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Status of the Evidence for a Magnetic Monopole

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In August, 1975, E.K. Shirk, W.Z. Osborne, L.S. Pinsky and I reported evidence[1] that we had detected a moving magnetic monopole, using a balloon-borne array of track detectors shown in Fig. 1. The Conference organizers have asked me to discuss the status of our evidence. I have agreed to do so, somewhat reluctantly since much remains to be done before the measurements of the accompanying ultraheavy cosmic rays are completed with all three types of detectors.

Our reasoning was straightforward. The very high, roughly constant ionization rate inferred from track etch rate measurements in the stack of Lexan detectors implies passage of a minimum-ionizing particle more highly charged than any known nucleus, yet the Cerenkov film detectors indicated a velocity less than ~0.68 c and the size of the track in the nuclear emulsion indicated a velocity ~0.5 c. At this velocity the ionization rate of a highly electrically charged particle would have changed dramatically with pathlength unless its mass to charge ratio were far greater than that of a nucleus.

It has been known for many years that the ionization rate of a magnetic monopole is roughly independent of velocity. Bauer[2] and Cole[3] showed that the rate is given by replacing the quantity $Z_e e$ in the Bethe-Bloch equation with $g\beta$, the product of magnetic charge and velocity. (Z_e is the effective charge.) Assuming the sensitivity of our Lexan detectors to be the same as that of Lexan used in previous balloon experiments[4] and in a Skylab cosmic ray experiment,[5] we found that $Z_e/\beta \approx 137$ or that $g \approx 137$ e. The fit to the expected behavior of a monopole with twice the Dirac charge (and equal to the Schwinger

charge) was so close that we were absolutely convinced of the validity of our evidence and decided to publish before carrying out the calibrations and analysis of the other events in the detector, which we knew would take nearly a year. .

The Lexan data single out the monopole candidate as not just the end member of a smooth distribution of heavily ionizing cosmic ray nuclei but as a unique particle with qualitatively different behavior. This is obvious in Fig. 2, which shows the variation of track etch rate with depth in the Lexan stack for the monopole candidate and for the other particles found in the flight. Because etch rate is an increasing function of ionization rate, the curves in Fig. 2 are somewhat like Bragg curves. The data for the monopole candidate fit a horizontal line at an etch rate of ~2.9 μm/h, far above the other horizontal lines between about 0.3 and 0.8 μm/h that correspond to minimum-ionizing ($\beta \gtrsim 0.95$) nuclei with Z up to ~83 that were detected on the flight. Only particles with steeply rising etch rate curves, corresponding to slowing nuclei of lower velocity, reach etch rates as high as that of the monopole candidate. In none of our previous ultraheavy cosmic ray experiments had we seen events with constant etch rates higher than 1 μm/h.

After publishing the Letter reporting our evidence, we found that the Lexan used in that flight was slightly different in composition from that used in our previous experiments. It did not contain the trace of a UV-absorbing dye that is normally added to Lexan to retard its deterioration in sunlight. Instead of increasing with Z_e/β as $(Z_e/\beta)^{\alpha}$, with α in the range 3.5 to 4 as had been found

previously,[4,5] the etch rate behaved as

$$v_T = 0.900(Z_e/90.18\ \beta)^{5.07}\ \mu m/h \qquad (1)$$

This required a downward revision of Z_e/β from ~137 to ~114. The higher value of the exponent meant that this Lexan was capable of detecting smaller changes in ionization rate than could the previous Lexan. Our first reaction was one of dismay that the revised ionization rate seemed to be significantly lower than expected for a monopole of strength 137. Steve Ahlen, a student of mine, then found that in a condensed medium the ionization rate of a monopole is not a constant but decreases continuously as it slows down. The old prescription[2,3] for finding dE/dx by replacing Ze by $g\beta$ in the Bethe-Bloch equation neglected the density effect. Using a restricted energy loss model of track formation, Ahlen[6] derived the curves in Fig. 3. The track etch rate in Lexan for a monopole of strength 137 e and velocity $\beta = 0.5^{+0.1}_{-0.05}$ is equivalent to that of a relativistic nucleus ($\beta \simeq 1$) with $Z_e = 121 \pm 2$. In view of the approximations used in Ahlen's treatment and of the crudity of the restricted energy loss model, this number is consistent with our revised estimate of $Z_e/\beta \approx 114$ for the monopole candidate. Reasoning from the observed numbers $Z_e/\beta \simeq 114$ and $\beta = 0.5^{+0.1}_{-0.05}$, we now would infer a magnetic charge $g = 130^{+2}_{-4}$, with an additional uncertainty of at least ±5 charge units quoted in Ahlen's paper.

Criticisms

We expected and got a lively response to our paper.[6-19] Some authors have critized our evidence and offered alternative explanations;[7-10] some have derived constraints on the properties or mode

of production of the proposed monopole;[11-15] some have dealt with monopoles in general;[6,16,17] one reports a method of distinguishing a monopole from a nucleus by adding a linearly polarizing paint to a Cerenkov film detector;[18] and one reports a new negative search.[19] At the present stage of our calibrations, some of the criticisms of the evidence have become invalid, but some cannot be fully assessed until we are further along.

We and all our critics recognize that the constant, high ionization rate, together with the low velocity, would make a mundane explanation of the event impossible if the measurements were beyond reproach. Here are the criticisms:

1. There is a "glitch" in the Lexan data (see Fig. 4) that suggests that the ionization rate suddenly decreases and then increases gradually as would be expected if a fast nucleus underwent a nuclear collision in the Lexan, fragmenting into a slightly lighter nucleus.

2. The two data points in the upper sheet of Lexan can be rejected on the grounds that that sheet was separate and may have experienced a different mechanical, thermal and chemical history from the remainder of the stack.

3. The black points and triangles in Fig. 4 were obtained in sheets processed in two different etch tanks. A calibration was done only for the sheets corresponding to the black points; therefore, the triangular points can be rejected.

4. The method of velocity determination based on the track profile in nuclear emulsion has not been demonstrated to work. Further, in P.H. Fowler's model of track structure,

it would not be possible unambiguously to distinguish the radial dependence of track structure of particles with $\beta \gtrsim 0.45$. Therefore, the information from the nuclear emulsion should be disregarded.

5. The thickness of material between the upper Lexan sheet and the main Lexan stack was labeled incorrectly in the paper. The actual

thickness was less, reducing the difficulty of accounting for the data by a fragmenting nucleus.

Taking these points into account, the critics "explained" the event by a nucleus with $Z \approx 78$ or 79 that passed through the Cerenkov detectors with a velocity ~0.68 to 0.70 c, just below the velocity at which Cerenkov light would have produced a detectable number of photons. In order to maintain the right average ionization rate, the nucleus had to fragment twice in the main Lexan stack, losing about two charges each time. The second fragmentation is supposed to have occurred at the glitch in the data; the first fragmentation is not visible in the data.

6. To these published criticisms I shall add one of my own. Though the Cerenkov film technique has been discussed in detail in Pinsky's thesis[20] and measurements have been made of Cerenkov light images produced in the film by a few ultraheavy cosmic rays in a previous balloon flight,[4,20] the technique requires very exacting performance of Kodak's fastest experimental film and needs to be tested thoroughly on the ensemble of particles that include the monopole candidate.

The Thickness of the Stack

Not only did we overestimate the thickness of material between the upper Lexan and the main Lexan stack, but we made a highly schematic drawing of the detector assembly that omitted two thin Lexan sheets, one of the Cerenkov detectors, a thin emulsion, a cellulose triacetate sheet, two Mylar sheets and the details of the layers of opaque wrapping paper around the emulsion and Cerenkov detectors. We simplified the drawing in order to emphasize the main features of the experiment within the spatial confines of a Letter. Figure 1 of the present paper gives a more detailed breakdown of the

stack showing all Lexan sheets, both Cerenkov detectors, the main emulsion, and the correct thicknesses in g/cm^2 Lexan equivalent, but still in somewhat simplified form. In Fig. 2 of ref. 1, reproduced here as Fig. 4, we took the thickness of Cerenkov detectors, emulsion, and associated wrapping material to be 0.625 g/cm^2, whereas the correct thickness should be 0.347 g/cm^2 Lexan equivalent. Referring to the correct Fig. 1 of the present paper, this material extends from the depth 0.039 g/cm^2 to 0.386 g/cm^2. The upper triangular data point in Fig. 4 corresponds to sheet 6. It was plotted at ~0.74 g/cm^2 but should be at 0.462 g/cm^2. All lower points in that figure will appear at the proper depth if 0.278 g/cm^2 is subtracted. Our overestimate of the stack thickness is equivalent to a change in velocity of ~0.02 c for a nucleus with Z ~ 78 and an initial velocity of ~0.68. For an initial velocity of 0.73 c, it is equivalent to a change in velocity of only 0.015 c. As we shall see in the next section, when all the Lexan data unjustifiably omitted by Alvarez are included (having now been calibrated), they rule out fragmenting nuclei with velocities as high as 0.74 c. Whether one starts with a nucleus at $\beta = 0.68$ or 0.70 is thus irrelevant, and the error in stack thickness is unimportant provided either the emulsion or Cerenkov detectors can rule out velocities appreciably higher than 0.74 c.

The next four sections include a discussion of the remaining criticisms, which must be shown to be invalid before worrying unduly about other difficulties such as the negative results of other monopole experiments of much greater collecting power.

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Data and Calibration of the Lexan Detectors

The principles and applications of nuclear tracks in dielectric solids are treated in a recently published book.[21] Of all track-recording solids, Lexan plastic is the kind most used for identifying charged particles. Because of its low cost, high resolution, and insensitivity to lightly ionizing particles, it is ideal as a detector

of large collecting power to study the rare, ultraheavy cosmic rays and to search for hypothetical, heavily ionizing particles. In a solution of a suitable chemical reagent, material along the trajectory of a heavy particle is etched out at a rate that depends on the ionization rate, leaving cone-shaped etch pits whose lengths can be measured in a microscope. The track etch rate, v_T (defined as etch pit length divided by etch time), increases as some power of Z_e/β that must be determined for each batch of Lexan and exposure history. A single expression fits values of v_T extending over at least three orders of magnitude for $Z \gtrsim 20$ and $\beta \gtrsim 0.2$.

Figure 5 illustrates schematically how we determined the two constants in the power law relation for v_T. A scanning criterion was adopted that favored the selection of events with $20 \lesssim Z \lesssim 30$. Because of the pronounced cosmic ray abundance peak at $Z = 26$ (iron), the measurement of 50 to 100 events, each comprising several pairs of etch pits in consecutive Lexan sheets, sufficed to define a surve of etch rate vs. residual range for Fe. In this short account we show only the result, a curve labeled "Fe calibration." To first order, this curve, together with a table of range-energy relations, enabled us to determine both constants in eq. 2. The density of stopping Fe nuclei was sufficiently high that we were able to carry out the calibration in the very sheets containing the monopole candidate. The criticism in point 3 is invalid because we calibrated the sheets etched in both tanks with Fe tracks and found the same values for the constants in eq. 2 for both etchings.

Out of some 600 candidates found in a stereomicroscopic scan of the entire nuclear emulsion, we have thus far verified that 64 of

them have $Z \gtrsim 40$ and we have measured their etch pits in the Lexan sheets. Fourteen of them came to rest in the Lexan stack, producing tracks with extremely high etch rates near the ends of their ranges. Data for four stopping particles are shown in Fig. 5. The requirement that the data for these 14 particles of known range have the correct slope on the graph of v_T vs. range is a stringent check on the exponent in eq. 1.

We searched through the data for the 64 events with $Z \gtrsim 40$ for evidence of Lexan sheets with higher or lower sensitivity than given by eq. 1. We found that sheet 2 (in the notation of Fig. 1) was systematically only about 0.94 times as sensitive as the sheets in the main stack. However, the data in sheet 2 showed no larger dispersion than did data for sheets in the main stack, so that the criticism in point 2 is invalid.

In order to increase our lifting power, we flew part of the stack (10 m^2) on September 18, 1973, and 20 m^2 of the stack on a second balloon launched on September 25, both from Sioux City. Both portions stayed at float altitude (3 g/cm^2 and ~4.5 g/cm^2 respectively) for 60 hours. Our calibrations show that both portions have the same sensitivity.

Figure 6 shows the calibrated Lexan data for the monopole candidate. The data in sheet 2 are raised by the factor $(0.94)^{-1}$ and given error bars that represent the standard deviation about the factor 0.94 for this sheet based on the measurements for all 64 cosmic rays. No data exist for sheets 5 and 12, which had been etched for a long time (160 h) to form holes that allowed the event to be initially found by ammonia scanning. We initially set aside sheets 1,3,4, and 35, but

after our published evidence had been criticized we etched these sheets in a third tank and calibrated their sensitivity individually using the 64 cosmic rays. The results for the monopole candidate, with error bars, are shown in Fig. 6. The main Lexan stack, comprising sheets 4 through 35, was bolted together as a unit. We found that the outer surfaces of the stack (top of sheet 4, bottom of sheet 35) were somewhat more sensitive than the inner surfaces, and a correction has been applied to those

two data points in Fig. 6. Not surprisingly, the thin Lexan sheets (1 and 3), having been manufactured in a different batch from the other sheets, required slightly different constants in the etch rate equation. A detailed account of the calibrations will appear in a future paper.

Figures 7 through 11 show various attempts to fit the Lexan data with fragmenting nuclei having initial velocities $\beta_i c$ (in sheet 1) ranging from 0.7 c up to 0.98 c. I used eq. 1 and a range-energy table to generate the curves, trying in each figure to minimize the square error by judicious choices of Z, β_i, and ΔZ. In Figs. 7 and 8 I worked backward from the glitch.

For each curve I have listed the statistic χ^2, the number of degrees of freedom, and the confidence level for the fit. To compute χ^2 one needs to know σ. I want to test the hypothesis that one of the curves in Figs. 7 through 10 gives as good a fit as the line of zero slope at the average etch rate 2.88 μm/h in Fig. 11. For the main Lexan stack (excluding sheets 4 and 35), assuming a normal distribution of measured etch rates about the average rate, I calculate a fractional σ of 0.0337. Including the separately determined σ's for sheets 1 to 4 and 35, I get a root mean square $\sigma_{rms} = 0.0356$ for the monopole fit. This is quite a reasonable choice; about half of the fractional σ's of the data from the optimum curves from eq. 2 for the 62 cosmic rays with $Z \gtrsim 40$ fall between 0.03 and 0.04. This procedure of course insures that $\chi^2/\nu \approx 1$ for the line in Fig. 11 and thus avoids the error common in particle physics experiments of underestimating σ. (See Rosenfeld's discussion[22] of the Particle Data Group's use of a Scale Factor to inflate the quoted σ's in experiments so that $\chi^2/\nu \approx 1$.)

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We now wish to find the confidence levels associated with the larger values of χ^2 that I calculate for the curves in Figs. 7 to 10. The F-test is suited for comparing the variances of two curves through a set of data. The statistic F is defined as the ratio of reduced chi-squares for the two curves. Based on the F-test, in the figures and in column 6 of Table 1 I have listed the confidence levels that the curves in Figs. 7 to 10 are as good a fit to the data as is the straight line in Fig. 11. The values are more conservative (higher) by about a factor 10 than would be the values computed with a χ^2 test.

The doubly fragmenting nucleus with $Z \approx 78$ hypothesized by Alvarez[9] and by Fowler[10] has been widely publicized. I believe the Lexan data rule out that hypothesis and also the one shown in Fig. 8. When the number of degrees of freedom is very large, a reduced χ^2 as low as 2 or 3 leads to extremely low confidence levels. Figure 12, which compares the error distributions for the curve with two interactions in Fig. 7 and for the straight line fit in Fig. 11, makes the point quite clearly. In the case of the fragmenting nucleus, not just one or two but many points lie outside the Gaussian error envelope derived from the σ_{rms} of 0.0356 for the straight line fit. The Lexan data alone cannot rule out a fast nucleus of uranium, curium, or a superheavy element (Figs. 9-11). Only if the emulsion or Cerenkov measurements show that the velocity could not have been as high as 0.82 c or 0.86 c, respectively, can these scenarios be ruled out.

Fragmentation and the "Glitch" in the Lexan Data

In computing the overall confidence level for the fragmenting nuclei in Figs. 7 to 9, we must consider not only the fit to the Lexan

data but also the product of two quantities: the probability of a given number of fragmentations with just the right decrease of charge to follow the Lexan data, and the total number of nuclei in all balloon flights that entered the stack with initial ionization rates and velocities that could have simulated a monopole if the fragmentations occurred.

Alvarez[9] assumed "several hundred" nuclei and a total probability of order unity for a doubly fragmenting platinum nucleus to have been seen in some flight. Fleischer and Walker[8] did a more realistic calculation. They considered nuclei with three possible velocities at the emulsion--0.7 c, 0.65 c and 0.6 c--and concluded that at the highest velocity a fragmenting nucleus would be a reasonable interpretation, whereas at the lowest velocity only a monopole could account for the data. To fit the data in the main stack (ignoring the data in the upper sheet) they assumed 2, 3, and 8 fragmentations, each with $\Delta Z \approx 2$ to 4, for $\beta = 0.7$, 0.65, and 0.6, occurring with probabilities they calculated to be $\sim 10^{-3}$, 2.4×10^{-5}, and 7×10^{-15} per incoming nucleus. For the three cases they assumed 14, 13, and 8 nuclei in the right range of Z and β and arrived at total probabilities of 0.017, 3×10^{-4}, and 6×10^{-13} for $\beta = 0.7$, 0.65, and 0.6.

I have followed the procedure of Fleischer and Walker to calculate the numbers in column 5 of Table 1, making two changes to make the calculations more realistic.

(1) Shirk and I examined all previous ultraheavy cosmic ray experiments to see how many nuclei were detected in a suitable range of Z and β. Flights launched from the southern U.S. could collect

none because the geomagnetic cutoff rigidity excludes nuclei with $\beta \lesssim 0.8$ to 0.85. Flights from the northern U.S. fall into two categories. Those by the Bristol-Dublin collaboration employ very thick stacks (~5 g/cm^2) with enough material to detect velocities less than ~0.85 c with no difficulty. In our Minneapolis experiment[4] we detected no particle in a suitable range of Z and β. In our Skylab experiment[5] we detected one lead nucleus (Z = 82) with $\beta = 0.68$ and with Z/β increasing from 121 to 153 through the stack. In our Sioux City flights we detected two nuclei with initial Z/β near that of the monopole candidate. Their etch rate curves are labeled in Fig. 2. One of them actually fragments, but with a loss of 34 charges. Figure 13 shows the data for that event, plotted with the same scale as in Fig. 6 for the monopole candidate. Thus, instead of the 13 candidates assumed by Fleischer and Walker, we use the observed number of four particles (including the monopole candidate) that should multiply the probability of a sequence of fragmentations by a single particle.

(2) I assumed the same fragmentation mean free path as did Fleischer and Walker, but with a window in ΔZ that was two instead of three units wide.

Is the glitch in the Lexan data an "obvious fragmentation," as claimed by Alvarez? If it were, then the above estimates are irrelevant, this paper is irrelevant, and I would immediately go back to the research I was doing before last July. ("Monopoles don't fragment.") Without having seen other Lexan data, it is quite natural to interpret the glitch as a sudden loss of charge. However, correlated variations in etch rate occurring over several consecutive sheets are not uncommon. Some show upward glitches; most of them must be attributed to the

chemistry and physics of the plastic and of the etching process, not to nuclear or atomic processes. Figure 13 is an example of large fluctuations in the data that appear to the eye to be correlated.

In the course of our studies of ultraheavy nuclei we have seen four definite fragmentations with $\Delta Z \approx 3$ to 6 and another ten with larger ΔZ, including the one in Fig. 13. The data following the fragmentation have a shallower slope than those preceding the fragmentation, for the simple reason that a fragmenting nucleus loses charge and mass but continues on at about the same velocity and thus has a greater range and a smaller gradient to its Bragg curve than it would have had. The glitch in the data for the monopole candidate is different and unphysical in that the data following the step have a much higher slope than the data preceding the step.

Measurements and Tests of the Nuclear Emulsions

As early as 1969 W.Z. Osborne had the idea that a single layer of nuclear emulsion could be used to estimate both Z and β of a heavy particle, for velocities between ~0.3 c and ~0.7 c. As a first test of his method we exposed a stack of Lexan below a layer of emulsion in a spectacularly long balloon flight (14 days) launched from Minneapolis in 1970.[4] The results, though encouraging, have not been thoroughly analyzed until recently and have not been published even though the Lexan data were published several years ago.[4] It is thus true that the method must be regarded as untested. Here I give a brief account of it and show results for 32 cosmic rays with $Z > 50$ from the Minneapolis flight and for 77 cosmic rays with $26 \leq Z \leq 83$

from the Sioux City flights. The procedure in all the flights was to scan all the emulsions in a stereomicroscope at Houston, locating tracks with large core and halo radii (defined below) that might correspond to cosmic rays with $Z > 26$. Coordinates, azimuth and zenith angles, and core and halo radii were recorded and sent to Berkeley. We etched the Lexan sheets, followed these tracks until they either ended or penetrated the entire stack, and determined Z and β for the heaviest events and for a number of the Fe tracks.

In G-5 emulsion the track of a heavy nucleus consists of a solid core of fully developed silver grains, extending to a radial distance that depends on Z/β virtually independently of β, surrounded by a halo of silver grains whose density decreases radially until it is indistinguishable from the background grain density. The radial distribution of silver grains is determined by the energy and angular distribution of δ-rays, which depend on Z and β of the incoming particle, and by the radial transport and energy deposition of these δ-rays. Osborne has used the model of Katz and co-workers to compute the probability of grain development as a function of Z, β and radial distance, using the Mott cross section instead of the less accurate Rutherford cross section. Figure 14 shows a set of Osborne's radial profiles for various velocities at a constant value of $Z/\beta = 114$ pertinent to the monopole candidate. The probabilities corresponding to an opaque core and to the background gray level are marked. For higher or lower Z/β the curves move up or down.

It is probably fair to say that the dependence of core radius on Z and β is uncontroversial, because at distances of less than a few microns from the particle's trajectory most of the blackening is caused

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by electrons of low energy, for which the assumption of diffusive transport due to the intense multiple Coulomb scattering in nuclear emulsion is valid. The dependence of core radius on Z and β has been studied in a series of papers[24] by a Swedish emulsion group, who find that the model of Katz and co-workers fits their measurements of cosmic ray track widths over a wide range of β and charges up to 26.

Figure 15 shows our measurements of core radius, made by eye with a reticle and an oil immersion objective, as a function of Z/β. Here β refers to the velocity at the emulsion as determined from the value of Z and β measured for the same event in the Lexan stack. In agreement with the Swedish group, we find a pronounced zenith angle effect: steep tracks have an apparently larger core width than do shallow tracks with the same Z/β. For the extremely heavily ionizing events we have studied, two effects may contribute. (1) When looking down a nearly vertical track, it appears black out to a greater distance, corresponding to a smaller probability of grain development (note the curves in Fig. 15 corresponding to probabilities of 0.2, 0.3, and 0.4), than does a shallow track. (2) During fixing, the undeveloped silver halide grains are removed, the emulsion shrinks in thickness, and the solid mass of silver grains in the core, being incompressible, may be displaced outward for a very steep track more than for a shallow track.[7]

The monopole candidate, which came in at a zenith angle of 11°, is plotted in Fig. 15 with the same (but enlarged) symbol as are other events with zenith angles from 0 to 20°. From the fact that it follows the trend with Z/β of the other steep events (near the curve P = 0.2), one can say that its core radius of 6 μm is consistent with its having a value Z/β between ~100 and ~140. Thus, I conclude that the portion

of nuclear emulsion traversed by the monopole candidate was neither anomalously sensitive nor insensitive compared to the other emulsions in the flights.

At low probabilities of grain development, corresponding to transport and energy deposition of δ-rays at distances of many tens of microns out from the trajectory, the shapes of the curves in Fig. 14 are disputed. Using a simple diffusion model and additional simplifying assumptions, Fowler[10] was able to integrate his expression for the energy deposition by δ-rays as a function of Z, β, and radial distance. For values of $\beta \gtrsim 0.45$ he has claimed that his curves are so close together that one can tell nothing about the velocity of the particle (point 4 of the criticism). They are so different from Osborne's curves that at least one of the two models must be wrong. Fowler's statement that Osborne's method cannot work at $\beta \gtrsim 0.45$ has been widely publicized and has been cited by Alvarez[9] as his justification for rejecting our emulsion evidence that $\beta \simeq 0.5$ for the monopole candidate.

Osborne has pointed out that Fowler's own published data[25] on radial profiles of ultraheavy cosmic rays are inconsistent with his diffusion model. Alvarez has privately expressed doubts to Fowler that his random walk model is valid for the more energetic electrons. Ray Hagstrom (LBL) has shown that all of Fowler's simplifying assumptions act in the same direction to underestimate the velocity-dependence of the radial distribution of the energy deposited by fast electrons. He concludes that Fowler's model is invalid, and he is developing his own model of track profiles.

Of course, ultimately the test of correctness of a model is the extent to which it agrees with experiment. Osborne is now testing a computer-driven image-recognition system that records the positions of all silver grains outside the core region and calculates a radial profile. Until we have such profiles for the events in the Sioux City flights, we must use measurements made by eye. The eye cannot recognize quantitatively the probability of grain development, P, but it can estimate the radial distance at which the halo of grains around a track fades into the background of randomly developed grains. The background typically corresponds to $P \approx 10^{-3}$. For the Minneapolis and Sioux City flights we use the value $P = 1.6 \times 10^{-3}$ as the level at which the eye sees the "edge" of the halo.

Independent observers at Houston and at LBL have measured the halo radius of the monopole candidate, obtaining values ranging from 50 to 55 μm. These values imply a velocity ~0.5 c if the curves in Fig. 14 are correct, if the dispersion about the expectation value is small and if the eye correctly locates the radius at which $P = 1.6 \times 10^{-3}$.

To assess these questions, in Fig. 16 I have plotted Osborne's observed halo radius as a function of the value calculated from the model, using as inputs the values of Z and β (at the emulsion) determined in the Lexan stack and $P = 1.6 \times 10^{-3}$. <u>I believe this figure contains the most important new results since our original publication.</u>

Let us examine this comparison of experiment with theory for any trends. First of all, I find that the "errors" are uncorrelated with zenith angle. One of Fowler's[10] criticisms of Osborne's model was that, due to the escape of high-energy δ-rays from the surface of an emulsion of finite thickness (the transition effect), the measured halo radius

should depend on zenith angle. Experimentally we are unable to detect such an effect. (Recall that we did for the core radius.)

Second, I find that the distributions of errors for the Minneapolis and Sioux City flights are indistinguishable. This is a reassuring result, showing that data taken four years ago, long before the monopole candidate was found, follow the same trend as the recent data, using the same value $P = 1.6 \times 10^{-3}$. Let me point out that the measurement most susceptible to subjective judgment, relying wholly on the human eye, is made first, without any information from the Lexan, followed by a set of ~60 etch rate measurements in the Lexan.

Third, notice the correlation of errors with velocity. Events with $\beta \gtrsim 0.7$ lie within a tight band, about ±10 μm wide, with a sharp edge at low observed halo radii, below which there are no stragglers. Events with lower velocity tend to lie higher and show a large dispersion toward positive errors. Consider, for example, the shaded area labeled "Fe." A conscious effort was made to reject the 10^5 to 10^6 Fe tracks in order to concentrate on the tracks of rare, heavier nuclei, yet many of the events with halo radii between 30 and 50 μm, thought to have $Z \gtrsim 35$, turned out to be Fe when measured in the Lexan. They tended to be at small zenith angles, which meant that their core radii were fatter than for shallow tracks (Fig. 15). This, together with their larger than average halo radii, caused them to be recorded as candidates for $Z > 35$.

The large positive errors for the particles with lowest velocities cannot be strictly a physiological defect of the human eye. The event with a halo radius of 105 μm and a calculated radius of only 58 μm was

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measured with Peter Fowler's photodensitometer in Bristol and verified to have a light-absorbing halo extending out to more than 100 μm.

One or both of two possibilities seem likely: (1) The theory underestimates the average radial distance to which electrons ejected by particles with $\beta < 0.7$ diffuse. (2) The theory does not take into account fluctuations in the distance diffused. It seems intuitively reasonable that very steep radial profiles for low β (Fig. 14) are more vulnerable to positive fluctuations in radial distance by δ-rays than are the shallow profiles for high β. The essence of diffusion is to reduce concentration gradients. It would be very unphysical to have a large dispersion toward lower observed halo radii. Inward fluctuations of the few electrons at the edge of a halo would be swamped by outward fluctuations of the more numerous electrons from regions closer to the core. Note that a complete radial profile would not be so sensitive to fluctuations in diffusion distance of those few electrons that travel to the edge of the halo. This is so because fast electrons cause the greatest blackening near the end of their range, and the distribution of δ-ray energies decreases as $(\text{energy})^{-2}$. A quantitative model of these effects is being developed by Hagstrom.

Where should the point for the monopole candidate appear in the figure? The horizontal lines at an observed halo radius of ~55 μm indicate the values calculated for the various nuclear scenarios shown in Figs. 7 to 11 and for a monopole of velocities 0.45 c to 0.55 c. Recall that the Lexan data are incompatible with fragmenting nuclei with Z = 76 to 83. A nuclear explanation of the event would require an extremely large negative fluctuation in electron diffusion distances not exhibited by any of the data in Fig. 16. The emulsion evidence provides strong support for the claim that the event is unique. It would appear to be

compatible, within the framework of Osborne's model, with a monopole of velocity ~0.45 to ~0.6 c.

We now need to assess the confidence level that the measured halo radius is compatible with a nuclear interpretation. A complete physical model would allow a realistic error distribution to be computed, one that is clearly asymmetric about a 45° correlation line. Even at this stage we could construct a Gaussian distribution of errors that would clearly err on the conservative side because of its symmetric shape. I shall be even more conservative and say that the hypothesis that the event was a nucleus has been tested at the level N^{-1}, where N = 110, the number of events studied. In column 7 of Table 1 I assigned a confidence level "less than 10^{-2}" to the consistency of the emulsion measurement with the various nuclear hypotheses. A confidence level based on the magnitude of the negative error would appear to be far lower.

Measurements and Tests of the Cerenkov Detectors

Figure 17 illustrates the principle of the Cerenkov method developed by L.S. Pinsky.[20] A particle with $\beta > \beta_c = n^{-1}$ (where the refractive index $n \approx 1.51$) generates a cone of Cerenkov photons along its path in a plastic radiator coated on the bottom by a layer of Eastman Kodak film 2485, the fastest film currently available. For the simplest case of vertical incidence this light falls on a circular area of radius $T \cdot \tan \theta_c$, where T = radiator thickness and $\theta_c \equiv \arccos (n\beta)^{-1}$ is the angle at which the photons from each element of pathlength are emitted.

At a radial distance r, the number of photons per unit area that

reach the film is given by

$$I(r) = \frac{\alpha Z^2 \Delta k (1-n^{-2}\beta^{-2})}{2\pi r \tan \theta_c} = \frac{\alpha Z^2 \Delta k \sin \theta_c \cos \theta_c}{2\pi r} \qquad (2)$$

where α = fine-structure constant and $\Delta k = 2\pi[\lambda_2^{-1} - \lambda_1^{-1}]$, the band pass for a particular film and radiator.

For an extremely heavy nucleus the region in which to look for the Cerenkov image is pinpointed by the solid black ionization spot that fills the depth of the 12 μm film and has a radial extent from a few to 30 μm, depending on Z/β. If this ionization spot cannot be found, it is difficult to locate the Cerenkov halo, because the coordinates of the track are precise only to a few mm. The Cerenkov halo has a much lower grain density than the ionization spot. Within a series of rings around the ionization spot Pinsky counts developed grains, corrects for the background grain density, and computes $I(r)$. In favorable cases (large Z, intermediate β) he sees a sudden drop in intensity that directly gives him the Cerenkov angle and therefore the velocity. If β is very high, the angle will be so large that $I(r)$ will decrease to the background level at a radial distance less than $T \tan \theta_c$. He can still estimate β from the radial variation of $I(r)$, solving eq. 2 for $\sin \theta_c \cos \theta_c$, which is single-valued for $\theta_c = 0$ to 45°, corresponding to velocities from 0.66 c to 0.94 c, and is roughly 0.5 for higher velocities.

Table 2 summarizes the measurements Pinsky has made on detectors from the Minneapolis and Sioux City flights. Because the Minneapolis payload crashed and was dragged miles across country, some of the

Cerenkov films were destroyed. Data from the usable films are shown. The results from this flight were encouraging. Where he saw no Cerenkov image, the velocity determined in the Lexan was consistent with the inequality $\beta < 0.68$ except for one particle with $Z = 64 \pm 6$, $\beta = 0.74 \pm 0.04$. In the ten cases where he saw a Cerenkov image, his estimate of β was consistent with that from the Lexan.

At this writing only a few observations have been made of the detectors from the Sioux City flights. No quantitative determinations of β have been made. Though the qualitative observations of Cerenkov images in both films for 14 events in Table 2 are encouraging, I believe it is too early to use the absence of Cerenkov images at the monopole candidate to further lower the confidence level for a nuclear interpretation. The detection of a Cerenkov image requires establishing the existence of a small signal above a large background of developed grains. To make quantitative profiles of grain density around the ionization spots of the events, Pinsky plans to use the same computer-operated image-recognition system Osborne will use on the emulsion.

The Cerenkov data will be most convincing in assessing confidence levels for the nuclei with largest Z and β. At a given radial distance in the Cerenkov film, the photon intensity for the three nuclear candidates with $Z = 92$, 96, and 112 would exceed that for a nucleus with $Z = 65$ and $\beta = 0.7$ by factors of 3.1, 3.5, and 4.8 respectively. From the qualitative results in Table 2 it appears that signals from nuclei with $Z \geq 65$ at $\beta \geq 0.7$ are detectable. The response curve of Kodak film 2485 is such that one would expect signals greater than three times the minimum detectable signal to be impossible to miss unless one argued

that the film was locally damaged or locally abnormally insensitive. The existence of two independent Cerenkov radiator-film combinations, each in its own protective wrapping, would require a critic to argue that both films were locally damaged or locally abnormally insensitive. The Cerenkov film thus provides a constraint that complements the constraint imposed by the Lexan data.

Discussion

Table 1 gives, I believe, a conservative view of the status of our work. The last column is

simply the product of the numbers in the previous three columns. I use the undefined term "figure of merit" to warn the reader that one should not literally interpret the number as a probability or an overall confidence level. It is a convenient way of summarizing the relative merits of the various nuclear scenarios.

The rows labeled "hypothetical particles" indicate that there are two classes of particles that are equally consistent with all the data. A monopole has the attractive features that the charge of $g \approx 130$ e inferred from our data (if $\beta \approx 0.5$) is consistent with the predicted value $g = 137$ e, and the lower limit of 875 amu for its mass, inferred by Ahlen[6] from the absence of a negative slope to the Lexan data, is consistent with 't Hooft's theoretical model[26] in which monopoles exist with mass $\approx 137\ M_W \approx 10^4$ amu, where M_W is the mass of the intermediate vector boson. A monopole has the unattractive features that it has not been detected in experiments with up to a million times greater collecting power, and it is hard to account for its low velocity without rather contrived assumptions. Most previous experiments would have missed seeing monopoles if they have masses greater than $\sim 10^4$ amu, which is consistent with our lower limit. For example, the collecting power of the lunar experiment of Alvarez and co-workers[27] decreases rapidly for monopoles of large mass, which bury themselves at great depths instead of in the shallow subsurface soil. Let it suffice to say that the history of physics shows that theorists have a way of explaining apparent conflicts with nature if sufficiently compelling experimental evidence requires it.

We cannot rule out the second hypothetical particle, one with

electric charge given by $Q/e = \beta \cdot (Z/\beta) \simeq 0.5 \times 114 \simeq 60$. In order that its Bragg curve, at $\beta \simeq 0.5$, not rise any faster than the Lexan data permit, its mass must exceed ~2000 amu. Such a particle has the attractive feature that its flux does not conflict with flux limits set by other experiments that have sought highly electrically charged particles. Yock[28] has proposed that hadrons consist of "subnucleons" with large mass and strong electrical charge, bound by Coulomb forces. His heaviest subnucleon is consistent with the charge and mass that we require.

It seems conceivable that a "collapsed" or "abnormally dense" nuclear particle, as discussed by Bodmer[29] and by Lee and Wick,[30] might have a huge mass and a charge of ~60. Bodmer has pointed out that, if the potential well is deep enough, the state of lowest energy may be one in which some of the nucleons convert into neutral hyperons with their own Fermi levels, so that Z/A is far less than that for normal nuclei.

To bring this discussion back to reality, let me close by affirming what all scientists believe, that science advances by criticism, painful as it may seem to those on the receiving end. In the absence of strong criticism we might have pressed ahead with plans for a further series of balloon experiments, neglecting the critical measurements of the other events on the Sioux City flights. Through the efforts of Steve Ahlen, Ray Hagstrom, and others, we are learning more about the expected behavior of monopoles and about the capabilities of nuclear emulsions and Cerenkov film detectors. It is possible that, when we have generated radial profiles of all the tracks in the emulsion and made quantitative measurements

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of the Cerenkov images, the question of the uniqueness of this event may be settled. It cannot be proved to have been produced by a monopole, but if it can be shown at a high confidence level not to have been produced by any nucleus, future experiments of expanded scope will be justified.

Acknowledgments

I am indebted to my colleagues Edward Shirk, Zack Osborne, and Larry Pinsky for continuing the investigation of this event. The notoriety and pressure which have made it difficult for us to carry out our research with untroubled minds are a direct consequence of my own eagerness to announce what I thought was strong evidence for a monopole. Following my return from the Munich Cosmic Ray Conference, there was strong pressure to publish a retraction and not to carry out calibrations and further measurements. A number of friends, among them Sumner Davis, Ed McMillan, John Reynolds, Emilio Segré, and Charles Townes, supported me in my determination to complete the investigation with an open mind. I am grateful to Steve Ahlen, Brian Cartwright, Fred Goldhaber, Maurice Goldhaber, Ray Hagstrom, and Ed McMillan for numerous valuable ideas and suggestions. It was a great pleasure to have had several conversations with Paul Dirac while at the Conference.

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Table 1. Confidence Levels for Nuclear Explanations of the Monopole Candidate

Z	Mass (amu)	β	No. of frags.	Total prob. to occur in some flight	Conf. level for fit to Lexan data[1]	Conf. level for fit to emulsion data[2]	Figure of merit
				Nuclear Explanations			
76	192	0.70	2	2×10^{-3}	3×10^{-6}	$<<10^{-2}$	$<<6\times10^{-11}$
79	197	0.70	3	3×10^{-5}	10^{-5}	$<<10^{-2}$	$<<3\times10^{-12}$
81	205	0.74	2	2×10^{-3}	3×10^{-3}	$<<10^{-2}$	$<<6\times10^{-8}$
83	209	0.74	3	3×10^{-5}	10^{-2}	$<<10^{-2}$	$<<3\times10^{-9}$
92	238	0.82	1	10^{-1}	10^{-1}	$<<10^{-2}$	$<<10^{-4}$
96	247	0.86	0	1	10^{-1}	$<<10^{-2}$	$<<10^{-3}$
112	296	≥0.98	0	?	1	$<<10^{-2}$	$<<10^{-2}$
				Hypothetical Particles			
g/e=137	>875	~0.5	0	?	1	1	?
Q/e≈60	≳2000	~0.5	0	?	1	1	?

[1]Based on F-test. The χ^2 test gives ~10 times lower confidence level.

[2]Based on measurements of halo radii for 110 nuclei.

Table 2. Performance of Cerenkov Film Detector

Minneapolis flight (refs.4,20); single radiator and film			Sioux City flight (in progress); two separate radiator film combinations			
Z(Lexan)	β(Lexan)	β(Cerenkov)	Z(Lexan)	β(Lexan)	Image in 200 μm Cer. detector?	Image in 100 μm Cer. detector?
90±3	.70±.01	.720±.013	83	0.95	yes	yes
80±8	.76±.05	.701±.011	83	0.76	yes	yes
76±2	.79±.01	.829±.012	82	0.93	yes	yes
74±6	.72±.04	$.741^{+.028}_{-.022}$	82	0.66	yes	yes
>60	>.65	.684±.005	77	0.86	yes	yes
>60	>.75	.793±.005	77	0.77	yes	yes
>60	>.7	$.717^{+.039}_{-.026}$	76	0.93	yes	yes
>60	>.7	>.95	76	0.82	weak	weak
>60	>.75	>.95	75	0.68	yes	yes
>50	>.7	$.810^{+.124}_{-.074}$	74	0.73	yes	yes
80±3	.56±.01	no spot	68	0.71	yes	yes
69±1	.70±.03	" "	59	0.77	yes	weak
68±7	.70±.03	" "	58	0.70	weak	weak
66±6	.60±.02	" "	58	0.64	yes	yes
65±3	.60±.05	" "	49	0.62	yes	yes
>65	>.6	" "	81	0.60	no	no
64±6	.74±.04	" "	65	0.80	no	no
63±3	.70±.05	" "	62	0.67	no	no
63±2	.63±.04	" "	61	0.60	no	no
60±4	.69±.02	" "	61	0.75	no	no
58±3	.58±.02	" "	60	0.71	no	no
56±2	.53±.02	" "	57	0.67	no	no
55±5	.63±.03	" "	53	0.67	no	no
54±6	.67±.03	" "	52	0.70	no	no
52±3	.52±.02	" "	monopole candidate		no	no
51±2	.58±.01	" "				
50±4	.55±.02	" "				

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Figure Captions

Figure 1. Detector array (schematic) with depths in g/cm^2 Lexan equivalent.

Figure 2. Response curves of the majority of the ultraheavy particles from the Sioux City balloon flights. A few slow particles with very steep curves are not plotted.

Figure 3. Effect of a monopole of strength $g = 137\ e$ in Lexan detectors, calculated by S.P. Ahlen (ref. 6). Upper curve shows velocity-dependence of energy loss to electrons with less than 350 eV, which produces etchable tracks in Lexan. Lower curve shows the equivalent charge of a highly relativistic ($\beta = 1$) nucleus that would produce the same etch rate in Lexan as a monopole of velocity given by the abscissa.

Figure 4. Original Lexan data for monopole candidate (Fig. 2 of ref. 1). Upper two points are from sheet 2; top two triangular points are from sheet 6; bottom two triangular points are from sheet 34.

Figure 5. Response curves of several stopping ultraheavy nuclei as a function of residual range, along with the curve resulting from measurements of numerous stopping Fe nuclei. The curves of Fig. 2 become nearly straight when plotted with residual range as abscissa, using log-log paper.

Figure 6. Calibrated Lexan data for monopole candidate.

Figure 7. Best fits for doubly and triply fragmenting nuclei with $\beta = 0.7$ at the Cerenkov detector.

Figure 8. Best fit for a triply fragmenting bismuth nucleus with $\beta = 0.736$ at the Cerenkov detector.

Figure 9. Best fit for a once-fragmenting uranium nucleus with $\beta = 0.82$ at the Cerenkov detector.

Figure 10. Best fit for a curium nucleus with $\beta = 0.856$ at the Cerenkov detector.

Figure 11. Best fit for a straight line of zero slope.

Figure 12. Error distributions for (a) a twice-fragmenting nucleus with $\beta = 0.7$ at the Cerenkov detector (Fig. 7) and (b) a straight line of zero slope (Fig. 11). The curves are identical Gaussians with $\sigma = 0.035\ \bar{v}_T$ (see text). The confidence levels are 3×10^{-6} for the fragmenting nucleus and ~1 for the straight line.

Figure 13. Data for the nucleus that comes closest to simulating the monopole candidate. Both the emulsion and the Cerenkov film indicated that it had $\beta > 0.7$. It fragmented with loss of 34 charges at 1.1 g/cm^2.

Figure 14. Probabilities of grain development around the track of a particle with $Z/\beta = 114$, calculated by W.Z. Osborne.

Figure 15. Measurements of core radius in emulsion for particles with various zenith angles and values of Z/β inferred from Lexan data. The curves for different probabilities of grain development were calculated by Osborne. The large black circle is for the monopole candidate.

Figure 16. Measurements of halo radius in emulsion for particles with various zenith angles. The abscissa gives halo radius calculated using Osborne's model with $P = 1.6 \times 10^{-3}$ for the edge of the halo and using Z and β measured with

the Lexan stack. The line segments at an observed halo radius of ~55 μm show where the point should be plotted if our event was a monopole at various velocities or one of the nuclear candidates.

Figure 17. The Cerenkov method of Pinsky (ref. 20). Two radiator-film units were used in our Sioux City flights. The two plastic radiators were 100 μm and 200 μm thick; the Kodak 2485 film was 12 μm thick.

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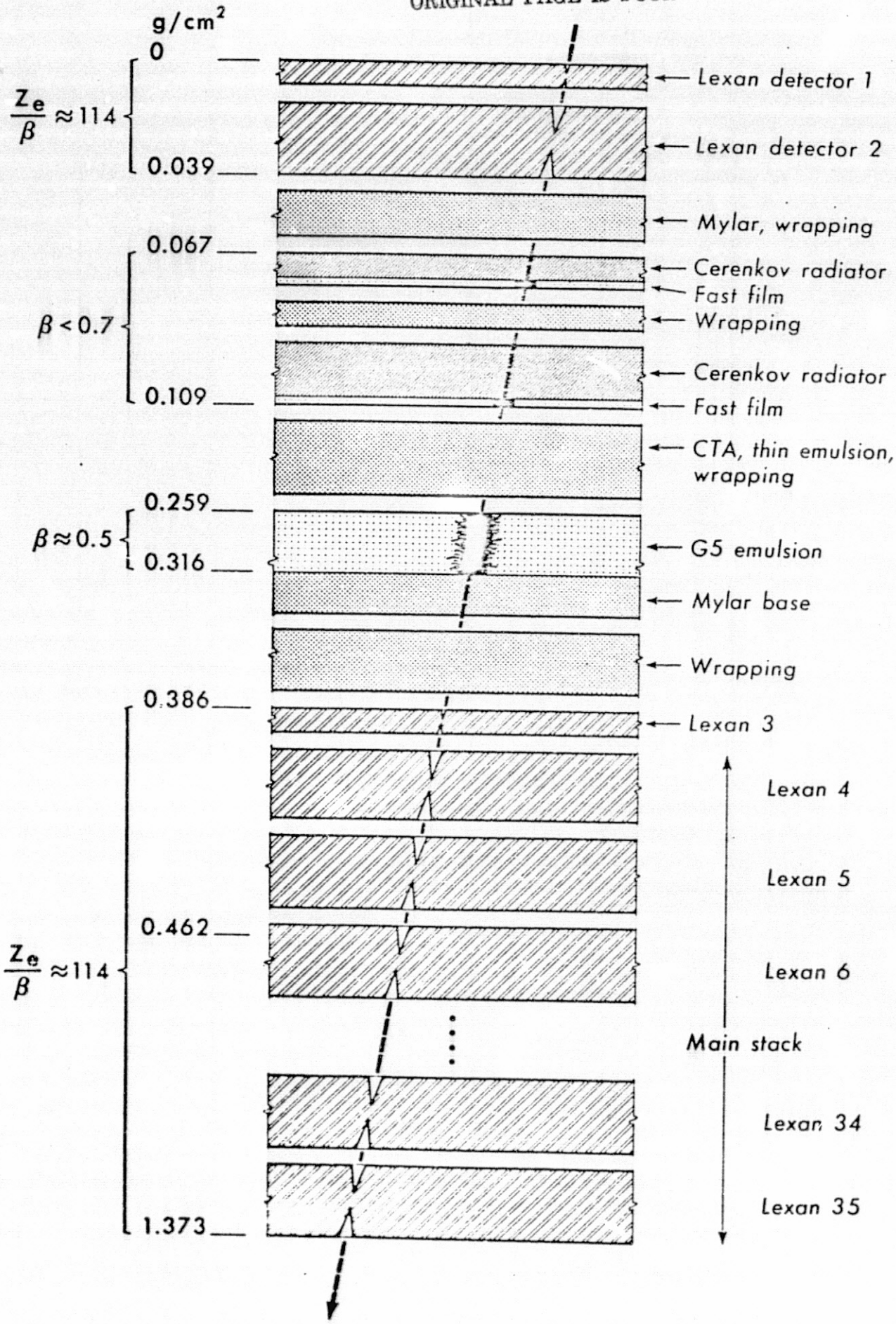

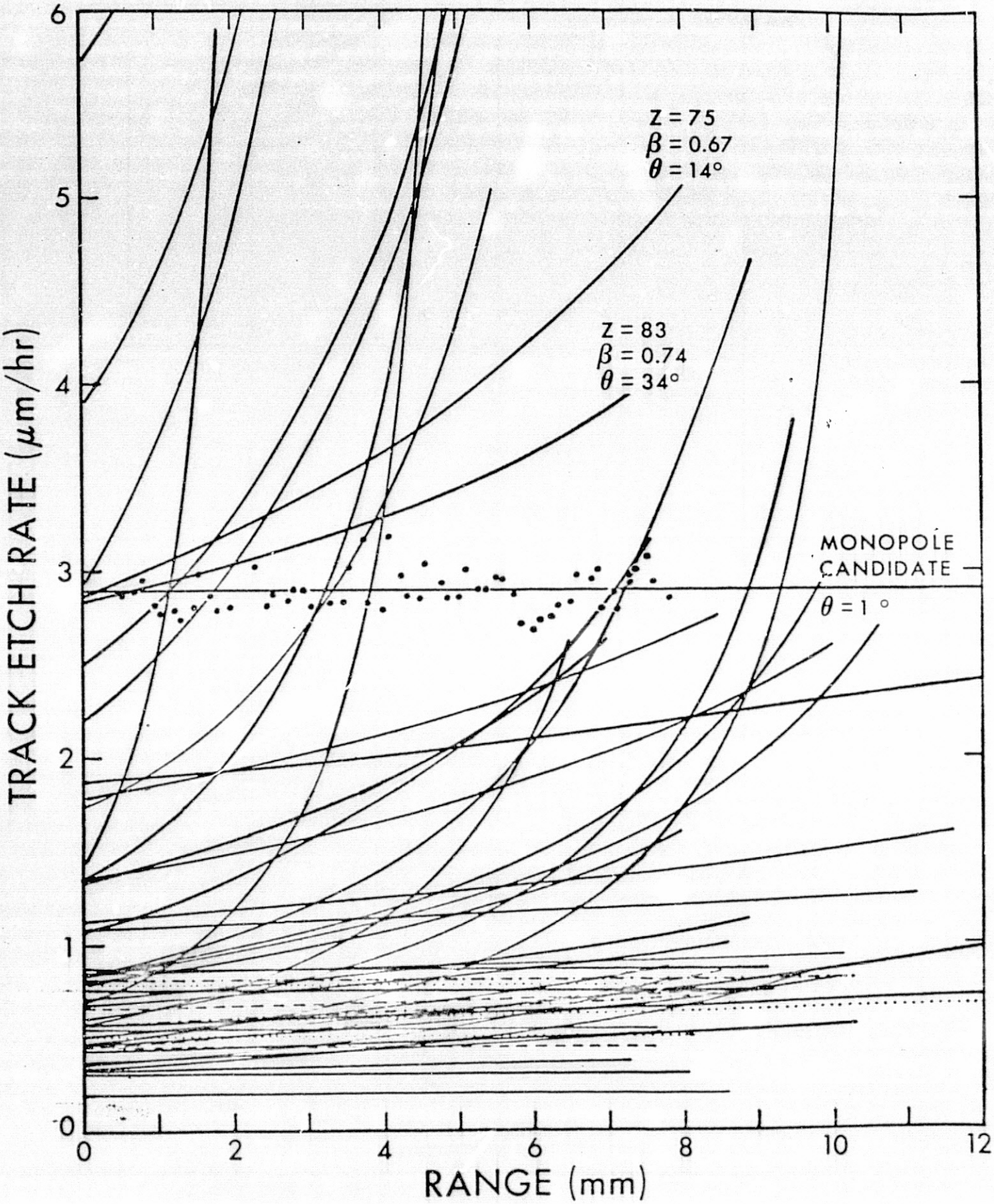
Z = 75
β = 0.67
θ = 14°
Z = 83
β = 0.74
θ = 34°
MONOPOLE
CANDIDATE
θ = 1 °
TRACK ETCH RATE (μm/hr)
RANGE (mm)
0
1
2
3
4
5
6
0
2
4
6
8
10
12

Appearance of a monopole in a plastic detector

S. P. Ahlen

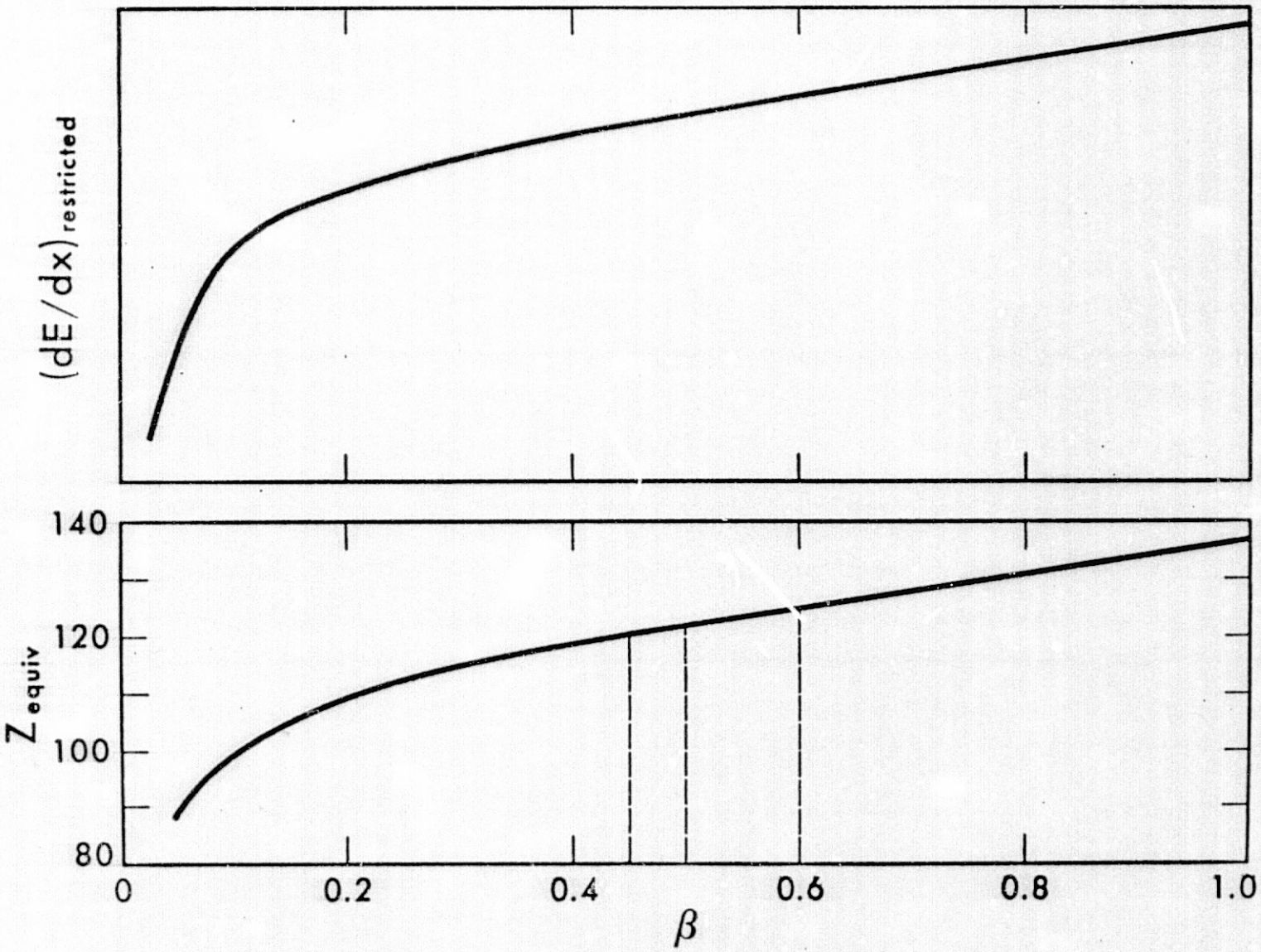

Fig. 3

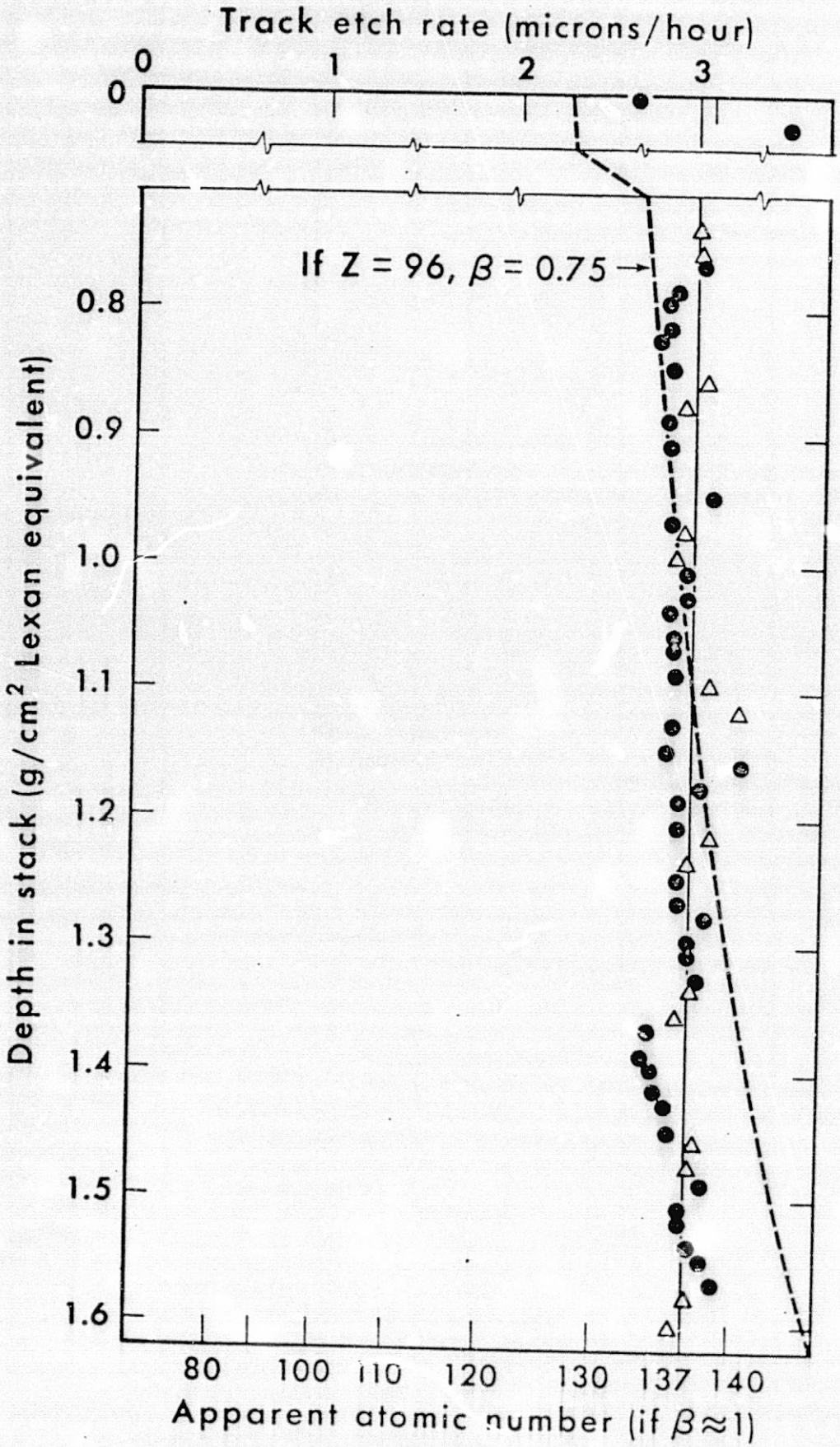

Fig. 4

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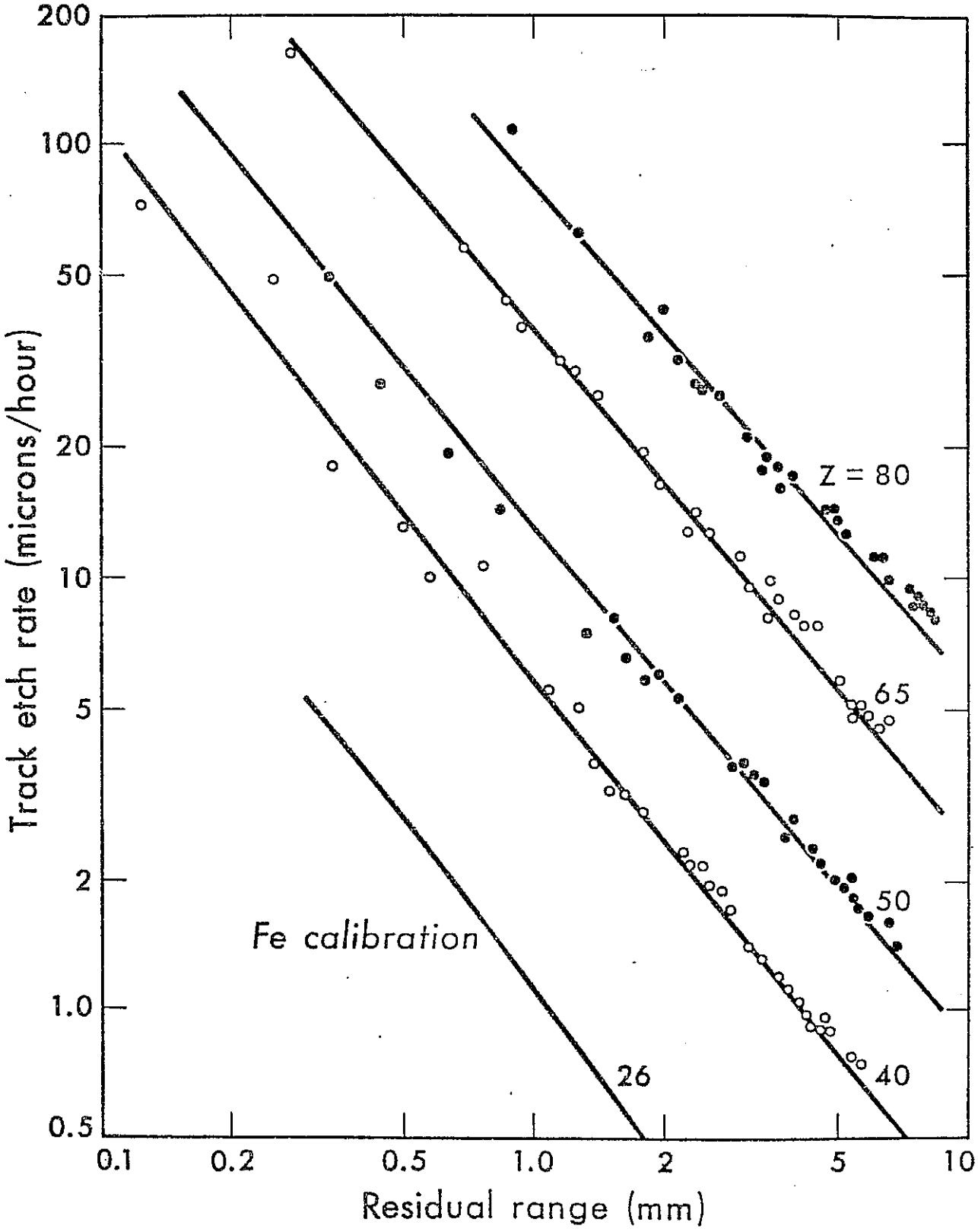
200
100
50
20
10
5
2
1.0
0.5
Track etch rate (microns/hour)
0.1
0.2
0.5
1.0
2
5
10
Residual range (mm)
Z = 80
65
50
40
26
Fe calibration

Fig. 5

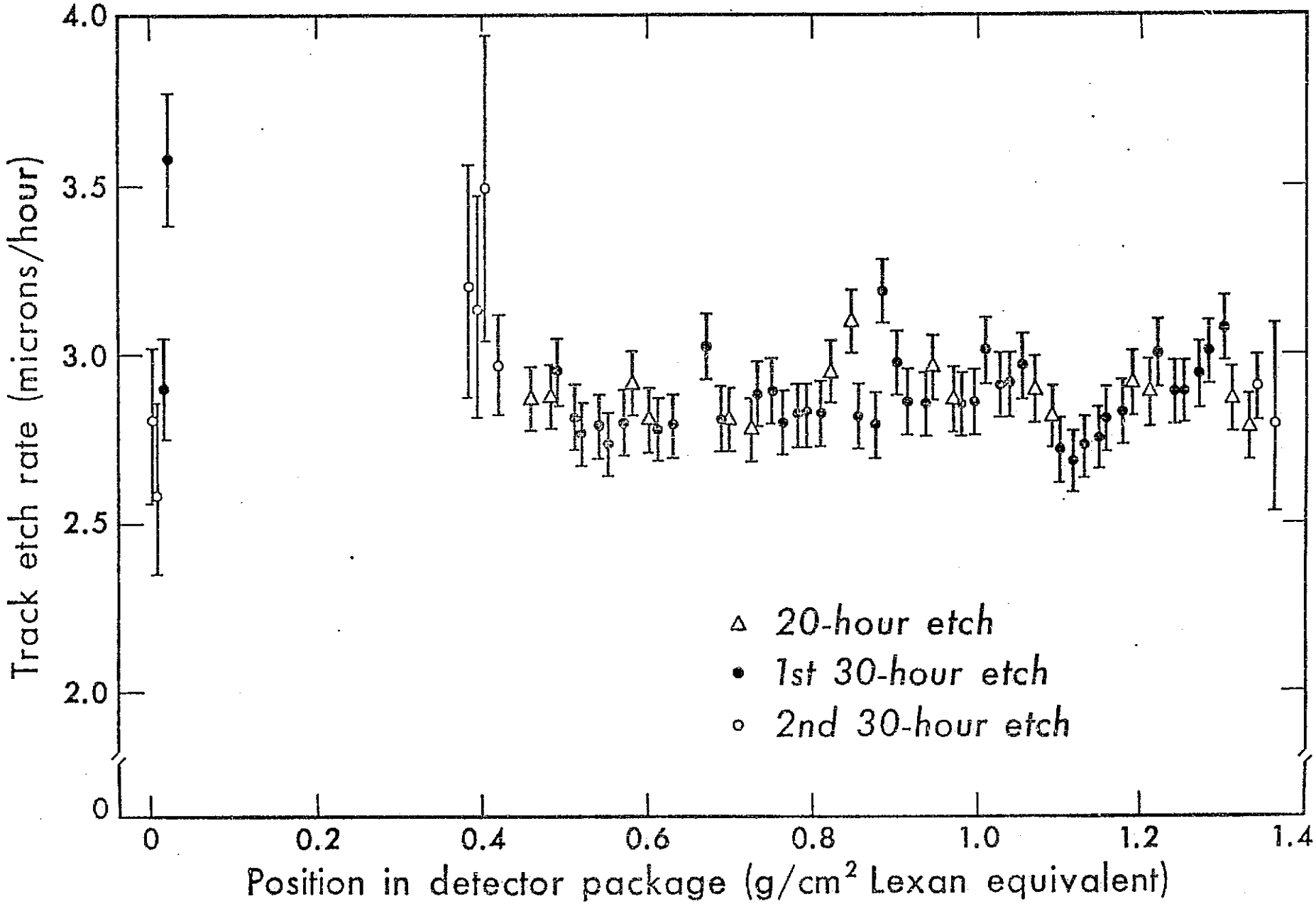
4.0
3.5
3.0
2.5
2.0
0
Track etch rate (microns/hour)
0
0.2
0.4
0.6
0.8
1.0
1.2
1.4
Position in detector package (g/cm² Lexan equivalent)
20-hour etch
1st 30-hour etch
2nd 30-hour etch

Fig. 6

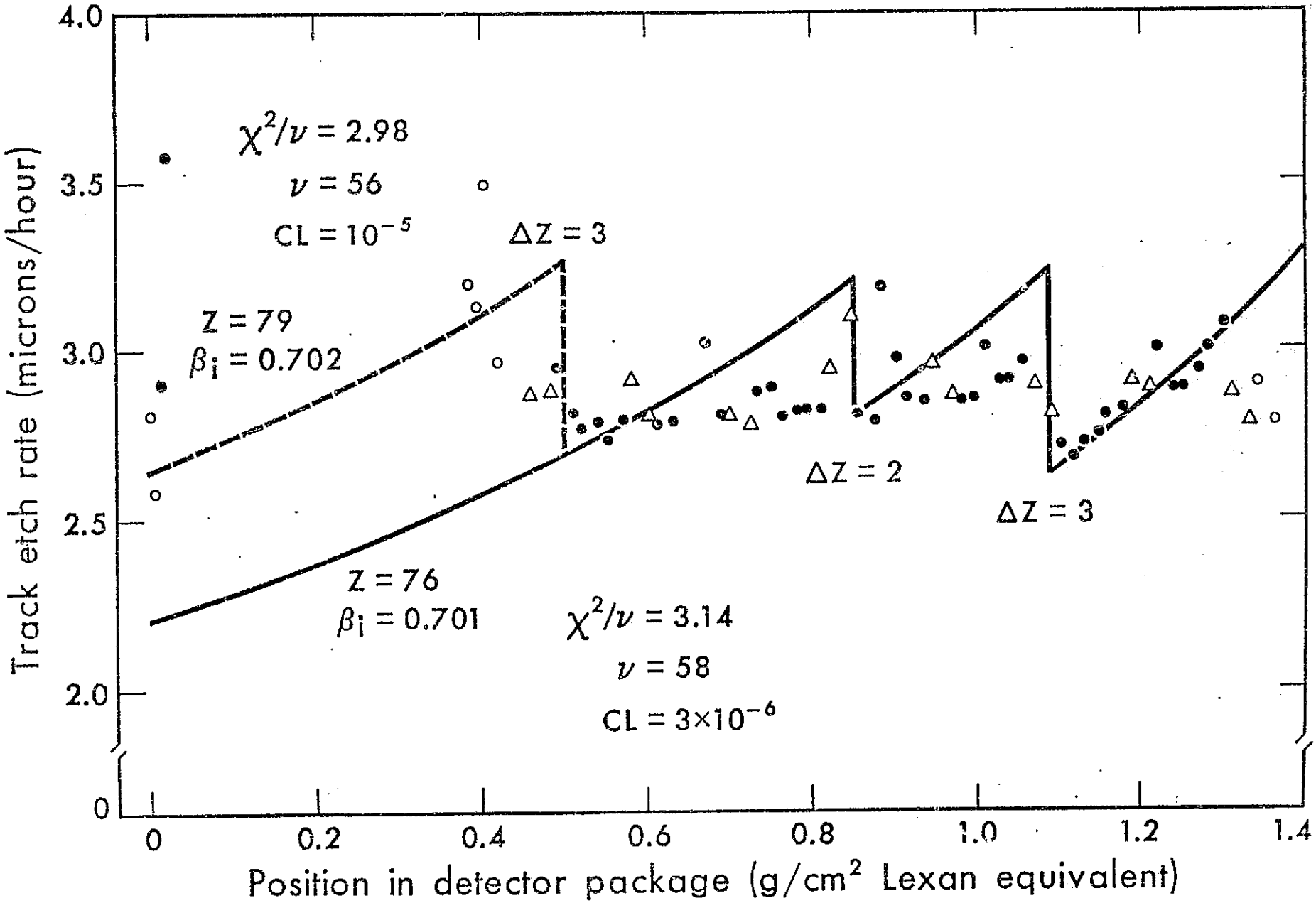

Fig.7

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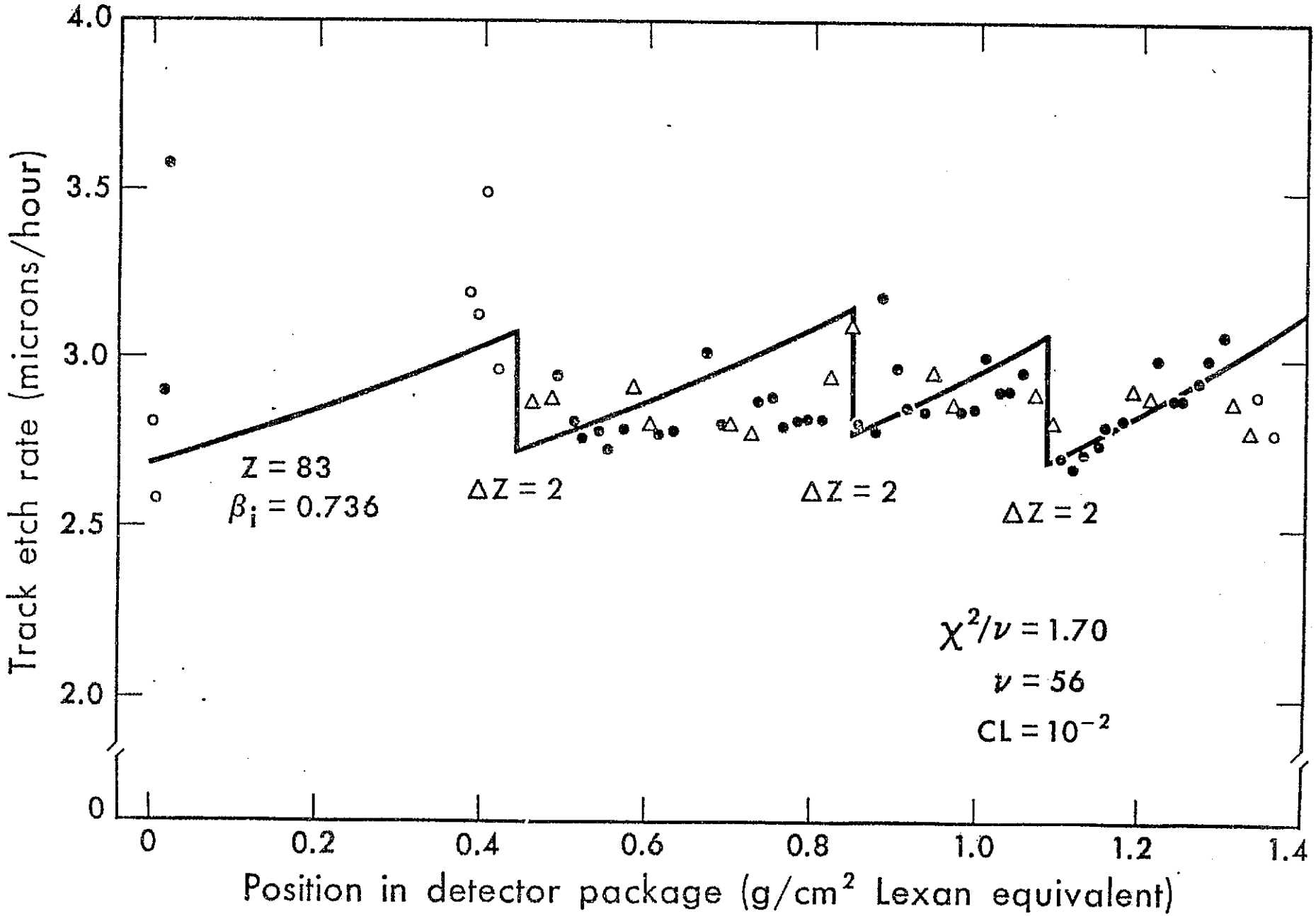
Track etch rate (microns/hour)
4.0
3.5
3.0
2.5
2.0
0
Z = 83
β_i = 0.736
ΔZ = 2
ΔZ = 2
ΔZ = 2
$\chi^2/\nu = 1.70$
$\nu = 56$
CL = 10^{-2}
0
0.2
0.4
0.6
0.8
1.0
1.2
1.4
Position in detector package (g/cm² Lexan equivalent)

Fig. 8

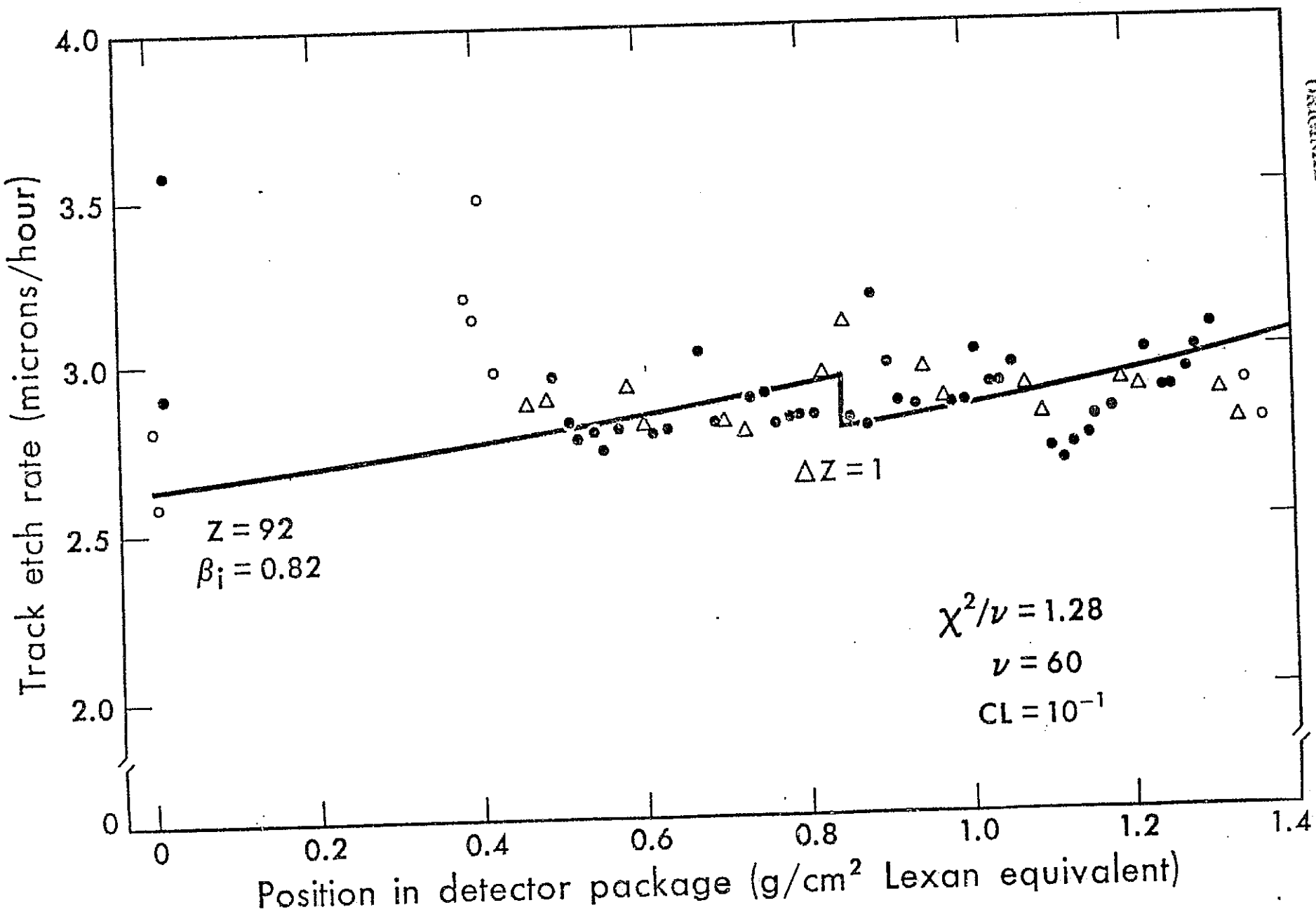

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Fig. 9

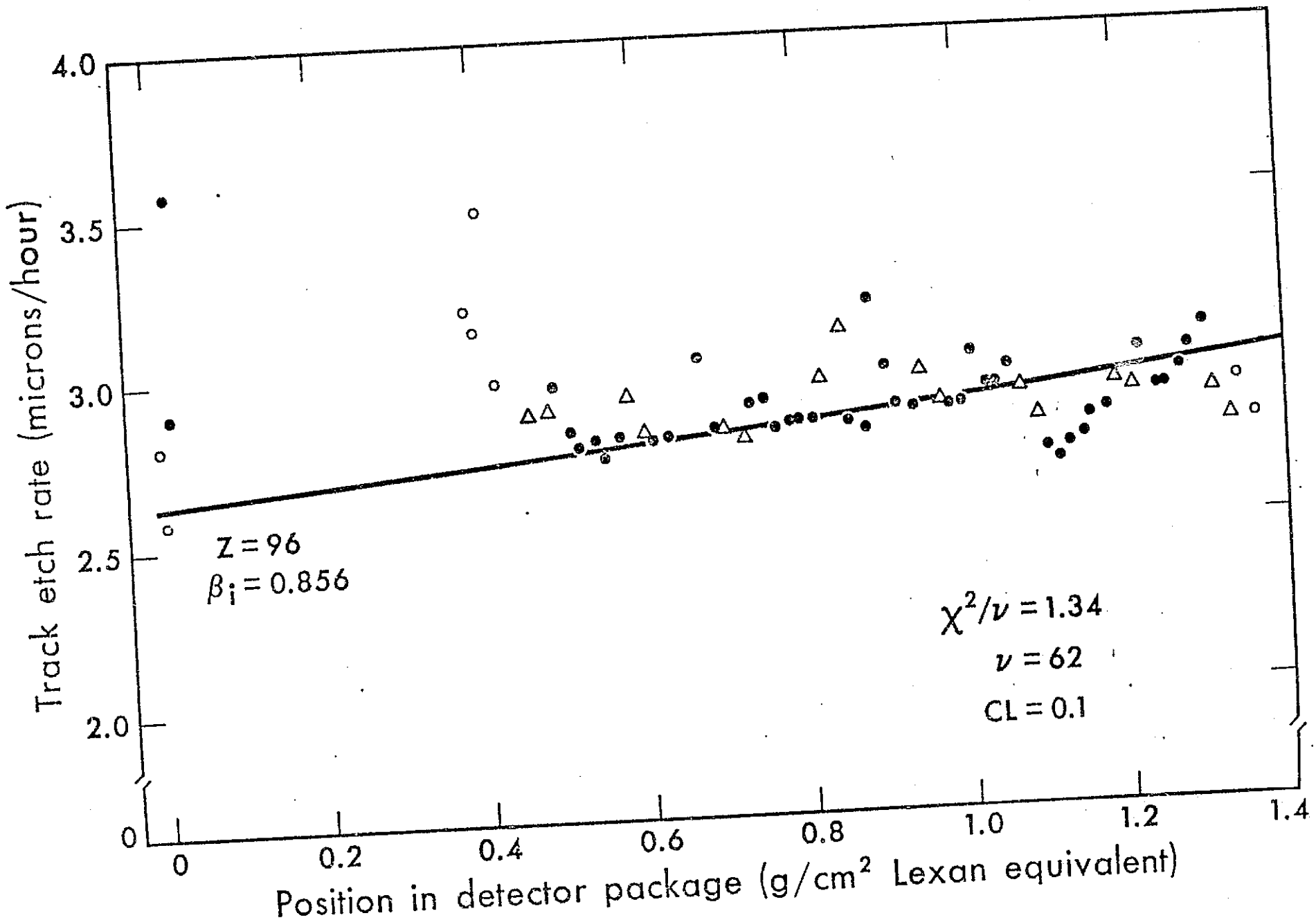
Track etch rate (microns/hour)
4.0
3.5
3.0
2.5
2.0
0
Z = 96
$\beta_i = 0.856$
$\chi^2/\nu = 1.34$
$\nu = 62$
CL = 0.1
0
0.2
0.4
0.6
0.8
1.0
1.2
1.4
Position in detector package (g/cm² Lexan equivalent)

Fig. 10

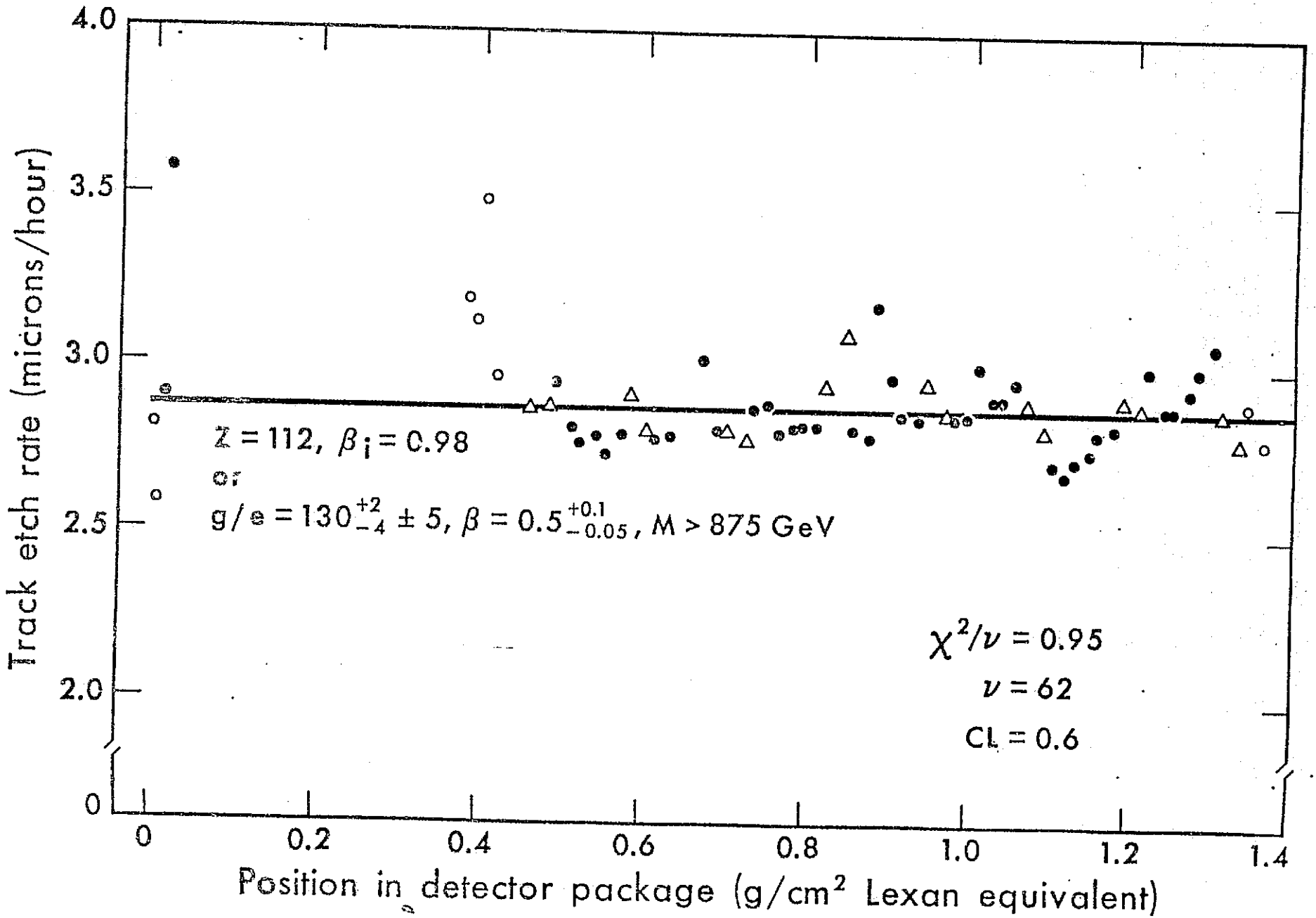

Fig. 11

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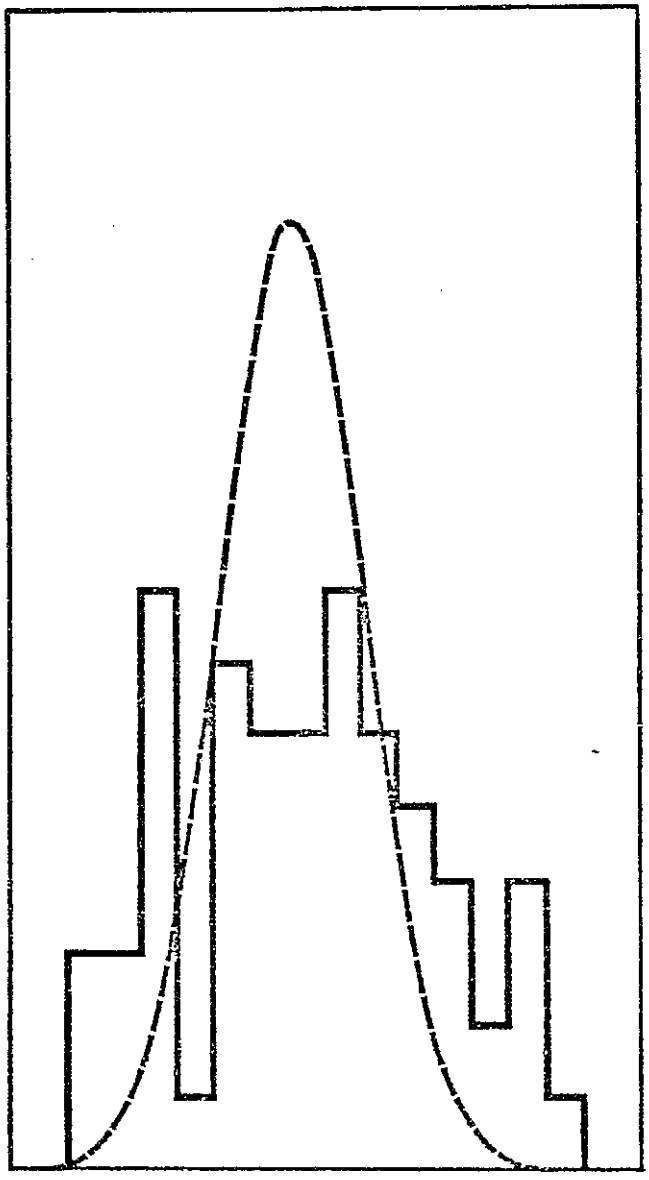
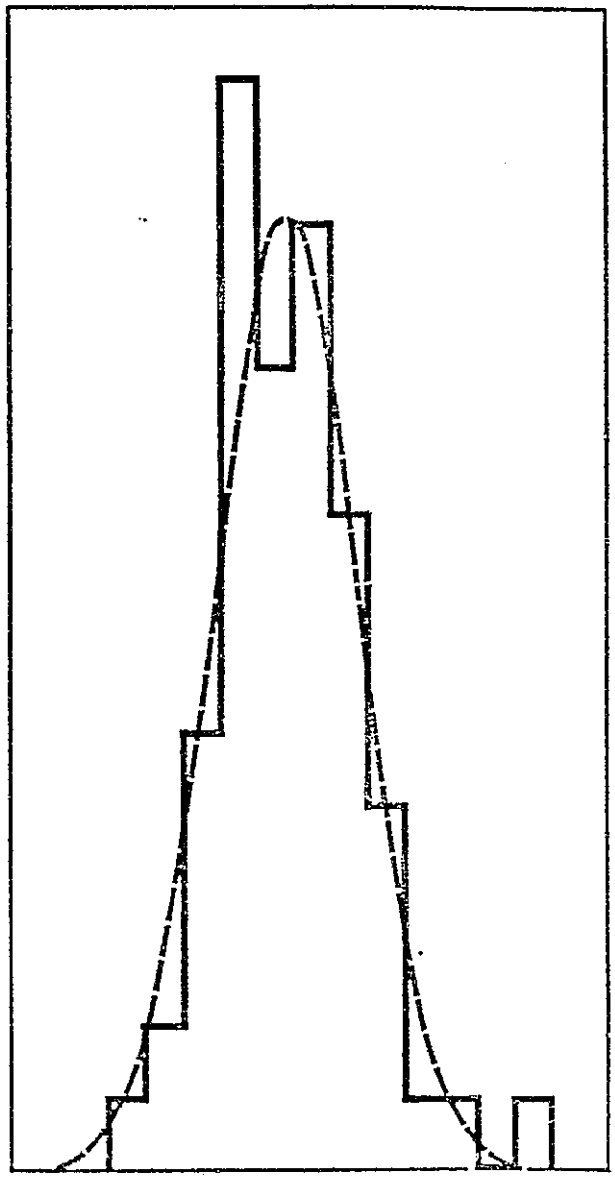


Twice-fragmenting nucleus with $Z = 76$, $\beta = 0.70$.

$\chi^2/\nu = 3.14$, with $\nu = 58$



Straight line of zero slope.

$\chi^2/\nu \approx 1$, with $\nu = 62$

Fig. 12

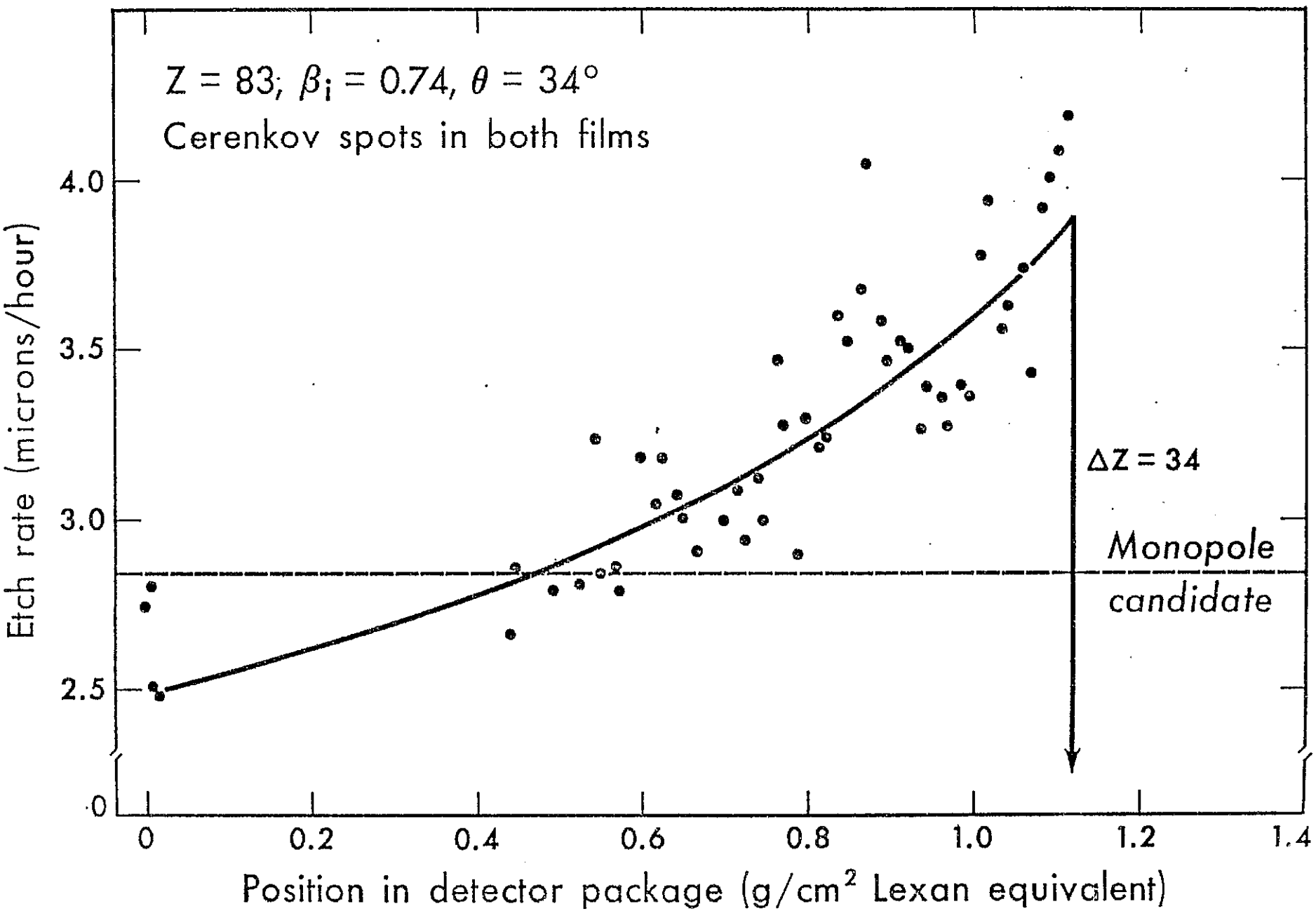

Z = 83; β_i = 0.74, θ = 34°
Cerenkov spots in both films
4.0
3.5
3.0
2.5
0
Etch rate (microns/hour)
ΔZ = 34
Monopole
candidate
0
0.2
0.4
0.6
0.8
1.0
1.2
1.4
Position in detector package (g/cm² Lexan equivalent)

Fig.13

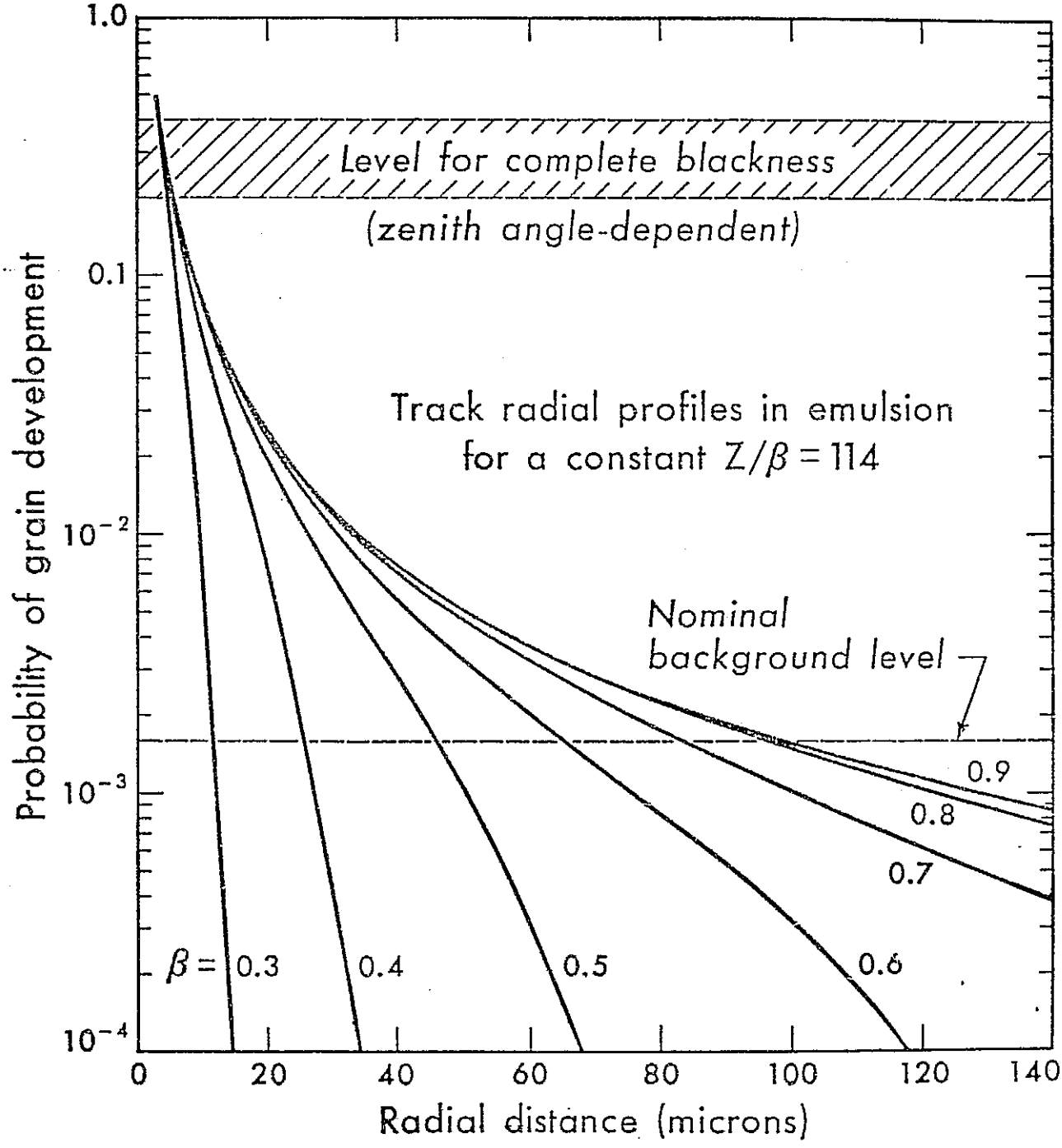

1.0
0.1
10^{-2}
10^{-3}
10^{-4}
Probability of grain development
Level for complete blackness
(zenith angle-dependent)
Track radial profiles in emulsion
for a constant $Z/\beta = 114$
Nominal
background level
0.9
0.8
0.7
0.6
$\beta =$ 0.3
0.4
0.5
0
20
40
60
80
100
120
140
Radial distance (microns)

Fig. 14

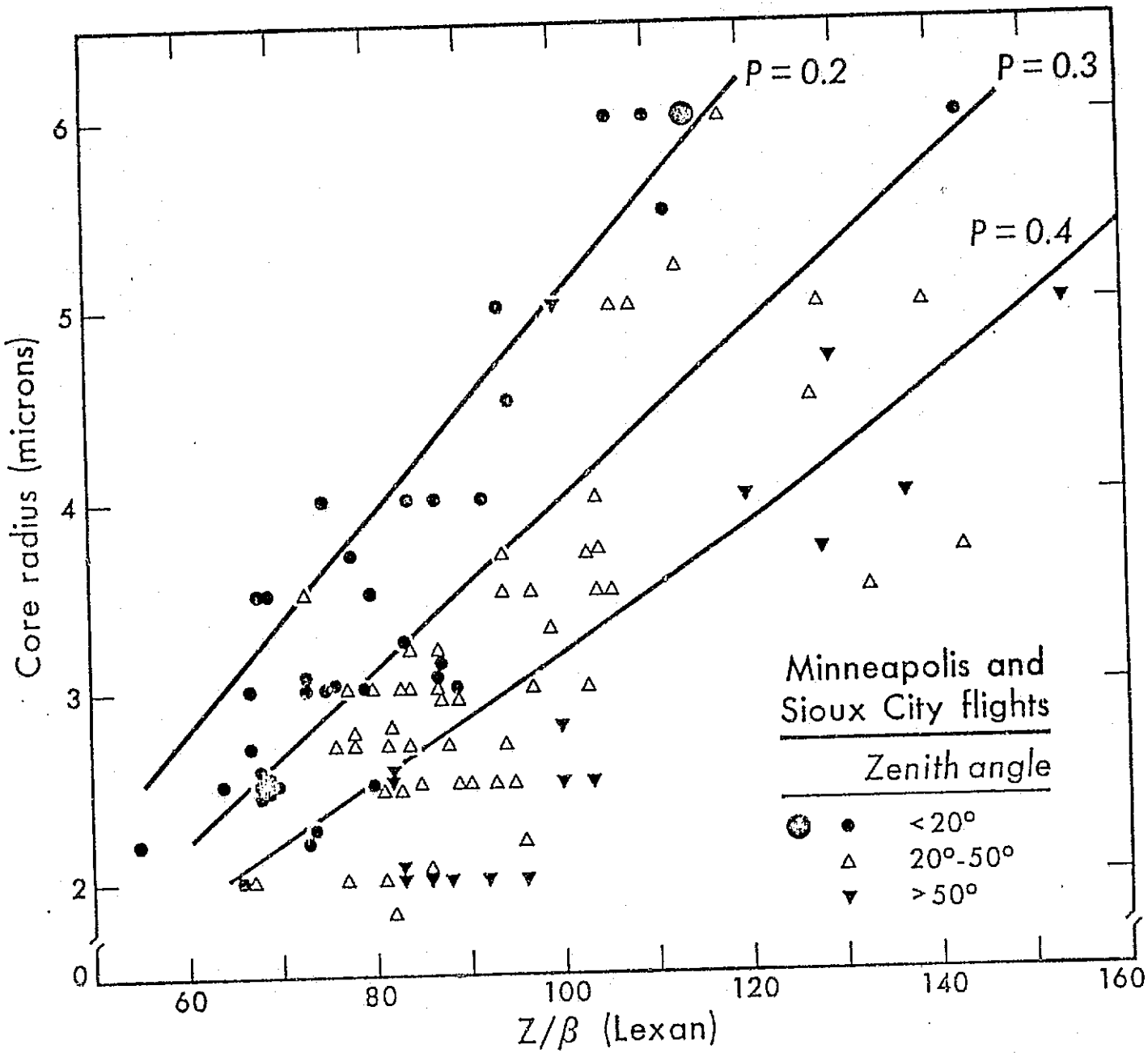

P = 0.2
P = 0.3
P = 0.4
Core radius (microns)
0
2
3
4
5
6
60
80
100
120
140
160
Z/β (Lexan)
Minneapolis and
Sioux City flights
Zenith angle
<20°
20°-50°
>50°

Fig.15

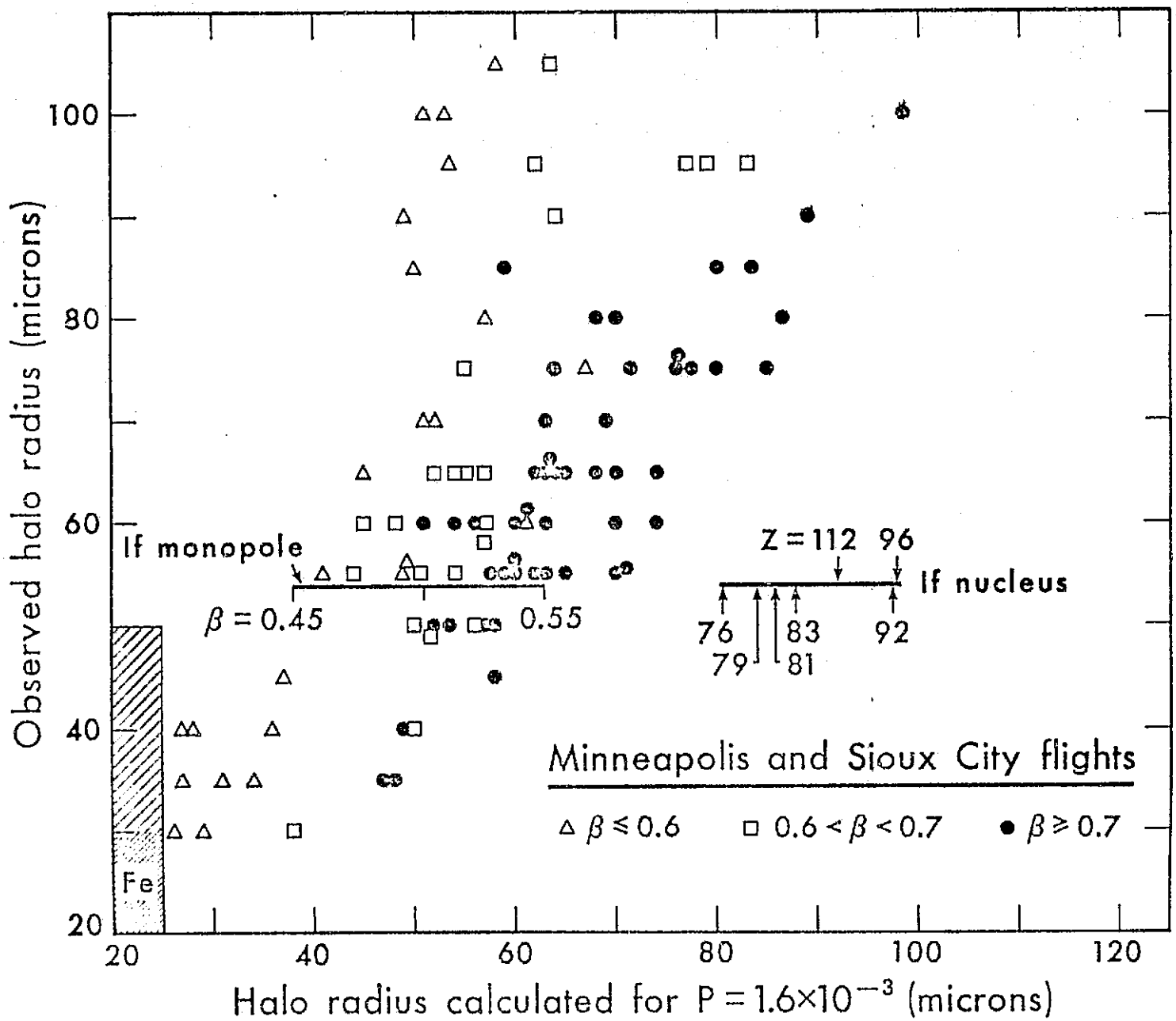


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Fig. 16

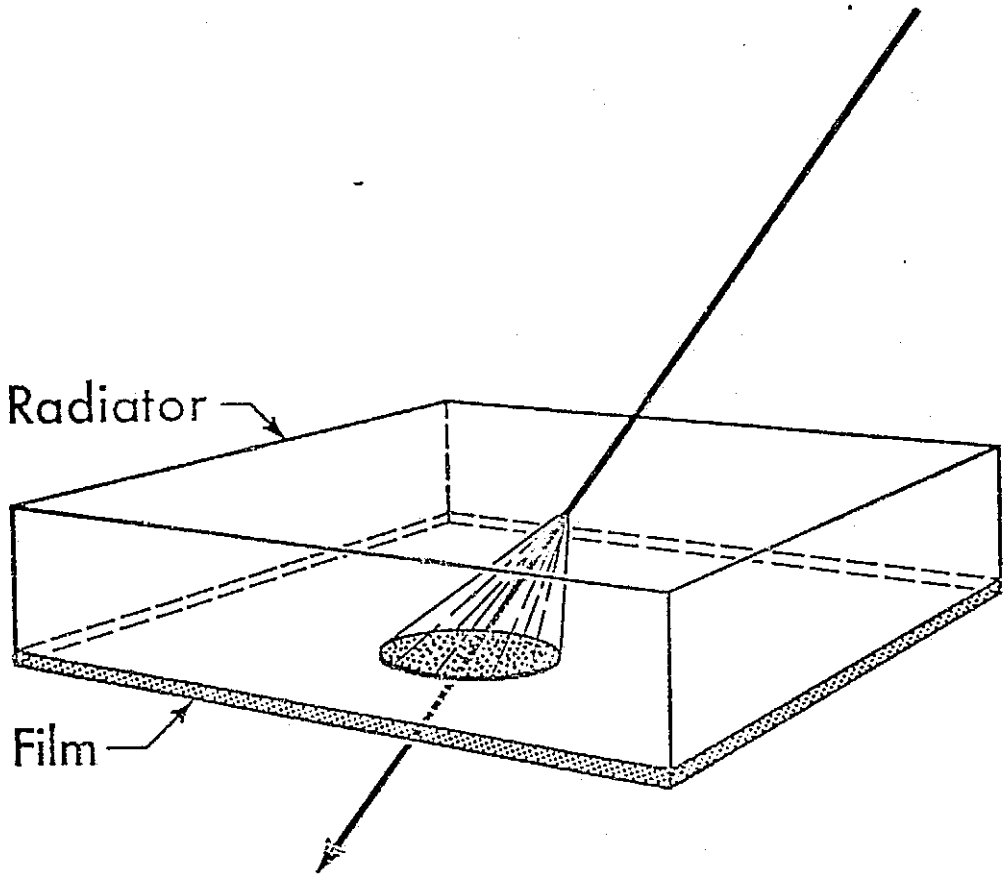

Fig. 17