g-2 TECHNIQUES: PAST EVOLUTION AND FUTURE PROSPECTS

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ABSTRACT

Some history, especially the resolution of early doubts as to the reality of a magnetic moment in the free electron are given. A survey is made of various techniques that have been proposed or used for the electron, positron and muon. Experiments currently under way are described. The situation in respect to precision is summarized.

INTRODUCTION

I have been invited to give an over-view of the researches on the magnetic moment anomaly, or g-2, of free leptons--how they got started and where they may be going. Since I have been in the business more or less from the start, I probably will see more when I look backwards than when I look into the crystal ball. I will touch mainly on some points that have intrigued me and that are not generally found in research papers, rather than attempt to make the coverage comprehensive. A review in full detail is available elsewhere[1]. I was hesitant about giving this talk to this group, for there is not very much in it that will apply to high energy problems. I hope you will find it interesting anyway.

SOME BACKGROUND

At the heart of g-2 experiments is the picture of the electron (and later the lepton) as a spinning magnet precessing in a magnetic field in a purely classical fashion. But the idea that one was allowed to think of a free electron precessing, or in fact to think of its magnetic moment as having any meaning at all was a very long time coming. I think the bit of history leading to that turnabout in viewpoint is interesting enough to deserve a few minutes of our time. It starts with the Stern-Gerlach experiment, performed before the electron spin was discovered.

You recall that Stern and Gerlach sent a beam of neutral atoms through an inhomogeneous magnetic field and found that the beam was split into two components, indicating orientations of the magnetic moment parallel and anti-parallel to the magnetic field. The impact of that experiment was in the fact that the beam was split into two parts, not a smear, showing that the magnetic moment was quantized with respect to the field direction. When Goudsmit and Uhlenbeck came along just a few years later (1925) with the discovery that the electron had a spin, including a magnetic moment, speculation must have arisen quickly as to whether its magnetic moment could be demonstrated in a Stern-Gerlach type of experiment. But any such dreams were discouraged by an elegant little proof, said[2] to have been given by Niels Bohr, in one of his lectures at about that time. He applied the uncertainty principle to any experiment by which it might be attempted to separate spin states of the electron by passage through an inhomogeneous magnetic field.

In essence Bohr's argument runs so: Since the magnetic field is inhomogeneous, the Lorentz force on the moving electron, $e\vec{v} \times \vec{B}$, depends on the location of the electron path. The location of the path is uncertain to the order of the DeBroglie wavelength. This in turn makes an uncertainty in the Lorentz force that is of the same order as the force due to the magnetic moment of the electron. Everything else cancels out. The proof is a textbook classic. The way in which this proof was interpreted over the ensuing decades is curious, to me at least. A case of over-kill. It was taken to mean that the magnetic moment of the free electron is unobservable in principle, and that therefore the assignment of a magnetic moment to the free electron is meaningless. Unless there is something in the proof that does not meet my eye, it showed not that the separation of spin states would not occur, but only that the natural widths of the beam spots would be of the same order as their separation. Experimenters, even in those times, were not easily deterred by large line widths. But the experiment on electrons was never tried.

In 1929, N. F. Mott[3] invented the double scattering method of studying the polarization of particle beams when he did his well-known paper on the polarization effects in the scattering of fast electrons on nuclei. But in the course of it, to use his words, he found a trap that had to be avoided. It was that if, according to then current ideas, electron spins aligned either parallel or anti-parallel to a magnetic field, then even a weak magnetic field parallel to the beam incident on his scatterer would kill the effect he described; the reason being that his effect is an asymmetry in the scattering due to polarization perpendicular to the plane of the incident and scattered beams. He says, then, that he was forced to the view that electron spins must be thought of as precessing about the direction of a magnetic field, rather than as aligned parallel or anti-parallel. And, clearly, if it were to save the double scattering effect, he must have meant the precession to be an observable thing, not just a mathematical device[4]. Mott's way out of his dilemma was, I believe, the first break toward thinking of electrons as precessing magnets. It was bold: it was in apparent contradiction to the Stern-Gerlach result, and it was made before there was any experiment to show that the double scattering actually worked. Mott did not pursue his idea, however, and suggest that it opened a way of measuring the precession and therefore the magnetic moment. If he had, he would have described the experiment that Louisell[5] did for his thesis in our laboratory much later (1953). But then nobody, including ourselves, took Mott's hint. When we finally did the experiment it was because we had invented it over again.

History repeats. When we got around to planning the Louisell experiment the ghost of Bohr's proof came out of the woodwork both in our own camp and outside. Therefore Mendlowitz and Case[6] in our laboratory undertook to find whether the rotation of the plane of polarization in a magnetic field was consistent with Dirac theory. They showed that it was. Several years earlier Tolhoek and DeGroot[7] had published a paper containing essentially the same conclusion. The seeming conflict with Bohr's proof was disposed of in another way by both Rabi and Bloch, who were skeptics at the beginning. They concluded that since the two Mott scatterings are quantum events, and since the exact trajectory between them was not specific, Bohr's requirement was in fact met.

Meanwhile, or in parallel, researches in hyperfine structure were coming to a head, that were to create a great need for high precision measurement on g of the free electron. It is well known how this culminated in the brilliant experiments by Rabi's group at Columbia beginning about 1947, and how the leading theorists joined in to open a whole new field. It is not possible within the scope of this talk to detail the steps or the names involved. They are well known and there are good review papers.[8] I would like only to pinpoint what it was that turned the direction of thinking. In 1928 Dirac[9] showed that a g of 2 came out of a proper relativistic treatment of the wave equations for an electron. This was taken as a kind of basic fact of nature. So when around 1937 discrepancies began emerging between theory and experiment on the hyperfine levels in hydrogen[10], a g value different from 2 was not suspected as the cause; rather, explanations in terms of the effects of the nuclear size were sought. Experimentally, the attack was through the comparison of the fine structures of hydrogen and deuterium, in which only the size of the nucleus is different. The impact of the Columbia experiments was to sharpen the discrepancies to the point where the possibility of an explanation on the basis of nuclear size had to be given up. The sacrosanct g value of 2 had to be looked at. Gregory Breit [11] was the first to put his neck out in print and say there might be something to g in addition to Dirac's 2. Things then went rapidly,then, as you know. As the anomaly in g was explained as vacuum polarization by quantum electrodynamics, which was itself shaky at the time, the effort became as much a development of QED as of the g-value. A triumph all around, and a very bright spot in physics history.

A great opportunity was open. We at Michigan literally walked backwards into it, as has been recounted in a Scientific American article.[12] We had a synchrotron whose only working part was a 400 kev electron gun, and we were looking for some interim experiment to do with that gun. 400 kev was just right for Mott scattering, and Louisell needed a thesis problem. That's how we got in. 23 years have passed and we still are not out!

METHODS: BEAT COUNTING

I would like to turn to some comments on the particular classes of methods for measuring g-2. The first to consider is the one we developed at Michigan, and its variations, one of the variations being the series of beautiful experiments at CERN on the g-2 of the muon.[13] The methods consists, essentially, of finding the frequency of the beat between the rotation of the spin direction and the orbital or "cyclotron" rotation when the particle is trapped in a magnetic well. The beat is at about a thousandth of either of the other frequencies. The initial polarization is held fixed, and a component of the final polarization is measured and plotted against the length of time the particle is allowed to rotate in the trap. You probably have seen one of these sinusoidal plots, either for electrons or muons, which gives the beat frequency.

In the case of the electron both the initial polarization and the analysis at the end are done by Mott scattering in a gold foil.

In the case of positrons the work so far has been done with a radioactive source, so the initial polarization is therefore ready made. The final polarization is found by a clever scheme that was proposed by Valentine Telegdi.[14] Positrons, when stopped, form positronium in two states having different lifetimes to annihilation. In a strong magnetic field the ratio in which the states are formed depends on whether the spin of the positron is parallel or antiparallel to the magnetic field. The ratio of the two states, and therefore the polarization, can be found by counting the delayed vs. the prompt annihilation radiation. In the muon experiments such tricks are unnecessary: the muons are born polarized, and they reveal their final polarization through the directions of their decay products.

The three applications described enjoy a common advantage, but each reaches a limit of precision in its own way. The common advantage is that by measuring the beat, or difference frequency, rather than the spin and cyclotron frequencies separately, one is ahead in precision by a factor 1000 at the start. A difficulty common to these variations of the method is that the time average magnetic field the particle experiences in the trap must be determined. The relation is as follows: g-2 = 2a where a is the "anomaly", equal to ω_D/ω_0. ω_D is the beat (angular) frequency, measured directly. ω_0 is the <u>zero energy</u>, or non-relativistic, cyclotron (angular) frequency, equal to eB/m_0c. Since all of these experiments are run at relativistic velocities, ω_0 cannot be measured directly but must be found from the magnetic field. By definition a well, or trap, is not a uniform field. The particles in the trap are spread over some range of energy levels in the well, and they are also oscillating in the z direction (parallel to the field). You can see that the effective field for the electron while it is in the trap is hard to determine precisely. To minimize the error from this source the well is made shallow, that is, as near as possible to a uniform magnetic field. But this is a trade-off against the efficiency of trapping particles at injection. In the case of our electron experiments the well was made only about 0.1% deep, but still that source of error predominated over others, such as the pitch angle of the orbits, stray electric fields and the counting statistics.

In the case of positrons the accuracy has, so far, been limited primarily by the statistics of counting, rather than by the ω_0 error. With a radioactive source the direction, momentum and time of emission are not controllable, and after narrow cuts are made in all three of these parameters, even with a source of several curies strength, the yield is extremely small. In our experiment[15] a positron was trapped in about every 100 repetition cycles. One continuous run lasted a month. Graduate students set up camp alongside the apparatus. If the final polarization had been measured by Mott scattering, rather than by the Telegdi method, the same run would have lasted 10 years—somewhat too long even for a thesis student.

The muon experiment has the problem of ω_0 and an additional basic limitation in that the rest lifetime of the muon is short, only about 2.2 μsec. This limits the number of beats that can be observed, and therefore the accuracy of ω_D. But both these limits have been pushed far out. The decay slows down at high energy by the factor γ, but the anomalous precession (in lab coordinates) does not slow down. ω_D is independent of γ. It is proportional to B. So the number of cycles of ω_D that can be observed goes up with γB. That is the reason why a very large, high field storage ring is used as the trap. Significant data are obtained out to 80 cycles of the anomalous precession and nearly 15 times the rest lifetime. (See ref. 13.)

The ω_0 problem has been solved in an ingenious way. At relativistic energies a radial electric field produces a change both in the cyclotron frequency and the spin precession frequency. These depend in different ways upon γ, and there is a "magic" γ at which they cancel in their effect on the anomalous precession frequency. Therefore at this γ, a uniform magnetic field can be used, and the muons held in orbit by an electric field. The effective magnetic field is then just the uniform field and there is no correction for the electric field. The latest experiment was done in that way. The only drawback is that the magic γ is only 29.3, and one would like to have it higher so that the lifetime would be longer. The same trick has not been practical for the electron experiments, because it calls for an energy of 14 Mev.

Before leaving the beat-counting methods I want to mention a variation that is unique in that it allows the measurement to be continuous, rather than by batches, or pulses. It was done in 1963 by Farago[16] and his group at the University of Edinburgh. They used beta rays, which were initially polarized, and Mott scattering for analysis. A weak electric field crossed with a strong uniform magnetic field caused the particle orbits to drift slowly across the magnetic field, striking the Mott scatterer after the order of 1000 revolutions. It worked, but it did not compete in accuracy with experiments in which the particles are trapped and allowed to make a far larger number of revolutions.

METHODS: SPIN RESONANCE

The determination of the spin precession frequency by the application of a radio frequency (rf) field has long had an appeal, mainly because a frequency is easily and precisely measured. Ideas along this line in fact pre-date those on beat counting. A number of experiments have been devised. Some have worked. None, except very probably the most recent, have come up to the accuracy of the beat method. But progress is now fast, and it promises to be a hot field in the future. I will pass quickly over some earlier work and get to the part that intrigues me very much, namely resonance studies of the electron in its ground quantum state in a magnetic field.

As early as 1958 Dehmelt[17] found a value for g-2 by resonating electrons precessing in a magnetic field, with rf. They were in a buffer gas, and he used interactions with polarized atoms for polarizing and analyzing the electrons. Tolhoek and Degroot[7] proposed a scheme in 1951 in which a magnetic field and an rf field would be interposed between the first and second Mott scatterers, and in which destruction of the asymmetry would indicate resonance. It would have been practical if he had envisioned a trap; but as he proposed it there would not have been enough cycles of the spin precession to give a well defined frequency. Next in the evolution came an experiment by Gräff and co-workers[18] at the University of Bonn and at the University of Mainz. They used a polarization and analysis scheme similar to that used by Dehmelt, but they held the electrons in a trap, in a vacuum, during the resonance part. This gave a fairly accurate g-2 value. In our own laboratory, in 1972, Rich and co-workers[19] did a spin resonance experiment on an apparatus in which electrons were held in a magnetic well between first and second Mott scatterers. The novel feature of this was a way in which rf of the difference frequency, ω_D, rather than the spin precession frequency, was made to rotate the polarization. The idea, due to Telegdi, is quite simple. The precession frequency in a frame that rotates with the momentum of the particle as it goes around the orbit is ω_D. An rf field that is symmetrical about the axis and of frequency ω_D will match the spin precession in that rotating frame. This is accomplished by means of rf current in a wire stretched along the center axis of the trapping chamber. It produces lines of force that are circles, concentric with the orbits. If the rf is held on for the right length of time the polarization is turned from the plane perpendicular to the main magnetic field to the direction parallel to it, and the asymmetry in the Mott analyzer disappears. It comes back again if the rf is held on twice as long. Like spin echoes. One continues to be struck by how classically it all works!

To come to ground-state electrons, Rabi[20] calculated the level structure in 1928, and Felix Bloch[21] in 1953 was the first to call attention to the application to g-2 experiments. Rabi showed that $E_{n,m} = p_z^2/2m_0 + (2n + 1 + gm)B_z\mu_0$. The first term is just the energy of the linear motion, due to the pitch of the helix, and is not of much interest. In the second term n and m are the orbital and spin quantum numbers, $n = 0,1,2,\ldots$ and $m = \pm 1/2$. The interesting things happen when n = 0 or a small integer, and this in fact will occur frequently for electrons that are in thermal equilibrium at liquid helium temperature. Because g is slightly greater than 2, the second term changes sign when n=0 and m=-1/2. Since this term is responsible for the axial force, electrons in that state will be pushed out of the well, while those in all other states will be pulled inward toward the center; a 100% sieve for electrons in that state! Even for somewhat larger n values the change in total moment due to a spin flip is relatively great.

We at Michigan got in the habit of calling this creature "zeronium", to signify an atom without a nucleus—atomic number zero. Recently Van Dyck of the University of Washington told us that out there they call it geonium, since through the magnetic field it is really bound to the earth. We defer to him, because he has them in captivity and we do not. A point one might argue about over the nth bottle of beer is whether inducing a spin flip in zeronium or geonium any longer amounts to a measurement of g-2 for the free electron. It probably does qualify, since the main hazard of the measurement of g-2 in atoms is the nuclear size effect, and that is absent.

Principally the groups at Stanford University and the University of Washington have, over a long period, pioneered the g-2 measurements using "cold" electrons. Bloch[21] at Stanford, was the first to design an experiment using the ground states as the means of detecting spin transitions. Electrons were to be held in a well, rf applied, and spin transitions detected by the escape from the well. Actual measurements did not materialize. Later, (1965) in Fairbank's group at Stanford, L. V. Knight[22] did a thesis in which he applied rf to ground state electrons, but in a drift tube rather than a magnetic well. Magnetic potential hills were used as the means of preparation and detection of the states. Some results were obtained, geonium was identified, but a value for g-2 was not obtained.

Just recently things have been happening in Dehmelt's laboratory at the University of Washington in Seattle that appear to be a real breakthrough in the use of cold electrons for g-2. All of their work is done with a small (order of a cm in dimension) Penning trap, and at liquid helium temperature. The Penning trap, you recall, is a magnetic and an electric well, having a common axis of symmetry, superimposed. Since, at non-relativistic energies, the magnetic well acts only on the total magnetic moment (orbital plus spin) of the particle and the electric well acts only on the charge, there is great flexibility in playing one of these parameters against the other to give the trap any desired characteristics. In several of the experiments mentioned earlier, the Penning trap principle was used. It, for example, allows a uniform magnetic field to be used, with the trapping done entirely by the electric field. The Gräff experiment used such a field, The latest CERN muon experiment is a variation of it, although for relativistic particles.

In Dehmelt's group in 1970, F. L. Walls[23] did a thesis using cold electrons in a Penning trap and got a value for g-2. The big break has come recently, when it has become possible to hold a <u>single</u> electron in captivity for hours or even days, flip its spin repeatedly by rf and detect the spin flips without ejecting or using up the electron. This stretches modern signal-to-noise techniques to the limit. The feasibility of making resonance measurements on a single particle was shown by D. Wineland and others[24] and currently the application to a g-2 measurement is being carried on by R. Van Dyck, Jr., with the Seattle group.[25] A change in either n or m is sensed through the resulting change in the electron's axial (z) oscillation. The electrical signal is a slight change in the non-dissipative loading by the electron of a resonant circuit that is connected between the end-caps of the Penning chamber. The rf frequencies necessary to make transitions in both n and m are found in this way, and the anomaly, a is (for cold electrons) directly the ratio of these frequencies. It is interesting that spin flips are induced by the difference frequency ω_D. Rf at ω_D is applied to drive the z motion, and the non-linear static fields in the trap couple the z motion to the spin precession in the frame that rotates with ω_c. (This is the kind of coupling that this high energy accelerator audience knows too well as a destructive resonance. One man's poison....) The Seattle group already has a precision in g-2 that is up to the best obtained with the beat method, and the full possibilities have not been realized.

I should not end the technical part without saying what we are up to at Michigan. Rich and his group are preparing a resonance experiment at high energy (1 Mev) and high field (a 10 kg cryogenic solenoid) on positrons and electrons. It should be operating within a year.

WHERE ARE WE?

The electron experimental results are now at about the level of 3 ppm in the anomaly[26], and they agree with the calculated value within a standard deviation. The calculation, by QED, has been carried to terms somewhat smaller than 3 ppm, but not by more than an order of magnitude. The fine structure constant, in terms of which the theoretical value of the anomaly is expressed is known to high precision. It will be interesting, when g-2 measurements improve by another order of magnitude, to see just what is being tested. If for example α is not also improved, we may have the choice of assuming that QED theory is accurate to that level, and of using the g-2 experiments to test the value of α. The same is not true yet of the positron and muon measurements. The precision of the positron g-2 is only to about 1000 ppm[15] and that of the muon about 23 ppm.[13] Both agree with QED theory, when the particle is treated as a simple point charge. As these precisions improve one will not expect to find anything from the positron that was not found from the electron. In the case of the muon, new couplings will be looked for, although there has been no sign of them as yet. But whether or not new levels of precision find immediate use, the game stays exhilarating!

REFERENCES

1. For a detailed review of the field A. Rich and J. C. Wesley, Rev. Mod. Phys. 44 250 (1972).
2. Attributed to Bohr by W. Pauli, Handb. d. Phys. 24/1 236 (1933).
3. N. F. Mott, Proc. Roy. Soc. A124 425 (1929).
4. Notwithstanding, Mott and Massey, "Theory of Atomic Collisions" editions 1933 and 1949 say, after reproducing the Bohr proof, that the assignment of a magnetic moment to the free electron is meaningless.
5. W. H. Louisell, R. W. Pidd and H. R. Crane, Phys. Rev. 94 7 (1954).
6. H. Medlowitz and K. M. Case, Phys. Rev. 97 33 (1955).
7. H. A. Tolhoek and S. R. DeGroot, Physica 17 17 (1951).
8. P. Kusch, Science 123 207 (1956).
9. P. A. M. Dirac, Proc. Roy. Soc. A117 610 (1928).
10. W. V. Houston, Phys. Rev. 51 446 (1937).
11. G. Breit, Phys. Rev. 72 984 (1947).
12. H. R. Crane, Sci. Amer. 218 72 (Jan., 1968).
13. F. J. M. Farley (a review) Contemp. Phys. 16 413 (1975).
14. See L. Grodzins, Progr. Nucl. Phys. 7 219 (1959).
15. J. Gilleland and A. Rich, Phys. Rev. Let. 23 1130 (1969).
16. P. S. Farago, R. B. Gardiner, J. Muir and A. G. A. Rae, Proc. Phys. Soc. (London) 3 82 and 493 (1963).
17. H. G. Dehmelt, Phys. Rev. 109 381 (1958).
18. G. Gräff, E. Klempt and G. Werth, Z. Phys. 222 201 (1969); G. Gräff, K. Huber, H. Kalinowsky and H. Wolf, Phys. Let. A41 277 (1972).
19. G. W. Ford, J. L. Luxon, A. Rich, J. C. Wesley and V. L. Telegdi Phys. Rev. Let. 29 1691 (1972).
20. I. I. Rabi, Z. Phys. 49 507 (1928).
21. F. Bloch, Physica 19 821 (1953).
22. Thesis, Stanford University (unpublished).
23. Thesis, University of Washington (unpublished).
24. D. Wineland, P. Ekstrom and H. Dehmelt, Phys. Rev. Let. 31 1279 (1973).
25. R. Van Dyck, Jr., P. Ekstrom and H. Dehmelt, Bul. Am. Phys. Soc. Ser. II, Vol. 1, No. 5 818 (1976).
26. See ref. 1 for a diagram of all of the experimental values except those of ref. 25, with error bars, and theoretical values.

DISCUSSION

Koester: (U. of Illinois) I haven't thought about this, but will the experiments with the Josephson tunneling help to resolve some of these ambiguities?

Crane: They are already giving more precise values for α, and I would guess that the Josephson experiments will be able to keep ahead of these others by finding more accurate values of α.