Origins of the FW Transformation: A Memoir

This five-page typewritten document was found among the papers of Professor Foldy by Robert W. Brown who organized them for the Foldy family. It had several small penciled corrections on it. It has probably not been published elsewhere. I thank Professor Foldy's widow, Roma Foldy, for permission to include it here.

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I have chosen to discuss in this memoir some of the circumstances surrounding the discovery of the Foldy-Wouthuysen (FW) transformation, the work for which I am probably best known. This work is concerned with the relationship between the Dirac equation which describes an electron relativistically and the Schrödinger equation which gives the more familiar non-relativistic description of its behavior. This problem had been explored by both Pauli and Schrödinger, and by Dirac himself but the resolution gave rise to various mysterious concepts like an imaginary electric dipole moment for the electron. In addition, the equation itself in a conventional interpretation predicts that any component of the electron's velocity is equal to the speed of light, yet has the beneficial predictions that the electron has spin ½ as well as a magnetic moment, but with an "anomalous" gyromagnetic ratio, and that the electron has an anti-particle: the positron. The FW transformation clears up most of these problems at a single stroke. But our discovery of it did not arise out of an attempt to exorcise these "ghosts'; it arose as a by-product of an attempt to solve a very practical problem.

Siegfried (Sieg) Wouthuysen (pronounced Vout'-high-sen), a Dutch student of H. A. Kramers (the "father of renormalization" in the context of quantum field theory) before World War II, was in hiding in Belgium during the Nazi occupation, and then came to the University of California/Berkeley at the time that I was a graduate student there. He, I and another graduate student, Harold Lewis, were Oppenheimer's last three doctoral students and we accompanied him to the Institute for Advanced Study in Princeton in 1947 when he became its Director. While Sieg and I were good friends we did not work together there. I became involved in a problem on renormalization in classical electrodynamics with Bram (Abraham) Pais). We came close but did not succeed in solving this problem (this was done substantially later in a very elegant fashion by Fritz Rohrlich) but in the process I learned a great deal about canonical transformation theory from the study of Schwinger's papers of that period. The three of us received our PhD.'s at the end of that academic year. While I had been offered (and had accepted) a position at Case Institute of Technology (now part of Case Western Reserve University) for the following academic year, I had no position for the intervening summer. Bob Marshak, then head of the physics department at the University of Rochester had visited the Institute and offered me a summer position at Rochester to work with him on a problem—namely meson production in nucleon-nucleon collisions. Sieg, who was married that summer, had accepted a position at Rochester for the coming academic year. I worked that summer on the meson-production problem, which Marshak and I had decided to do in the approximation that the nucleons are treated non-relativistically and succeeded in getting some interesting results. Actually some sign errors sneaked through (which were not decisive) and it was only some time afterwards that it was realized that important isotopic spin selection rules played a role in getting results that some of the meson-production cross-sections vanished. In any case I spent the next nine months at Case working on some other problems.

During the summer of 1949, Richard Feynman was scheduled to give lectures on his novel approach o quantum electrodynamics at the annual Theoretical Physics Summer School at the University of Michigan so I arranged to attend these and then to spend some time again at Rochester. While in Ann Arbor I met another young theoretical physicist, Cecile Morette (later Cecile Morette DeWitt), and learned that she also had worked on the meson production problem getting results different from ours which she attributed to our having dropped some terms which she claimed contributed substantially. I contended that these were relativistic terms but I did not know how to establish this. In this case it was nucleons rather than electrons which were under consideration. (Of course it was not then known that the proton was made up of constituent particles.) I was still preoccupied with this problem of separating relativistic from non-relativistic terms when I arrived later in Rochester. Arthur Wightman, who had just received his Ph.D. at Princeton that summer, was there and I talked about the problem with him. He reminded me of Pauli's method of handling the problem but when I went off to think about it I realized that it simply would not work well in the context of the problem with which I was concerned. The reason is that essentially the Dirac equation involves a four-component wave function and the Schrödinger equation involves a two-component wave function. For a non-relativistic particle the two "upper" components of the wave function are large and the two "lower" components are small (of order v/c compared to the upper components). Pauli used a method of eliminating the two lower components in terms of the upper components to get a Schrödinger-like equation correct to second order in v/c. Unfortunately the resultant "effective Hamiltonian" of this equation contains the "imaginary" terms referred to earlier, and is therefore non-Hermitian with the unfortunate consequence that a "Schrödinger interpretation" of the resultant wave function leads to non-conservation of probability.

It was at this point that the light flashed on in my mind: the solution was to eliminate coupling terms between the upper and lower components by canonical transformations patterned after the transformations used by Schwinger to eliminate certain coupling terms between matter and the electromagnetic field – namely those corresponding to so-called "virtual processes". In fact, the elimination of those terms in the Dirac Hamiltonian which couple upper and lower components in the Dirac four-component wave function also corresponded to elimination of a type of virtual transition: in this case virtual transitions of the Dirac electron from positive to negative energy states, or in Dirac's theory of the filled sea of negative energy states where an empty negative energy state corresponds to a positron, virtual transitions of an electron from a negative energy state to a positive energy state or what we would now call an electron-positron pair state. The problem with Pauli's method (which can be repaired, but with considerable effort) is that one essentially throws away the lower components and with it conservation of probability. This is why it is expedient to perform a canonical transformation which eliminates the terms coupling upper and lower components and not just to discard the lower components of the wave function. From what I had learned about the classical case and Schwinger's methods it was an easy matter to construct the required canonical transformation to eliminate such terms to any order of v/c or equivalently in inverse powers of c. For the case of a free particle I obtained for the Hamiltonian just the expansion of the usual classical relativistic energy in powers of the momentum with the first term just the rest energy, the second the usual non-relativistic kinetic energy and then terms of fourth and higher order in the momentum p. Working out the same result for the case of an electron in an electromagnetic field I again obtained the proper Pauli Hamiltonian with the well-known second order relativistic corrections: the magnetic moment-magnetic field interaction the spin-orbit coupling and the so-called Darwin term to second order, and could get the higher order terms straightforwardly. (This Darwin is Charles G. Darwin, a physicist, and grandson of the more famous Charles G. Darwin of the Origin of Species). When I started to obtain these last results, I told my friend Sieg about what I had found in the hope of getting his help in carrying out and checking these and planned future calculations. A day or two later he came beck to me with the result that he could obtain the canonical transformation for a free particle in closed form (rather than as an infinite series) which would completely decouple the upper and lower components of the Dirac wave-function. It contained a square-root of an operator involving the momentum! I cannot recall whether Sieg appreciated its importance but I soon realized (though I do not recall whether this was minutes or days later) that here was the key to most of the puzzles about the Dirac equation. I will explain this in a moment but first I want to say something else.

Ever since our collaboration Sieg has always deprecated his contribution to this work, regarding it as a rather trivial extension of my own, and I have tried to convince him of the important role this contribution played. I do not know whether I have really succeeded. The last time we had occasion to see each other (about eight years ago) he was somewhat upset that Max Dresden in his biography of Kramers had at one point in the text credited our work to him without mentioning me (though Dresden's citation to the work contains both our names). He had told Max that this was a distorted view of what occurred and Max had promised to correct this in a second edition, should one ever be published. During that meeting Sieg and I put forward our recollections of our collaboration. There were various discrepancies in what I think were details but no real disagreements of substance, even after forty years. Actually, I discovered later that the transformation for a free particle had been discovered previously by Smio Tani during the war and published in the Japanese journal "Progress of Theoretical Physics" and even earlier by the German theoretical physicist R. Becker (but in this case only to second order in v/c). *(ed. note: Tani, who became Foldy's research associate at Case in 1957, has recently pointed out that his discovery was actually made after the war.* Prog. Theor. Phys. (Japan) ***6*** *267 1951)* Neither dealt with the problem of interaction with an external field. Now to return to my principle subject, the importance of the form for the canonical transformation obtained by Sieg was that it is essentially a non-local transformation on the wave function with the non-locality of the order of the Compton wavelength of the electron. What this means is that the transformed wave function at any point depends on the values of the original wave function on a set of points lying within a distance of the order of the Compton wave-length of the electron from the original point. Another way to express this fact is that the operator which represents the position of an electron in the usual form of the Dirac equation represents a different observable after the canonical transformation, an observable which we called the mean position. For a free particle the mean position moves with constant velocity equal to the momentum of the particle divided by its energy, as we would expect from classical relativity. On the other hand the original (Dirac) position moves with a velocity of the order of the velocity of light over a region of radius of the order of the Compton wave-length (the so-called zitterbewegung). Since the different components of the operator representing the Dirac velocity do not commute with one another in general it is not possible to measure more than one component at a time and such measurement will always yield a value equal to the velocity of light. With this picture we can understand qualitatively the features of the interaction of a particle with an external field exhibited by the Dirac equation, and in particular, the origin of the magnetic moment, the resultant spin-orbit coupling, and the Darwin term which has just the form of an interaction of an extended spherically-symmetric charge distribution of Compton wave-length radius with an electrostatic field. These ideas are described in substantial detail in our paper. With this methodology available I could immediately see that I had not dropped any non-relativistic terms in our meson-production calculation, regardless of what other errors or oversights that paper may contain. In fact it soon became clear that the perturbation method on which virtually all such calculations, including ours, depended at that time was quite unreliable for hadronic quantities since the meson-nucleon coupling was so strong.

This is not really quite the end of the story. I presented a paper on this work at a meeting of the American Physical Society which was held at Columbia University in February of 1950 (the society was very much smaller then than it is now). As I was waiting to be called I noticed with some trepidation that Pauli was in the room. Pauli was as expert as anyone on relativistic quantum mechanics and the Dirac equation and he was also the *enfant terrible* of physics. I recognized Pauli since I had heard him deliver some lectures at Princeton during the war and I knew his reputation so I was, to say the least, quite distressed to see him there. Shortly after I started delivering my paper, Pauli's head began to nod back and forth and I felt relieved that he seemed to be accepting what I had to say. He made no comment on the paper but I learned later that this nodding was a well-known tic of Pauli's when he was in an audience, but of this I was not aware. I have often wondered how I would have felt and responded if his tic consisted of shaking his head from side-to-side. In 1963-64 I spent a year at CERN in Geneva and discovered that Pauli had left his collection of journals to CERN. I looked up the FW paper to see whether had had made any notes beside it. I found some pencil marks, which at lest suggested that he had read it, but there were no remarks of any substance, as I recall. The paper seems to have been fairly well received. My first graduate student, Richard Osborne, wrote a thesis on extending the transformation to a system of two particles and some formal difficulties arose which we did not resolve but were soon after clarified by Z. V. Chraplevy and a student W. A. Barker, at St. Louis University. Someone, though I cannot recall who, once told me that Dirac had known of, but did not publish, the FW transformation, presumably again for a free particle. I would find it hard to believe that had he been aware of the transformation for a particle interacting with a field that he would not have presented it in his book in place of the rather unaesthetic treatment he does give which involves the imaginary electric dipole moment term in the effective Hamiltonian.

The work contained in the FW paper had some far-reaching consequences for me, personally. A year after its publication I discovered an application of the transformation to understanding the problem of the so-called "electron-neutron interaction". The result of that work was "instant fame" of a sort for reasons I cannot go into here but hope to discuss in another memoir. A year after that, while returning from Copenhagen by ship, I realized that the FW transformation decomposed the Dirac equation into irreducible representations of the Poincaré group, which I soon after discovered were explored by Wigner and his student, T. D. Newton. This ultimately led me to a line of investigation which I continued to follow well into the mid-1970's and was linked to another influential paper of Dirac's published not long after the end of World War II. This also I hope to recount in another memoir.