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Comments on GUT Monopole Energy Loss and Ionization

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Today I will be making a few comments about the likely behavior of the electromagnetic energy loss and ionization rates of super-slowly moving magnetic monopoles. Let me first establish my personal credentials in these matters: I am not a full-time practitioner in this field; I merely have been highly interested in these matters as an adjunct to my work in various fundamental physics fields. I write the discussion on Passage of Charged Particles through Matter in the Wallet Card, but I will defer to the judgment of the real experts in this field. The ideas here will be circulated among these experts for revision and correction; I will be glad to pass along their judgments as they come in, but I think that it is now time to set down the problem in a form which I hope will make meaningful dialogue possible between the various monopole searchers (myself included) and the true experts. The reader is, therefore, advised that, while I believe the following conclusions to be reasonable, my point of view has not passed the test of dialectic.

We will be looking at the questions of energy loss rates and ionization rates for super-slow monopoles passing through matter, concentrating on aspects of these issues which affect practical detection techniques. It is worthwhile here to emphasize that there is a potentially great distinction between energy loss rates and ionization rates and that the magnitude of this distinction is really the great issue which must be settled in order to understand the significance of experimental results from present and proposed investigations of the slow monopole question. We easily can understand that energy loss here means the total dE/dX of the projectile due to interactions

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with the electrons of the slowing medium. To the extent that nuclear collisions can be neglected, this so-called "electronic" energy loss is the relevant quantity in questions about whether monopoles stop within the Earth's crust, whether they are slowed by interstellar plasmas, or the signal in a truly calorimetric measurement (measuring temperature rises along the trajectory), etc. Most of our successful detection techniques depend upon the promotion of ground state electrons into states which lie above some energy gap in the material of the detector: Electrons must be knocked completely free from the gas atoms in a proportional chamber gas, electrons must be promoted to a higher band in solid scintillator plastics. I will generically identify these processes as "ionization" and we will keep in mind that this terminology is non-standard.

Let us first turn to the question of dE/dX . Fig. 1 shows my impression of an expert's impression of the behavior of dE/dX for electrically charged projectiles. Elementary particle physicists are very much used to thinking that

$$dE/dX = \frac{4\pi}{m} \frac{\alpha^2}{\beta^2} N_e \left\{ \ln \left(\frac{2m\beta^2 \gamma^2}{I} \right) - \beta^2 \right\} .$$

This sort of formula accounts only for the right half of Fig. 1. The hairy action in Fig. 1 occurs in the left half of the graph and that is where the experts (and the monopoles) tend to live. I have met one certifiable expert who did not apparently even know about the relativistic rise in dE/dX due to the logarithm. Of course, the number of elementary particle physicists who are ignorant of the left half of the graph is quite large. Here is what we learn from Fig. 1.

1. The Planetary System model of the atom is demonstrably false: The following "fundamental limitation" has been "discovered" by altogether too many elementary particle physicists: "A slow projectile, moving at speed V can collide with an atomic electron at rest yielding a maximum recoil speed $2V$ and thus a maximum recoil energy $2mV^2$. Thus, when V is less than valence atomic speeds, there will never be collisions violent enough to excite discrete atomic transitions." We see from Fig. 1 that the

electronic energy loss rates are greatest for speeds lower than this. In fact, one of the earliest triumphs of quantum mechanics (before the advent of the wave mechanics) was Bohr's explanation of total energy loss rates in terms of violation of this Planetary System model of the atom. It is well-known that the cut-off of ionization occurs when the power spectrum of the perturbation does not contain power in the frequencies exceeding the transition energy via the Planck relation.

2. dE/dX falls linearly for the smallest projectile speeds: The earliest successful computation of this linear behavior is due to Fermi and Teller. This and all other successful calculations which I know rely upon evaluate the interactions of the slowing projectile with a degenerate Fermi gas of non-interacting electrons. Note in particular that this linear energy loss realm appears to be explained in terms of multiparticle effects such as the excitation of plasma oscillations within the stopping medium, and that it does not apparently involve any promotion of any electron across any bandgap. I comment here that I am quite skeptical of any plan to exploit this energy loss in the linear region of V as if it were ionization until we hear some pretty convincing arguments in behalf of the scheme.
3. There is a second peak in dE/dX due to nuclear collisions: It is not to be denied that nuclear effects may be important in total stopping calculations. I will not present any here because I do not know any for sure, but I will guess that the mere substitution of $(gv)^2$ for e^2 in the calculations of Coulombic nuclear stopping powers will suffice for the electromagnetic part of monopole stopping due to nuclei. Strong interactions of monopoles are a matter of mystery to me.

So much for dE/dX for electric charges; what about monopoles? I believe that this issue has been carefully treated in the recent preprint of Ahlen and Kinoshita¹, and I believe that their result is correct. The monopoles have a linear behavior of dE/dX with velocity at slow speeds. There has been a good deal of nonsense published in this field to date by several other authors, and I suggest that Ahlen and Kinoshita should be the starting place for discussion of this issue in the future.

Few individuals appear to be inclined to look for stopped monopoles in matter so that the real issue with broad consumer appeal is the question of ionization rates of monopoles within detectors. Discussions of this issue are not developed to anywhere near the clarity of the dE/dX question. Let us start with a feeble computation which I know to be quantitatively dubious. I compute the energy transfer to a harmonic oscillator at fixed impact parameter with respect to the trajectory of the infinitely massive projectile, then integrate the total energy loss rate to an ensemble of such harmonic oscillators randomly distributed around the trajectory. This computation is performed in the so-called "dipole approximation." The integration over impact parameters is cut off at an arbitrary lower limit, b . We can perform this calculation exactly for three types of projectiles, bare magnetic charges, bare electric charges, and shielded electric charges, modeled as Yukawa potentials. The asymptotic results for slow projectiles are:

$$\left(\frac{dE}{dX}\right)_{\text{ionization}} = g^2 \alpha \frac{\pi^2 N_o^2}{m} e^{-\frac{2Ib}{V}} \{1\} \text{ for monopoles ,}$$

$$\left(\frac{dE}{dX}\right)_{\text{ionization}} = \alpha \alpha \frac{2\pi^2 N_o^2}{m} e^{-\frac{2Ib}{V}} \left\{\frac{1}{V^2}\right\} \text{ for point electrical charges ,}$$

$$\left(\frac{dE}{dX}\right)_{\text{ionization}} = \alpha \alpha \frac{2\pi^2 N_o^2}{m} e^{-\frac{2Ib}{V}} \left\{\frac{1}{V^2} + \frac{b}{VIa^2}\right\} \text{ for shielded electrical charges .}$$

(a = shielding distance)

Note the common behavior of these predictions. There is a deadly rapid cutoff for slow speeds as $\exp(-bI/V)$, where I is the energy level separation of the harmonic oscillator levels. There is, in addition, a much more modest polynomial dependence modulating the cutoff. The polynomial reflects the dynamics of the situation while the cutoff seems to be of a more fundamental nature. I do not accept the quantitative behavior of this cutoff with V . I believe that this exponential cutoff reflects the fact that the wave function basis which I have used is for a harmonic oscillator. More realistic wave

functions will result in less steeply falling cutoff behavior. I have translated a work by Walter Henneberg, 1934 Zeitschrift Physik, in which it is asserted that, in the limit of slow projectile speeds, it is accurate to employ the Born Approximation to evaluate transition amplitudes for these types of processes. This violates a commonly held "wisdom" that the Born Approximation is only reliable for $V/c > 1/137$. I find no fault with the Henneberg result (in fact, I find it to be a delightful bit of work), but I will not feel comfortable with this result until a more competent individual does a translation. My translation is available on urgent request only. If the Henneberg result is accepted, then we get a more believable result that the cutoff goes as $(V/bI)^{10}$ or so. I suspect that this is true.

Now let us directly realize the practical significance of a sharp cutoff in ionization rates for super slow particles: When designing a detector, it would appear to be an unattractive choice to allocate much of the finances toward detecting feeble levels of ionization. The rapid nature of the cutoff means that the range of velocities which will yield measurable signals, will be only marginally extended at the low velocity limit. Most directly, I comment that the Baksan detector array appears to provide useful limits despite any reservations which one might have about the Russians' capability to trigger properly on super-slow sub-minimum-ionizing tracks....there certainly is some level of ionization which is sufficient to trigger their apparatus, and if that were even ten times higher than advertised, the lower limit on detectable velocities would be increased, perhaps, by 30%. Thus, I am led to conclude that, in order to extend the Baksan result, one is well-advised to develop detection techniques which have lower values than the cutoff velocity which applies to scintillator detectors. Although I can offer no formal justification, I am forced to appeal to my naive model of ionization rates for a quantitative estimate of this cutoff velocity: $V_{\min} = bI$. Here, the parameter b represents the minimum impact parameter (I will naively take $b = 1 \text{ \AA}$ throughout), and I represents the activation energy for the detectable transitions. My qualitative conclusion is that one must develop detection techniques which require smaller activation energies than scintillators to get optimal increases in our knowledge of monopole flux limits.

I think that the time has come for us to calibrate the quality of these conclusions by appealing to existing experimental data. Fortunately for us, the experts at the International Commission on Radiation Units have given a great deal of consideration to understanding the difficult (and frequently conflicting) experiments which apply to a closely related phenomenon. They have compiled², as ICRU Report #31, a lucid discussion of experimental determinations of the parameter, W , the mean energy loss per ion pair in various stopping media. This document is required reading for all practitioners in slow monopole searches. I have selected the "best" of their many data sets ("best" in terms of having the greatest internal consistency of results among the greatest number of independent investigations) as Fig. 2, W for alpha particles stopping in methane gas. I have appended the initial speeds of the alpha particles along the upper x-axis of this plot. The reader is advised not to miss the datum in the upper left corner. We are forced to conclude that W does not, in fact, remain constant independent of speed of the projectile. W rises by a factor of two from its relativistic value of 29 eV/ion-pair even at the modest slowest speeds represented on this plot. Of course, many of the interesting speeds where we would look for monopoles are well off scale to the left on this plot! I attribute this rise in W to be due to a decrease in the efficiency of slowly moving projectiles to excite ionization because we have no doubt that the total energy of the α -particle was given up to the gas because the α -particle was stopped in the gas. I note that the cutoff speed computed from my naive theory is 0.006 c, which agrees decently with the visual impression of the position of the knee in the data on Fig. 2.

Next, we want to investigate the dependence of the experimental low velocity cutoff of ionization rates upon the activation energy of the slowing medium. Regrettably, the ICRU has not reviewed low I materials so that I have had to search the literature myself. The best source of information which I have located is for Germanium. Here "ionization" means promotion of valence electrons into the conduction band. The activation energy in Ge is 0.9 eV. Fig. 3 shows my interpretations of the data of Abroyan and Zborovskii³ in terms of W , normalized to the measured value at the highest speed

investigated. The rather large error bars represent my estimated uncertainty in certain Cee-thru ruler measurements at the 100 micron resolution level on a Xerox copy of a graph from the pages of Doklady. The cutoff speed which is predicted by my naive theory is 0.0005 c, and I think that there is some justification for qualitative confidence of my notions of this low velocity cutoff, at least for electrically-charged projectiles.

Now let me briefly comment on a recent highly innovative proposal of MacIntyre and Webb to use plastic scintillator arrays to search for the slow monopoles. I feel that there will be a cutoff speed below which the proposed apparatus will not see the quarry and that the real issue is to understand where the cutoff lies, i.e., what the activation energy really is for this scintillator material, in the terms of my simplistic models. There are two candidate values for I here, $I \sim 4.5$ eV for the lowest lying singlet state, and $I = 2.5$ eV for the lowest lying triplet state. Dipole selection rules suppress the triplet state excitation for the stopping of normal electrically-charged particles so that $I \sim 4.5$ eV is the proper choice from any data now available. The question of whether magnetic charges efficiently excite the triplet level is far from settled in my present understanding, and I am unable to produce convincing arguments one way or another on this hard question. There are ways in which Nature could evade the apparent limitations which my simplistic model seems to impose upon scintillator searches: collective effects are not allowed for in my model, spin effects are missing, etc.

I am not too optimistic that spin effects play a dominant role in the slow monopole ionization question. There is the encouraging computation that the force at 1 A distance of the monopole's magnetic field gradient upon the magnetic dipole moment of the electron exceeds the force of the induced electrical field on the electron's charge. When one looks at the exact solutions which exist for elastic scattering of free "electrons" from monopoles, one does not see tremendous modifications when the spin effects are included. Furthermore, the best available computations of dE/dx for monopoles which allow computations of equal footing with and without electron spin (due, again, to Ahlen and Kinoshita), one does not see dramatic modifications due to spin effects.

There is one common misconception which is widely held within the elementary particle community on this general topic, which I think deserves criticism: When the monopole passes a molecule, its static magnetic fields are strong enough to cause various energy levels of the molecule to cross. This is certainly true. The belief is that, when the levels become degenerate, there is a strong mixing of the populations, i.e., if an occupied level crosses an empty level, the result when the perturbation is removed will be a large number of molecules left in the excited state regardless of the speeds at which this perturbation is applied and removed. This would be a great breakthrough for monopole detection via Zeeman Effect pumping. I believe, however, that in the limit of adiabatic application and removal of the perturbation, the molecules will return to their ground state. This belief is based primarily upon the "Too Good to be True" notion: One otherwise could take ordinary materials, expose them to intense pulsed magnetic fields causing the levels to cross, resulting in a medium highly pumped in the optical region. This would allow orders of magnitude increase in the efficiency of lasers, allowing laser fusion to come up from behind the pack and solve the Energy Crisis. "Too Good to be True" (but if it works, remember, you heard it first here!).

Now I want to turn, in my remaining time, to a brief description of the most attractive possibilities which I have investigated as techniques to allow large-area searches for the super-slow monopoles. These techniques rely upon deploying arrays of detectors with activation energies in the 1 eV range, i.e., in the near infrared. The most obvious such technique is to use high-quality Si or Ge solid-state ionization detectors. When cooled, such detectors can provide good timing resolution (on the order of 10 nsec) together with phenomenal ionization resolution (better than 0.1 times the ionization of a relativistic singly-charged particle). The best battery of silicon detectors known to myself to be now available at modest cost (perhaps \$50K and 2 man-years) belongs to the experimental group on the HISS heavy ion spectrometer at LBL. The total area of silicon is 1200 cm². My best design for deploying these disc-shaped detectors consists of nine planes spaced a couple of centimeters apart, each of which has a sparse array of detectors on

a honeycomb lattice. Each set of three consecutive layers has three distinct translations of the honeycomb pattern, so that each set of three layers when viewed in projection forms an over-dense honeycomb pattern. Thus, in effect, one has three dense time-of-flight panels, each of which is composed of three sparse arrays of detectors. Fig. 4 shows one of my Monte-Carlo designs for this array in projection, each circular image represents the projection of three detectors, the axes are labelled in cm. When deployed optimally, see Fig. 4, the array has an effective area of about 150 cm^2 with full angular acceptance for three or more planes hit. Computations indicate that the apparatus can operate at sea level even without an active shield. This project has been stalled for 17 months, principally for lack of manpower. Anyone interested in investigating the possibility of reviving this project might be well advised to contact me at ANL or Doug Greiner at LBL.

To push on toward the desired huge area searched, single crystal silicon detectors hold little promise on economic grounds. I would like to bring to your attention, however, an idea which I find to be quite intriguing. This relies upon deploying large arrays of modified Lennard phosphors and measuring time-of-flight. The most likely candidate known to me is a 6-element compound consisting of Sr, S, K, Cl, Ce, and Sm. This lovely material is a translucent powder which has the capability of "wavelength shifting" infrared photons (0.9 eV to 1.8 eV) into blue photons (2.6 eV) with considerable quantum efficiency. Of course, this "lighter than air act" proceeds only at the expense of optically pumping the phosphor with ultraviolet photons (4 eV). Fig. 5 schematically shows the career of an electron in this medium. For a charming reference which truly hints at the difficulties in devising and manufacturing such a material, see Urbach and Pearlman⁴. To the best of my knowledge, this material has not been manufactured anywhere for a couple of decades. I have played around with a similar material which is commercially available, but which has Eu substituted for the Ce in my favorite stuff (this yields yellow radiation instead of blue and is impractical because of the economics of sensitive photodetection). Anyone who cares to may come upstairs to my lab and put it through its paces. Kodak Research Labs, the apparent world leader in infrared phosphors, has agreed, at their own expense, to provide me with

samples of the blue-emitting phosphor as soon as they can make satisfactory preparations of the material. This is another situation where I am understaffed to take advantage of the opportunity so that I invite interested individuals to contact me.

There are three time constants of interest to the practical application of this speculative technique to search for monopoles. First, the response time (60 nsec), which determines how accurately timing may be done. Second, the spontaneous de-excitation time (3 months) which determines how quiet the phosphor can be in the absence of signals. Third, the development time (completely unknown) which determines how rapidly feasibility and practicality can be demonstrated. To set the scale on this, the material as described in 30-year-old literature is quite a ways from being practical. I believe that the way to improve it is to decrease the spontaneous glow by, say, six orders of magnitude. This does not appear to be a very imposing challenge to me now, but when I get the blue-emitting phosphor samples this summer, I will be able to come rapidly to grips with the real practicalities of the problem.

Although, I remind you, the feasibility of this phosphor scheme to detect monopoles remains very much in doubt, the subject comes up as to the optimal deployment and location of such an apparatus. The spontaneous glow of the phosphor will be limited by cosmic-ray induced blue photon emission at sea-level, provided that I can meet the desired improvement in the truly spontaneous glow. It seems fairly clear that a considerable saving in expense of highly segmenting the photodetector readout apparatus would result from an underground siting. Fig. 6 shows a schematic of an idealized first-generation stand-alone deployment. It is an amusing basis for speculation to wonder how one might be able to leapfrog the tedious effort of deploying such an apparatus on a small scale: It seems to me that one might look for ways to retrofit existing proton-decay apparatus, especially those devices which already have photodetectors in place, to exploit this material. I already know of quite a few practical constraints on how such a serendipitous marriage could be implemented, but it might bear some serious investigation.

References

1. Ahlen and Kinoshita, recent University of California at Berkeley preprint, submitted to Phys. Rev. D.
2. "Average Energy Required to Produce an Ion Pair," ICRU Report #31, H. O. Wyckoff, Chairman (1979).
3. Abroyan and Zborovskii, Sov. Phys. Doklady 7, 417 (1962).
4. Urbach, Pearlman, and Hemmendinger, JOSA36, 372 (1946).

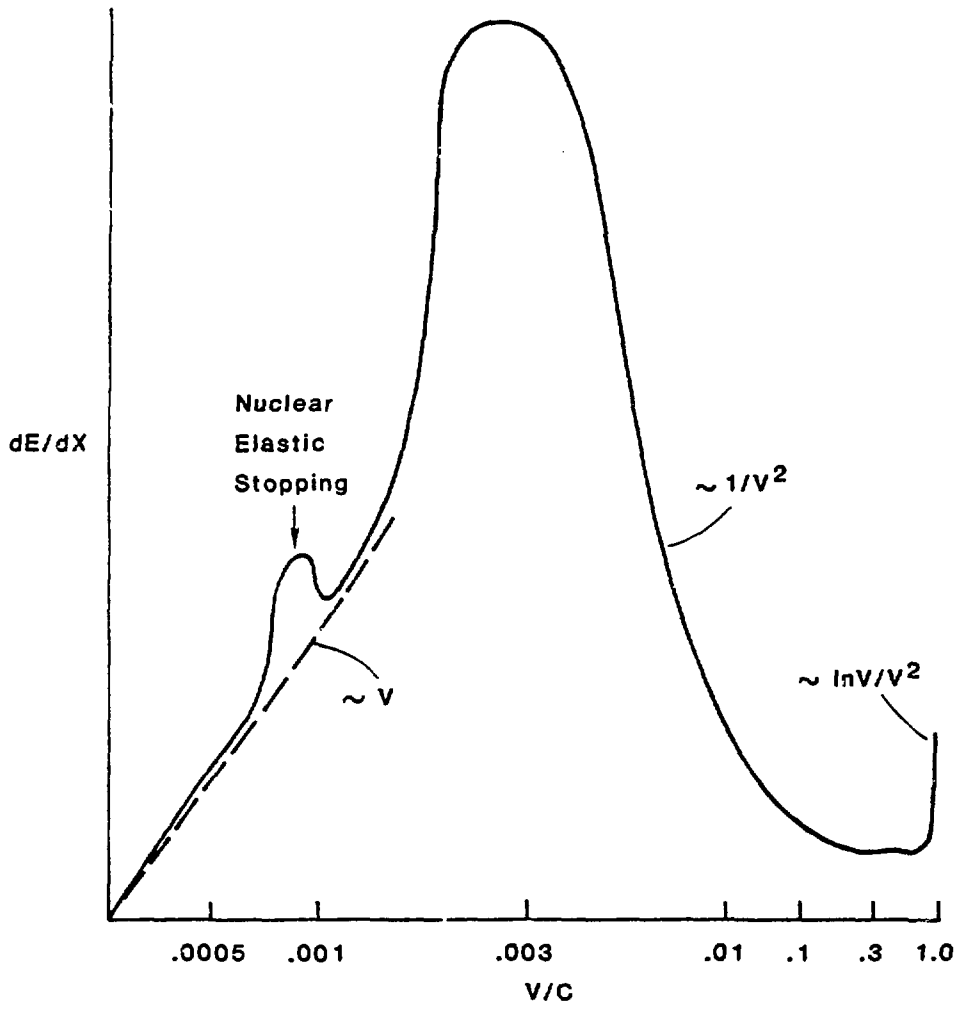


Fig. 1. My impression of an expert's impression of the velocity dependence of dE/dx .

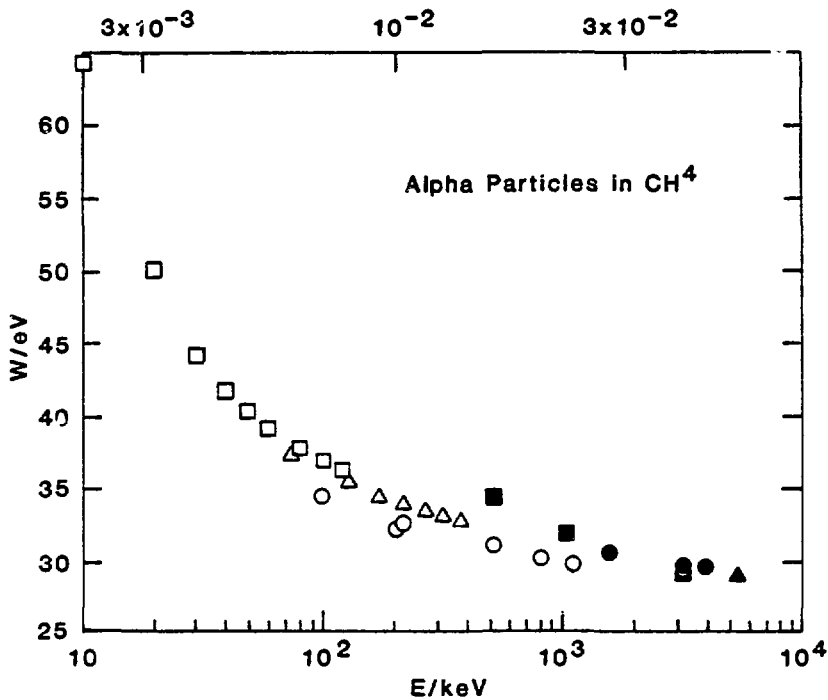


Fig. 2. Experimental values of W (eV/ion pair) for alpha particles stopping in methane gas. Energy E refers to the initial energy of the alpha. Source: ICRU #31. Data points and authors' stated uncertainties are:

- MacDonal and Sidenius (1969); $\pm 3\%$
- Kuhn and Werba (1978); $\pm 3\%$
- Jesse (1968); $\pm 1\%$
- △ Nguyen et al. (1979); $\pm 3\%$
- ▲ Mean value for 5.3 MeV, from ICRU #31; $\pm 0.7\%$
- Varma and Baum (1978); $\pm 0.4\%$

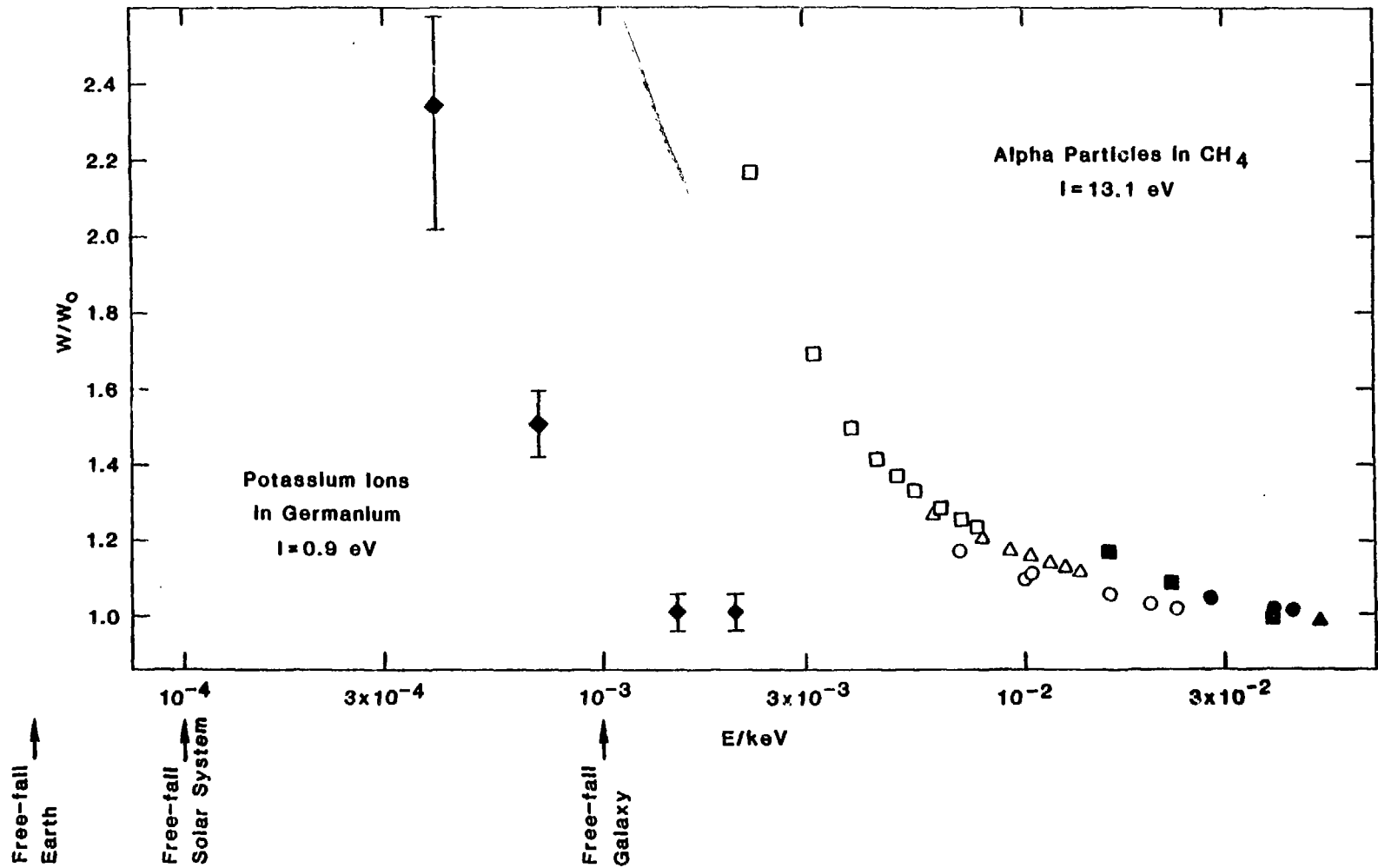


Fig. 3. Comparison of low-velocity-cutoff of ionization for high and low activation energy slowing media. Data on CH_4 from ICRU #31. Data on Ge from my reading and interpretations of Abroyan and Zborovskii.

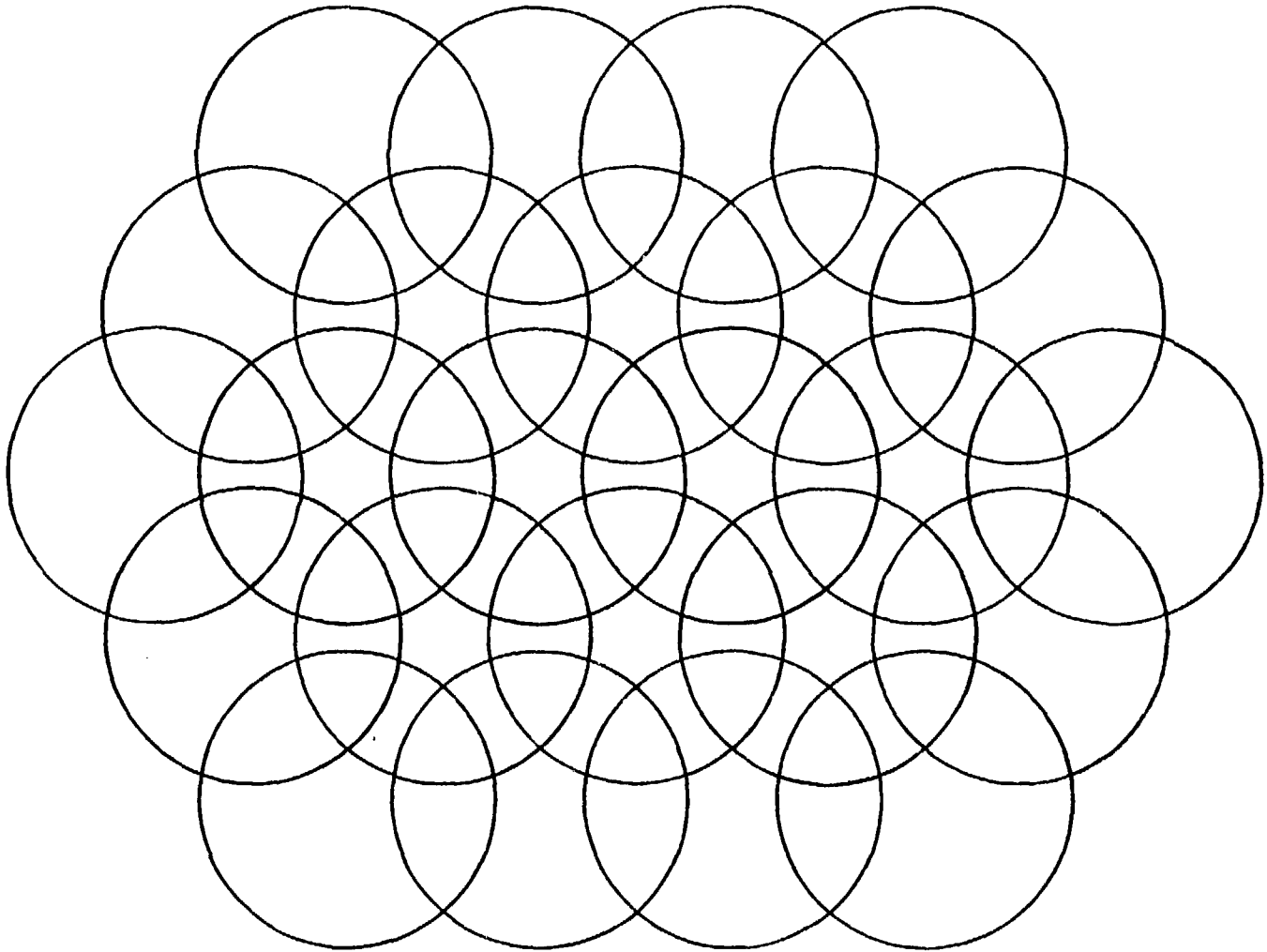
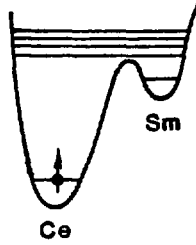
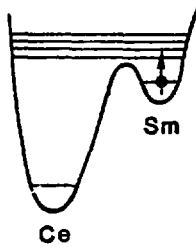


Fig. 4. End view of 9-layer silicon detector array. Each circle represents the projection of three 4.6-cm diameter detectors.

Ground-State



Pump with U.V.



Stimulate with I.R. from
Monopole Weizacker-
Williams Field

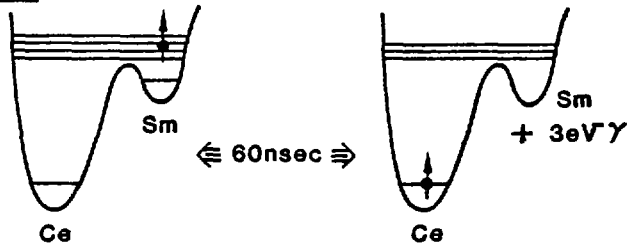


Fig. 5. Schematic career of active electron in Sr-S-K-Cl-Ce-Sm infrared-sensitive phosphor.

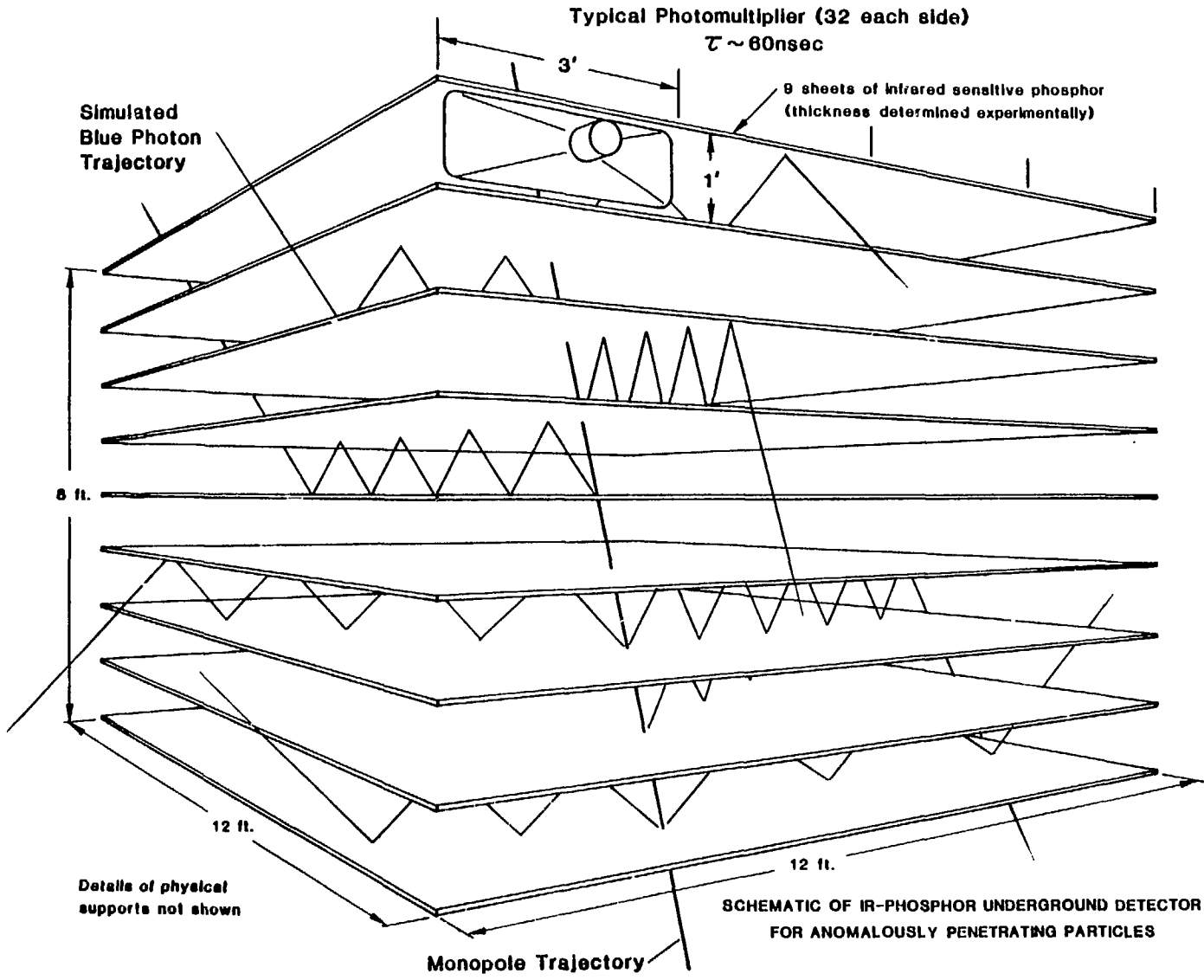


Fig. 6. Schematic of early-generation stand-alone monopole detector using infrared phosphors in 8 sheets.

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