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Decay Rate and Other Properties of the Positronium Negative Ion

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ABSTRACT

A new method for detecting the positronium minus ion is described, and the possibility of a long positronium mean free path in a solid is discussed.

1. INTRODUCTION

I am going to talk about the decay rate and other properties of the positronium minus ion. This is a workshop, so I don't have to apologize for the fact that you're catching our experiment in mid air: we don't have an answer yet, unfortunately, for Y. K. Ho's table. The reason I put "other properties" into the title is that in the process of trying to do the experiment we found out a little bit more than what we wanted to know about how positrons and positronium interact with a foil. I will be asking my theoretician friends to help out in figuring what a positronium atom does going through a foil. How does it break up? By way of introduction, I'll remind you about John Wheeler's famous paper, in which he invented the polyelectron at the same time as a couple of other people invented positronium; I'll describe the slow positron source that is used to do these experiments with positrons; I'll show you the ancient method for the production of positronium minus by beam foil and the old lifetime measurement; I'll tell you briefly about our new effort to detect positronium minus by double charge exchange; Finally I'll be asking what's wrong. This will be the meat of the talk where you can help me out. I will show you our one pitiful lifetime curve which unfortunately needs to be extrapolated to infinite energy to get the answer: we're still working on it. At the end I'll say just a couple of words about what's next.

2. POLYELECTRONS

Lest we forget the inventor of the polyelectron, John Wheeler, I will remind you that his 1946 article asked the question, "Can you get clusters of various of various sizes of electrons?"(1) Wheeler predicted that positronium and the positronium minus ion would be bound, but he was unable to get binding for positronium molecules with his simple wave function. You have heard from Y. K. Ho that lots of work has been done since that time.(2) In particular, the lifetime of Ps^- has been calculated and would be interesting to measure accurately because of the current interest in the the triplet lifetime being measured by the Michigan group for the last 10 or 15 years.(3) There is a descrepancy, and we do not know whether the theory is really going to be right. As an additional test it would be interesting to measure the singlet lifetime, but its eighth of a nanosecond lifetime makes it pretty hard to do. An alternate would be to measure the lifetime of positronium minus ions which contains in it a large factor that is due to the singlet lifetime. We would need to achieve parts in ten to the three or four accuracy in order to make a useful contribution towards the solution of the controversy. Unfortunately, I can only tell you about why we haven't gotten that accuracy yet.

3. EXPERIMENT

The whole experiment starts with the usual slow positron beam,(4) where slow

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positrons are made by moderating them in a layer of some material, either an insulator or a metal. For this particular experiment we're using a solid neon moderator.[5] We obtain a beam of roughly a quarter of a million positrons a second using a 5 mCi source of Na^{22}. Positronium minus ions can be made by putting relatively slow positrons through a thin foil.[6] In the first experiment the ions were accelerated with a grid into a field free region where they annihilated, giving Doppler shifted photons that were counted by a germanium detector. In the spectrum shown in Fig 1 we see a line from positrons that annihilated somewhere in the foil, and a Doppler shifted line that moves when you apply more electric field to shift the positronium minus velocity in the direction of the detector.

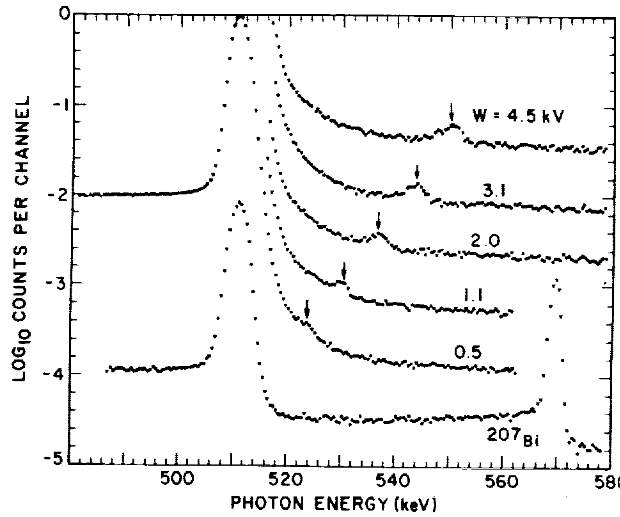

Fig. 1 Annihilation γ-ray energy spectra obtained for five different ps^- acceleration voltages W. [From Ref 6]

Especially relevant to our problems today is Fig 2 which shows (large error bars) the yield of positronium minus as a function of the energy with which the positrons are implanted into the foil. The small dots are the the transmission of the positrons through the foil as a function of energy. My interpretation at the time was that you get the most positronium minus when you have the greatest density of straggling particles near the surface of the foil. The six or seven measured data points agree with what you would expect: the derivative of the stopping curve does have a peak roughly coinciding with the maximum yield of Ps^-. It looks like the yield has a single broad peak, but more precise data suggest that things are more complicated.

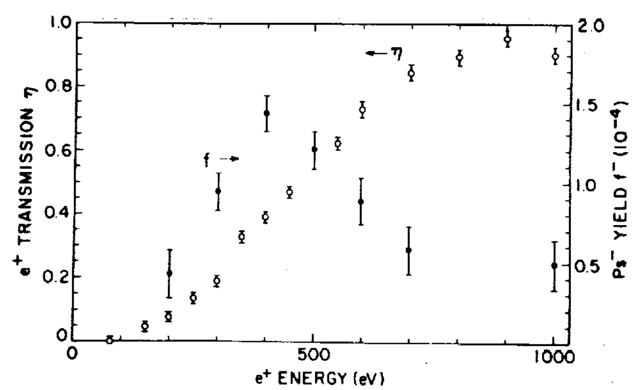

Fig. 2 Positron transmission probability η and Ps^- formatioin fraction f^- plotted vs positron incident energy E. To account for grid losses, F^- should be multiplied by 2.0 ± 0.2. [From Ref 6]

The lifetime was measured some years ago by carefully determining the amplitude of the Doppler shifted peak again with the germanium detector.[7] As you change the distance between the formation foil and the acceleration foil, the proper time that the positronium minus spends is proportional to the distance. By plotting amplitude versus calculated time, you can get the lifetime, as shown in Fig 3. Unfortunately, the positronium minus is coming out of the foil with velocities comparable to atomic velocities, so you have to extrapolate to infinite acceleration in order to get the right answer. The extrapolation to infinite energy is right on top of Y. K. Ho's prediction.[8]

To do a better experiment, we would like to get rid of the germanium detector, which is inefficient, and we have to go to higher voltages to reduce the size of the extrapolation needed. There has to be an improved way of moving the foil because, in the previous experiment, the foil was on the end of a manipulator about one foot away, and I had to measure the distance with a traveling microscope. The present attempt has a much better moving mechanism: three synchronous linear motion vacuum feedthroughs define the foil position to 10^{-2} mm precision.

Our new effort uses a tandem acceleration method depicted in Fig 4. A positronium formation foil is bombarded by a quarter of a million positrons a second. Any positronium minus formed is accelerated by what we call "the analysis grid", which has a potential W across it for measuring the lifetime. As before, the distance d between the analysis grid and the formation foil is variable. By varying d while measuring the count rate, you determine the lifetime. Following the analysis grid is another electrode that accelerates the positronium minus to some large voltage on the order of fifty kilovolts. At this point, there is a thick carbon film that is supposed to strip the positronium minus and turn it back into two electrons and a positron. On the other side is a grounded electrode that repels the electrons, but accelerates the positrons. The positrons emerge with four-thirds times the acceleration potential on the stripping foil, which would be about 67 kilovolts if the stripping potential is 50 kV. We thus have a definite Ps^- signature of rather high energy positrons which cannot be produced any other way except by having taken a torturous route of making positronium minus and getting stripped. About two and a half meters away, to get rid of gamma rays, we have a charged particle detector (a silicon detector) which detects the energy spectrum of the positrons to distinguish them from any background that might be there from ions.

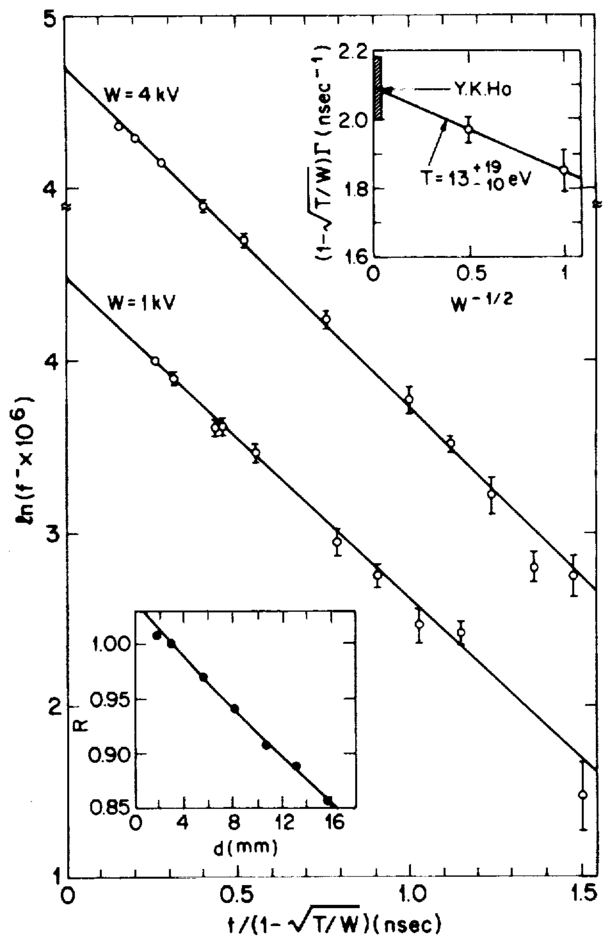

Fig. 3 Log of the relative amount of Ps^- surviving for a time t corrected for the initial Ps^- kinetic energy T. The inset shows the extrapolation of the decay rate to infinite acceleration potential W. [From Ref 7]

An ion shield, one tenth mil of mylar, covers the detector. Unfortunately, there were so many ions that we had to use a plastic scintillator in coincidence to cut the background rate.

Fig 5 shows spectra taken with the silicon detector at three different acceleration voltages. The potential applied to the stripping foil is 20, 34, or 45 kilovolts. The peak due to the positrons that make it through the whole apparatus is evident, and there is a sloping background due to ions which looks like it's not important, at least at the higher acceleration potentials. There is a plateau below the peak, and I don't know what that could possibly be, since the fraction of particles that scatter [9] is supposed to be only about 15%. If you spread that fraction over a large energy range, it should not give a 10% amplitude. That is problem number one; but at least we are producing tandem generated positrons.

Using our double charge exchange Ps^- signal, we have remeasured the yield of positrons as a function of energy in Fig 6. As in Fig 2, we get a blob as a function of energy, peaking at slightly higher energy because the film is a little thicker. The film is nominally 15 angstroms thick, a cloudy carbon film on top of a glass slide that is slid off onto water, to be picked up with a grid. The thicknesses are nominal, since there are obviously layers of grease and water. Notice in the new data at the low energies, there seems to be a plateau and a real threshold at a ridiculously low energy of 25 volts. I have no idea what this structure means. If the film is really only 15 angstroms thick, I suppose that is an average thickness, and once in a while there could be a flake that's only one crystal layer thick that might be 5 angstroms. However, I would think that there would be a series of plateaus for different thicknesses and that they shouldn't occur down at 50 volts. If anybody has a suggestion, I would be happy to hear it. It will go right into the book if you have anything to say.

Another mystery is why is the yield so small, about five times smaller than we saw in 1981 and 1983. We have mapped out the count rate as the detector is moved around. As far as I can tell, all the fast positrons seem to be hitting the detector. The grids that the foil is on and the acceleration grids have 90% transmission. Putting in all the grid correction factors does not account for the apparent losses. The grid corrections are just about the same as they were in 1981.

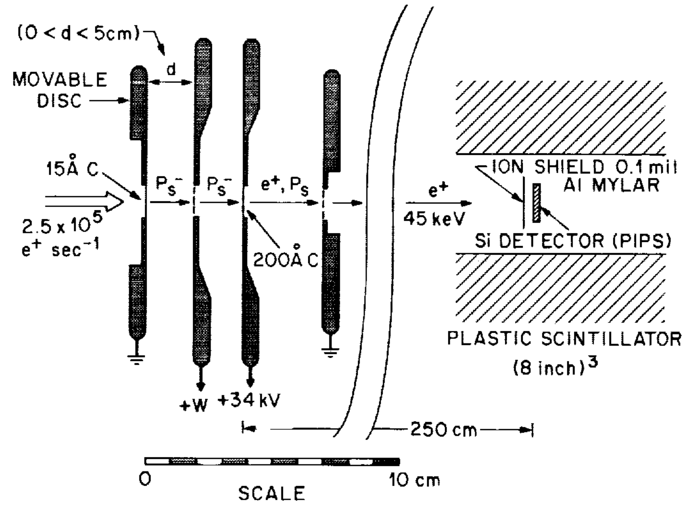

Fig. 4 Tandem acceleration method of detecting Ps^-.

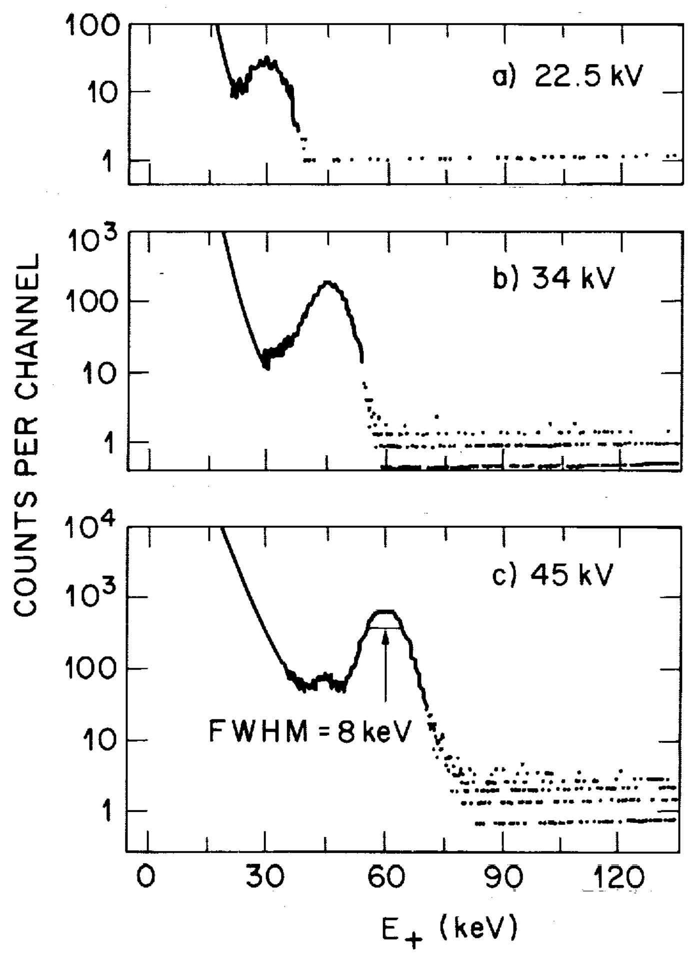

Fig. 5 Energy spectra of the fast positrons detected by the surface barrier detector.

Table I shows the yield of positronium minus measured at 20, 34, and 45 kilovolts. The coincidence rate is corrected for decay loss in various places where the positronium minus is being accelerated. There's a negligible loss in the mylar foil.[10] The positron beam rate is what you divide by in order to get the total fractional yield of positronium minus. There's a constant grid transmission coincidence efficiency, and a little bit of back-scatter loss. The net result is a positronium minus yield of about 7×10^{-5} independent of the energy. That is a surprise to me because the only way that I can think of to make the yield smaller than the $2-3 \times 10^{-4}$ found previously is to have the stripping foil be less efficient.

4. DISCUSSION

Now we come to the central point of the talk where I ask you what happens to positronium and positronium minus when it gets stripped. There are several convenient theories. The simplest theory, which turns out to be the same as Surko's Theory that he told me about at breakfast, is that you simply use multiple scattering calculations and an independent particle approximation. Let's just talk about positronium going through the foil. In the time scale over which the particles are in the solid the positron and an electron don't orbit at all. They just go straight through the solid without moving relative to each other. In this approximation, you would say each particle gets an independent kick from scattering off the potential which, in this case, would be a frozen potential of the solid because the electrons don't have time to move either. It's very easy to calculate the perpendicular kick that each particle gets: it will be the perpendicular electric field integrated times dt. The amplitude for making a transition turns out to be the perpendicular momentum kick times the dipole matrix element. Summing all the dipole moments that lead to the continuum gives a transition probability that is perpendicular kick squared over 2m

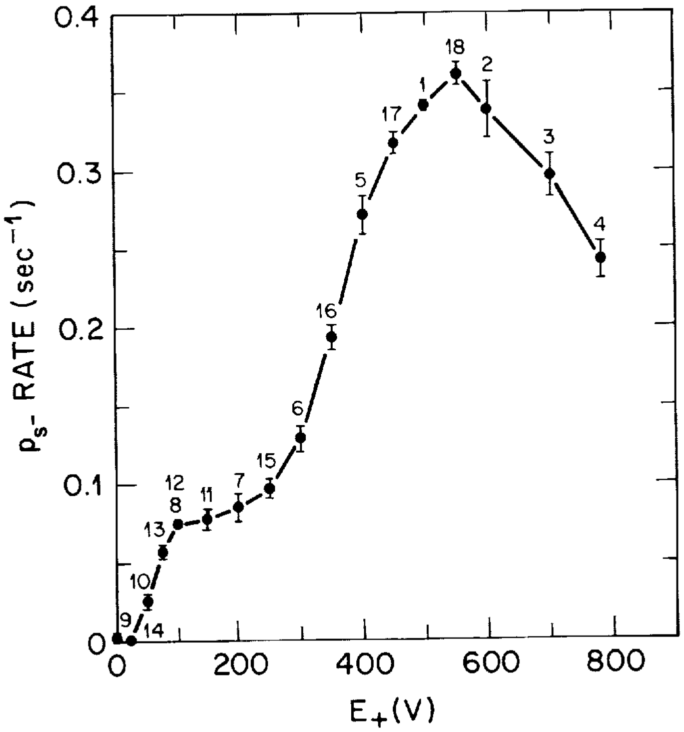

Fig. 6 Yield of Ps^- versus positron energy uncorrected for transverse beam motion.

Table I

Accel. Pot. [keV]	22.5	33.75	45
coinc. rate [sec^{-1}]	0.225(6)	0.341(3)	0.385(2)
decay loss $e^{-\Delta t/\tau}$	0.113	0.135	0.140
loss in mylar [%]	14[a]	6.5[a]	3.5[a]
beam rate [$e^+ sec^{-1}$]		2.5×10^5	
grid transmission		0.45 ± 0.1	
coinc. efficiency		0.4 ± 0.1	
backscatter loss [%] from Si detector		15[b]	
f^- [10^{-5}]	6.1 ± 2	7.1 ± 2	7.5 ± 2

a) R. D. Evans, *The Atomic Nucleus*

b) V. E. Cosslett & R. N. Thomas, Brit. J. Appl. Phys. *16*, 770 (1965)

Y. K. Ho: You showed a slide with a laser interacting with the Ps^- beam; can you measure the photoionization cross section and the electron affinity?

Mills: Yes, Marv Leventhal and I are working on it, and Lewis Rothberg has given us a laser. We'll have the answer for you in ten years.

Y. K. Ho: So you can measure the binding energy of Ps^-?

Mills: Maybe, but its pretty hard to find the binding energy from the photoionization threshold because the cross section vanishes at threshold.

Marv Leventhal: We can find the binding energy quite accurately from the location of the Feshbach resonances.

Alex Weiss: What are the wigglers that you showed? [See Fig 23 of Ref 16]

Mills: The wigglers are to excite the triplet-singlet hyperfine resonance in a fast monoenergetic positronium beam. By moving two identical wigglers one can obtain Ramsey fringes in the triplet positronium abundance, and so measure the hyperfine interval accurately.

Richard Drachman: Is there a preliminary lifetime result from the new data?

Mills: Unfortunately, no.

Alex Weiss: Have you thought about making a tuneable gamma-ray source?

Mills: Not very hard. The gamma rays are emitted isotropically in the center of mass, so its not like having a laser. If you had a very intense relativistic beam of Ps^-, the photons would be foreward directed, and it would be a good idea.

6. *POSTSCRIPT*

In a subsequent experiment using a Ge detector in a geometry similar to that of Ref 7, we found that the Ps^- yield of the 0.3 $\mu g cm^{-2}$ foil (15 Å thick) is in fact about an order of magnitude less than a 0.6 $\mu g cm^{-2}$ foil, and the yield of the latter is in agreement with the measurements of 1981 and 1983. The stripping foil was observed to be damaged over a significant portion of its area. We conclude that

1) very thin carbon films are perhaps multiply connected like lace;

2) more care is required to prevent high voltage damage to the stripper foil;

and 3) there is no evidence to suggest that energetic positronium has a particularly long mean free path.

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