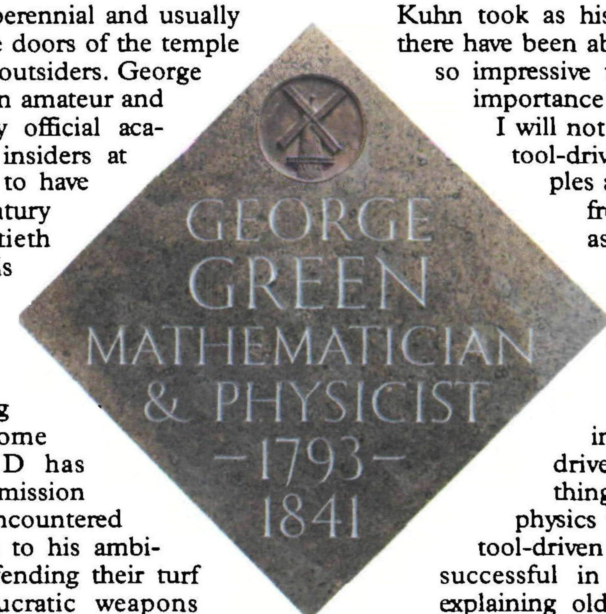
Invented in 1828 and successfully applied to classical electromagnetism and acoustics, Green's functions were the essential link between the theories of quantum electrodynamics proposed by Schwinger, Feynman and Tomonaga in 1948 and are still alive and well today

George Green and physics

FREEMAN DYSON

ALTHOUGH George Green was born 200 years ago, his life and work exemplify two themes that are still highly relevant to the progress and public understanding of science. The first theme is the perennial and usually unsuccessful struggle to keep the doors of the temple of science open to amateurs and outsiders. George Green was a prime example of an amateur and outsider, someone without any official academic credentials, beating the insiders at their own game. He was lucky to have lived in the early nineteenth century rather than in the late twentieth century. He was, in spite of his social and educational deficiencies, allowed to enter the temple, and his achievements were recognised by the insiders. If George Green were living today, since science has become professionalised and the PhD has become a necessary ticket for admission to the temple, he would have encountered much more formidable barriers to his ambitions. The insiders are now defending their turf against outsiders with bureaucratic weapons unknown in the 1830s.



Tools and concepts

The second theme that George Green's work exemplifies is the historical fact that scientific revolutions are more often driven by new tools than by new concepts. Thomas Kuhn in his famous book, *The Structure of Scientific Revolutions*, talked almost exclusively about concepts and hardly at all about tools. His idea of a scientific revolution is based on a single example, the revolution in theoretical physics that occurred in the 1920s with the advent of quantum mechanics. This was a prime example of a concept-driven revolution. Kuhn's book was so brilliantly written that it became an instant classic. It misled a whole generation of students and historians of science into believing that all scientific revolutions are concept-driven. The concept-driven revolutions are the ones that attract the most attention and have the greatest impact on the public awareness of science, but in fact they are comparatively rare. In the last five hundred years we have had six major concept-driven revolutions, associated with the names of Copernicus, Newton, Darwin, Maxwell, Einstein and Freud, besides the quantum-mechanical revolution that Kuhn took as his model. During the same period there have been about 20 tool-driven revolutions, not so impressive to the general public but of equal importance to the progress of science.

I will not attempt to make a complete list of tool-driven revolutions. Two prime examples are the Galilean revolution resulting from the use of the telescope in astronomy, and the Crick–Watson revolution resulting from the use of X-ray diffraction to determine the structure of big molecules in biology. The effect of a concept-driven revolution is to explain old things in new ways. The effect of a tool-driven revolution is to discover new things that have to be explained. In physics there has been a preponderance of tool-driven revolutions. We have been more successful in discovering new things than in explaining old ones. George Green's great discovery, the Green's function, is a mathematical tool rather than a physical concept. It did not give the world a new theory of electricity and magnetism or a new picture of physical reality. It gave the world a new bag of mathematical tricks, useful for exploring the consequences of theories and for predicting the existence of new phenomena that experimenters could search for. The Green's function was a tool of discovery, like the telescope and the microscope, but aimed at mathematical models and theories instead of being aimed at the sky and the microbe.

The invention of the Green's function brought about a tool-driven revolution in mathematical physics, similar in character to the more famous tool-driven revolution caused by the invention of electronic computers a century and a half later. Both the Green's function and the computer increased the power of physical theories, particularly in the fields of electromagnetism, acoustics and hydrodynamics. The Green's function and the computer are prime examples of intellectual tools. They are tools for clear thinking. They helped us to think more clearly by enabling us to calculate more precisely. ▶

But this article is not about the evils of the PhD system or the importance of tools in science. Rather it stems from a request by Professor Challis from Nottingham (where Green was born and is buried) to discuss "the events associated with the first introduction of Green's functions to a quantum mechanical treatment of electrodynamics". In other words, to describe what happened in 1948 when the words "Green's function", which had been part of the accepted language of classical electrodynamics and fluid mechanics for a hundred years, suddenly began to be spoken by quantum theorists. I will describe the physics of the 1940s. This will include an account of the scientific communities in Europe and the United States at the time, and of my own modest role as a messenger transmitting knowledge from Europe to America.

Classical and quantum Green's functions

To understand what happened in the 1940s we must begin with some historical background. There are two kinds of physics, classical physics beginning with Galileo and Newton in the seventeenth century, and quantum physics beginning with Planck and Bohr in the twentieth century. Classical physics describes big things such as rocks and planets. Quantum physics describes small things such as atoms and electrons. Next, cutting across the division of physics into classical and quantum, there is a division of physical objects into discrete and continuous. A rock is a discrete object. A flowing liquid or a magnetic field is a continuous object. Discrete objects are described by a finite set of numbers specifying their positions and velocities. The physics of discrete objects is called mechanics. Continuous objects are described by fields specifying their distribution and movement in space and time. The physics of continuous objects is called field theory. We have then four varieties of physical theories – classical mechanics, classical field theory, quantum mechanics and quantum field theory.

A highly compressed account of the history of theoretical physics goes like this. Physics is a drama in six acts. Act one, the classical physics of discrete objects, was worked out by Galileo and Newton. Act two, the classical physics of continuous objects, was worked out a hundred years later by Euler, Coulomb and Oersted. Euler did hydrodynamics, Coulomb did electrostatics and Oersted did magnetism. So it happened that, at the beginning of the nineteenth century, the classical field theories of hydrodynamics, electrostatics and magnetism were well established. Act three, in 1828 George Green revolutionised classical field theory by introducing his new tool, the Green's function, which described directly the causal relationship between the behaviour of a field at any two points in space and time. The Green's function measures the local response of the field at a given point at a later time to a local disturbance of the field at another given point at an earlier time. Green used the Green's function to clarify in a fundamental way the causal relationships between electric and magnetic fields. Helmholtz subsequently used Green's functions to clarify in an equally fundamental way the causal relationships between pressure and velocity in acoustics.

Friend, teacher and most unselfish scientist – Nicholas Kemmer (left) with the author

Act four, Heisenberg and Schrödinger worked out the quantum physics of discrete objects, describing the behaviour of atoms and electrons with the theory that became known as quantum mechanics. Act five, Fermi and Heisenberg and Dirac invented quantum field theory to describe the quantum physics of continuous objects. The quantum field theory that described electricity and magnetism was called quantum electrodynamics. But quantum field theory did not work well as a practical tool. It was unreliable and tended to give absurd answers to simple questions. You asked a quantum field theory the question, "What is the mass of an electron?" and the answer came back, "Infinity". That was not very helpful. As a result of these well publicised absurdities, the majority of practical physicists, especially in America, wrote off quantum field theory as useless and probably wrong. So at the end of act five in the 1930s, physics was divided into two disconnected parts, the classical field theories which worked beautifully in the classical domain, and the quantum mechanics of particles which worked beautifully in the quantum domain. There was no connection between the two domains. Green's functions were a convenient working tool in the classical domain, but there were no Green's functions in the quantum domain. The quantum field theories, which should have been the link between the two domains, were discredited and generally believed to be useless. That was the situation at the beginning of the 1940s. Act six was the resurrection of quantum field theories and the introduction of quantum Green's functions at the end of the 1940s. Act six is the main subject of this article.

To set the stage for act six, I looked at the four books from which I learned physics as a student – *Theoretical Physics* by Georg Joos (written in 1932), *The Principles of Quantum Mechanics* by Paul Dirac (1930), *The Quantum Theory of Radiation* by Walter Heitler (1935) and *Quantentheorie der Wellenfelder* by Gregor Wentzel (1942) – to see how often the name of George Green appears in them. All four books are classics, full of beautiful writing and clear thinking and I still refer to them frequently as sources of useful information. It turns out that George Green is mentioned only twice, by Joos and by Dirac, in both cases in connection with Green's theorem. Green's theorem is one of Green's major contributions to science, establishing an exact relation between the sources and the fluxes of two fields. It relates the sources of two fields inside any region of space to the fluxes of the same fields through the surface bounding the region. The theorem is applied by Joos to a problem in classical electrostatics, by Dirac to a problem in quantum scattering of a particle. But Green's more important discovery, the Green's function, is not mentioned by name in any of the books. If you look closely at the books by Heitler and Wentzel, you will see that Green's functions are lurking on many of their pages, but they are not labelled as such. The Green's functions appear mainly in equations and are called commutators, or potentials when they are mentioned in the text. I only learned much later, after I became a professor and had to learn something of the history of physics in order to teach it, that these elegant and useful tools had been borrowed from George Green.

Nicholas Kemmer

I now begin my narrative of act six as I experienced it, first in England and then in America. In 1946 I came to Cambridge with the intention of learning modern physics. When I arrived experimental physics was at a low ebb. The experimenters had been away during the war and in 1946 they were still struggling to get started on new enterprises which were to achieve huge success within a few years, the new sciences of radio-astronomy and molecular biology. I understood that Martin Ryle with his radio receivers and Max Perutz with his haemoglobin crystals were doing exciting stuff, but the stuff they were doing was clearly not physics. If I had wanted to be in a place where world class experimental physics was being done, I should have gone to Bristol where Cecil Powell and his team of scanners, with their microscopes and photographic emulsions, were developing the techniques which led within two years to the discovery of the pion (pi-meson). But I was a mathematician by training. At Cambridge I had been a student of Besicovitch, who had taught me the fine art of combining geometrical with analytical reasoning. I enjoyed talking with experimenters, but my more urgent need was to talk to a competent mathematical physicist. I needed to find somebody in Cambridge who could tell me what the important unsolved problems in theoretical physics were, and how I might use my mathematical skills to solve them.

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Numbers man – Hans Bethe

My first stroke of luck was to find Nicholas Kemmer. He was the teacher I needed. He rapidly became a friend as well as a teacher, and I am happy to say that our friendship is still alive and well after 45 years. Kemmer gave two courses of lectures in Cambridge, one on nuclear physics and one on quantum field theory. The quantum field theory course was a distillation of the wisdom of Europe, at that time still unknown in America. It happens that quantum field theory, a rigidly formal mathematical discipline, was invented in Europe and was for a long time more highly regarded in Europe than in America. In 1946 the only textbook on quantum field theory was Wentzel's *Quantentheorie der Wellenfelder*, written in Zürich and published in Vienna in 1943 during the middle of the war. Kemmer had a copy of Wentzel's book and allowed me to borrow it. It was at that time a treasure without price. I believe there were then only two copies in England. It was later reprinted in America and translated into English. But in 1946, few people in America knew of its existence, and even fewer considered it important. Kemmer knew it was important. He not only lent it to me but also explained why it was important.

Kemmer belonged to the generation of scientists whose careers were maximally disrupted by the war. As a young man in 1938 he had published a theory of nuclear forces mediated by a symmetric triplet of meson fields, one positive, one negative and one neutral. The purpose of the symmetric triplet was to achieve equality of the neutron–proton and proton–proton forces. In 1938 not one of the three hypothetical mesons had yet been discovered. The symmetric meson theory was considered a wild speculation. Ten years later, all three mesons were found, and the theory was proved to be a correct description of a new symmetry of nature. It is in fact one of the most brilliant predictions in the history of physics, comparable in brilliance with Yukawa's original prediction of the existence of the meson. But Kemmer received little public acclaim when his theory was confirmed. To blow his own trumpet was not in his nature.

Kemmer had spent most of the wartime years working on the Canadian atomic energy project at Chalk River. After the war, he was given a lecturing job in Cambridge with a heavy teaching load and an enormous number of undergraduates to supervise, although he never complained. As a result he had no time left over to resume the research career so brilliantly begun ten years before. Kemmer and I were both living in Trinity College. He was a college lecturer and was treated by the college as a drudge, while I was a junior fellow with no duties and complete freedom to do whatever I liked. This was a monstrously unfair division of labour, but Kemmer seemed to accept it without any sign of resentment. He was as generous in spending time with me as he was with his students. He always had time to advise me, to explain the difficult points in Wentzel's book, and to share with me his vision of quantum field theory as the key to a consistent mathematical description of nature. He was, and is, the most unselfish scientist I ever knew.

During my year at Cambridge I decided to go to America and make a fresh start there. In spite of my friendship with Kemmer, I found Cambridge depressing. I wanted to be in a place where I would be involved in an active group of young people doing research. By chance I met Sir Geoffrey Taylor, who had a little hand-made wind-tunnel in a cellar under the Cavendish Laboratory and did classic experiments on turbulence in the traditional Cavendish string-and-sealing-wax style. He was also the world's greatest expert on blast-waves, and had been at Los Alamos during the war to make sure that the bombs were exploded at the correct height to achieve the maximum blast damage. I told him I was planning to go to America and asked where I should go. He said at once, "Oh, you should go to Cornell and work with Bethe. That is where all the brightest people from Los Alamos went when the war was over". The conversation was over in one minute. At that time I hardly knew that Cornell existed, but I took Sir Geoffrey's advice, and a year later I was a student of Hans Bethe and a friend of Richard Feynman. That was my second enormous piece of luck.

Physics in 1948

My personal memories of the state of physics during my first year in America, 1948, are hopelessly unreliable 45 years later. A much more reliable and informative view can be obtained by looking at volume 73 of the *Physical Review*, the leading American physics journal which was started at Cornell in 1893. The journal came out twice every month in the 1940s and volume 73 contains the issues from January to June 1948. It covers all areas of physics – experimental and theoretical, atomic, nuclear and astronomical, quantum and classical.

Looking through volume 73 today, I see a great number of familiar faces belonging to old friends. Almost every paper in it is interesting, and many of them are memorable. In 1948 issues of the journal were thin enough to be read from cover to cover. Many of us did just that. Nowadays, of course, the journal is fragmented into six parts, each of which is so fat that nobody even attempts to read it. In 1948 it was possible to read the whole journal and obtain an overview of everything that American physicists were doing.

The paper that impressed me most in 1948 and still impresses me today is entitled "Relaxation Effects in Nuclear Magnetic Resonance Absorption", a monumental piece of work, 34 pages long, by Bloembergen, Purcell and Pound, three Harvard physicists. All three are still going strong and two are still at Harvard. I worked through all the details of this massive work with intense pleasure. Nuclear magnetic resonance means "tickling" the nuclear magnets inside a piece of solid or liquid material by alternating magnetic fields applied from the outside. In 1948 nuclear magnetic resonance was a recently discovered phenomenon. It is now, 45 years later, the basis of the medical technique known as MRI (magnetic resonance imaging) which enables doctors to obtain clear pictures of brain tumours and other soft tissue abnormalities in their patients.

The paper of Bloembergen, Purcell and Pound addresses the question: What are the effects of the environment, in which the nuclei are embedded, on the detailed behaviour of their magnetic resonance? The paper reports a comprehensive series of experiments together with an equally comprehensive theoretical analysis. It is one of the finest examples of the American style of physics, with experiment and theory working together as inexorably as a steam-roller and squashing a problem flat. The paper provided a fundamental understanding of the various ways in which the physical and chemical properties of the environment are linked with the shape of the nuclear resonance. It demonstrated that nuclear resonance could be made into a powerful new tool for exploring the properties of matter. It provided the essential foundation of knowledge on which the development of MRI as a tool of medical diagnosis could be built 30 years later.

In the same volume of the *Physical Review* are many other wonderful papers on the most diverse subjects: Alpher, Bethe and Gamow on the origin of chemical elements; Gleb Wataghin on the formation of chemical elements inside stars; Edward Teller on the change of physical constants; Lewis, Oppenheimer and Wouthuysen on the multiple production of mesons; Foley and Kusch on the experimental discovery of the anomalous magnetic moment of the electron; Julian Schwinger on the theoretical explanation of the anomalous moment; and Paul Dirac on the quantum theory of localisable dynamical systems. This was a vintage year for historic papers and I mention these seven just to give the flavour of what physicists were doing in the early post-war years.

The Alpher–Bethe–Gamow paper proposed that the chemical elements were formed by the successive capture of neutrons on protons during the initial expansion of the Universe from a hot dense beginning. Bethe had nothing to do with the writing of the paper but allowed his name to be put on it to fill the gap between Alpher and Gamow. This joke, which was Gamow's idea, made the paper famous. Meanwhile, Wataghin's paper, which proposed that the elements were formed in neutron stars, or more precisely in the process of rapid expansion of neutron stars into interstellar space, received much less attention. Wataghin was then living in Brazil and was not widely known. Unfortunately, it took many years to collect the evidence which proved that, at least for the great majority of the elements, Alpher–Bethe–Gamow were wrong and Wataghin was right.

One fact which I found remarkable in 1948 and still find remarkable today is that, among the hundreds of papers in volume 73 (almost all of them worth reading), the paper by Dirac is the only one concerned with quantum field theory. Dirac was a voice from another world. The vast majority of the papers are like the paper of Bloembergen, Purcell and Pound, sticking close to experiments and using a minimum of theory. The American scientific tradition was strongly empirical. Theory was regarded as a necessary evil, needed for the correct understanding of experiments but not valued for its own sake. Quantum field theory had been invented and elaborated in Europe. It was a sophisticated mathematical construction, motivated more by considerations of mathematical beauty than by success in explaining experiments. The majority of American physicists had not taken the trouble to learn it. They considered it, as Samuel Johnson considered Italian opera, an exotic and irrational entertainment. Dirac's paper, although published in America, found few readers. It was read by the small community of experts in general relativity who were themselves isolated from the mainstream of American physics.


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Sin-Itiro Tomonaga – Japanese physicist in the European tradition

Thus it happened that I arrived at Cornell as a student, and found myself, thanks to Nicholas Kemmer, the only person in the whole university who knew about quantum field theory. The great Hans Bethe and the brilliant Richard Feynman taught me a tremendous lot about many areas of physics, but when we were dealing with quantum field theory I was the teacher and they were the students. Bethe and Feynman had been doing physics successfully for many years without the help of quantum field theory, and so they were not eager to learn it. It was my luck that I arrived with this gift from Europe just at the moment when the new precise experiments of Lamb and others on the fine details of atomic energy levels required quantum field theory for their correct interpretation. When I used quantum field theory to calculate an actual number, the Lamb shift separating the energy levels of two of the states in a hydrogen atom with a spinless electron, Bethe was impressed. He said it was the first time he had seen quantum field theory do anything useful. For Bethe, formal mathematical machinery was pointless unless it could be used for calculating numbers. For him, and for almost all American theorists at that time, calculating numbers was the object of the game. Since the little gift that I brought from Europe to America could be used for calculating numbers, and the numbers could be checked by experiment, the gift received a friendly reception.

Quantum field theory and Green's functions

Julian Schwinger had known all about quantum field theory long before. But he shared the American view that it was a mathematical extravagance, better avoided unless it should turn out to be useful. In 1948 he understood that it could be useful. He used it to calculate the fine details of atomic physics revealed by the experiments of Lamb and Retherford, and Foley and Kusch. But he used it grudgingly. In his publications he preferred not to speak explicitly about quantum field theory. Instead, he spoke about Green's functions. It turned out that the Green's functions which Schwinger talked about and the quantum field theory that Kemmer talked about were fundamentally the same thing. In Schwinger's papers I could recognise some of my old friends, functions that I had seen before in Wentzel's book. This was one of the ways that Green's functions came to occupy a central place in the particle physics of the 1950s.

The second way that Green's functions emerged in particle physics was through the work of Richard Feynman at Cornell. Feynman had never been interested in quantum field theory. He had his own private way of doing calculations, based on things that he called "propagators". These were probability amplitudes for particles to propagate from one space-time point to another. He had rules for calculating the propagators and calculated the probabilities of physical processes by adding up the propagators. Each propagator was represented graphically by a collection of diagrams. Each diagram gave a pictorial view of particles moving along straight lines and colliding with one another at points where the straight lines met. When I learned this technique of drawing diagrams and calculating propagators from Feynman, I found it completely baffling, because it always gave the right answers but did not seem to be based on any solid mathematical foundation. Feynman called his way of calculating physical processes "the space-time approach", because his diagrams represented events as occurring at particular places and at particular times. The propagators described sequences of events in space-time. It later turned out that Feynman's propagators were identical with Green's functions. Feynman had been talking the language of Green's functions all his life without knowing it.

The third way that Green's functions appeared in the particle physics of the 1940s was in the work of Sin-Itiro Tomonaga, who had developed a new and elegant version of relativistic quantum field theory. His work was done in the complete isolation of wartime Japan, and was published in Japanese in 1943. The rest of the world became aware of it only in the spring of 1948, when Hideki Yukawa sent an English translation to Robert Oppenheimer at Princeton. Tomonaga was a physicist in the European tradition, having worked as a student with Heisenberg at Leipzig before the war. For him, in contrast to Schwinger and Feynman, quantum field theory was a familiar and natural language in which to think about particle physics. Tomonaga and Dirac were on the same wavelength. In his *Physical Review* paper of 1948, Dirac mentions Tomonaga in the text and in a footnote, but does not refer to Schwinger or Feynman.

After the war, Tomonaga's students in Japan had been applying his ideas to calculate the properties of atoms and electrons with high accuracy, and were reaching the same results as Schwinger and Feynman. When Tomonaga's papers began to arrive in America, I was delighted to see that he was speaking the language of quantum field theory that I had learned from Kemmer. It did not take us long to put all the various ingredients of the pudding together. When the pudding was cooked, all three versions of the new theory of atoms and electrons turned out to be different ways of expressing the same basic ideas. The basic idea of all three ways was to calculate Green's functions for all atomic processes that could be directly observed.

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Julian Schwinger (left) thought quantum field theory a mathematical extravagance while Richard Feynman talked the language of Green's functions all his life without realising it

Green's functions appeared as the essential link between the methods of Schwinger and Feynman, and Tomonaga's relativistic quantum field theory provided the firm mathematical foundation for all three versions of quantum electrodynamics.

Physics after 1950

The history of physics did not end in 1950. One of the major early advances beyond Tomonaga, Schwinger and Feynman was made by Rudolf Peierls in Birmingham in 1951, and published as "The Commutation Laws of Relativistic Field Theory" in the *Proceedings of the Royal Society*. Peierls is like Kemmer, an exceptionally unselfish person as well as a brilliant physicist. When I returned from America to England in 1949, he welcomed me as a member of his department in Birmingham and gave me all the privileges that he never asked for himself. He had a heavy teaching load; mine was minimal. He and his wife Genia were responsible for a big household and four children; I was a guest in their home. The idea was that he would take care of all the mundane chores while I would have freedom and leisure to make important discoveries in physics. Of course, things did not work out that way. During my two years in Birmingham, the most brilliant discovery made in Peierls' department was the general commutation law of relativistic field theories. The discovery was his, not mine. I remember the surprise and delight when he told me about his discovery in the garden of his house on Carpenter Road. It gave for the first time a deep and general understanding of the connection between Green's functions and the commutation relations between fields. It also clarified the meaning of the correspondence principle which connects classical and quantum field theories. It put Green's functions where they belong, in the logical foundation of classical and quantum physics.

During the 1950s, the new methods of calculation, using Green's functions to describe the behaviour of quantum fields, were successfully applied to a variety of problems in electrodynamics. Experiments and theory were pushed to higher and higher levels of accuracy. When we began these calculations, we had hoped to find a clear discrepancy between theory and experiment. A discrepancy would reveal some fundamental information about the many known and unknown particles and interactions that quantum electrodynamics did not take into account. Since quantum electrodynamics does not pretend to be a complete theory of everything, it must at some level of accuracy disagree with experiment. But all attempts to find a discrepancy ended in disappointment. As each quantity was measured and calculated to more and more decimal places, the measured and calculated numbers remained obstinately equal. Quantum electrodynamics turned out to be a more accurate description of nature than anybody in the 1940s had imagined possible. It now agrees with experiment to ten or eleven decimal places. We are left with an unsolved mystery to explain. How could all the important fields and particles that lie outside the scope of quantum electrodynamics have conspired to hide their influence on the processes that lie inside? To solve this mystery, even more accurate calculations will be required.

Green's functions were once again the working tools of calculation, both in particle physics and in condensed matter physics. And so they have remained up to the present day

After the Green's function method had been successfully applied to quantum electrodynamics, the next big step was to apply the same method to many-electron systems in the physics of condensed matter. I began the application to condensed matter physics in 1956 with a study of spin-waves in ferromagnets. I found that all the Green's function tricks that had worked so well in quantum electrodynamics worked even better in the theory of spin-waves. The spin-wave is the simplest propagating mode of disturbance in the condensed assemblage of electrons inside a ferromagnet, just as the photon is the simplest propagating mode in the electromagnetic field in free space. I was able to calculate the scattering of one spin-wave by another, using the same tricks that Feynman had used in quantum electrodynamics to calculate the scattering of light by light.

Meanwhile, the Green's function method was applied systematically by Bogolyubov and other people to a whole range of problems in condensed matter physics. The main novelty in condensed matter physics was the appearance of temperature as an additional variable. In quantum electrodynamics we had considered atoms and electrons in free space, an environment with zero temperature. In condensed matter the temperature can never be zero, and many of the most interesting questions concern the effect of temperature on the properties of the system. The appropriate tools for analysing condensed matter properties are therefore thermal Green's functions. A beautiful thing happens when you make the transition from ordinary Green's functions to thermal Green's functions. To make the transition, all you have to do is to replace the real frequency of any oscillation by a complex number whose real part is frequency and whose imaginary part is temperature. Thus thermal Green's functions are just as easy to calculate as ordinary Green's functions. To put in the temperature, you simply give the frequency an imaginary component. This is mathematical magic which I will not attempt to explain. Green's functions make such magic possible. That is one of the sources of their power and their beauty.

Soon after thermal Green's functions were invented, they were applied to solve the outstanding unsolved problem of condensed matter physics, the problem of superconductivity. They allowed Bardeen, Cooper and Schrieffer to understand superconductivity as an effect of a particular thermal Green's function expressing long-range phase-coherence between pairs of electrons. The Bardeen–Cooper–Schrieffer theory of 1957 explained satisfactorily all the observed features of superconductors as they were then known. The only thing the theory did not do was to give any hint of the high-temperature superconductors that were discovered unexpectedly 30 years later.

In the 1960s, after Green's functions had become established as the standard working tools of theoretical analysis in condensed matter physics, the wheel of fashion in particle physics continued to turn. For a decade, quantum field theory and Green's functions were unfashionable in particle physics. The prevailing view was that quantum field theory had failed in the domain of strong interactions, and that only phenomenological models of strong interaction processes could be trusted. Then, in the 1970s, the wheel of fashion turned once more. Quantum field theory was back in the limelight with two enormous successes, the Weinberg–Salam unified theory of electromagnetic and weak interactions, and the gauge theory of strong interactions now known as quantum chromodynamics. Green's functions were once again the working tools of calculation, both in particle physics and in condensed matter physics. And so they have remained up to the present day.

In the 1980s, quantum field theory moved off in new directions, to lattice gauge theories in one direction and to superstring theories in another. The Wilson Loop is the reincarnation of a Green's function in lattice gauge theory and there is a corresponding reincarnation of Green's functions in superstring theory. And as we move into the 1990s, Green's functions are still going strong, ready to help us again as soon as the wheel of fashion turns once more and the next new theory of everything emerges.

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