The Antiproton and How It Was Discovered

John Eades

University of Tokyo

Abstract. The antiproton celebrates its 50th birthday this year. Although its existence had been suspected since the discovery of the positron in 1932, there was still doubt in some quarters that such a companion particle to the proton could exist. I will try to trace the scientific history of the antiproton from that time to the publication of the definitive paper by Chamberlain, Segrè, Wiegand and Ypsilantis in November 1955, with a brief look at what happened next. The narrative will be supplemented with thoughts and opinions of some of the main actors, both at the time and in retrospect.

Keywords: Antiproton, Discovery, Dirac
PACS: 03.65.P 01.65.+g 11.30.Er 25.75.Dw

We tend to think that the antiproton was discovered in a flash, perhaps during a single night shift at the Berkeley Bevatron in 1955. The real story is much more interesting than that and goes back long before that date. Owen Chamberlain, who got the Nobel prize with Emilio Segrè for the discovery of the antiproton, began his talk at the 1985 Symposium 'Pions to Quarks – Particle Physics in the 1950s' with the words *' I believe that the antiproton story starts with P.A.M. Dirac, who in 1930 published his paper 'A Theory of Electrons and Protons''.* It is as good a place as any to start.

The 1930s

At the time, as everyone knows, Dirac was trying to make sense of the negative energy solutions that turned up when he introduced his relativistic wave equation for electrons. Usually the important point is overlooked that this problem occurs even classically, since we get from Einstein the equation :

$$E^2 = p^2c^2 + m^2c^4 \qquad (1)$$

which is going to give negative square roots for the energy E as well as positive ones. Indeed, the transformation of equation 1 into a linear operator equation acting on an electron wave function is where the Dirac equation starts. Classically, however, we can simply ignore the negative roots because discontinuous energy changes like the $2mc^2$ gap that occurs for $p = 0$ between the positive and negative continua do not occur in classical physics.

CP793 *Physics with Ultra Slow Antiproton Beams*
edited by Yasunori Yamazaki and Michiharu Wada

We cannot do this in a quantum theory, where discontinuous quantum jumps in energy are the name of the game, and where mass energy of particles appears or disappears discontinuously in collisions and radioactive decays. This was the reason why Dirac was so reluctant to reject the infinity of negative energy solutions, and again as everyone knows, proposed that they really existed but were usually all occupied.

New problems appear

This immediately caused new problems because it gives us an infinite charge density ρ everywhere, so the Maxwell equation:

$$div\ \boldsymbol{E} = -4\pi\rho \qquad (2)$$

results in an infinite divergence for $\boldsymbol{E}$, now the electric field vector. Dirac dodged this one by what looks suspiciously like renormalisation in QED : he just redefined the vacuum as that state where all negative energy solutions are occupied by electrons, so that only departures from this infinite charge density distribution contribute to $div\ \boldsymbol{E}$:

Let us assume that there are so many electrons in the world that all the states of negative energy are occupied except perhaps a few of small velocity. …. We shall have an infinite number of electrons in negative energy states and indeed an infinite number all over the world, but if their distribution is exactly uniform we should expect them to be unobservable.

Dirac, 1930 [1]

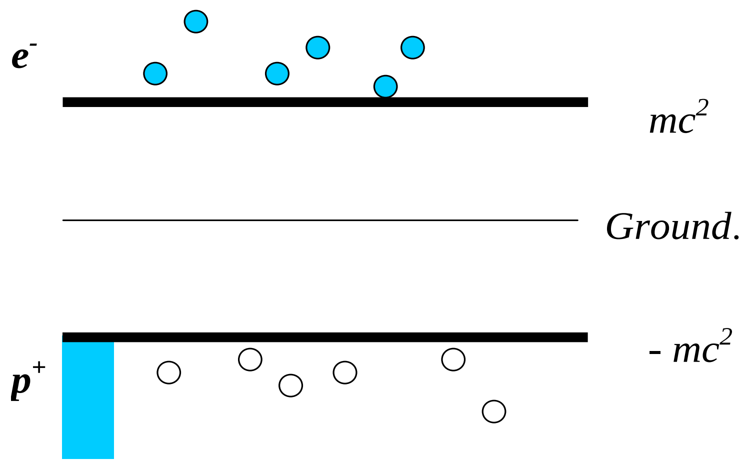

FIGURE 1. Dirac's Unitary Theory of Electrons and protons

It is well attested in his writings that at the time he really did believe in the reality of this kind of Aether of negative energy electrons filling up the entire universe. One anonymous critic said that it all sounded like something an engineer might have dreamed up. Almost no-one now recalls that Dirac's first degree was in Electrical Engineering, and that he is said to have replied *'But I AM an engineer!'*

Accepting both ideas – vacuum 'renormalisation' and fully occupied negative energy states – allows us for all practical purposes to return to the classical physics situation of ignoring the negative energy states, but only as long as every one is occupied by an electron, the exclusion principle for fermions then being sufficient to forbid any $\Delta E \geq 2mc^2$ discontinuous transitions. Strictly speaking, unobservability would mean that these gymnastics would really have no content, but Dirac tried to supply some observable consequences by introducing what he later referred to as a *Unitary Theory of the Nature of Electrons and Protons*, in which the proton, the only other known fundamental particle at the time, was tied to the idea of *unfilled* negative energy states:

Only the small departures from exact uniformity brought about by some of the negative energy states being unoccupied can we hope to observe… We are led to the assumption that the holes in the distribution of negative energy electrons are the protons.

Dirac, 1930 [1]

Contemporary reactions

Even before the 1930 paper, the audacity and counter-intuitiveness of many of the consequences of Dirac's equation made sure that almost nowhere was it accepted with great enthusiasm :

In spite of the wonderful progress which we owe to Dirac, new difficulties are revealed every day … A new particularly instructive example has recently been brought to light by Klein.

Bohr, Letter to C.W. Oseen 5 Nov 1928 [2]

The wonderful progress, was of course the automatic inclusion of spin (which Pauli had, 'without new justification' as he put it, introduced to explain the interactions of the electron with magnetic fields), giving an almost correct value for the electron's magnetic dipole moment, the Thomas factor and so forth. Klein's paradox was that more electrons would be reflected from a sharp potential barrier (one of height $>> mc^2$ over a distance $\sim h/2\pi mc$) than were incident on it. Heisenberg was still more pessimistic:

The saddest chapter of modern physics is and remains the Dirac theory.

Heisenberg, Letter to Pauli, 31 Jul 1928 [3]

while after hearing a talk by Dirac, Landau sent a one-word cable to Bohr on September 9, 1930 which said simply *'Quatsch'* (gibberish).

What is striking however in retrospect is the extent to which many of these counter-intuitive ideas were, in some other form, and much later, to become part and parcel of our present ideas of particle physics. Any contemporary who considered the cosmological consequences of Dirac's equation would surely have remarked that the deexcitation of most of the electrons in the universe to negative energy states at some time in the past, must have filled it with radiation. This radiation must now have reached a dynamic equilibrium condition with the particle content of the universe, under which those electrons excited to real positive energy states, together with the real protons left behind, constitutes the material substance of the universe we experience. Secondly there was the redefinition of the vacuum – far from being a place containing no matter, the vacuum now had an infinite density of it everywhere. The non-empty vacuum concept was to reappear within QED in the late 1940s, but it must have been very difficult to accept in 1930. Next, it is important to remember that in 1930 the only known particles of matter were the electron and proton, and that everyone was hoping to include them in what we would call today a Standard Model of particle physics. If Dirac's *Unitary Theory* (that the holes left behind among negative states represent real protons) was correct, why did the masses of protons and electrons differ so much? Dirac suggested a kind of proto-Higgs argument - that the Coulomb interaction between the holes and the electrons might somehow give the protons their mass, although he deferred any attempt at calculation until some unspecified later date. Finally, he was resurrecting a new version of the Aether at a time when no one had any use for it. It was perhaps no wonder that Pauli suggested jokingly at the time that authors be subjected to their own theories, and that Dirac should therefore be annihilated !

Oppenheimer and Weyl

By March1 1930, Oppenheimer had published a two page letter in Physical Review pointing out several inescapable difficulties with the Unitary Theory, among which was that Thompson scattering (scattering of light by an electron gas) should occur from both positive and negative energy states with the same matrix elements, and that any gaps (as he called Dirac's holes) in the negative 'sea' would be rapidly filled up. He calculated the rate for :

$$p^{+} + e^{-} \rightarrow 2\,\gamma \qquad (3)$$

and got the result:

$$T = (m + M)^2\, c^3 / \, 64\, \pi^5\, e^4\, n_p \qquad (4)$$

m and M being the electron and proton masses, and n_p the local density of protons. He is known to have dropped a factor $(2\pi)^4$ on top. The numerical result is still much too short to keep the universe in existence, and he concludes:

…. The mean lifetime for ordinary matter will be only about 10^{-10} seconds… If we return to the assumption of two independent elementary particles , of opposite charge and dissimilar mass , we can resolve all the difficulties raised in this note, and retain the hypothesis that the reason why no transitions to states of negative energy occur, either for electrons or protons is that all such states are filled.

Oppenheimer, 1930 [4]

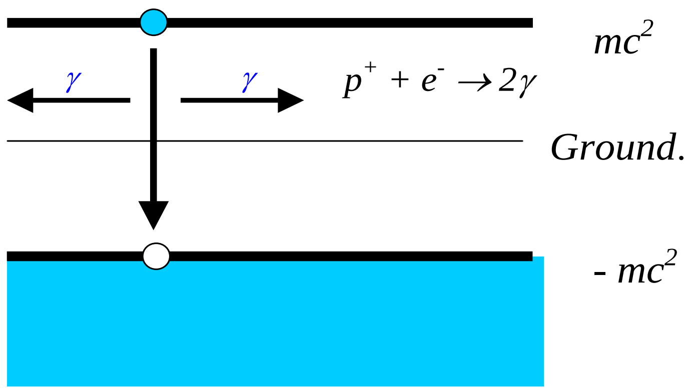

FIGURE 2. Oppenheimer, Dirac - Calculations of annhilation rate .

This is the first hint of the antiproton although it is not explicitly said that there must be some unfilled negative energy proton states.

A few weeks later, probably before he saw this, Dirac sent the Proceedings of the Cambridge Philosophical Society a paper entitled *On the Annihilation of Electrons and Protons*, in which he came to similar conclusions about the instability of matter, for which he calculated a lifetime:

$$T = m^2\, c^3 / \, \pi\, e^4\, n_e \qquad (5).$$

The question of the mass of the proton having been deferred, Dirac plugged in the average of the electron and proton values and got 10^{-9} seconds. By October he had read Oppenheimer's paper and concluded :

Oppenheimer… does get over these difficulties …but only at the expense of the unitary theory of the nature of electrons and protons. …..There being now no holes which we can call protons, we must assume that the protons are really independent particles . The proton will now itself have negative energy states, which we must again assume to be all occupied.

Dirac, 1930 [5]

Shortly afterwards, Weyl also showed that according to Dirac's own theory, the holes would in any case have to have the same mass as the electron. Early in 1931, Dirac finally felt obliged to give up the unitary idea and quoted both Oppenheimer and Weyl as having provided the decisive arguments that the holes were new particles.

Subsequent investigation has shown that this particle necessarily has the mass of an electron and if it collides with an electron, the two will have a chance of annihilating one another much too great to be consistent with the known stability of matter… We must abandon the identification of holes with protons… A hole, if there were one, would be a new kind of particle, unknown to experimental physics, having the same mass and opposite charge to the electron. We may call such a particle the anti-electron. We should not expect to find any of them in nature …

Dirac, 1931 [6]

This paper is also the first in which the antiproton is explicitly mentioned:

The protons, on the above view, are quite unconnected with with electrons. Presumably, the protons will have their own negative energy states, all of which are normally occupied, an unoccupied one appearing as an anti-proton.

Dirac, 1931 [6]

It is also, incidentally, the paper in which Dirac first showed that magnetic monopoles are not precluded by quantum mechanics.

The positron appears

Dirac's cautiousness about finding positrons in nature was based on the assumption that they would have to be produced via the collision of two intense beams of radiation. In fact, when doing his hole-electron annihilation rate calculation, he had not used QED (which he said was unnecessarily complicated) but worked out the stimulated transition rate for this inverse two-photon process, and divided it by the ratio of stimulated to spontaneous emission. Decades before the invention of the laser, he was apparently quite ready to entertain the idea that one day, colliding beams of 512kV radiation might become available to do this (we are still waiting for them). Of course the very next thing was that the positron DID appear in nature, but in cosmic rays, as reported by Anderson, and removed Klein's problem, as the extra electrons reflected from a potential barrier could now be seen to be pair-produced positrons! In the very paper reporting the positron discovery, Anderson himself speculates about antiprotons:

The greater symmetry, however, between the positive and negative charges revealed by the discovery of the positron should prove a stimulus to search for the evidence of negative protons… While this paper was in preparation press reports have announced that P.M.S. Blackett and G. Occhialini in an extensive study of cosmic-ray tracks have also obtained evidence for the existence of light positive particles confirming our earlier report.

Anderson, 1933 [7]

The error on the mass with respect to the electron was about 50%. With regards to the second remark, the Blackett-Occhialini paper may have been accepted as the real discovery, had they been less cautious about publishing. They say in their paper:

We have recently developed a method by which the high speed particles associated with penetrating radiation can be made to take their own photographs… It will be shown that it is necessary to come to the same conclusion that has already been drawn by Anderson from similar photographs. This is that some of the tracks must be due to to particles with a positive charge but whose mass is much less than that of a proton…

Blackett and Occhialini, 1933 [8]

Whatever the truth, the Nobel prize was awarded to Anderson 4 years later, in 1936.

The Idea of the Antiproton

Dirac now drew the explicit conclusion that what was true for electrons might also be true for protons:

One would like to have an equally satisfactory theory for the proton. One might perhaps think that the same theory (as the electron one) could be applied to protons. This would require the possibility of the existence of negatively charged protons *forming a mirror image of the usually positively charged ones…. We must regard it as an accident that the earth (and presumably the whole solar system) contains a preponderance of negative electrons and positive protons. It is quite possible that for some of the stars it is the other way about…. There would be no way of distinguishing them by present astronomical methods.*

Dirac, 1934 [9]

So the hint of the antiproton had now become a serious proposition and the possibility of a baryon-symmetric universe had been seen for the first time.

The situation in 1955

The fact that Anderson had found positrons in cosmic rays made it seem natural to keep one's eyes open for antiprotons in later cosmic ray exposures of cloud chambers and in emulsions. There had indeed been several potential annihilation star candidates in cosmic ray experiments as far back as 1947. With the hindsight of knowing what the cosmic ray flux of antiprotons is near sea level, we can see how weak most of these claims were. A clear experimental demonstration of the existence of the antiproton was one of the factors considered in deciding the energy of the Bevatron. A parallel emulsion search had been installed by members of the same research team, in the same Bevatron beamline, during the same period in 1955, and it was probably anybody's guess who was going to produce the first evidence.

Or indeed any evidence at all, since as Tom Ypsilantis often said, there was quite serious doubt in many people's minds as to whether the antiproton existed at all. The main reason given for this was that the proton's large anomalous magnetic moment could be taken to mean that it might not be describable by the Dirac equation, or at least that it was not a simple Dirac particle. Dirac himself had recognized as much in his Nobel Prize lecture:

There is however some recent experimental evidence obtained by Stern about the spin magnetic moment of the proton, which conflicts with this theory for the proton. As the proton is so much heavier than the electron it is quite likely that it requires some more complicated theory, though one cannot at the present time say what this theory is.

Dirac, 1934 [9]

Now the electron of course had an anomalous moment too, but its value was a few per mil, not almost two hundred percent, and it was understood in terms of QED. This doubt concerning the antiproton resulted in some now famous bets made in and around Berkeley, which ranged over several orders of magnitude:

Speculations about the existence or non-existence of antiprotons were rife during the *planning stages of the experiment, and even included a 25 cent bet between Physics Division Head Ed McMillan and Segrè. (By the way 25 cents was a heretofore unimaginably high amount when it came to bets by Segrè). Most of the members of our group were on the pro-antiproton side. The detailed technical planning for the experiment was done primarily by Chamberlain and Wiegand. Tom (Ypsilantis) soon joined the effort and played an active role in all aspects of this work. I was a graduate student who was fortunate enough to be sucked in as well.*

Herbert Steiner, private communication

As to bets my brother Maurice did indeed bet [Hartland Snyder] $500 against the existence of the antiproton. As I understand his motivation 1) No antiproton galaxies, 2) The proton had an anomalous magnetic moment i.e. not obviously a Dirac particle like the positron. This was long before CP violation and Sakharov's argument.

Gerson Goldhaber, private communication

One is reminded of similar bets made a year or so later against parity non-conservation, notably by Pauli. An important point about the anomalous moment was that no satisfactory Relativistic Quantum Field Theory existed for hadrons in 1955 to explain the huge anomaly. Nor (see below) would there be one for decades afterwards.

The experimental discovery of the antiproton

The upshot of these previous inconclusive searches and falsely identified events was the counter experiment method as described in the Physical Review Letter paper announcing the observation of some 60 antiprotons produced by the Berkeley Bevatron . This paper notes in the introductory paragraphs that:

There have been several experimental events recorded in cosmic ray investigations which might be due to antiprotons although no sure conclusion can be drawn from them at present.

Chamberlain et al., 1955 [10]

Tom Ypsilantis recalled this inconclusiveness about annihilation searches many years later:

When we started thinking about an experiment to find the antiproton (1953-1954) we decided to build a spectrometer which could measure both mass and charge rather than trying to observe the annihilation process. This decision… turned out to be crucial …

T. Ypsilantis [11]

The emulsion experiment mentioned above was nevertheless thought necessary as a kind of insurance policy and was already well under way using the same beamline:

I arrived as a postdoc to Berkeley in September 1955 and could follow the progress of the counter experiment. I got deeply involved in the parallel emulsion search and discovery of the annihilations.

Gösta Ekspong, private communication

The point here is that tracking through a magnetic spectrometer does not give the full record of an emulsion star, which contains ionization information for identifying the annihilation products and therefore for determining the annihilating mass. The counter experiment alone might not therefore have shown unambiguous evidence that the antiproton hypothesis was both necessary and sufficient to explain any candidate events seen. So neither experimental approach seems to have been regarded as having a cast iron guarantee of success.

Furthermore, magnetic tracking does not of course determine the particle mass itself but its radius of curvature, which is proportional to the charge to mass ratio times the velocity (or rather $\beta\gamma$). To ensure that their charge Q was e and not $2e$ or more it was therefore necessary to check that the antiproton candidates had the same pulse height as protons, and also to add a velocity measurement. This was done by measuring the time of flight, between two scintillation counters, $S1$ and $S2$, $12\ m$ apart in the beam. The time of flight for π or K mesons (both with $\beta > 0.96$) for this distance was $40\ ns$, while for antiprotons ($\beta \sim 0.76$) it was $51\ ns$.

This would have been enough were it not for the possibility of mesons interacting in, for example, the air, or a scintillation counter after the magnetic bend, and producing a lower momentum pion that looks like an antiproton. With 10^5 mesons per antiproton even very unlikely occurrences of this kind have to be foreseen. So in fact there were three bends in the beamline (including the Bevatron magnet around the target) so that these spurious antiproton events were repeatedly swept out of the beam. Effectively the momentum was measured three times. Still more redundancy was added by measuring the velocity by an independent method:

Since the antiprotons must be selected from a heavy background of pions it has been necessary to measure the velocity by more than one method…. C2 is a Cerenkov counter that counts particles only within a narrow velocity interval $0.75 < \beta < 0.78$ *... the requirement that the particle be counted in this counter constituted one of the determinations of the velocity of the particle….*

Chamberlain et al., 1955 [10]

In both the text and the figure included in the paper (reproduced as Fig 3 below) the labels C1 and C2 seem to have been interchanged. In spite of all this care taken to reject spurious events, the authors still thought that:

As outlined so far, the apparatus has some shortcomings … Accidental coincidences between S1 and S2 cause some mesons to count …. [and] C2 could be actuated by …. [one of these] if the meson suffered a nuclear scattering in the radiator of the counter [C2]. … Both of these deficiencies have been eliminated by the insertion of the guard counter C1 , which records all particles of $\beta > 0.79$. A pulse from C1 indicates a particle (meson) moving too fast to be an antiproton of the selected momentum and indicates that this event should be rejected

Chamberlain et al., 1955 [10]

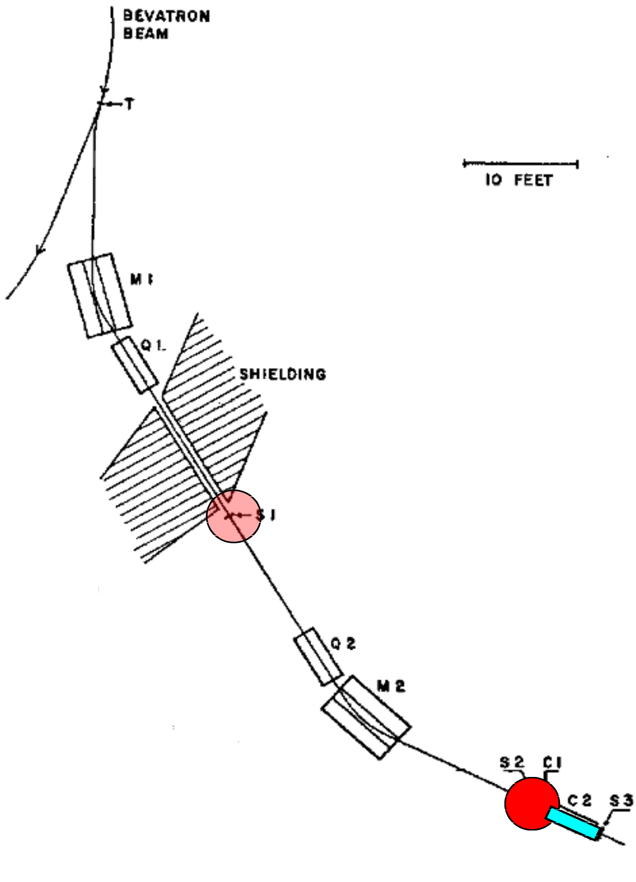

FIGURE 3. The Berkeley antiproton spectrometer, Chamberlain et al., Phys. Rev. 100 (1955) p 947.

Even then, the 'golden' annihilation event which came somewhat later from the emulsion experiment [12] seems to have been taken as the conclusive evidence for the antiproton. Indeed the emulsion group might have reported the first antiprotons, because the Bevatron energy – chosen to be 6.2 GeV in order to be above the theoretical six proton-mass threshold for antiproton production from a stationary target – was thought to maximize the beamline yield at 1.19 GeV/c, and this was the momentum to which the beamline was tuned. This was too high to allow the antiprotons to stop in the emulsion, and $132g/cm^2$ of copper had to be placed in the beam to degrade its energy sufficiently for this. Now the internal Fermi energy within the beryllium target played an important role in both experiments by reducing the threshold for antiproton production to 3.5 Gev/c so the emulsion experiment could have been done at a lower beam momentum without the degrader, with better sensitivity :

However, the major crime was forgetting Fermi motion. … For protons on a copper target at the Bevatron, the true antiproton threshold was only 3.5 GeV. At 6.2 GeV, the lab antiproton momentum distribution peaked at 0.6 – 0.7 GeV/c, not 1 GeV/c. So no degrader was necessary, just retune of the beam! Of course that was done later. In the first experiment, we could in any case not have reduced the momentum, because the Segrè experiment was designed for 1 GeV/c. You can see therefore that the antiproton "discovery" was a shambles all the way round….

D. Perkins, private communication

There was some consolation for the emulsion group in the acceptance of the emulsion star as the crucial piece of evidence for the antiproton. Incidentally the event from reference [12], as sketched by Gerson Goldhaber, was reproduced in TIME magazine on Feb 13 of the following year. The overall experimental uncertainty quoted on the equality of the proton and antiproton masses was about 5% (it is now a few parts in 10^8). As with the positron experiment, the Nobel prize was only awarded four years later, in 1959.

Aftermath - P, C, T and CPT

In 1957, quite soon after the antiproton was discovered, parity was shown to be violated in weak interactions by Wu's experiment at Columbia University, New York. It is not widely known that Dirac had always believed that it was unwise to assume either parity or time reversal invariance anyway. Pais asked him in 1959, why parity was never mentioned in his book *Principles of Quantum Mechanics* and he replied '*because I did not believe in it'* , and in 1949 he had written:

A transformation of this (inhomogeneous Lorenz) type may involve a reflection of the coordinate system in three spatial dimensions and it may involve a time reflection… I do not believe there is any need for physical laws to be invariant under these *reflections, although the exact laws of nature so far known do have this invariance.*

Dirac, 1949 [13]

Nevertheless it has been said that he was very reluctant to accept the possibility of C-violation (which after the Wu experiment had to be violated to keep CP invariance) until the experimental evidence for this came from an experiment at Liverpool, in 1957. C-invariance, up to then was thought to be the condition that would ensure the equality of particle and antiparticle properties:

Originally, properties 1-4 (charge, mass, spin, magnetic moment) were derived from C-invariance – a possible physical situation is transformed into another possible physical situation by changing the sign of all electric charges. Since this principle is violated in weak interactions, it is important to point out that it is not necessary to establish the properties listed above, but that the weaker requirement expressed by the invariance under CPT is sufficient ….

Segrè, 1958 [14]

It has also been said that even after these events Dirac was not convinced of the usefulness of the CPT theorem, which replaced individual C-P- and T- invariance from 1957. If this was true (and I have found no statement by Dirac himself to suggest that it was) the reason may have been that the CPT-theorem is a theorem about relativistic quantum fields, and RQFTs for the strong interaction had more or less been abandoned by then :

In retrospect we can now see that the reason field theory failed back in the 1950s and 1960s to give an adequate account of the strong interactions was not that it was wrong but that it was misapplied. The fundamental fields of the strong interactions correspond not to the hadronic quanta but rather to the quarks, and the gluons that bind them together. In the mid 1970s theoretical physicists finally invented a successful field theory of the strong interaction – quantum chromodynamics – based on interacting quarks and gluons.

Pagels, 1983 [15]

It is implied in reference [16] that what did finally put the CPT theorem on firm grounds as far as Dirac was concerned was the appearance of the antideuteron in 1965. As with the positron and to some extent the antiproton, there were two almost simultaneous antideuteron experiments, first at CERN [17] and then at BNL [18] by a Columbia University team. Both were based on the fixed-momentum, long path time-of –flight method used in the antiproton discovery, with now multiple flight paths and momentum bends for additional redundancy since there was now only one antideuteron per 10^8 mesons, while there had been one antiproton per 10^4 or 10^5 of them. Thus the antideuteron experiments had much longer beamlines (*65 m* and *120 m* instead of *24 m*). In the CERN experiment, the beam was partly separated electrostatically, so the added redundancy was not so crucial. In the Brookhaven experiment, there were five bends and the time of flight was measured five times over overlapping flight paths and was supplemented by the same kind of 'velocity-window' Cerenkov counter used in the antiproton experiment. Indeed, the BNL experiment was probably over-redundant for antideuterons because it was looking for heavy, metastable unitary symmetry triplets rather than antideuterons, which were nevertheless expected as a by-product. Now time-of-flight measurements are not a particularly precise way of arriving at a particle's mass, and in both experiments the error on the antideuteron mass was $\sim 3\%$. Since this far exceeds the deuteron binding energy, neither of them could in fact really be said to constitute a real test of the invariance of nuclear binding expected under CPT, which makes the assertion of reference [16] somewhat difficult to understand.

Conclusion

A physicist writing about history walks on the same dangerous ground as the historian discussing quantum chromodynamics. I am conscious of the fact that in doing so one is confronted with a confusing mass of ideas, papers and books, without the connections and sequences presented in textbook accounts. All this surrounded by complete mystery as to how much person A knew about what B was doing, and when did he know it. Furthermore, the symbols, terminology, and modes of expression of the time are often now unfamiliar to us. All this makes science history even more difficult than science itself. Nevertheless, I hope I have here conveyed some of the flavour of a long-gone period in particle physics history when the tools of the trade were slide rules rather than computers, and author lists usually took up only a single line.

ACKNOWLEDGMENTS

It is a pleasure to express my thanks to Gösta Ekspong, Gerson Goldhaber, Don Perkins and Herbert Steiner for sharing their recollections with me of the events surrounding the discovery of the antiproton at Berkeley in 1955. Sadly missing from among us is Tom Ypsilantis, a constant source of ideas and information on the art of experimentation and a friend and colleague, with whom many of us now wish we had discussed the discovery of the antiproton at greater length. For the reasons outlined in the previous paragraph, I would here like to apologize for any errors, omissions and misattributions, sole responsibility for which is mine.

REFERENCES

1. Dirac,, P.A.M. , Proc Roy Soc (London) A126 (1930) p 360 .
2. Reprinted in Niels Bohr, *Collected Works*, North Holland, Amsterdam, 1972.
3. Reprinted in W. Pauli, *Scientific Correspondence*, vol 1, Springer New York, 1979.
4. Oppenheimer J.R., Phys. Rev. 35 (1930), p 562.
5. Dirac, P.A.M., Nature vol 126 (1930), p 605.
6 Dirac P.A.M., Proc. Roy. Soc,. A133, 61 (1931) p 60.
7. Anderson C., Phys. Rev. Vol 43 (1933), p 491.
8. Blackett P.M.S, and Occhialini G.P.S. , Proc. Roy. Soc. A139 (1933) p 699
9. Dirac, P.A.M., Nobel Prize lecture (1934)
10. Chamberlain, O., Segrè, E., Wiegand C. and Ypsilantis, T., Phys. Rev. 100 (1955) p 947.
11. Ypsilantis T., in *The Discovery of Nuclear Antimatter*, L. Maiani and R.A. Ricci eds., Italian Physical Society, Bologna 1996, p 37
12. Chamberlain, O, *et al.*, Phys. Rev, 102 (1956), p. 921.
13. Dirac, P.A.M., Rev. Mod. Phys. vol 21 (1949) ,p 392.
14. Segrè, E., , Ann. Rev. Nuc. Sci., vol 8 (1958), p 127
15. Pagels. H., *The Cosmic Code,* Hollen Street Press, Slough 1983, p 295
16. Zichichi, A, in *The Discovery of Nuclear Antimatter, op.cit.*, p 123
17. T. Massam et al., Il Nuovo Cimento, Serie X, vol 39 (1965), p10
18. D. Dorfan et al., Phys. Rev. Lett. vol 14 (1965) p 1003