this case the only coordinate she will have fed along is vertex point. In each of the views one can make an appropriate scan around this vertex to try to pick up points on tracks radiating from the vertex. These you feed to the one view PEPR system just as though they came from its clear point locator routine. From here on, nothing downstream knows the difference. If the girl on the scanning table chooses to hit the appropriate switch that says find the vertices yourself, there is still another routine that first and this is a rather amusing one, that first scans the appropriately parameterized entrance window to pick up all beam tracks; it then uses these as clear points to go back to the old one view PEPR program which we know works very nicely and this time the one view PEPR program simply tracks everyone of these beam tracks through the chamber. When this is done in each and every one of the three views, the program throws out beam tracks that go all the way through, only retaining those who stop within an appropriately parameterized fiducial volume. The program then calls MATCH and matches all these beam tracks and calls an end point finding routine which will presumably find the end point of those beam tracks that have stopped in some fiducial region. There we have a vertex. Then we go into what I have described so far to sweep around the vertex, find clear points, go into the one view point guidance PEPR, MATCH again, and so forth.

This is the philosophy and the strategy of a three view system. Esentially nothing is written except the control program and a few of these packaged tracking programs which we did have to provide for testing. I am sure in their first versions these programs will not be adequate but they can be developed independently of the whole strategy. This then is the philosophy that I was asked to discuss.

DISCUSSION

MULVEY: (Oxford) Concerning matching and track following perhaps in 3 views, how do you see this in the photographs of the bigger chambers in the future where perhaps the extent to which you are making use of simpler optics is a less good approximation.

SNYDER: (Illinois) MATCH makes use of first order optics only because, shall we say, we can get away with it in that particular case. Better optic transformation routines could be provided. In fact, the 3 view track follower that I just described, does have to use much better optics than MATCH does. MATCH is not necessarily restricted by the precision of the optics transformation routines that you chose.

PLANO: (Rutgers) Would someone like to comment on the expected time scale with the evolution of this philosophy.

PLESS: (MIT) The contract that I had with Jim was that next to getting production out this would be the highest priority. Unfortunately getting production out, as everyone is aware is not trivial. Basically the major programs that Jim coded, have been assembled and some of them have even been tested; I think in a certain sense, the software will not be the greatest hold-up in the initial stage, namely to find out whether the strategy works or not. Getting to that point will be almost solely dependent on the hardware, eg. getting the correct cathode ray tube, the correct film gate and making sure all that works. It's the hardware at the moment that holds us up and as soon as we can sort of get out of our difficulties we expect we will work on that hardware. To give a guess estimate to what all these troubles will lead to: sometime this fall we should have hardware and sometime soon after that we'll know how badly this system doesn't work.

HARRIS: (Oxford) When you actually get into 3 D track following, when you have left over pairs, and you have defined points in space and get what you call clear points, then rather than tracking in space one simple strategy would be to try tracking on that third view. I wasn't clear whether this is what you are going to try first, it seems to me as a very last resort one actually wants to go into space, because, as you point out, you have all the calculation overheads.

SNYDER: (Illinois) That's probably a very good strategy to develop, it just is not in the scheme as presently written.

HARRIS: (Oxford) I would think this is the way we would intend doing it at Oxford. We would use left over pairs of tracks on 2 views to define, in your terminology, clear points, maybe up to three clear points. Then we would have a very quick try at following; it would take only 30 - 40 milliseconds and it would seem that's the best thing to do.

SNYDER: (Illinois) It is certainly a very good idea and could be done.

HARRIS: (Oxford) Concerning MATCH and the pair test, I wasn't quite clear how comprehensive the test was, in the sense of how many points along a track in the two views are used?

SNYDER: (Illinois) Every measured point.

HARRIS: (Oxford) Do you check the distance between points in track - views for monotonic increase?

SNYDER: (Illinois) No.

HARRIS: (Oxford) Why not? If the track is all in the XY plane it's a very good approximation that that distance should be constant and it should increase monotonically as a function of the dip angle. It would seem to me that you ought to put in as many checks as possible at the pair stage so that you don't end up in this messy business that takes all the time.

SNYDER: (Illinois) It's well known that there are 87 different tests that one can apply and in fact many MATCH routines are built up of applying in sequence such tests, such as: don't try to match tracks with the opposite curvatures, do the tracks lie on the same or opposite sides of the stereo axis, and so on. The hope is by applying a sufficient number of these tests, one eventually disentangles the tracks. I make no brief for having not used some of these more obvious tests, but the pair test is very simple, it takes every point of the one track of a possible pair, projects it over to the other in the standard way and sees if that point reconstructed in space is above or below the chamber. The first time the answer to this question is yes for any possible pair candidate, that possible pair is rejected.

BASTIEN: (Washington) First of all concerning higher order optics in MATCH, we thought about trying to make a third order optic correction in the triple estimator calculation. It's a possibility that presumably would really improve the estimator. We didn't do it because we felt the program was working very well and probably it wouldn't speed it up.

Next concerning the strategy that Jim Snyder has used in his for three view PEPR. At Seattle, we would like to do the best we can in one view without having the three view film transport, then put all the tracks on a disc. If the event does not pass, then we write out fine mesh area scans on the storage disc implement exactly the same type of thing. We bring in MATCH and match the tracks, if all the tracks match we go to TVGP. If the tracks do not match, then we bring in our fine mesh area scans and do something similar to this, namely three view tracking or three view help or something of that nature. Then, in fact, we have a super help which includes TVGP and MATCH and everything and we lay the events to rest.

MULVEY: (Oxford) This completes this morning's session and perhaps I can be excused for making a few, I hope, brief remarks. We've come to the end of this meeting in which I think for all of us, certainly for me, it has been very enjoyable as well as very informative. I think the organization has been very good, there has been plenty of time for informal discussion and I think if you'll bear with me, there are one or two remarks I should like to make, some of them put in my mind perhaps by the remarks made by Weisskopf last night. I think he reminded us that as experimentalists in high energy physics we are making a contribution, we're working in one of the frontiers which is a very exciting frontier, making a contribution , to knowledge which will be lasting. I think it's perhaps a good thing he reminds us not to be defensive, on the defensive about this situation, I think it is probably also true, as he said, that one should perhaps try to do more to convey some of this excitement to other people. I think one of the things that people in the field like myself, feel at times, is that the physics itself is becoming rather difficult. It is not so easy to see one's way towards the answers. Of course, this doesn't diminish the importance and the challenge of the problems that one is trying to tackle; again, Weisskopf by reminding us of Feynman's talk, showed that one should occasionally not be too over-awed by the very complex theoretical structures that are sometimes created. It's also often been seen in the past that one doesn't find one's way through problems until the appropriate technique has actually come along, and I think that it's true that the technique for the use of bubble chambers has certainly been advancing. Everybody knows that one of the most important advances has been in the use of automatic and semi-automatic methods of reducing the data, the vast amount of information that lies on each bubble chamber picture and I think it's here that now the CRT devices, PEPR's, POLLY etc. are now begining to make important contributions. Here, we can't predict exactly what solutions are going to be found, how one will find one's way further through the tunnel of strong interaction physics, but certainly it's my belief that the devices that we've been hearing about during these past few days are going to make vital contributions. I think also, we shouldn't forget as we pursue this path to keep an eye open for other applications of these devices, which are certainly very powerful systems.

As "PEPR bubble chamber specialists", it's clear that we are all in debt to Irwin and his collaborators over the years for showing that the use of CRT's is not only intuitively sensible but in fact, which is much more difficult, in fact possible. I think it is here that POLLY can be added to the list. It's also characteristic, that although as proponents of particular designs, we all enjoy some healthy competition, nevertheless, everyone, I think, has certainly been able to learn from the experiences of others, and here it is relevant to say that this applies to the whole history of this particular branch of the technique, including Spiral Readers and all sorts of other developments. There has always been a very free exchange of information with those of us who start latest. We are always able to profit from, and it is certainly true of Oxford, we've benefitted considerably from everything that's gone before us.

One feature in this connection that I couldn't help noticing, is that many groups represented here are using Astrodata PEPR devices comercially built, and they all seem to be working extremely reliably. I didn't notice that anybody had come across any problems with these PEPR's. I think this says a lot for the high standard of the work done by Astrodata, and also very particularly of the very high order of professional skill and expertise shown by Bernie Wadsworth and his collaborators at M.I.T. in drawing up the specifications so well and supervising the whole project. I am sure, although we at Oxford are not members of this group of Astrodata users, perhaps we can also appreciate just what a triumph that was. I must say that we were very skeptical at the beginning of that project. One of the reasons we didn't do it was because we couldn't really see how it could work so well as it undoubtedly has.

So now, on your behalf, may I thank all of those who have worked to make this meeting run so well and so enjoyable, most of the organizing work has been done by Terry Watts and we are very grateful to you Terry, and all your colleagues on the organizing committee and particularly to all your most beautiful helpers, who are also very efficient. It has seldom been so pleasant to go and have my onward ticket confirmed. I am sure everybody would agree, in spite of some early mutterings when one saw the map about distance out from the center of civilization, that since the very first moment we arrived, everyone has been thoroughly delighted with the choice of Endicott house for the place of this meeting. It is a glorious place and has contributed to the pleasant and happy atmosphere in which all the discussions have taken place. We should thank the staff of Endicott house for making this a pleasant stay and M.I.T. for letting us use it. I mustn't forget the memorable dinner that we had, and as Peyrou mentioned, a classically good Bordeaux.

Finally, as some of you may have noticed, this is the second time in a few weeks that a person from Oxford has been the last Chairman at a Cambridge meeting, no doubt this is something to do with the fact that there are several Cambridges but only one Oxford. However, it does begin to appear like a conspiracy. In order to avoid this becoming a very unfortunate tradition maybe we should reverse the roles of host and visitor.

LIST OF PARTICIPANTS

W. W. M. Allison	ANL
F. Anelli	Bari
Pierre Antoine	Institut de Physique Nucleare, Paris
J. Ballam	SLAC
D. Ballantyne	MIT
P. L. Bastien	Univ. of Washington
H. Baumel	MIT
A. Bettini	Padova
Gene Binnall	LRL, Berkeley
D. Bogert	Yale
J. E. Brau	MIT
A. E. Brenner	Harvard
D. Brick	MIT
C. B. Brooks	Oxford
John Brown	SLAC
R. Budde	CERN
R. Caw	Johns Hopkins
S. Centro	Padova
G. Charlton	ANL
C. Chatzky	Johns Hopkins
W. Chien	MIT
M. K. Choe	MIT
B. Cox	Johns Hopkins
Geoff Crask	Astrodata
M. Cresti	Padova
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P. T. Demos	MIT
D. Drijard	CERN
R. Engler	MIT
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T. H. Fields	ANL
D. Fong	Brown
C. Forte	MIT
L. R. Fortney	Duke
Jerry Fram	Maryland
R. G. Glasser	Maryland

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Raymond Hanft	NAL
J. F. Harris	Oxford
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S. Heiselman	MIT
J. Hendriks	Nijmegen
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J. Kim	Harvard
V. Kistiakowsky	MIT
R. Knop	Rutgers
E. L. Koller	Stevens Institute of Technology
D. Kropp	Heidelberg
J. Lach	NAL
D. Lord	CERN
H. J. Lubatti	Univ. of Washington
Peter Lucas	Yale
T. Ludlam	Yale
W. Lund	Yale
John Maloney	Brandeis
P. Marcato	MIT
H. J. Martin	Indiana
R. McIlwain	Purdue
D. C. Miller	MIT
J. E. Mott	Indiana
Judith Mueller	Harvard
J. H. Mulvey	Oxford
A. Nakkasyan	MIT
A. Napier	MIT
N. Oberlack	Heidelberg
B. Orum	Astrodata
T. C. Ou	MIT
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S. Pascaud	Orsay
C. Peyrou	CERN
R. J. Plano	Rutgers
M. Plesko	MIT
I. A. Pless	MIT
Vera S. Pless	Air Force Cambridge Research Labs.
E. Quercigh	CERN
C. J. Robinson	Glasgow
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E. Sartori	MIT
G. Schulze	MIT
A. M. Shapiro	Brown
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L. K. Sisterson	Harvard
P. F. Slattery	Rochester
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R. Socash	DEC
H. D. Taft	Yale
A. Thorndike	BNL
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Helmut Walz	SLAC
G. Wolsky	Tufts
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