Analysis of a Reported Magnetic Monopole*

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In a recent publication (PRL 35, 487, 1975) Price, Shirk, Osborne, and Pinsky have described a cosmic ray event which is consistent with the hypothesis that it was caused by a magnetic monopole. In their paper, they say "the etch-rate data (in the Lexan) admit of only two alternatives:

(1) The particle was a nucleus with $Z \geq 125$, $\beta = \geq 0.92$.

(2) The particle was a monopole with $g = 137e$ and any velocity sufficient to penetrate the 1.6 gm/cm^2 stack.

The data from the nuclear emulsion and Cerenkov film enable us to reject the first alternative."

No one could disagree with this way of presenting the arguement; to put oneself into a position where the monopole hypothesis can be discussed as a possibility, it is indeed first necessary to prove that all other explanations in terms of known particles can be "rejected".

It will be shown below that several substantive errors are to be found in the published article, so the "rejection of the first alternative" is no longer valid. Since, as I will show, the data points fit beautifully to the hypothesis that the responsible particle is a Platinum nucleus ($Z = 78$) fragmenting to Osmium ($Z = 76$) and then to Tantalum ($Z = 73$), it is, in my view, quite improper to discuss the question, "which fit to the data points is better, the Pt nucleus or the monopole". Such a question would be very much in order if one were deciding between the following

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two hypotheses: 1) The track is due to a Pt nucleus (Z = 78) or 2) The track is due to a Pb nucleus (Z = 82). Then, all the apparatus from Statistics could be brought to bear on the problem -- χ^2, goodness of fit, confidence level, etc. --and one could arrive at the best explanation of the event. But such an analysis is absolutely proscribed by the "rules of Physics", in the present decision-making situation -- heavy nucleus vs. magnetic monopole -- so long as the heavy nucleus gives even a poor, but acceptable fit. (It will be shown below that the Platinum explanation gives an excellent fit, but that is not essential to the arguement.)

To illustrate the fact that I didn't invent these " rules of Physics" this morning, let us recall the single essential ingredient in the discovery of the positron.

1) Most physicists would say that the discovery of the positron involved the observation that an electron-like track in a magnetic cloud chamber bent the wrong way. But that would not be correct, since others had previously seen electron-like tracks curving the wrong way in cloud chambers; the effect was always attributed to electrons going in the opposite direction. In fact, Skobeltzyn (the first person to build a magnetic cloud chamber) commented on the strange behavior of electrons - - they occasionally scattered through almost exactly 180°! (With hindsight, we now recognize that he was seeing pair production, but he believed that the positron was an electron going the other way.)

Anderson's great discovery of the positron rests entirely on the fact that he knew which direction his positron was going; he placed a lead plate in his cloud chamber, and saw the particle lose energy, and "curl up", after it went through the plate. Many observers had seen particles that were consistent with the positron hypothesis, but Anderson was the first one to be able to <u>reject</u> all other alternatives. That is why we recognize him as the discoverer of the positron. (And the fact that his discovery was based on a <u>single</u> event should be noted by any of you who might have criticized the present observation as resting on a single event.)

As long as we are discussing the severe criteria that are involved in a <u>great</u> discovery, it is interesting to recall what happened next. Anderson did not yet feel entitled to announce the discovery of the positron. He first had to reject another alternative -- the magnetic field could be reversed. A study of pictures from the same roll showed that most downward travelling tracks were made by negative electrons. You will now agree that having checked the field, he could then publish. But no! -- one possibility remained; the engineering students at Caltech are notorious practical jokers, so Anderson had to reject the possibility that during the night, when the cloud chamber was periodically taking pictures, some students had reversed the direction of the field, and then restored its original direction. This alternative hypothesis involved an interval of time that could be estimated, (turning off the generator, unscrewing the heavy lugs, etc.) and to reject it, Anderson had to prove to his own satisfaction that neighboring pictures, whose data boxes displayed a clock, showed ordinary electrons, rather than positrons. Only when he was so convinced did he feel he could speak publicly of positrons. <u>He had rejected all other alternatives.</u>

Now that we have "reviewed the rules" that are applicable to great discoveries -- as that of the monopole would certainly be acclaimed -- we can return to the article under discussion. The paper of Price et al proceeds for the first two pages almost as though the authors had read and believed what I've just said about the rules governing great discoveries. But if one follows this route, he must actively seek out alternative explanations for any puzzling observations. An example to show that this was not done as vigorously as possible will now be given.

No mention is made in the paper of a phenomenon well known to the authors, which tends to smooth out the etch rate (approximately proportional to the square of the energy loss per cm.), and make the rising Bragg curve less apparent. This is particularly surprising in view of what happens to the etch rate points at a tabulated depth of about 1.35 gms/cm^2. (Figure 1) Anyone looking at the circles in this plot will note that if the bottom 12 circles are slid as a group horizontally to the right, they will neatly lie parallel to the dashed line (labeled $Z = 96$, $\beta = 0.75$), and all the circles will appear to trace out the classic Bragg curve of a heavy nucleus slowing down.

Therefore, my first strong disagreement with the authors comes from their not including a third alternative:

(3) A heavy nucleus that fragments once or twice in its passage through the Lexan sheets.

To neglect to mention this obvious explanation of the non-Bragg-like <u>average</u> behavior of the points, is to me a serious error, particularly when the first question after Buford Price's early Berkeley talk was, "What's that glitch at 1.35 grams per cm^2 due to?" To fit a straight line through the points displaced in the glitch is like fitting the points on a log-log X-ray absorption curve with a straight line, ignoring an obvious K edge. Knowing that K edges exist would make most experimentalists fit such a curve with two straight lines, displaced paralled to each other at the K-edge. It is unthinkable that fragmentation was not discussed by these experienced heavy ion physicists as a possible explanation for the glitch. It is in this light that I find the omission of the third alternative such a serious flaw in the arguments presented by the authors.

Before leaving the "obvious fragmentation", which will be my name for the glitch, I'll now show all 40 circles in a slightly different way. First, I'll rotate the graph 90° counter clockwise, to make the axes conform to a normal Bragg curve, and I'll expand all the etch rate points by a factor of 4. Figure 2 is the new curve, with its now even more obvious fragmentation, and figure 3 shows the same data with the 12 circles to the right of the fragmentation moved upward to give a good "eyeball fit" to the earlier points. I am confident that any physicist who looked at Figure 3, and the scale at the left would immediately say, "That's a beautiful Bragg curve of a high Z nuclear particle; you must have found it in a balloon flight, or perhaps in Skylab." Then, if you showed him figure 2, he'd say, "My, what a beautiful fragmenting heavy nucleus."

Since I've talked only of the 40 circles (in Figure 1) in the main Lexan stack, omitting the 2 circles in the top sheet, and the 16 triangles, let me now say why. My reasons for omitting the 2 circles from the top sheet are twofold: 1) Buford Price, in his Berkeley talks, said that the two points from the top sheet were not reliable because that sheet had had a different treatment from the others; "it was manhandled". 2) Steve Ahlen in his talk today said the two points in the top sheet weren't reliable because they might have received more ultraviolet light exposure than the sheets in the lower stack, and so etched more rapidly. (From my last comment concerning Steve Ahlen's talk -- which was given after mine -- it is obvious that I am including some points that I did not mention in my talk. I have done so to make my written comments more complete.

I reach no new conclusions in the written version, that were absent from the talk I gave. But I have included some material I later learned from Peter Fowler concerning his parallel activities in analyzing the event.)

My reason for ignoring the triangles is that the plastic sheets yielding those points were etched at a different time, and in a solution of NaOH different from that used to etch the sheets that gave the circles. The "circle sheets" were calibrated on the cosmic ray "Iron peak", and that brought the value of Z/β corresponding to the monopole fit down from 137 to 121. The "triangle sheets" weren't calibrated by the Fe peak, and two of Buford Price's students told me that although they would like to have me include the triangles, they couldn't in good conscience predict that the Iron peak calibration would make the values of Z/β decrease by the same factor of 12% observed with the circles.

I will now return to the change in the value of Z/β corresponding to the monopole fit. That value was given by Buford Price in his Berkeley talk as 137 ± 0.5 (statistical) ± 2 (systematic). It was published as 137, and we have been told that the best value is 121; but Steve Ahlen said today that it might go higher or lower than that in future calibrations. With that erratic record, it would seem reasonable to consider only the 40 circles; they comprise a complete and separate experiment, with enough points to be statistically significant. One of the fundamental principles of Statistics is that a subset of normally distributed data is a valid set as long as the selection of points is made by a method that isn't designed to bias the results. By using all 40 points from the same etching solution, which moreover is the totality of points from the calibrated Lexan sheets, I believe I am acting in accordance with the "rules of statistics".

Two of the reasons that caused the authors to reject the heavy nucleus hypothesis have been discussed. 1) The failure to take seriously the fragmentation possibility, and 2) the incorrectly stated value of Z/β -- 137 instead of 121. Using the higher value, the authors were able to set up a "nuclear candidate" with $Z = 96$, and $\beta = 0.75$. (Shown by the dashed line in Fig. 1.) They could easily discredit this, because 1) no nuclei with $Z = 96$ have ever been seen in the cosmic rays, 2) the Cerenkov detector would have been triggered by such a fast particle, and 3) the new nuclear emulsion technique for measuring β forbade such a high value of β.

When Z/β was lowered from 137 to 121, by calibrating the Lexan, the parameters of the "nuclear candidate" were lowered to $Z = 85$, $\beta = 0.75$. Such a value of Z is still of no use, since the "Actinide gap" excludes all values of Z between 84 and 89, and the high value of β is still forbidden. But if we now lower both Z and β, we can keep the required value of $Z/\beta = 121$, and by making use of the "obvious fragmentation", we find an acceptable fit in the Lexan stack. (For the moment, we will forget about the two photographic limits on β, and look only at the Lexan data. We will return to the limits on β, later in the talk.) The fit becomes really impressive if we let the incoming nucleus -- now identified as Pt -- fragment about half way between the top of the Lexan stack and the "obvious fragmentation".

(According to Henry Crawford, who recently did his Ph.D thesis in the Price group, studying the fragmentation of heavy nuclei in Lexan, the probability for one additional fragmentation in the path of such an "obviously fragmenting heavy nuclei" is 3% in the upper 0.6 gm cm^{-2} of Lexan. He estimates the fragmentation cross section to be about 30% of the total cross section, which in turn corresponds to a mean free path of about 5.3 gm cm^{-2}. If one wonders, in addition, about the probability that a heavy nucleus fragments twice in 0.8 gm cm^{-2}, he should multiply the Price group's observed population of heavy nuclei -- several hundred -- by the square of 4%. The answer is close to unity.)

At this point, one can see that the jagged curve shown in figure 4 fits the 40 circles about as well as the vertical line corresponding to the monopole. As I stated at the outset, the relative values of χ^2 for the monopole and Pt fits are of no interest to one trying to decide which hypothesis to consider; as long as χ^2 for the Pt fit is not absurdly large, one should not even discuss the possibility of the monopole hypothesis. It is obvious from inspection that the two fits are comparable, so the monopole hypothesis is ruled out -- if we can find a way to get the Pt nucleus past the "Cerenkov gate" near the top of the complete stack.

Although I've said as firmly as I can that the relative values of χ^2 for the two fits are of no interest to us, some of you will want to know what the values are, anyway. So in order to save you the time required to make the measurements, I'll let you see the numbers, which I have of course calculated. χ^2 is normally evaluated as the sum of the squares of the individual deviations of the points from the theoretical curve, divided by the square of the standard deviation for a single point. I measured the deviations of the 40 points from the monopole line, and from the jagged fragmenting heavy ion line, in millimeters, on the etch rate curve in the preprint of Price, et al. Since you may see the same figure at a different magnification, you will want the sum of the squared deviations in less arbitrary units. Since one can equate small displacements on the graph, near $z/\beta = 121$, to changes, ΔZ, in nuclear charge I will state the sums of the squares of the errors in units of Z^2. The jagged line in figure 4 has an upper break with a $\Delta Z=2$, and a lower break with a $\Delta Z=3$. These breaks then set the scale for the determination of the sums of the squared deviations. The 40 point sums have the values:

$$\Sigma(\text{monopole}) = 20.8 \text{ square charges}$$

$$\Sigma(\text{Pt-Os-Ta}) = 22.0 \text{ square charges.}$$

These sums can be divided by the square of the standard deviation, ΔZ, for the determination of a nuclear charge from a single etched pit, to give the corresponding χ^2. Normally one measures many etched pits to find Z/β to some fraction of a unit charge. From these numbers, one can see that both values of χ^2 correspond to high confidence levels. Nothing useful would be accomplished by pursuing these calculations further.

Since I've been discussing the problems that are always involved in analyzing another person's data, I should mention some special problems that have made it even more difficult in this case. I've recounted the changing value of Z/β, but that has changed only once. In the recent conversion of mm. on a graph to ΔZ, I had to contend with three quite different values of the exponent in the equation: etch rate = k $(\frac{Z}{\beta})^n$. The article says n = 4, the scale at the bottom of figure 1 was constructed using the value n $\cong$ 3.4, and Steve Ahlen told us today that the best value for n, from the latest calibration, is n=4.4. (I used n=4 in all my calculations.)

I have found that there is enough flexibility in the parameters available to me, to make a satisfactory fit to changing values of the "monopole Z/β", and to changing values of the exponent, n. One can accomodate to a change in the calibration of Z/β, simply by choosing a new value of Z, at the same β. It turns out that as one integrates heavy ions through the stack, the behavior of β as a function of grammage is fairly insensitive to Z or A. For example, if one starts with a heavier nucleus at some initial point in the stack, (with $\beta=\beta_o$) that nucleus has more energy (from A), and it loses energy faster (from Z), and over the range of interest, β varies at nearly the same rate as before. One can equally well accomodate to a change in the exponent, n, by changing the value of β that is used as an initial condition. This corresponds to sliding the Bragg curve left and right, in figure 3. A change of n from 4.0 to 4.4 changes the slopes of any curves by a factor of about 10 per cent, and obviously preserves the etch rate at the normalizing value of Z/β, which is taken to be 121, but which could be whatever value is at the moment in vogue.

At this point, I have shown that all the Lexan points can be explained satisfactorily as being caused by a ${}_{78}Pt^{195}$ nucleus that entered the main Lexan stack with $\beta = 0.664$, then lost an alpha particle to become $_{76}Os^{191}$, and then lost 3 charges at the "glitch", to become $_{73}Ta^{185}$. I find it extraordinarily interesting that yesterday afternoon, (the day before the talk) I learned that Peter Fowler in Munich had independently come to the same conclusions -- not to similar conclusions -- but to the identical sequence: Pt → Os → Ta, with the fragmentations at the same places. (It is my understanding that Peter Fowler is writing up his work, for inclusion in the Munich Cosmic Ray Proceedings, as I am doing with my work, for these Proceedings.)

Let us now ask the question, "If Peter Fowler and I came to identical explanations of the etch rate points in the Lexan stack, 6000 miles apart, why did not the authors of the paper come to the same conclusion. And more importantly, why did they feel so confident that there was no explanation of the event in terms of heavy ions, that they announced the discovery of a magnetic monopole."

The answer is that neither Peter Fowler nor I could have put forward our two quite different explanations for the appearance of the doubly fragmenting Platinum nucleus in the Lexan stack, if we had believed all three of a triad of numbers that appear in the paper. These are 1) the value of $\beta = 0.5\,^{+0.1}_{-0.05}$, obtained from the G-5 emulsion, 2) the maximum value of $\beta = 0.68$, from the Cerenkov detector, and 3) the thickness of material (mostly photographic -- see figure 5) above the highest etched points in the main Lexan stack -- shown as $t = 0.74$ gm/cm^2 (Lexan equivalent), in figure 1. The authors believed these numbers, and because they did, they could invent no way for a fragmenting Pt nucleus to arrive at that depth in the total stack, and behave the way I've shown it was behaving. (Neither could Peter Fowler nor I do so under the same "groundrules.") The arguments are strong enough to "reject all other alternatives," in my opinion, but only if the second and third numbers quoted above were absolutely reliable, and not subject to change.

Peter Fowler and I both rejected the emulsion value of $\beta = 0.5$, but for different reasons. I rejected it because the method is new, unpublished, and therefore unrefereed -- hardly the linchpin on which to base a great discovery. Peter Fowler rejected it for solid technical reasons; he says the method is not valid for $\beta > 0.45$, and as the world's most experienced observe of heavy ions in nuclear emulsion, his evaluation must be given great weight.

We are therefore left with two critical numbers, that if unimpeachable, truly make it impossible to explain the event as anything but a monopole.

1) $\beta_0 \lesssim 0.68$ from the Cerenkov detector

2) $t = 0.74$ gm/cm^2 Lexan equivalent (Figure 1)

As we shall now see, Peter Fowler learned (in Munich) from Larry Pinsky, who built the Cerenkov detector, that it might pass a Pt nucleus with $\beta = 0.70$, without showing a "Cerenkov ellipse" on the fast photographic film. That fact, assuming that the thickness, t, was 0.74 gm/cm^2, permitted him to explain how the Pt nucleus could penetrate the 0.74 gm/cm^2, and show the "signature" I've described above.

I took the other approach; I tended to believe the Cerenkov threshold, and inquired of the Houston people (in particular Ed Hungerford) if the thickness of material above the Lexan stack might not in fact be somewhat less than half of the indicated 0.74 gm/cm^2. I was given the surprising information that the photographic package was indeed less than 0.30 gm/cm^2 Lexan equivalent, from the top of the thick Cerenkov detector to the bottom of the wrapping paper which kept light from exposing the films.* No explanation for this substantive and acknowledged error in the grammage above the topmost etched Lexan sheet in the main stac has been forthcoming, but regardless of the reason for the error, its existence made the complete explanation of the event essentially obvious.

I was at first surprised that Peter Fowler could explain the event without knowing what I've just disclosed concerning the incorrectly tabulated value of the grammage above the Lexan stack. But now that I understand it, I'll share that knowledge with you. Everyone: the authors, Peter Fowler and I agreed that if one traced a Pt nucleus with $\beta o = 0.68$ at the top of Cerenkov detector, down through 0.7 gm/cm^2 of Lexan equivalent, it would show a very much higher etch rate than was observed, and more importantly, its high rate of change of etch rate with depth would make it impossible to fit to the observed points. One could start with ions of lower Z, at $\beta = 0.68$, but that would not permit a fit to the points; the Bragg curve of such an ion is rising almost preciptously as its velocity drops toward zero.

But if we reverse the procedure, as Peter Fowler probably did, by assuming (from the "signature") the velocity of the Pt at the top of the main stack to be 0.664 (my estimate), then the value of β_o at the top of the Cerenkov detector (0.7 gm/cm^2 Lexan equivalent higher) rises only to 0.705

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* In my actual talk, I quoted a larger number of gm cm^{-2} for this interval. That number was the true grammage, which I corrected in my calculations, for the decreased relative stopping power of emulsion. Now that I've noticed that the ordinate scale on Figure 1 is in "Lexan equivalent", the proper number to quote is slightly less than 0.300.gm cm^{-2} Lexan equivalent.

(my estimate). Then Larry Pinsky relaxed his estimate of the Cerenkov threshold to 0.70. The reason it is impossible to fit the "signature" to a Pt ion with $\beta = 0.68$ at a distance of 0.7 gm/cm^2 above the main stack, while requiring only a minor change in β_o, from 0.68 to 0.70, to explain the event on tracing the track backwards from its observed "signature" is the following: the slope of the Bragg curve is very low when β is high, and it is very steep when β is somewhat smaller.

Although I would probably have come to Peter Fowler's explanation of the event, had I not learned of the critical error in the thickness, I find this latter explanation more satisfying. One doesn't have to "stretch" any of the numbers, which can now be accepted as originally published, or as corrected by the authors. The only number that must be rejected is β = 0.5, as measured in the emulsion, and for reasons given earlier.

Finally, it is interesting to note that the heavy ion shown to be responsible for this event is $_{78}Pt^{195}$, with $\beta = 0.68$, and therefore an energy of 67 GeV. If we add the energy it lost in traversing the residual atmosphere above the balloon, its original energy was about 120 GeV, and its magnetic rigidity was 3.1 GV/c. The explanation of the event given in this talk could have been ruled out had the balloon been flown from the most common launch site in this country -- Palestine, Texas. The vertical geomagnetic cutoff in Texas is 4.9 GV/c, so the required particle, with R = 3.1 GV/c could not have been incident there on the Lexan stack, nearly vertically. But at Sioux City, where the flight was launched, the geomagnetic cutoff is 2.0 GV/c, so the required Pt ion is not only allowed, but has a very probable momentum value.

Pt has been shown by the Price group to be the most abundant element in the cosmic ray spectrum, with $Z > 60$. So the required Pt nucleus is near the peak of both the momentum spectrum and the charge spectrum.

I'll conclude by saying that what I find most convincing about the analysis presented here is this: my firmly held belief that what we were seeing in the Lexan stack was a fragmenting Pt nucleus made me question the thickness of the material overlying the Lexan stack. One does not often question the published parameters of an experimental apparatus, and to do so, one must have a very good reason. The fact that the critical error in thickness was uncovered in this manner is what will undoubtably convince most physicists that this analysis is correct.

ACKNOWLEDGEMENTS

Most of us have devoted countless hours to criticizing the manuscripts of papers brought to us by our friends and colleagues before publication. That is an important facet of the "scientific method", that has received almost no public attention. In that spirit, I was the first person outside the Price group to whom they disclosed their very unusual event. I told Buford of my concern that his discovery seemed to be in strong conflict with the most sensitive previous searches for monopoles, in particular one by his group, one by our group, and one by an M.I.T. group. I elaborated on those concerns during the two seminars Buford gave in Berkeley, before the paper was completed, and before the four authors departed for Munich, where they now are.

I've had much help from several persons with special knowledge of the experiment. I'd like to thank Dick Eandi of Houston for several long conversations concerning the Cerenkov detector, Ed Hungerford for two valuable conversations concerning different aspects of the experiment, three persons with experience in the Price group -- Hank Crawford, Steve Ahlen and Gregg Tarlé -- for several discussions and important information, and Peter Fowler, for a phone call telling me first hand what I had previously learned third hand. I am particularly indebted to Rich Muller for checking every one of my calculations by an independent method. We both recognized that my credibility as a critic would be severely damaged if someone could point to an obvious flaw in any of my calculations. Rich found an error that didn't affect my conclusions, but which would have embarassed me if some one else had found it.

Most importantly, I wish to thank Buford Price for his complete openness and obvious desire to have all the facts in the case made known. This is my first appearance in the role of "open critic", and what otherwise might have made for a tense situation --no one really likes to have his firmly held conclusions questioned -- was ameliorated by the fact that Buford and I are friends and long time respected colleagues. I hope that if any of you ever finds himself in the position I'm in today, you also have the good fortune to have as your "debating partner" someone who was raised in the tradition of the "Southern gentleman".

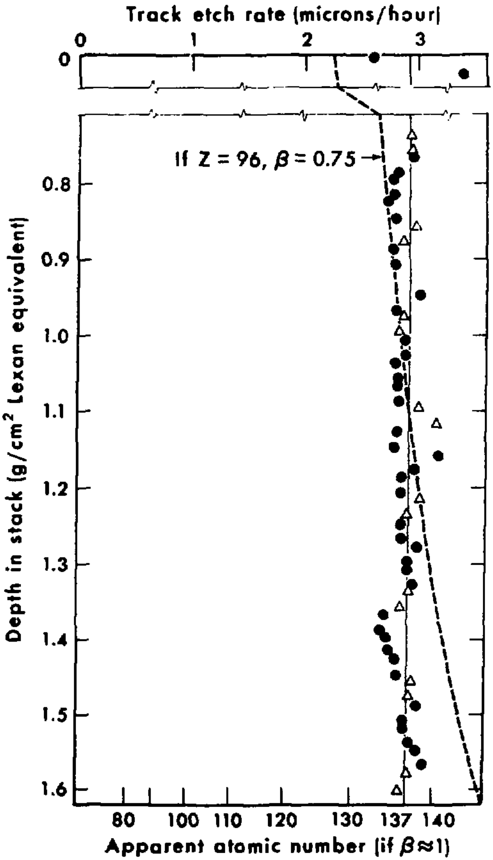
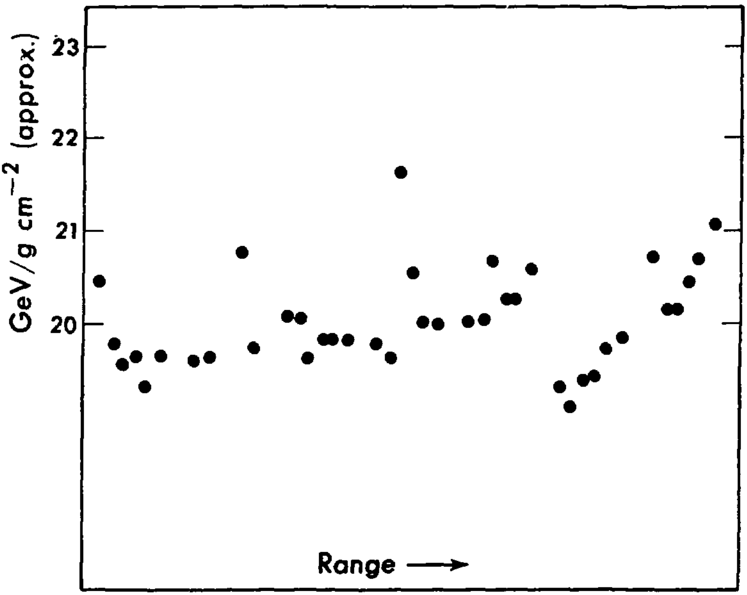


Fig. 2. Etch-rate data. See text. Emulsion data rule out dashed curve or any fit to a nucleus with $\beta > 0.5^{+0.1}_{-0.05}$

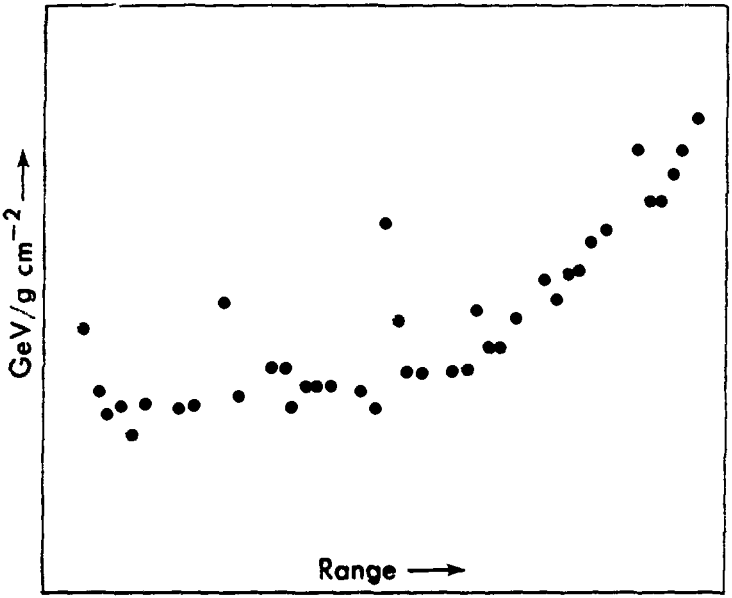
FIGURE 1

XBL 759-8353



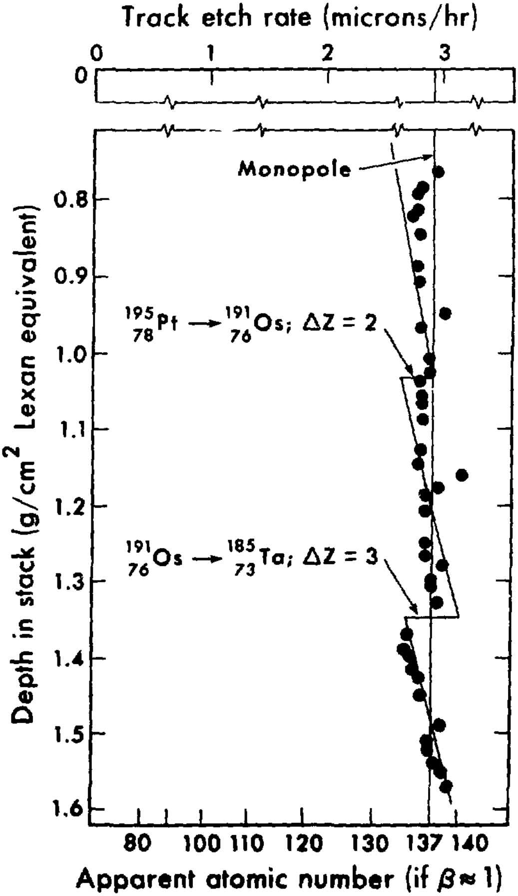
XBL 759-4060

Fig. 2



XBL 758-4050

Fig. 3



XBL 759-4058

Fig. 4

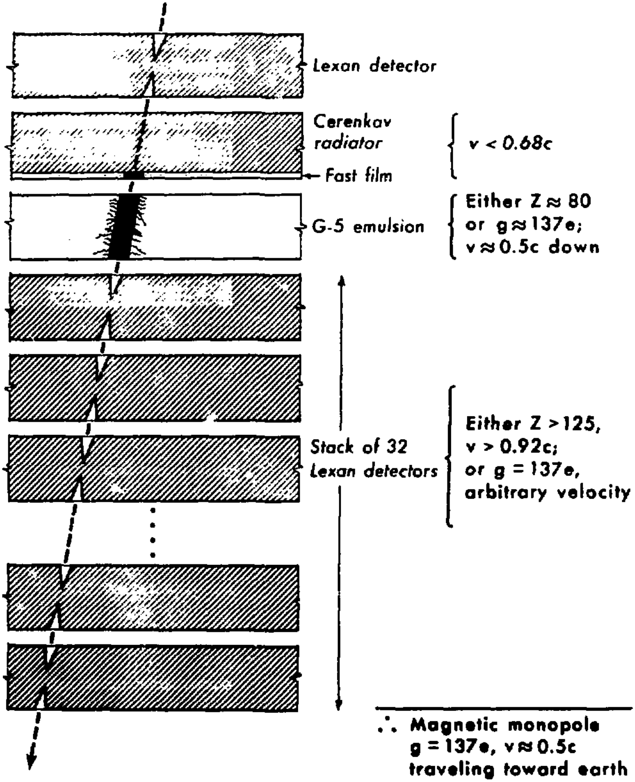


Fig. 1. Stack of balloon-borne detectors.

FIGURE 5

XBL 759-8352