

THE GREENING OF QUANTUM FIELD THEORY: GEORGE AND I*

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The young theoretical physicists of a generation or two earlier subscribed to the belief that: If you haven't done something important by age 30, you never will. Obviously, they were unfamiliar with the history of George Green, the miller of Nottingham.

Born, as we all know, exactly two centuries ago, he received, from the age 8, only a few terms of formal education. Thus, he was self-educated in mathematics and physics, when in 1828, at age 35, he published, by subscription, his first and most important work: "An Essay on the Applications of Mathematical Analysis to the Theory of Electricity and Magnetism." The Essay was dedicated to a noble patron of the "Sciences and Literature," the Duke of Newcastle. Green sent his own copy to the Duke. I do not know if it was acknowledged. Indeed, as Albert Einstein is cited as effectively saying, during his 1930 visit to Nottingham, Green, in writing the Essay, was years ahead of his time.

There are those who cannot accept that someone, of modest social status and limited formal education, could produce formidable feats of intellect. There is the familiar example of William Shakespeare of Stratford on Avon. It took almost a century and a half to surface, and yet another century to strongly promote, the idea that Will of Stratford could not possibly be the source of the plays and the sonnets which had to have been written by Francis Bacon. Or was it the earl of Rutland? Or perhaps it was William, the sixth earl of Derby? The most recent pretender is Edward deVir, seventeenth earl of Oxford, notwithstanding the fact that he had been dead for 12 years when Will was put to rest.

I have always been surprised that no one has suggested an analogous conspiracy to explain the remarkable mathematical feats of the miller of Nottingham. So I invented one.

Descended from one of the lines of the earl of Nottingham was the branch of the earls of Effingham, which was separated from the Howards in 1731. The fourth holder of the title died in 1816, with apparently no claimant. In that year, George Green, age 23, could well have reached the maturity that led, 12 years later, to the publication of the Essay. And what of the remarkable fact that, in the same year that the earldom was revived, 1837, George Green graduated fourth wrangler at Cambridge University?

The conspiracy at which I hint darkly is one in which I believe quite as much as I think Edward deVir is the real Shakespeare.

I consider myself to be largely self-educated. A major source of information came from my family's possession of the Encyclopedia Britannica Eleventh Edition. I recently became curious to know what I might have, and probably did, learn about George Green, some 65 years before.

There is no article detailing the life of George Green. There are, however, four brief references that indicate the wide range of Green's interests.

First, in the article Electricity, as a footnote to the description of Lord Kelvin's work, is this:

In this connexion the work of George Green (1793-1841) must not be forgotten. Green's "Essay on the application of mathematical analysis to the theories of electricity and magnetism," published in 1828, contains the first exposition of the theory of potential. An important theorem contained in it is known as Green's theorem, and is of great value.

It was, of course, Lord Kelvin, or rather William Thomson, who rescued Green's work from total obscurity.

Then, in the article Hydromechanics, after several applications of Green's transformation, which is to say, the theorem, there appears, under the heading "The Motion of a Solid through a Liquid":

The ellipsoid was the shape first worked out, by George Green, in his "Research on the vibration of a pendulum in a fluid medium" (1833).

On to the article Light under the heading "Mechanical Models of the Electro-magnetic Medium." After some negative remarks about Fresnel, one reads:

Thus, George Green, who was the first to apply the theory of elasticity in an unobjectional manner ...

This is the content of "On the Laws of Reflexion and Refraction of Light" (1837).

Finally, the paper "On the Propagation of Light in Crystallized Media" (1839) appears in the Britannica article Wave as follows:

The theory of waves diverging from a center in an unlimited crystalline medium has been investigated with a view to optical theory by George Green.

The word "propagation" is a signal to us that, in little more than 10 years, George Green had significantly widened his physical framework. From the static three-dimensional Green function that appears in potential theory, he had arrived at the concept of a dynamical, four-dimensional Green function. It would be invaluable a century later.

To continue the saga of George Green and me — my next step was to trace the influences of George Green on my own works. Here I spent no time over ancient documents. I went directly to a known source: THE WAR.

I presume that in Britain, unlike the United States, the War has a unique connotation. Apart from a brief sojourn in Chicago, to see if I wanted to help develop The Bomb — I didn't — I spent the War years helping to develop microwave radar. In the earlier hands of the British, that activity, famous for its role in winning the Battle of Britain, had begun with electromagnetic radio waves of high frequency, to be followed by very high frequency, which led to very high frequency, indeed.

Through those years in Cambridge (Massachusetts, that is), I gave a series of lectures on microwave propagation. A small percentage of them is preserved in a slim volume entitled *Discontinuities in Waveguides*. The word propagation will have alerted you to the presence of George Green. Indeed, on pages 10 and 18 of an introduction there are applications of two different forms of Green's identity.

Then, on the first page of Chapter 1, there is Green's function, symbolized by G . In the subsequent 138 pages the references to Green in name or symbol are more than 200 in number.

As the War in Europe was winding down, the experts in high power microwaves began to think of those electric fields as potential electron accelerators. I took a hand in that and devised the microtron which relies on the properties of relativistic energy. I have never seen one, but I have been told that it works. More important and more familiar is the synchrotron.

Here I was mainly interested in the properties of the radiation emitted by an accelerated relativistic electron. I used the four-dimensionally invariant proper time formulation of action. It included the electromagnetic self-action of the charge, which is to say that it employed a four-dimensionally covariant Green's function. I was only interested in the resistive part, describing the flow of energy from the mechanical system into radiation, but I could not help noticing that the mechanical mass had an invariant electromagnetic mass added to it, thereby producing the physical mass of an electron. I had always been told that such a union was not possible. The simple lesson? To arrive at covariant results, use a covariant formulation, and maintain covariance throughout.

Quantum field theory, or more precisely, quantum electrodynamics, was forced from childhood into adolescence by the experimental results announced at Shelter Island early in June, 1947. The relativistic theory of the electron created by Dirac in 1928 was wrong. Not very wrong, but measurably so.

A few days later, I left on a honeymoon tour across the United States. Not until September did I begin to work on the obvious hypothesis that electrodynamic effects were responsible for the experimental deviations, one on the magnetic moment of the electron, the other on the energy spectrum of the hydrogen atom.

Although a covariant method was in order, I felt I could make up time with the then more familiar non-covariant methods of the day. By the end of November I had the results. The predicted shift in magnetic moment agreed with experiment. As for the energy shift in hydrogen, one ran into an expected problem.

Consider the electromagnetic momentum associated with a charge moving

at constant speed. The ratio of that momentum to the speed is a mass — an electromagnetic mass. It differs from the electromagnetic mass inferred from the electromagnetic energy. Analogously, the magnetic dipole moment inferred for an electron moving in an electric field is wrong. Replacing it by the correct dipole moment leads to an energy level displacement that was correct in 1947, and remains correct today at that level of accuracy as governed by the fine structure constant.

I described all this at the January 1948 meeting of the American Physical Society, after which Richard Feynman stood up and announced that he had a relativistic method. Well, so did I, but I also had the numbers. Indeed, several months later, at the opening of the Pocono Conference, he ran over to me, shook my hand, and said "Congratulations, Professor! You got it right," which left me somewhat bewildered. It turned out he had completed his own calculation of the additional magnetic moment. Later we compared notes and found much in common.

Unfortunately, one of the things we shared was an incorrect treatment of low energy photons. Nothing fundamental was involved; it was a matter of technique in making a transition between two different gauges. But, as in American politics these days, the less important the subject, the louder the noise. When that lapse was set right, the result of 1947 was regained. Incidentally, even Lord Rayleigh once made a mistake. That's one reason for its being called the Rayleigh-Jeans law.

To keep to the main thrust of the talk — the evolution of Green's function in the quantum mechanical realm — I move on to 1950, and a paper entitled "On Gauge Invariance and Vacuum Polarization."

This paper makes extensive use of Green's functions, in a proper-time context, to deal with a variety of problems: non-linearities of the electromagnetic field, the photon decay of a neutral meson, and a short, but not the shortest derivation of the additional electron magnetic moment. The latter ends with the remark that "The concepts employed here will be discussed at length in later publications." I cannot believe I wrote that.

The first, rather brief, discussion of those concepts appeared in a pair of 1951 papers, entitled "On the Green's Functions of Quantized Fields." One would not be wrong to trace the origin of today's lecture back 42 years to these brief notes. This is how paper I begins:

The temporal development of quantized fields, in its particle aspect, is described by propagation functions, or Green's functions. The construction of these functions for coupled fields is usually considered from the viewpoint of perturbation theory. Although the latter may be resorted to for detailed calculations, it is desirable to avoid founding the formal theory of the Green's functions on the restricted basis provided by the assumption of expandability in powers of the coupling constants. These notes are a preliminary account of a general theory of Green's functions, in which the defining property is taken to be the representation of the fields of prescribed sources.

We employ a quantum dynamical principle for fields which has been described in the 1951 paper entitled "The Theory of Quantized Fields." This (action) principle is a differential characterization of the function that produces a transformation from eigenvalues of a complete set of commuting operators on one space-like surface to eigenvalues of another set on a different surface.

In one example of a rigorous formulation, Green's function, for an electron-positron, obeys an inhomogeneous Dirac differential equation for an electromagnetic vector potential that is supplemented by a functional derivative with respect to the photon source; and, the vector potential obeys a differential equation in which the photon source is supplemented by a vectorial part of the electron-positron Green's function. (It looks better than it sounds.) It is remarked that, in addition to such one-particle Green's functions, one can also have multiparticle Green's functions.

The second note begins with:

In all the work of the preceding note there has been no explicit reference to the particular states on (the space-like surfaces) that enter the definitions of the Green's functions. This information must be contained in boundary conditions that supplement the differential equations. We shall determine these boundary conditions for the Green's functions associated with vacuum states on both (surfaces).

And then:

We thus encounter Green's functions that obey the temporal analog of the boundary condition characteristic of a source radiating into space. In keeping with this analogy, such Green's functions can be derived from a retarded proper time Green's function by a Fourier decomposition with respect to the mass.

The text continues with the introduction of auxiliary quantities:

The mass operator M that gives a non-local extension to the electron mass; a somewhat analogous photon polarization operator P ; and T , the non-local extension of the coupling between the electromagnetic field and the fields of the charged particles. Then, in the context of two-particle Green's functions, there is the interaction operator J .

The various operators that enter in the Green's function equations M , P , T , J , can be constructed by successive approximation. Perturbation theory, as applied in this manner, must not be confused with the expansion of the Green's functions in powers of the charge. The latter procedure is restricted to the treatment of scattering problems.

Then one reads:

It is necessary to recognize, however, that the mass operator, for example, can be largely represented in its effect by an alteration in

the mass constant and by a scale change of the Green's function. Similarly, the major effect of the polarization operator is to multiply the photon Green's function by a factor, which everywhere appears associated with the charge. It is only after these renormalizations have been performed that we deal with wave equations that involve the empirical mass and charge, and are thus of immediate physical applicability.

In the period 1951-1952, two colleagues of mine at Harvard, and I, wrote a series of papers under the title "Electrodynamical Displacements of Atomic Energy Levels." The third paper, which does not carry my name, is subtitled "The Hyperfine Structure of Positronium." I quote a few lines:

The discussion of the bound states of the electron-positron system is based upon a rigorous functional differential equation for the Green's function of that system.

And,

Theory and experiment are in agreement.

As for the rest of the 1950's, I focus on two highlights. First: although it could have appeared any time after 1951, it was 1958 when I published "The Euclidean Structure of Relativistic Field Theory." Here is how it begins:

The nature of physical experience is largely conditioned by the topology of space-time, with its indefinite Lorentz metric. It is somewhat remarkable, then, to find that a detailed correspondence can be established between relativistic quantum field theory and a mathematical image based on a four-dimensional Euclidean manifold. The objects that convey this correspondence are the Green's functions of quantum field theory, which contain all possible physical information. The Green's functions can be defined as vacuum-state expectation values of time-ordered field products.

I well recall the reception this received, running the gamut from "It's wrong" to "It's trivial." It is neither.

Second (highlight):

Another Harvard colleague and I had spent quite some time evolving the techniques before we published a 1959 paper entitled "Theory of Many-Particle Systems." It was intended to bring the full power of quantum field theory to bear on the problems encountered in solid state physics, for example. That required the extension of vacuum Green's functions, which refer to absolute zero temperature, into those for finite temperature. This is accomplished by a change of boundary conditions, which become statements of periodicity, or anti-periodicity, for the respective BE or FD statistics, in response to an imaginary time displacement.

As an off shoot of this paper, I published in 1960, "Field Theory of Unstable Particles." Here is how it begins:

Some attention has been directed recently to the field theoretic description of unstable particles. Since this question is conceived as a basic problem for field theory, the responses have been some special device or definition, which need not do justice to the physical situation. If, however, one regards the description of unstable particles to be fully contained in the framework of the general theory of Green's function, it is only necessary to emphasize the relevant structure of these functions. That is the purpose of this note. What is essentially the same question, the propagation of excitations in many-particle systems where stable or long-lived "particles" can occur under exceptional circumstances, has already been discussed along these lines.

One might be forgiven for assuming that this saga of George and me effectively ended with this paper. But that was 1/3 century ago!

To set the stage for what actually happened, I remind you that operator field theory is an extrapolation of ordinary quantum mechanics, with its finite number of degrees of freedom, to a continuum labeled by the spatial coordinates. The use of such space-time dependent variables presumes the availability, in principle, of unlimited amounts of momentum and energy. It is, therefore, a hypothesis about all possible phenomena of that type, the vast majority of which lies far outside the realm of accessible physics. In honor of a failed economic policy, I call such procedures: trickle-down theory.

In the real world of physics, progress comes from tentative excursions beyond the established framework of experiment and theory — the grass roots — indeed, the Green grass roots. What is sought here, in contrast with the speculative approach of trickle-down theory, is a phenomenological theory — a coherent account of the phenomena that is anabatic (from anabasis: going up).

The challenge was to reconstruct quantum field theory, without operator fields. The source concept was introduced in 1951 as a mathematical device — it was a source of fields. It took 15 years to appreciate that, with a finite, rather than an unlimited, supply of energy available, it made better sense to use the more physical-if idealized-concept of a particle source. Indeed, during that time period one had become accustomed to the fact that to study a particle of high energy physics, one had to create it. And, the act of detection involved the annihilation of that particle.

This idea first appeared in an article, entitled "Particles and Sources," which recorded a lecture of the 1966 Tokyo Summer Lectures in Theoretical Physics. The preface begins with:

It is proposed that the phenomenological theory of particles be based on the source concept, which is abstracted from the physical possibility of creating or annihilating any particle in a suitable collision. The source representation displays both the momentum (energy) and the space-time characteristics of particle behavior.

Then, in the introduction, one reads:

Any particle can be created in a collision, given suitable partners, before and after the impact to supply the appropriate values of the spin and other quantum numbers, together with enough energy to exceed the mass threshold. In identifying new particles it is basic experimental principle that the specific reaction is not otherwise relevant. Then, let us abstract from the physical presence of the additional particles involved in creating a given one (this is the vacuum) and consider them simply as the source of the physical properties that are carried by the created particle. The ability to give some localization in space and time to a creation act may be represented by a corresponding coordinate dependence of a mathematical source function, $S(x)$. The effectiveness of the source in supplying energy and momentum may be described by another mathematical source function, $S(p)$. The complementarity of these source aspects can be given its customary quantum interpretation: $S(p)$ is the four-dimensional Fourier transformation of $S(x)$.

The basic physical act begins with the creation of a particle by a source, followed by the propagation (aha!) of that particle between the neighborhoods of emission and detection, and is closed by the source annihilation of the particle. Relativistic requirements largely constrain the structure of the propagation function — Green's function.

We now have a situation in which Green's function is not a secondary quantity, implied by a more fundamental aspect of the theory, but rather, is a primary part of the foundation of that theory. Of course fields, initially inferred as derivative concepts, are of great importance, as witnessed by the title I gave to the set of books I began to write in 1968: *Particles, Sources, and Fields*.

The quantum electrodynamics that began to emerge in 1947 still bothers some people because of the divergences that appear prior to renormalization. That objection is removed in the phenomenological source theory where there are no divergences, and no renormalization.

As another example of such clarification I cite a 1975 paper entitled "Casimir Effect in Source Theory." The abstract reads:

The theory of the Casimir effect, including its temperature dependence is rederived by source theory methods, which do not employ the concept of (divergent) zero point energy. What source theory does have is a photon Green's function, which changes in response to the change of boundary conditions, as one conducting sheet is pushed into the proximity of another one.

A few years later, I and two colleagues at the University of California (UCLA), who had joined me from Harvard with their new doctorates, extended this treatment to dielectric bodies where forces of attraction also appear.

Having said this, I can move up to the present day, and the fascinating phenomenon of coherent sonoluminescence.

It has only recently been discovered that a single air bubble in water can be stabilized by an acoustical field. And, that the bubble emits pulses of light, including ultra violet light, in synchronism with the sonic frequency.

During the phase of negative acoustical pressure the bubble expands. That is followed by a contraction which, as Lord Rayleigh already recognized in his 1917 study of cavitation, turns into run away collapse. The recent measurements find speeds in excess of Mach 1 in air.

Then the collapse abruptly slows, and a blast of photons is emitted. In due time, the expansion slowly begins, and it all repeats, and repeats.

When confronted with a new phenomenon, everyone tends to see in it something that is already familiar. So, when told about this new aspect of sonoluminescence, I immediately said "It's the Casimir effect!" Not the static Casimir effect, of course, but the dynamical one of accelerated dielectric bodies. I have had no occasion to change my mind.

I can imagine a member of this audience thinking: "That's nice, but what is the role of George Green in this?"

Looking in at the center of the water container, one sees a steady blue light. A photomultiplier tube registers the succession of pulses, each containing a substantial number of photons, which can be an incomplete count because, deep in the ultraviolet, water becomes opaque.

A quantum mechanical description seeks the probabilities of emitting various numbers of photons, all of which probabilities are referred to the basic probability, that for emitting no photons. The latter probability dips below one — in some analogy with synchrotron radiation — because of the self-action carried by the electromagnetic field, as described by Green's function. And that function must obey the requirements imposed by an accelerated surface discontinuity, with water, the dielectric material, on one side, and a dielectric vacuum, air, on the other side. Carrying out that program is — as one television advertiser puts it — job one. Very fascinating, indeed.

So ends our rapid journey through 200 years. What, finally, shall we say about George Green? Why, that he is, in a manner of speaking, alive, well, and living among us.