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Professor Hideki Yukawa
Department of Physics
Kyoto Imperial University
Kyoto, Japan

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PERSONAL CORRESPONDENCE

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~~NEW YORK 27, N. Y.~~

January 2, 1948.

Professor Hideki Yukawa
Department of Physics
Kyoto Imperial University
Kyoto, Japan

Dear Professor Yukawa:

I have been very glad to receive the two sets of issues of Progress of Theoretical Physics. If you have any of volume 1, number 1, I would be happy to have it.

Some months ago, I inquired of the local post office if I could send reprints to Japan, and was told that I could, so that I sent you a complete set of the war time reprints of my work. However, the envelope was returned, and I found out that it is still forbidden to send any printed matter to Japan. I think this is outrageous, but can't do anything about it, except to keep the reprints for you until I can send them. It is permitted (I think!) to write personal letters, and so if you excuse the informality, I will write you somewhat of the state of physics here. First I give a list of the titles of the reprints I would have sent you.-

1940 Passage of Uranium Fission Fragments through Matter. (You may have the first few already.)

1939-1940 Deviations from the coulomb law for a proton. (Argument with Heitler, etc. over meson theory predictions about electron-proton interaction.)

1941 On the Einstein Condensation Phenomenon. (Letter pointing out that ideal Bose gas in a gravitational field would really condense in physical space as well as momentum space.)

1941 Internal diamagnetic fields. (Calculation on Fermi-Thomas and Hartree fields of diamagnetic magnetic field at nucleus of an atom. Correction used in molecular beam experiments on nuclear magnetic moments.)

1943 The propagation of order in crystal lattices. (Statistical mechanics of two dimensional Ising model, and connections with work of Kramers-Wannier and Zernicke.)

1944 Extraction of electrons from a metal surface by ions and metastable atoms. (Straightforward calculation, useful in my thinking about later work on metastable hydrogen atoms, see ~~below~~ over.)

1944 Effect of Nuclear electric quadrupole moment on the energy levels of a diatomic molecule. (Applied to molecular beam work.)

1946 Theory of a microwave spectroscope. (Steady state response of a large metal cavity, in which vapors can be placed in order to study their absorption of radar frequencies. War-time research)

1947 Space charge frequency dependence of a magnetron cavity. (Also war time research.)

1947 Fine structure of the hydrogen atom by a microwave method. (by L. and Retherford)

Of these, I think you will be most interested in the last, and I will give you some details of the method and results. As you know, the spectroscopic work on the hydrogen alpha line has for years suggested some deviation from the Dirac theory of the fine structure. Yet, Pasternack's analysis of Houston's and Williams's data was rather discredited by the 1940 observations of Drinkwater, Williams and Richardson. It turns out that Pasternack was right, and the 2s level is raised by about 0.033 cm^{-1}. During the war, I worked on magnetron oscillators of high frequency, even leaving theory enough to learn to build some myself. I then became aware of the fact that the separation of the two 2p levels in hydrogen was just in the frequency range in which we had facilities (10,000 megacycles and up.) The obvious idea would be to observe the absorption in such frequencies in a discharge tube containing excited hydrogen atoms (this was tried in Germany 1932-35, but failed for several reasons.) We managed to hit on a better idea. Namely, to form a beam of excited 2s atoms, which can be done because of the metastability of the 2s level. Such a beam can be detected by the process of electron ejection when the excited atoms hit a metal plate. If transitions can be induced by radio-frequency waves from 2s to either 2p state, the detected current will decrease and resonance curves can be taken in this way. We found resonance frequencies corresponding to 9,950 and about 1000 megacycles, which indicates clearly that the 2s level is higher than the $2p_{\frac{1}{2}}$ level by 1000 m.c. while the separation $2p_{\frac{1}{2}}$ to $2p_{3/2}$ is roughly the 10,950 m.c. predicted by theory. The present accuracy is probably only 100 m.c. but work is continuing, and we hope to do much better. The beam is formed by bombardment of a stream of H atoms by electron(s) and the H atoms are formed from H_2 by thermal dissociation in a tungsten oven. The metastable atoms have to be protected from electric fields in order to avoid Stark effect mixing of 2s and 2p. We are working also on the same problem for singly ionized helium.

Following some conference remarks by Oppenheimer, Weisskopf, and Schwinger, Bethe made a very quick non-relativistic theory of this level shift. I will describe a similar calculation, but the difference from his is not essential, at least non-relativistically. Consider the fully non-relativistic hamiltonian for an electron interacting with the transverse electromagnetic field. Calculate the second order self-energy of an electron in a bound state. This diverges logarithmically (worse, unless you keep in the recoil energy in the denominators.) However, the difference between the self-energy for 2s and 2p converges, and the result is

$$W_{ns} - W_{np} \simeq \frac{8}{3\pi}\left(\frac{e^2}{\hbar c}\right)^3 \text{Rydberg} \frac{Z^4}{n^3} \log \frac{2mc^2}{\Delta E} \qquad \Delta E = 17.8 \text{ Ryd. for } n=2.$$

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This gives somewhat too high a shift, and actually Bethe's result doesn't have the 2 in the logarithm, and gives 1040 megacycles, which is in very good agreement with the present rough measurements. He got this by assuming that a relativistic calculation of the self-energy would make the effective cut-off for virtual quantum frequencies equal to mc^2/h. At least five people* are busy now trying to calculate the shift with relativistic theory. At present, the following is clear: the expressions converge, and give a value very close to that obtained by Bethe, with the modified logarithm. It is not clear, however, that all the methods being used correctly take into account the structure of the electron in an invariant way, if indeed, such is at all possible. The numerical differences however, will probably be small. No one knows what will happen in higher order than second.

*Schwinger
Weisskopf
Bethe
Lamb
Student of Fermi
Pauli?

You can regard the forgoing as the effect of an electric field on the electromagnetic self energy of an electron. If instead, you calculate the effect of an external magnetic field on the self energy (even of a free electron) you find that the effective Lande g-value is not 2, but higher by the factor (1 plus alpha over two pi) This result is due to Schwinger, and is supposed to be on the basis of relativistic hole theory. (If such a calculation were made for a non-relativistic electron with a Pauli type spin, the g value would be less than two, corresponding to the fact that an electron emitting and absorbing magnetic dipole type radition will some of the time have its spin against the magnetic field. That the relativistic result is g greater than two must be somehow related to the zitterbewegung.) Now this change in the g-value has probably been observed in two ways. First, in the hyperfine structure of the ground state of hydrogen, where the splitting is greater than that calculated on the basis g value of two. Second, by observations on the atomic beam spectrum of gallium for which moments of $p_{3/2}$ and $p_{\frac{1}{2}}$ states were made in the same magnetic field, and also on the $s_{\frac{1}{2}}$ state of sodium. Here, there could be interconfiguration mixing, but the probabilities are against it.

You will see that despite the divergences of quantum electrodynamics, it is possible to get interesting, possibly correct results, by a process in which the singularities are not exactly thrown away, but let us say, reinterpreted, namely as being part of the mass of the electron. Most of the electron mass is regarded as mechanical, and the electromagnetic contribution must somehow be regarded as made convergent at something like 137 mc^2. Since, however, the fine structure shift and the correction to the g-value are given for frequencies below mc^2, the exact method of cutting off isn't too important. (I think that it probably is, nevertheless, although perhaps not numerically important.) Such tricks might be of interest in mesotron theory.

Another bit of physics which you might find of value: Fermi, and also Rabi et.al. have done experiments which set an upper bound on the interaction between an electron and a neutron (apart from the magnetic interaction of Bloch.) The experiments involve the study of the interference scattering of neutrons by heavy atoms, in which the wave scattered by the nucleus and by the electrons is studied. The angular dependences and energy dependences are different, so that it possible to determine the contribution of the electrons. Fermi is able only to set an upper limit for the interaction: (in terms of a square well potential with the electron radius for radius 300 plus or minus 5000 volts.) Rabi's result, probably capable of improvement is that there is a repulsive potential of about 5000 volts. The interpretation has been that this is just due to the static interaction of the electron with the mesotron cloud about the neutron and the proton core at the center. The value of g^2 depends on the mesotron mass + theory assumed, but seems to indicate the weak or intermediate coupling theories instead of the strong coupling theories.

The magnetic moments of both hydrogen three and helium three have been measured. For the former, the result is 7% greater than the proton moment, and I don't recall the latter, ~~but both are compatible with~~ but neither ~~are~~ is compatible with calculations based on additivity of moments of proton and neutron and mixing in of various ammounts of higher configurations. A Swiss, Villars, has pointed out that the additional magnetic moments from the exchange currents can just explain the effect. There has also been strong evidence for exchange in the scattering of hundred million volt neutrons at Berkeley.

I hope this rambling letter will not be too confused to help you, and I would be glad to answer any questions (that I can) should you care to write them to me. Much will depend on how rapidly the mail goes from here to Japan.

Allow me to express my admiration for the high quality and quantity of the theoretical work in Japan under the present difficult conditions.

Yours sincerely,

Willis Lamb

Willis E. Lamb, Jr.