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WALTER M. ELSASSER

1904—1991

A Biographical Memoir by

HARRY RUBIN

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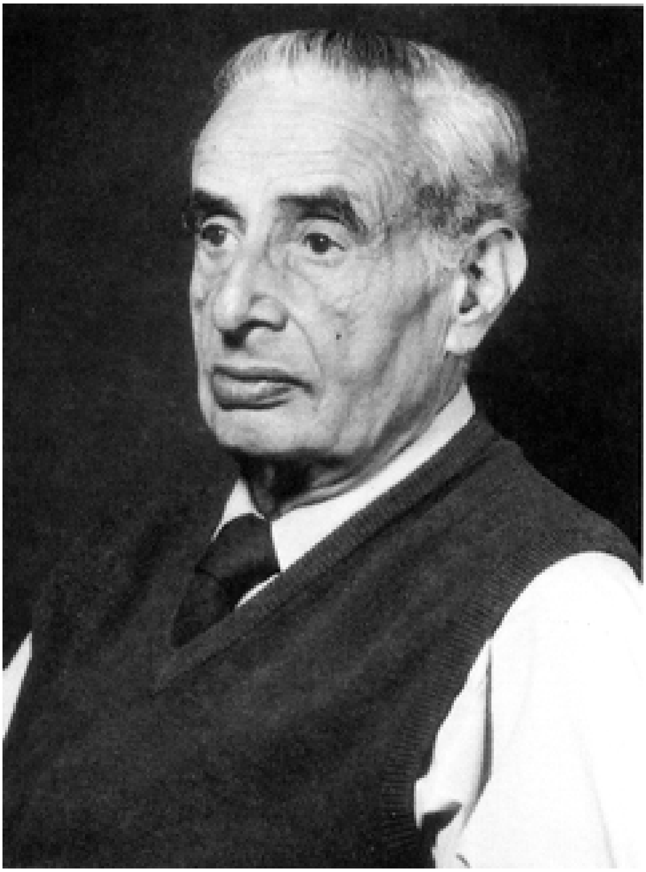

Walter M. Elsasser

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March 20, 1904–October 14, 1991

BY HARRY RUBIN

WALTER ELSASSER WAS TRAINED as a theoretical physicist and made several important contributions to fundamental problems of atomic physics, including interpretation of the experiments on electron scattering by Davisson and Germer as an effect of de Broglie's electron waves and recognition of the shell structure of atomic nuclei. Circumstances later turned his interests to geophysics, where he had important insights about the radiative transfer of heat in the atmosphere and fathered the generally accepted dynamo theory of the earth's magnetism. He devoted a major part of the last fifty years of his life to developing a theory of organisms, concentrating on the basic features that distinguish between living and inanimate matter, and he produced four books on the subject. While his contribution to biology was not widely acknowledged, he felt it would eventually be seen as his major scientific achievement.

BACKGROUND AND YOUTH

Walter was born in Mannheim, Germany, the older of two children of Maurice and Johanna Elsasser. His sister, Maria, was three years younger than him. His grandparents were prosperous Jewish merchants, but his father was a lawyer who was caught up in the great wave of assimilation and both parents became nonpracticing Protestants. Walter was confirmed in the Evangelical Church and had no idea of his Jewish ancestry until the age of fifteen when an acquaintance unexpectedly asked him about it. His father gave evasive answers when he inquired about his ancestry, and it took about a year to learn the truth. Up to this time he had no notion of Jews as a separate group, but his Jewish identity was to prove a crucial factor for him in the rising tide of antisemitism that culminated in the Hitler regime. One of the first manifestations of that antisemitism occurred in his last year of high school, when he applied to join a fraternity at the urging of his father who thought it would help turn his son from somewhat of an oddball into an ordinary good German citizen. His application was rejected on the grounds of the so-called Nürnberg articles adopted in 1919 by a national organization of fraternities which specified that persons of Jewish descent were inadmissible.

Up to the age of thirteen Walter had a congenial upbringing, although there were severe food shortages because of World War I. His father, then in his forties, was called into the German army and because he was a lawyer was given a desk job at the headquarters of the Swiss border guard. Since he had earlier developed a severe ulcer, which was exacerbated by barracks food, he obtained permission to have his family join him. When headquarters were shifted from a small town to a Konstanz, the food shortage became more severe. His father's illness became so bad he was mustered out of the army and shortly after was promoted to a judgeship at the Superior Court of Heidelberg.

Walter attended Gymnasium, which had a nine-year curriculum, roughly equivalent to the fourth through twelfth grades in America. The emphasis was on classical subjects such as Latin, Greek, mathematics, history, and religion with only a smattering of physics and chemistry. This was in contrast to the alternative Realschule, which appeared in the nineteenth century and emphasized science, mathematics, and modern languages. He felt that the unpragmatic immersion into a past world tended to bring out introverted features that he already possessed. In any case, Germans predominantly thought of science as a philosophical enterprise, and Walter maintained a strong interest in the philosophical aspects of science throughout his career.

THE ROAD TO SCIENCE

Walter's first encounter with natural science came from a journal of popular science to which his father subscribed. The journal also issued a series of small books dealing with various subjects of scientific research, which he perused from the age of thirteen or fourteen. These were monographs covering all branches of science by carefully chosen authors who knew their fields well and had a knack for popular interpretation. He was particularly attracted to the books dealing with the mysteries of the discoveries of atoms and molecules, a curiosity that never left him. His mathematics teacher around the eighth grade took an intensive interest in him when he discovered Walter's interest in science and mathematics. They took long walks in the wooded hills around Heidelberg and discussed everything concerned with science and the nature of scientific inquiry. In the ninth grade, mathematics was chiefly concerned with solid geometry, which his teacher thought Walter could learn in a fraction of the time devoted to it. He then suggested Walter get a book of calculus problems and do them in lieu of solid geometry. Walter worked through these elaborate problems one by one and thereby acquired an extensive working knowledge of calculus long before graduating from

Gymnasium. He also had a fairly good intuitive understanding of avant garde thought in physics from the monographs on atoms and crystals, light and X rays, and stars and galaxies, which he read twice if not more often.

Walter also found books of a philosophical character in his father's library, among them Ernst Haeckel's enormously popular *The Riddles of the Universe*. He already recognized the book as a statement of a very coarse rationalism or straight materialism. Although he disliked Haeckel's crude philosophy, the shock he received from it opened his eyes to genuine problems in the philosophy of science, which occupied much of his thinking in later years. Under pressure of antisemitism he began to identify with his forebears, the authors of the Bible. These men were unanimous about one thing—that the understanding of nature, man, or God was not a wholly intellectual matter. This stood in contrast to Haeckel, whose world was nothing but Haeckel-like intellects trying to understand the world intellectually.

Stimulated by such quasi-philosophy Walter started thinking about philosophy, in particular Hegel's dictum that, when quantitative differences in some field become pronounced, they tend to turn into differences in quality. This contrasts with the view of the great philosophers of science, that the scientist in his methods has no place for qualities: they pertain to philosophy proper, usually expressed as metaphysics. An example is the notion of heat, which first appears to our perceptions as a quality but which physicists have shown is motion of molecules that can in all detail account for the properties of warm and cold bodies.

Philosophical thinking eventually led Walter to the realization that the purpose of the scientific method, which is to structure the multifarious data of experience, is neither simple nor obvious. This is apparent in the example that for thousands of years wise men studied the motions of stars and developed complicated mathematical descriptions for them. Around 300 B.C. Aristarchos proposed that the earth rotates about itself and moves in an orbit around the sun, as did the other planets. This view was ignored as idle speculation for eighteen centuries up to Copernicus. Walter grew to realize that acceptance of scientific ideas depends on whether they harmonize with the prevailing ideas of society. These ideas are controlled by unconscious tendencies and cannot be controlled by rational volition. He considered "the current unrelieved and brutal dominance of pragmatism in science, often clothed in terms such as political or other 'relevance'," a frightening development.

Walter tried his hand briefly at the commercial enterprise left by his grandparents. He was surprised at how much he liked the work, primarily because it provided a framework for his activities. However, he had become deeply committed to scientific activity and did not wish to abandon the sense of intellectual adventure found in any scientist of an inquisitive mind. He therefore returned to science with a renewed determination to become a physicist. He entered the University of Heidelberg, where the professor of physics was Phillip Lenard, who had received the Nobel Prize several years earlier. While still in high school Walter had occasionally skipped class to attend Lenard's lectures with their admirable demonstration experiments. Lenard was actively involved in right-wing politics and was later to retire from his professorship to devote himself entirely to the Nazi party, ending up as president of the Nazi Academy of Sciences. But in 1922 politics was far from Walter's mind when he entered the large lecture room for his first physics class. Every seat was taken as Lenard walked in wearing an impeccably tailored suit bearing an enormous silver swastika. This was unusual as Germany was still a place of law and order, and professors were not expected to brandish symbols of political extremism in class. But the students gave him the longest, loudest, and most dedicated ovation Walter ever witnessed either before or after. They had clearly voted for the swastika, and Walter, who knew what this meant for Jews, was deeply disturbed. A number of people advised him to leave after his first year, as he would then have to enter the laboratory where Lenard was as likely as not to grab him by the scruff of the neck and throw him out bodily. He therefore decided in the fall of 1923 to move to Munich, by far the best university in southern Germany.

There were two kingdoms of physics in Munich, one headed by the noted experimentalist Wilhelm Wien and the other by the theoretician Arnold Sommerfeld. Walter did considerable experimental work in Wien's institute and enjoyed it very much. During his third semester he worked assiduously on such complicated matters as the Millikan oil drop experiment and the electrostatic quadrant electrometer. But the chief influence on him was Sommerfeld, who was as brilliant a teacher as he was a research man. Walter considered Sommerfeld's classes the best he ever attended. In addition, he participated in Sommerfeld's weekly seminar on contemporary atomic physics attended by his assistants and a small number of students. The seminar stimulated Walter to read some scientific literature on his own. He particularly remembered a paper by James Franck of Göttingen on spectral lines that involved highly excited states of gaseous atoms. Calculations indicated orbits of the electrons that were incomprehensible from a simple mechanical point of view. This provided an unexpected glimpse into a different order of nature in the minute dimensions of the atom that would soon be expressed in the mathematical language of quantum mechanics.

In Munich Walter overlapped for one semester with Werner Heisenberg, who then obtained his Ph.D. and went on to

Göttingen. On several occasions he heard Heisenberg say that doing physics was fun. This came as a great revelation to Walter, who had grown up in the stolid environment of the German middle class and who could think of scientific research only as a matter of duty or personal ambition or just to make money. The insight of doing science for the fun of it left Walter exhilarated and deeply impressed.

Walter became fond of traveling and hiking during his years in southern Germany. While in Munich he took many weekend trips to the fore-Alps, staying in the numerous inexpensive youth hostels and hiking on the innumerable trails to the top of the mountains, often 2 kilometers high. He decided to become an experimental physicist, but early in his third semester in Munich an assistant professor approached him with the advice that every single member of the faculty of Wien's institute aside from the director and himself were card-carrying members of the Nazi party. It was, of course, in Munich in November 1923 that the abortive beer hall putsch by Hitler and Ludendorff, the former chief of staff of the German army during World War I, took place. Munich would therefore not be a favorable place for Walter to continue to work toward his Ph.D. He suggested that Göttingen was not only very good but "full of Jews." Walter asked Sommerfeld to write a letter of introduction to James Franck, whose work had so intrigued him, and he at once acceded to Walter's visit. When he arrived in Göttingen early in 1925, Franck accepted him almost immediately as one of his Ph.D. candidates.

THE WORLD OF GÖTTINGEN

Walter developed a close relationship with James Franck, whom he admired greatly. Franck kept his office open, and Walter often found himself sitting on one end of an old battered sofa in lively discussion with Franck at the other end. Franck's main interest was in the study of atoms and molecules by the simplest means possible, electrons and light. He had already shown that electrons transfer energy strictly in lumps or packages and had received the Nobel Prize for this achievement in 1925 along with his younger collaborator Gustav Hertz. Walter received for his thesis the subject of fluorescence, whereby one quantum of light is absorbed while another slightly different energy is emitted. While he was making technical preparations for this thesis work he would drop in now and then to Franck's office to question him about atomic physics, and Walter came to regard Franck as his main teacher of science.

Max Born, the theoretician, was also a professor in Göttingen and the growing international reputation of Born and Franck attracted many foreign students, among them Robert Oppenheimer, Robert Brode, H. P. Robertson, and Patrick Blackett. Paul Dirac was a frequent visitor. Walter also had close interactions with German students in the institute and developed close friendships with Fritz Houtermans and Wolfgang Harries. There were many interesting lectures in physics, especially theoretical physics. One course was a seminar titled "The Structure of Matter," which played a germinal role in the development of quantum mechanics. Although the seminar was long listed under the name of David Hilbert, the famous mathematician, he was no longer active and its conduct was left to Max Born. Walter was a regular attendant at the seminar throughout his stay in Göttingen.

One of the earliest presentations in this seminar was by Born's student, Friedrich Hund, who later made major contributions to the theory of atomic spectra. The report was about an experiment of Davisson and Kunsman, two physicists at Bell Telephone Laboratories in New York. They shot electrons at a platinum plate and observed how they were scattered back. They found that the intensity of the distribution of the electrons varied with the angle of scattering, showing maxima and minima. This was a mysterious and quite surprising result, but the source was unimpeachable. Born tried to explain the result by the variable deflection of the extraneous electrons by shells of electrons that were of different densities. Without calculations it was impossible to know whether this suggestion was correct. One day in May 1925 Walter found in the library two recent papers by Einstein on the effect of quantum theory on gases. Einstein showed that certain gases behaved like assemblies of waves rather than particles. Twenty years earlier Einstein had noted that light, which everyone thought to be of wave motion, also had particle properties and that light was emitted and absorbed in packets called quanta. Coming from Einstein this was highly significant news. Einstein then noted a thesis of Louis de Broglie, which Walter found in the university library. The thesis contained de Broglie's basic idea that all primary components of matter have wave properties and presented a simple formula connecting the wave length with the particle's velocity. Walter wondered whether Davisson and Kunsman's maxima and minima were diffraction phenomena similar to those produced by X rays penetrating crystals but produced by a slight penetration and reflection of the electrons. He easily calculated the energy of the electrons required for the maxima, and it came out just right. Since the experiments were still crude, his surmise was only a guess but an exceptionally interesting and promising one.

Walter talked with Franck about the problem and was encouraged by his opinion that the idea was interesting though speculative. Franck suggested Walter think it through carefully and write to *Naturwissenschaften.* A few weeks later he did so and, after receiving Franck's approval, sent it off.

He later learned that the paper was reviewed by Einstein, who indicated that he was not sure how literally the idea of waves associated with electrons should be taken but thought the paper should certainly be published, which it was shortly thereafter. In print it became a note about half a page in the folio-sized volume. Heisenberg wrote to Wolfgang Pauli about the importance of Walter's note, and it was reproduced by Max von Laue in 1944 in a book on matter waves. In 1927 the decisive publication of Davisson and Germer appeared which demonstrated the wave character of electrons without a doubt. The authors referred to Schrödinger's famous papers of 1926 but not to Walter's note. Born published an article in 1926 in which he treated the collision of an electron with an atom as the scattering of a de Broglie wave and then developed a whole mathematical machinery for wave scattering. In this article Born introduced the notion of probability for the first time in quantum mechanics; he proposed that the wave function was a statistical guide for the particles in the sense that the amplitude of the wave specifies a probability for the particle to travel in certain ways. At the end of the paper Born quoted Walter's note saying he had correctly interpreted the experimental results of Davisson and Kunsman.

Publication of his note turned Walter's head and he asked Franck if he could experiment with the scattering of electrons by metal surfaces. This was quite foolish because that type of experiment is technically very difficult, and Walter's skill was not up to it. Franck agreed as long as he would do it on his own, since Franck would not allow his group to engage in highly speculative exercises. Walter tried it for three months before he realized how silly it was for an inexperienced young man to undertake such formidably difficult experiments on his own.

Among Franck's six or eight graduate students, Walter felt he was the least successful at building apparatus, a skill that was essential to an experimental physicist of those days. He also recognized that he was the most passionately interested in and most knowledgeable about physical theory. This was apparently recognized by Max Born, who, in the summer of 1926, asked Walter if he would consider becoming a theoretical physicist and doing a thesis with him. After consulting with Franck he decided to accept the offer and undertook an uncomplicated study of the collision of an electron with a hydrogen atom. This involved straightforward mathematical techniques with a large pile of formulas and offered few difficulties to Walter. He had few opportunities to discuss his work with Born, who worked at home and exhibited little interest in seeing students. Walter was fortunate at this stage to have the help of Robert Oppenheimer, who could steer him to the proper place in a mathematical book when difficulties did arise. One conversation with Born Walter remembered well. Born, who was a mathematical virtuoso, told Walter that he was not outstanding in mathematics but strong in conceptual thought, where Born felt less secure. A common idea is that conceptual thought precedes precise mathematical analysis: models and patterns emerge out of the primal chaos of data and thoughts of human experience and often cannot be predicted. Walter had already recognized that his great strength lay in conceptual thought, and his self-confidence grew stronger with age, so he feared no competition in this area.

Walter chose astronomy and mathematics as his required minors in the Ph.D. program. The astronomy, which was mainly astrophysics, confirmed the ideas of the uniformity of atomic physics and its laws. Mathematics in Göttingen involved some of the great men of the field. It had been established there by Carl Friedrich Gauss, one of the most remarkable and versatile mathematicians who ever lived. Bernard Riemann, who had developed Riemannian geometry, had been a professor of mathematics in Göttingen. David Hilbert was considered the grand old man of mathematics while Walter was in Göttingen. The mathematician closest to the Franck-Born group of physicists was Richard Courant. Many mathematicians visited Göttingen, among them Norbert Wiener, who Walter met and occasionally encountered after he moved to the United States.

In his 1948 book Wiener generalized the concept of feedback, which had been discovered in neural circuits a century before. In the wake of the rise of computer technology the subject dealt with by Wiener is commonly called systems theory. It is the general mathematical theory of machines that operate in a causal manner. By the time Walter had read Wiener's book, quantum mechanics had led to a broad confidence in the uniformity of nature, and he felt that from there one could attack the central problem in the philosophy of science, the relationship of inorganic science to organic life. In the early days of science, the age of the clockwork universe, Descartes had declared that there were two substances, matter and mind; the body pertained to matter and was a machine pure and simple, an automaton. But to decide whether the body was simply an automaton, one had to know just what machines can and cannot do. This is exactly what modern systems theory claims to do, and Wiener had given the field its greatest impetus. He thus played a major role in framing Walter's approach to the problem of the relationship of inorganic science and organic life.

Another mathematician Walter met in Göttingen was John von Neumann, who later influenced his scientific thought as much as any individual. von Neumann was one of Hilbert's assistants and was charged with keeping Hilbert abreast of new developments in quantum mechanics. Walter only saw von Neumann in lectures and seminars but had regular discussions with him at Princeton between 1946 and 1949 on their common interest in hydrodynamics. He learned a useful lesson from von Neumann's celebrated 1932 book *The Mathematical Foundations of Quantum Mechanics*, namely that any model of organic life based only on the existence of statistical features of physics was likely to be false. Walter studied the book carefully, and it taught him that the introduction of probabilities into physics, which is the most distinctive feature of quantum mechanics, did not "loosen up" the framework of the theory but made it even more deterministic in the mean and more suitable for reducing everything to physics than Newtonian mechanics had ever been. Although this was a valuable lesson for Walter, it deeply dissatisfied him because he distrusted reductionism, especially as applied to organisms. He struggled with von Neumann's book for years, both to absorb its technical details and to explore its philosophical implications. It took some twenty years of this struggle for him to escape the philosophical impasse of the mechanistic nature of quantum mechanics and his distrust of reductionism in organisms. The escape consisted of comparing the infinite sets of symbols underlying mathematical description with the finite sets of observations that experience offers. This later took the form of heterogeneity or individuality among organisms as a key element in setting apart organic life from the inorganic world. More will be said about this in a later section dealing with Walter's theoretical work in biology.

THE TROUBLED YEARS

Shortly after receiving his Ph.D. in 1927, Walter received an unexpected invitation from Paul Ehrenfest, a well-known theoretical physicist, to be his assistant in Leiden, Holland, for a semester and possibly longer, and he accepted. He wondered who had recommended him since he did not know Ehrenfest and concluded that it must have been Oppenheimer. Shortly before leaving for Leiden, Walter received a long letter from Ehrenfest concerned not with physics but with the latter's psychological problems. Walter worried about this strange behavior of explaining the complexities of his soul to a stranger half his age, soon to be his assistant. Ehrenfest met him at the train station in Leiden and immediately took him on a long walk on which he recounted his psychological problems and appeared to be pleading for help. Walter offered to help as much as he could, but as events turned out this was not to be.

Walter settled down in Leiden, enjoying the people, the city, and the countryside. He loved paintings and recognized scenes painted by great Dutch artists. He came to know H. A. Lorentz, the famed mathematical physicist, who brought Ehrenfest to Holland to be his successor. Walter served as Lorentz's assistant at a series of lectures. The lectures were at first utterly strange to him, as they differed radically in style from that of the German university. Lorentz started with general propositions in a pleasantly undulating voice that had a hypnotic effect and after ten minutes or so shifted to a very precise description of an electron as a little charged ball. It then became clear how it moved in an electrical field. Walter drew a parallel between this lecture style and the clair-obscure style of Rembrandt's paintings with their varying shades of brown from which there emerges an intensely illuminated face or object. This visual simile allowed Walter to understand Lorentz's presentation, and he thereafter enjoyed the lectures immensely. Walter later wondered how much of his own thinking originated in those lectures. This involved the thought that a scientist ought to imitate nature in discovering how order can be created out of chaos. He then realized that there were two forms of creation, the first being the creation of raw inorganic matter in the "big bang" or other cosmic device. But anyone who admits that living things are not just automata has to assume that there is an ongoing creative process in organic life that is much closer at hand and more readily studied than the cosmic process.

Walter developed pleasant friendships in Holland, but his relationship with Ehrenfest deteriorated as the latter changed from aggressiveness to downright hostility, for reasons Walter could not understand. One day after Walter had had a haircut with the usual barber's pomade, Ehrenfest came into his office and accused him of wearing perfume. Ehrenfest said he hated perfume, grew furious, and ordered Walter out. A few days later he told Walter to go back to Berlin. It is perhaps not unrelated to this behavior that a few years later Ehrenfest killed his retarded son and committed suicide.

Walter was, of course, very upset by Ehrenfest's rejection and returned to his parents' home then in Berlin. He realized that the entire episode would greatly reduce his chances of employment abroad and that he would most likely end up as a teacher of science and mathematics in a high school. This in itself seemed a reasonable enough career, but given the ever rising tide of antisemitism in Germany he felt he would be spending much of his time defending himself against Nazi hoodlums. His growing alienation from the majority of his fellow citizens was not compensated by a strong positive feeling of being Jewish since he had never been provided with the opportunity to develop such identification. Feeling blocked in, he became severely depressed and lacked the initiative to formulate and carry through scientific research.

Walter's parents were sympathetic and agreed to let him spend another postdoctoral seminar away from home. He decided to study with Wolfgang Pauli in Zurich. Walter attended Pauli's lectures on quantum mechanics, which he found both powerful and elegant. Although he tried to work closely with Pauli, Pauli did not seem very interested. A few years later Pauli told Walter he had seemed so weak and shaken up at the time that he was afraid he would faint if he breathed on him. Many years later in 1958 Pauli read Walter's first book on "philosophical biology" and wrote asking questions, which Walter answered. Later that year they met in Berkeley, California, and had a pleasant visit.

While in Zurich Walter also became acquainted with the famous Swiss mathematician Herman Weyl. He read Weyl's popular book on relativity that was "all the rage" among physicists at the time. At first he was very taken with the book but later realized that despite its exhibition of prodigious mathematical skill it made only tenuous contact with reality. He began to be aware that one cannot grasp reality by a commitment to one technique or procedure. He dimly perceived that anyone who depicted nature in a manner that seemed wholly comprehensible should be approached with the greatest skepticism. Many years later he learned from experience that, as soon as one exhibits skepticism about the comprehensibility of scientific description, many otherwise sound scientists become uncomfortable and freeze into dogma.

At the end of the semester, Walter returned to Berlin, where his friend Fritz Houtermans procured for him a part-time position as an auxiliary assistant in the physics laboratory at the Polytechnic School. His parents continued to provide room and board at their apartment, which gave him the wherewithal to lead at least a limited life of his own. He stayed on for two years in Berlin. One man who impressed him deeply was Max von Laue, the discoverer of

X ray diffraction by crystals. von Laue was very favorably disposed toward Walter, partly because of Walter's paper interpreting the experiments of Davisson and Kunsman as demonstrating the diffraction of de Broglie waves. It was a sign of Walter's psychological difficulties in that period that he could not utilize von Laue's good offices to start over again in theoretical physics, where he had made a small but spectacular start.

While in Leiden Walter had become acquainted with a Russian experimental physicist named Obreimov, who later became director of a new physics research institute in Kharkov, a large industrial town in the Ukraine. Late in 1929 Obreimov asked Walter to come to Kharkov as a technical specialist and he agreed. He was the first non-Russian to be associated with the institute, so it was a challenging undertaking. The challenge of adventure in a foreign land, plus the likely deterioration of the economy in Western countries following the 1929 Wall Street crash, induced Walter to accept. Russia was in the midst of a great famine, but the numerous engineers who had come from the West to work in the factories were supplied with the limited food of a special store. When he tried inviting his Russian colleagues to share in his spare meals they politely but firmly refused. He was impressed with the transformation of society from conditions of bare survival in peasant communities to a more technologically oriented one. He was convinced that the revolution was primarily cultural and educational and only secondarily political and economic. It was a desire for cleanliness, orderliness, elementary decency, and honesty by people who had been confronted with a harsh environment they could not subdue and had sunk lower and lower century after century. He met some of Russia's outstanding physicists and toured to Odessa on the Black Sea with Paul Dirac, who was attending a meeting there. Walter traveled on through the Caucasus and then from Baku to Rostov before returning to Kharkov. He therefore had an opportunity to view a great deal of the countryside and its people. These experiences did his mental state much good, but he soon developed a severe case of infectious hepatitis, which required his return to Germany.

Walter felt that the half-year spent in Russia was the most profound external experience of his life. The 1,600-kilometer displacement to the east introduced him to a new universe, where almost none of the concepts he had grown up with were applicable. It made him aware of the tremendous heterogeneity among people, as contrasted with the uniformity of the behavior of matter that allows us to predict the behavior of atoms and molecules in distant galaxies. It also convinced him that any ideas of the unification of diverse societies are just illusions.

In 1931, after his recovery, he accepted an offer to be an assistant to Professor E. Madelung in Frankfurt. An aunt who lived in Frankfurt told him that it was an ancient mercantile town where Jews had flourished and that he need not fear discrimination. Within six months, however, Walter found that the racist disease had spread there, leaving little hope for a Jew to remain in a university position.

SKETCHES FROM BERLIN

In the six years between obtaining his Ph.D. and leaving Germany, Walter spent about half his time in Berlin. It had become a cultural capital for many groups from eastern and southwestern Europe, and many talented people flocked there. The most memorable was a group of Hungarians, all entirely or partly Jewish though far from the Jewish tradition. Many appear in the history of physics, such as von Neumann, Wigner, Szilard, Orowan, and Polanyi. They met in a weekly seminar at the Kaiser Wilhelm Institute at Dahlem, and Walter frequently took the long subway trip to attend these lively and stimulating discussions. He became well acquainted with Wigner, who introduced group theory as the mathematical tool of choice for atomic physics and later, along with his student Frederick Seitz, provided the quantum mechanical basis for solid-state physics. On one occasion when Walter was "somewhat too easy with my imagination," Wigner counseled him, "One should tackle a problem only when its solution seems trivially easy, it will then turn out to be just at the limits of the manageable; when it appears more difficult, trying to solve it is usually a hopeless undertaking."

Walter also came to know Erwin Schrödinger rather well. Schrödinger was called from his native Austria to Berlin in 1927 as the successor to Max Planck, who introduced the idea of quanta in physics a quarter-century earlier. One thing that drew Walter to Schrödinger was that both were primarily visual types in a field, theoretical physics, where most of the practitioners were passionate musicians. A second factor was Schrödinger's conception of science as a natural philosophy, that is, that science was a continuation of philosophy by other means. Still another basis for common interest was the fact that Schrödinger's great papers of 1925-26 started from de Broglie's insight on the wave nature of matter, which Walter had recognized as the explanation for Davisson and Kunsman's strange results on electron scattering, as later established by Davisson and Germer. Schrödinger wrote a series of thirteen small books, several of which dealt with theoretical physics and others with philosophy. He was a traditionalist who drew his spiritual nourishment from the society in which he grew up. Walter felt that it was Schrödinger's commitment to historical continuity that led as he grew older to doubt, along with Einstein and de Broglie, the statistical interpretation of quantum theory and to suggest that it must ultimately be replaced by a strictly causal theory. Walter could not understand how one acquires these predilections since he felt that "scientific theory consists in finding all the order that can be detected by observations in nature, and in representing it by suitable means, mathematical, logical or other." He felt that the preference for traditional physical causality was patterned after the behavior of machines and was not an innate direction of man's mind. It probably dates from the age of Galileo and Descartes, and since it was born in history it can disappear in history.

Although Walter thought that Schrödinger's books on physics and philosophy were pearls of the art of exposition, he was highly critical of his famous little book *What Is Life*, which influenced many physically trained scientists to turn to biology. Walter thought that the book was a failed attempt to reconcile Schrödinger's humanistic philosophy with biochemistry, now often called molecular biology. While he acknowledged Schrödinger's superior skill in presenting his case, he believed that the basic philosophy of strict rationalism on which the book was based was a throwback to Descartes's seventeenth-century view that the world is formed of two substances, matter and mind-soul. The body belongs to matter and is basically a machine obeying causal laws. Walter thought that it was necessary to replace this paradigm with a better one before any deeper understanding of organisms was likely to occur. Basically, he believed that the dividing line between the inanimate and the living was much more powerful than that between man with his rational soul and brute beasts. He rejected vitalism as a flimsy substitute for a real criticism of the machine nature of organisms. His encounter with Schrödinger, whom he clearly admired, brought out a hitherto somewhat latent passion for natural philosophy, which inspired much of his later life.

THE CONVERSION OF A RATIONALIST

While Walter was still in high school, the family doctor, a distant relative, often referred to him as a "bundle of nerves." Such remarks and others convinced Walter that he was a high-strung individual. This appeared to him only as a deficiency, especially in the considerably brutalized environment of pre-Hitler Germany. During his troubled years after receiving his Ph.D., he became an aficionado of Marcel Proust, reading in succession all twenty-two volumes that made up the original editions of his novel. He felt Proust was an excellent introduction to depth psychology, or exploration of the unconscious, which Walter had been contemplating for some time. He was particularly impressed that Proust had succeeded in putting his oversensitivity to work for him in creating a splendid portrait of a whole society. He began to think of using his own perceived weaknesses for constructive purposes and kept this idea with him for the rest of his life.

When Walter moved to Frankfurt, he entered psychoanalysis with Dr. Karl Landauer, a man of great perception. The analysis continued every weekday for eighteen months, until one day in April 1933 when the Nazis actually carried out their long announced seizure of power. This act marked the death of the Weimar Republic and the beginning of the Nazi terror that was to last for twelve years. When he arrived that morning for his session, Dr. Landauer advised him to leave the country before the Nazis closed the Swiss border. Walter's immediate reaction was an indignant resistance to the suggestion, but when he arrived at the university he was greeted by a gang of brownshirts, each with a rifle slung over his shoulder. Their leader demanded Walter's university identification card, which he then pocketed and told Walter to go home and await further orders. Seeing the handwriting on the wall, Walter left for Zurich a few days later while his passport was still valid and the border still open. He never saw Dr. Landauer again but learned that he perished in the Holocaust.

The full measure of the analytic procedure did not dawn on Walter until after its termination. He then had the phantom feeling that someone was drilling on his innards, though he consciously knew the drilling had stopped. Through his work with Dr. Landauer, Walter learned that he was a far stronger person than he had ever perceived by self-observation. Landauer pointed out that there was a core of aggressiveness submerged in Walter's unconscious that contrasted with the timid young man cowed by Nazi bullying that he fancied himself. If this was correct, Walter realized he had the strength of developing into a scientifically creative person by the standard psychological technique of sublimation.

As a result of his analytic experience, Walter could no more question the reality of the unconscious than that of the electrons or atomic nuclei of his professional work. But the reality of psychic phenomena was of a different kind, characterized by elusiveness, irregularity, disorder, disharmony, or, in Jung's terms, irrationality. He read the literature of the psychology of the unconscious and began to appreciate its depth and breadth. The knowledge of the human unconscious appeared to him a scientific discovery of as great a radical novelty as the discovery of atoms, molecules, and nuclei. Those who spoke of the unconscious before Freud were like those who spoke of gravity before Newton, correct but irrelevant. Newton did not discover gravity but the structure of its laws. Similarly, Freud and Jung did not discover the unconscious but the structure of its modes of behavior.

Such musing filled a gap in Walter's philosophical understanding and even of his science. As a physical scientist it was hard for him to think about the human psyche as purely abstract functionalities apart from anchoring in the material substance of the brain. This led him to an encounter with the ancient philosophical problem of the dualism of body and soul, an interest that never left him. He felt the methods of theoretical science were sufficiently advanced to rescue the central problem of philosophy, the body-soul dualism, from metaphysics in order to treat it as a specifically scientific problem. This would have remained a matter of speculation had it not been for his meeting with the physiologist Theophile Kahn in Paris, about a year after leaving Frankfurt. They met frequently, and Walter was impressed with Kahn's claim that biology was first and foremost the realm of utter complexity. The idea appealed to him because it was simple, abstract, and general and therefore exactly like the notions constructed and used by theoretical physicists. He found no shortage of examples for this complexity in biology from the molecular diversity of cells to the structure of the human body. Later, when he became a leading figure in geophysics, he found that the complexity of neither the turbulence of the fluid earth or the structure of minerals and rocks in the solid earth could compare with the formidable complexity of all higher organisms, nor even that of a single cell.

Walter contrasted the complexity and individuality of organisms with the simplicity and universality of physical science. He felt that these two pairs bring out the difference, otherwise frequently hidden, between the ways of thinking needed in the biological and physical sciences. This realization had an overwhelming intellectual effect on him. He realized that the worlds of physics and biology could be reconciled only if one of the groups yielded and that it is the man who thinks in traditional terms of physics who must change his ways and learn to accommodate complexity and individuality if he is to encounter life and understand its nature. In particular, the notion of creativity that arises in depth psychology, can be converted into scientific terms and applied at all levels of the living world from the cell upward. One must show that the complexity of life is broad enough to encompass the creativity of the organism. The complex structure of the organism, including that of cells, that creates the enchainment of ever-new and unpredictable individualities not only makes creativity possible, it *is* this creativity. Walter was convinced that creativity is a basic property of all life and that the transition from simplicity and universality toward complexity and individuality is essential to the development of a true science of life. As he became successful in his later scientific activity in the United States, he became more committed to these ideas. Their articulation in books and articles led to many clashes with the biomedical establishment.

As these ideas developed, Walter began to understand Born's remark that his strength was more in conceptual thinking than mathematics. This had made no sense to him as long as he believed along extravagantly rationalistic terms that all thinking could ultimately be expressed in mathematical form. But if the utter complexity of organisms at both the structural and logical levels impeded the application of mathematical analysis, then conceptual thought could and indeed had to have a respected role in the hierarchy of human mental endeavors. If biology is the locus of utter and perhaps irreducible complexity, it can be the locus of partial irrationality, that is, of our inability to order the phenomena exhaustively in logico-mathematical schemes. Under these conditions, organic structure serves as the vehicle of this irrationality, and the study of the nature of this vehicle is strictly a scientific task, which was to occupy many of Walter's later years.

RUE PIERRE CURIE, PARIS

Upon arriving in Zurich, Walter went directly to the physics building of the Polytechnic School, where he was heartily greeted by Pauli. Without a spoken word Pauli understood that Walter was the first of many to come, escaping the grim situation in Germany. Fortunately, Pauli had just received a letter from Frederic Joliot, the son-in-law of Madame Curie, saying that the nuclear physicists in Paris needed a theoretical man, and Pauli wrote that he would propose Walter for the position. Joliot wrote back without delay accepting the proposal but indicated that it would take some time to set the necessary bureaucratic machinery in motion. Walter waited two to three months in Zurich for another letter from Joliot and then decided he might as well wait in Paris. There Joliot told him that he was encountering some obstacles to funding a position for Walter, but the matter would be cleared up. Soon thereafter Walter found himself the recipient of a fellowship from the Alliance Israélite Universelle, a Jewish cultural organization. This piece of good fortune actually annoyed him since it was essentially an act of charity, although he had never had any connection with Judaism or its organizations. After a year his support was transferred to the Centre National de la Recherche Scientifique (C.N.R.S.). He was given a small table in the library of the Institut Henri Poincaré from which he had to remove all his books and papers every night. The spare nature of the facilities and the restrictive working conditions were typical of the way scientists were treated in France at that time.

Walter got to know some of the remarkable men who directed French physical science and its instruction. Among these was the Nobel Prize-winning experimental physicist Jean Perrin, who was the first to obtain a precise value of Avogadro's constant, the number of hydrogen atoms in a gram of hydrogen. He also met Louis de Broglie, whose introduction of matter-waves in physics had provided the foundation for Walter's first published paper in Göttingen. Joliot was clearly the moving spirit in the physical sciences, and since he was the one who had brought Walter to Paris, he decided to specialize in the structure and dynamics of atomic nuclei. The transition from his previous research to the application of quantum mechanics to the nucleus was not difficult. During his stay in Paris, Walter got to know Joliot and his wife, Irene Curie, rather well, and he admired them both. He remembered Joliot as a man without affectation, who could deal with every man he encountered and could design experiments of formidable simplicity.

In the fall of 1933 many more scientists found their way from Germany to Paris. Walter was the oldest of these, and since he was on excellent terms with Perrin and Joliot, the new arrivals, if they were physicists, were sent to him. He received great support in helping these people from Jean Langevin, son of the eminent physicist and himself a physicist. Langevin had an "excellent mind and a heart of gold" and put out great effort in trying to find a niche for the refugee physicists in the Greater Paris area. Once a potential position was identified, Walter would escort the man to his destination and, if nothing was available, take him to the Alliance Israélite to keep him from starving.

Walter was intensely aware that his publication record was poor for one aiming to be a professional research scientist. He realized that he had previously had inhibitions about seeing his work in print. Paris provided him with a last chance to make his mark. The two journals available for a physicist were the *Comptes Rendus* and the *Journal de*

Physique. There was no formal scheme for review of papers; they merely had to be submitted through a well-known person. The *Comptes Rendus* limited the length of an article to two and one-half printed pages. The pages of the journal were large and had two columns, which was space enough to clearly formulate any ordinary communication of the theoretical type that Walter would submit. There was also a limit of five of these notes per year for an individual author. Walter took his first note to de Broglie, who was closest to his interests, and asked him to submit it. De Broglie told Walter that he had no desire to read it but might read it later at his leisure when it was in print. He advised Walter to hand his manuscripts to him at the same time each week before the start of the regular academy session. A week later the official printer had the proofs, which Walter immediately corrected. The next issue of the *Comptes Rendus,* which appeared the following week, had the article. Walter tried to publish the limit of five articles for each of the three years he remained in Paris. In addition, he published a total of seven somewhat larger articles in the *Journal de Physique.* All of these were on the structure and dynamics of the atomic nucleus, a subject in which Walter made his second great contribution to physics.

At that time Niels Bohr and his colleagues in Copenhagen had proposed the liquid drop model of the nucleus in which there was proposed to be a homogeneous agglomeration of protons and neutrons without further internal structure to the nucleus. Though there was considerable empirical support for this model, Walter had certain doubts that led him to think the nucleus would have a degree of internal structure. A large part of his efforts in France were devoted to following up on this idea. He discussed the problem of how the nucleus is held together with a refugee physical chemist, K. Guggenheimer, who had found a temporary post in a laboratory at the College de France. Guggenheimer had a great deal of knowledge about how molecules are held together starting from atoms. There are many analogies of the molecular forces with nuclei, but the energies of nuclear binding are 100,000 times greater than those holding molecules together. It became clear though from a study of chemical reaction kinetics that variations in binding energies of nucleons would in many cases be reflected in nuclear abundances, many of which were known. Walter proposed a joint piece of research to Guggenheimer, but they failed to agree on collaboration. Guggenheimer published two articles on binding energies of nucleons in 1935 and then left for a position in England. Walter then found a trick to obtain at least an approximation of the binding energies of individual protons and neutrons from the directly measured disintegration energies of the very heavy, naturally radioactive nuclei. He could then show how the binding energy of a nucleon decreases sharply beyond the end of a nuclear shell. He was satisfied he had established the existence of nuclear shells, though these were not analogous to the electron shells of atoms. Deeper physical understanding of the physical forces that brought about the nuclear shell structures only became available two decades later. A more detailed theory of nuclear structure was then worked out by Hans Jensen and Maria G. Mayer, who had been a Ph.D. student of Born's just after Walter left Göttingen. Both the latter workers received the Nobel Prize in 1963, which they shared with Eugene Wigner in a long-delayed recognition of his many contributions to atomic and nuclear physics. Maria Mayer quoted Walter's earlier work in a 1964 article in *Science*, where she also pointed out that the mathematical theory could not have been complete before the knowledge of nuclear interactions had advanced to the level of the 1950s. Despite the importance and originality of this pioneering work of Walter's, it received little recognition, probably because of the impact of Bohr's liquid drop model, which made its debut a short time earlier.

Walter's remarkable insight into the structure of the atomic nucleus typified other discoveries he made in the physical sciences, some of which will be described later. He would enter a field, read exhaustively about the subject, and then through calculations and reflection develop concepts appropriate for structuring the published observations long before specialists had penetrated deeply into the subject. Once he had confirmed his primary insight, he would, for various reasons, not work through the full ramifications of his ideas. The one great exception was his theory of organisms, which occupied much of the thinking of his last five decades. He had few regrets about the limited recognition of his work in physical sciences, particularly in the matter of nuclear structure, where he would have had to remain a nuclear specialist to follow up on the insights. This ultimately would have involved him in developing the atomic bomb. He of course had no idea of this possibility when he was developing his ideas about nuclear forces since atomic fission was not discovered until 1938, several years after he had left research in nuclear physics. In addition to his work in Paris on nuclear binding, he and Frank Perrin published a theoretical explanation of the exceptionally large capture cross-sections for slow neutrons that certain nuclei had shown. Others published similar analyses at the same time, including Hans Bethe, whose treatment was more extensive and general than those of anyone else. However, Walter was the first to explain the large capture cross-section as the square of the de Broglie wavelength of the neutron rather than the cross-sectional area of the nucleus. The original papers of his French sojourn were hardly noticed, and he turned to other things. He remarked to Eugene Parker that when

Shakespeare wrote Hamlet he had Walter Elsasser in mind. Parker wrote me that, "Walter had a deep aversion to asserting himself in the generally cynical scientific arena."

PASSAGE TO THE NEW WORLD

By 1934 Walter's small position as investigator was fairly secure, since it was financed by the budding French Science Center, and likely to last almost indefinitely. He then realized that he had to make a decision about his career. If he decided to remain in France, he would have to become a French citizen, which required two years of military service. If not a citizen, he would have to fight endlessly with a bureaucracy determined to keep him out. The alternative was to go to the United States. Since this was the time of a worldwide depression, it was hard to get a job, even in science. Having a stable position in Paris made it difficult to decide for the American option. His decision to do so was finally determined by his parents' situation. His father in his early sixties was ill and had retired and moved to a small house in the Black Forest. By that time, however, the Nazi policies had become clear: in spite of the depression, they were going to rearm, and the property of the non-Aryans was going to finance at least part of the rearmament. That category included not only nominal Jews but those who had married into the middle and upper classes and changed their religion. His parents would be reduced to indigents, deprived of home, income, and normal medical care. A few years later in early 1939 Walter's foresight was justified, when his parents were able to go to England for temporary residence pending their passage to the United States. In 1940 they came to America, where they later lived with his sister in Chicago.

Walter applied for an immigration visa to the United States and after the usual bureaucratic delays received it in 1935.

He then took a large German steamer from Le Havre to New York. The ambiance on board was his last closeup of the subdued nazification he encountered. On board, however, he met Margaret Trahey, an American girl of Scotch-Irish extraction, who was later to be his wife. She was reading C. P. Snow's *The Search*, about the rise of a young man from modest circumstances to academic research as a physicist. Since Walter was the first of that strange breed she had met, it helped smooth the way to a further understanding of each other.

Walter always remembered vividly his first sighting of the Statue of Liberty when the steamer entered New York harbor. He felt that no one who had not undergone years of persecution could fully appreciate what this means to an immigrant—the chance to begin life over again without the impediments that Europe throws in the way of anyone not born of a privileged social class. Although free immigration had stopped when he arrived in the United States, the country still accommodated itself to the reception of persecuted minorities. In his mind it was the spirit that counted: comparable conditions for entry existed in South America but little that corresponded to the reception of the downtrodden masses at the feet of the Statue of Liberty.

Two days after disembarkation Walter met I. I. Rabi of Columbia University, whom he knew from the latter's stay in Europe. Rabi set him up in a dormitory room on the Columbia campus and introduced him to Harold Urey. He also visited Davisson at the old Bell Telephone Laboratories, where the latter had first demonstrated electron diffraction. Walter was alert to the possibility of obtaining a position in all his meetings, but there was none to be had. He traveled on to Ann Arbor to attend a summer course by Enrico Fermi whom he came to know personally to some degree. Although Fermi was perhaps the most scientifically gifted of all the physicists Walter had encountered, he was simple and modest in his personal contacts.

During his month-long stay in Ann Arbor, Walter frequently visited with a young theoretical physicist, David Inglis, whom he had first known in Pauli's institute in Zurich. Inglis and his wife Betty introduced him to many of the nuances of American life that are not explained in guidebooks. If in France he was impressed by the sensitivity of the residents, he was impressed by the generosity of Americans. Unlike Europeans, the prospect of an uncertain economic future does not breed cynicism or despair among Americans.

Walter went on to Chicago, where he met Arthur Compton, discoverer of the Compton effect, the most spectacular demonstration that light exhibits corpuscular in addition to the well-known wave properties of light. Compton was most unsympathetic to Walter's quest for a position because he thought it unethical to give positions to Europeans when many American scientists were out of work. He had in fact sent back to Germany a young German physicist, although the latter had married an American and wished to stay in the United States. Walter knew this young man from his Frankfurt days. He was of frail constitution and died soon after being drafted into the German army from the exertions of a forced march. Thereafter Compton was always the first and most vociferous among those who helped displaced scholars from Europe.

After Chicago Walter returned to New York to take the boat back to Europe for another year in Paris. His trip had shown him that the prospects for nuclear research in the United States were slim, and he began to think seriously about another specialty. Since many of the ideas described above under "Conversion of a Rationalist" had already developed in embryonic form, he began to think that much of the science of the future would be concerned with complex systems. Although the ideal place for such study would be biology, Walter was reluctant to commit himself to that field because it involved too much chemistry, to say nothing about the details of biology. He was later to approach biology by way of the semiphilosophical ideas described earlier. It occurred to him that in geophysics he could indulge the study of complex systems, using the tools of the theoretical physicist and some knowledge of astronomy, which had been a minor subject along with mathematics in his Ph.D. examination. He decided to cut loose from his moorings in Paris and on his next trip to the United States try his luck in California, specifically at CalTech. He wrote to Oppenheimer about his intention to go to Pasadena, where Oppenheimer spent a few months in the spring of every year as a part-time professor of physics. Oppenheimer answered promptly, suggesting that Walter first visit his wealthy father on his way through New York and then stop off at his ranch in New Mexico. Walter followed up on both invitations in 1936 after he arrived in the United States for the second time.

In a long, friendly conversation with Oppenheimer's father in New York, Walter found the man to be warm and generous. After a few days exploring the "marvelous and complex city" of New York, Walter moved on to Chicago, where he visited Margaret and they announced their engagement to her family. They also decided they could manage between their two small incomes. A year later they married and had a girl and five years later a boy. He derived the deepest happiness of his life from the seventeen years of their marriage, which ended with the loss of Margaret under extremely tragic circumstances. He did not remarry until ten years after the tragedy.

After several weeks in Chicago, Walter took the westward train headed for Albuquerque and Los Angeles and stopped off at a small station some distance from Santa Fe. He was picked up by Oppenheimer and driven to his ranch in the Sangre de Cristo mountains, where he could see through the trees the mountain range in which the Los Alamos Laboratory was later to rise. Included in his stay on the ranch was a daylong horseback trip with Oppenheimer into the surrounding mountains, which was exhilarating but tiring to one unaccustomed to riding. On their return Oppenheimer offered him a Chili drink he had prepared according to an old recipe. A mouthful of the vermilion fluid tasted like what Walter imagined sulfuric acid would taste, and he immediately spit it out. Oppenheimer told him he had passed the first but failed the second part of the initiation ceremony at the ranch.

After a few days of relaxation on the ranch, Walter took the train to Los Angeles and then on to Pasadena. There he met a physicist, Jesse DuMond, who introduced him to several people at CalTech, including Linus Pauling, who, though a young man, was already head of the chemistry department. Pauling almost at once offered him a position in his laboratory, but feeling he had no background in chemistry Walter refused. He considered this one of the wiser decisions he made in his life.

In 1936 DuMond made an appointment for Walter to see Robert Millikan, the head of CalTech, whom he was to see many times thereafter. He told Millikan that he was a theoretician who had worked in atomic and nuclear physics but was willing to work in other areas such as geophysics. Millikan told him, "If you want to do geophysics we can use you. If you want a place in nuclear physics or in astrophysics I can do absolutely nothing for you. . . . If you are serious about geophysics I can promise you that I will find some salary for you." Walter considered Millikan one of the most extraordinary men he had ever met. He was, of course, famous for the oil-drop experiment of his younger years, which established the unit charge of the electron. His forte was not original thought but subtle and controlled strength in everything he did—a human powerhouse. Among his many accomplishments was to build CalTech into a major scientific institution. He once told Walter, "You are a producer; if you were put on the top of a mountain you would still produce." Considering the eminence of the source, Walter thought of this as the highest compliment he ever received about his qualifications as a scientist.

Margaret came west in the spring of 1937 to find a job. She did find a tolerable but rather low-paying job, and they got married. Walter decided to make his break with nuclear physics gradually, so he made the acquaintance of Charles Lauritsen and his capable group of research students who represented nuclear physics at CalTech. Their work was at the forefront of studies on nuclear structure and dynamics then going on in the world. Oppenheimer was the official theoretician for the group, but he only spent two or three months each spring in Pasadena. Since Walter knew quite a bit about the subject, it was easy for him to converse with the group, which he did in the summer and fall of 1936. In return, men of the Lauritsen group alternated in teaching him one skill that was essential for a full life in the western United States, namely driving a car. Early in 1938 he and Margaret bought their first automobile, a somewhat aged Ford, in which they made many excursions. By the time Oppenheimer arrived in the spring of 1937, Walter had become deeply engrossed in the physics of the atmosphere, which seemed a weird subject to nuclear physicists, including Oppenheimer. As Walter had stopped reading the burgeoning literature of nuclear physics, he eventually lost touch with the congenial crowd of Lauritsen's laboratory.

EARTH SCIENCE

In 1937 Walter joined the fairly new Meteorology Department at CalTech at Millikan's suggestion. Millikan had gotten a contract from the Department of Agriculture to be administered by the Weather Bureau, which was then part of Agriculture. Walter knew nothing about meteorology, but he read a few textbooks and came up with some unsolved problems in which he could use his accumulated knowledge of atomic physics. He then began to receive a small salary to supplant the loans on which he had been existing up to that point.

He undertook the study of the effect of infrared radiation on the atmosphere. The American Meteorological Society had for years been urging the American Physical Society to do research on infrared radiation in view of its importance for the atmosphere, but nothing was done. The far infrared spectra of the atmosphere have a very complicated structure that required an understanding of quantum mechanics for its full elucidation. The basic material on infrared spectroscopy had been gathered and presented by the physicist Gerhard Herzberg, another refugee from Germany then living in Canada. Walter spent the years 1937-41 analyzing the properties of far-infrared atmospheric radiation from first principles. After lengthy calculations, he ended up with tables and graphs that a practitioner could use to find the heating and cooling of the atmosphere when he knew the distribution of temperature and moisture.

This was a time of rest and recovery for Walter, despite the pinched material circumstances under which he lived. The hot breath of Hitler was off his neck for the first time in twenty years. Extreme brutalities did not exist in the California environment, and he had a congenial wife. He had plenty of time to think, which allowed him to develop the two basic ideas that dominated the scientific ideas of his later life: those that led to the theory of the earth's magnetic field and the relation between quantum mechanics and organic life on the background sketched out above under "Conversion of a Rationalist."

After he had done the atmospheric research for a few years with emphasis on calculations and drafting, Walter felt frustrated at his previous inability to do experiments. To correct this deficiency, he befriended John Strong, a very successful experimentalist. Strong taught him the procedures used by a successful experimenter, including careful advance planning and breaking down the procedure into a number of little steps, mastering each before proceeding to the next. Walter then set out to measure the far-infrared transmission of the atmosphere along paths up to 300 meters in length. This was done on an athletic field next to CalTech with equipment he built with his own hands. He monitored the temperature and moisture continuously and wrote up the results. Although he realized he would never be a first-rate experimentalist, the experience restored his self-confidence and made him realize that the American ambiance had a relaxing and stabilizing influence on him.

In 1936, before he was officially employed, Walter decided to take a meteorology course. Almost all of the twenty some students in the course were career military officers sent to Pasadena for training as weather officers. They were well aware that in a few years their new skills might be required for a war against Hitler. Much of the course was practical plotting of weather characteristics and upper-air data. Walter learned much about the peculiarities and vagaries of the earth's atmosphere, which proved useful in his later work on the earth's magnetism. He learned that the earth's atmosphere is full of unpredictable contingencies on every level of its scale, which gives the meteorologist a feeling for reality that is radically different from that of the laboratory scientist who can minimize contingencies.

The Meteorology Department at CalTech was part of Aeronautical Engineering headed by an outstanding man, Theodore von Karman, who had come to Pasadena from Europe in 1930 after a brilliant academic career. He tried to induce Walter to leave his meteorological studies and turn to hydrodynamic problems such as turbulence and thereby become an aeronautical theorist. As a result of Walter's refusal, he never got closer to von Karman. He also began to see that von Karman had only accepted meteorology into his laboratory as an accommodation to Millikan, who believed it was his patriotic duty to foster that subject. Walter saw that the smallness of his position was the result of a protracted political accommodation between two powerful men, and he began looking for other employment. His efforts were unsuccessful because positions were still rare in the wake of the great depression, and he had no money to travel around looking for opportunities. He asked Oppenheimer about a job on one of the campuses of the University of California, particularly the Los Angeles campus, but was told, "We do not wish to encourage graduate teaching outside of Berkeley."

Walter's position at CalTech came to a rather ludicrous end in 1941. He had gone to Washington on some business connected with his research. While there he encountered Carl Rossby, who was head of research for the Weather Bureau. Walter had gotten to know him well in the summer of 1938 when Rossby was head of the Department of Meteorology at MIT and had invited him to lecture there. Rossby controlled scientific meteorology in the United States, a task he found overwhelming, and decided to delegate control of the western part of the country to a deputy, that deputy being Walter. Walter thought this was some sort of fantasy that would never work out when confronted with reality but said nothing. However, von Karman got wind of this plan from Rossby, who told him that the Meteorology Department at CalTech was inadequate, since its head, a young assistant professor, was more commercially than scientifically inclined. He said that there was already an excellent man on the spot, Elsasser, and that he had already discussed with Elsasser his future functions. To emphasize his point, he declared that the contract with the Meteorology Department would not be renewed. Von Karman reported to Millikan that Elsasser had used the federal government to pressure CalTech into an appointment for himself.

When von Karman informed Walter of the situation, he saw no sense in asserting his innocence. He walked out of von Karman's office and considered himself fired. It occurred to him that the logical course of action was to move east, where, with the war coming on, there were greater opportunities. He wrote to Charles Brooks, professor of meteorology at Harvard, who was in charge of the Blue Hill Observatory in South Boston. Brooks agreed to give Walter a position compiling tables of various data with spare remuneration. He and Margaret packed their few belongings in the car and drove off to Boston. While there he wrote a monograph on atmospheric radiation, which Brooks published in a series he edited. Rossby, who by then was suffering from a bad conscience, had most of the edition bought up and distributed to meteorology students.

Walter started thinking seriously about the source of the earth's magnetism around 1940. Three months after the Japanese attacked Pearl Harbor, he received a telegram from the U.S. Signal Corps Laboratories at Fort Monmouth, New Jersey, telling him to promptly report there for duty. He spent the first part of the wartime years there and the second part working in the Empire State Building in New York for the Radio Propagation Committee of the National Defense Research Council. The chief part of his theoretical work on the earth's magnetism was done during the war. He had plenty of time on the weekends to work on this problem, especially toward the end of the war. He used this time to do calculations and write articles that appeared in a series of three papers in the *Physical Review* in 1946-47. The first systematic summary of the physics of the earth's interior was given in an article in the *Reviews of Modern Physics* in 1950.

Full exploration of the earth's body had begun in the 1890s, when the first seismological stations were set up. In the decades following, the main features of the earth's interior came into view. The most important of these was the sharp boundary about halfway between the surface and the center of the earth separating the core from the mantle. The core is made of molten iron, the mantle of conventional rock. There is by now a thorough quantitative knowledge of all the mechanical properties of the earth's interior. The one characteristic of the earth as a whole that was not included in this scheme was magnetism, a property studied scientifically since 1600. Early thinking was that the earth behaves like a bar magnet, magnetized along the earth's axis. But mysterious deviations were later discovered in the regularity of the earth's magnetic field. These irregularities occurred not only from place to place but from time to time, leading physicists to speak of the secular variation in the earth's magnetic field. In the first half of the nineteenth century, the famous Göttingen mathematician, Gauss, showed that if one knows the magnetic field over all of the earth's surface the field can be mathematically divided into a part whose sources are inside the earth and a part whose sources are outside. The overwhelming part of the field arises from sources within the earth. Toward the end of the century, it became clear that the internal source could not be of the bar magnet type, since the temperatures inside the earth are too high for this sort of magnetism to occur.

Two competing theories arose to replace the idea of a bar magnet. The first, espoused by a prominent group of theoretical physicists with Albert Einstein at the head, believed that a theory must be created in which large bodies such as the earth are magnetic by the very fact of their rotation. This involved finding a new term in Maxwell's equations to provide a connection between gravity fields and electromagnetic fields. The alternative theory was that electric currents flow in the conducting molten iron of the outer core. This idea had its origin in the observation in 1908 by the astronomer George Hale, founder of the Mount Wilson Observatory in Pasadena, that all sunspots have large magnetic fields. The British physicist J. J. Larmor attributed the sun's magnetism to a dynamo effect in analogy to the engineering term for power station machines that convert mechanical motion to electrical currents, the conventional rotating generators. Although Larmor vaguely suggested in 1919 that the earth's magnetism might be explained along these lines, not a single article appeared about it in the intervening years. The likely explanation for the total absence of papers on the subject is that the limited number of those qualified to handle the solution of problems of theoretical physics was absorbed by the two main streams of inquiry then existing in physics, namely relativity and quantum mechanics.

Walter's work was truly pioneering because there was no theoretical literature in the field. The main problem was to find a mechanism to sustain the electric currents for billions of years. The decisive step was the discovery that there could and should be a toroidal field inside the earth. Walter spent a lengthy period making calculations on the basis of different models and found that only the dynamo model gave an adequate numerical magnitude. Reassuringly for the dynamo model, the calculations came out just right. He then adapted a two-step strategy to resolve the problem. The first step was to assume a constant main magnetic field and calculate how it would be modified by eddy-type motions in the core. Then he constructed the mathematical machinery that could describe the field as it was generated by currents inside the core. Using his meteorological knowledge about the dynamics of the atmosphere, he derived rules about the character of the fluid motions needed to produce the field. He thus showed how a metallic fluid dynamo can be self-sustaining indefinitely as a result of ordinary electromagnetic induction. This involved mathematical demonstration of how the kinetic energy of fluid motion in the earth's core can be converted to electromagnetic energy. The key element is a feedback process in which energy is exchanged between two components of the magnetic field, that extending to the earth's surface and beyond (the poloidal field) and that contained within the core (the toroidal field). The convective motion of the fluid is the agent in the exchange process.

Walter was greatly aided in this work by magnetic maps consisting of lines of constant rates of secular or long-term variation, prepared under the direction of E. H. Vestine. These closely resembled ordinary weather maps. From these maps Walter calculated the average speed of fluid iron in the core, which was 0.03 cm/second. He also derived an important formula for predicting the strength of magnetic fields in celestial objects. By balancing Coriolis and Lorentz forces acting on the conducting fluid, he concluded that the earth's dynamo is characterized by a particular value of a dimensionless parameter that includes the magnetic field strength, the electrical conductivity of the core, and the rotation rate of the earth. This parameter is now called the Elsasser number and is the critical parameter in modern dynamo theories.

After the war, Walter renewed his acquaintance with John von Neumann, then at the Institute for Advanced Study in Princeton, who had gotten interested in the problems of magnetohydrodynamics during his work at Los Alamos. Von Neumann listened with interest but, like everyone else, refused to believe that magnetic fields could be created by fluid motions alone. Over many visits in the course of a year Walter convinced him that his arguments were mathematically unassailable. Stimulated by remarks of von Neumann, a distinguished British hydrodynamicist, K. G. Batchelor, carried out calculations showing that when an electrically conducting fluid is in random turbulent motion, a random stray magnetic field that happens to exist in the field will always be amplified by random shears engendered by the turbulence. When Batchelor's work appeared in 1950, the objections to dynamo models disappeared, and in a few years the dynamo idea was fully accepted, as indicated by Walter's election to membership in the National Academy of Sciences in 1957. This gave him a strong sense of personal achievement since his was mainly an individual accomplishment. It is perhaps of more than passing interest that this work was done in his spare time and had no direct connection to his war work. A similar pattern will be seen in his later biological work.

When the concept of plate tectonics revolutionized the geological sciences in the 1960s, Elsasser turned his attention to the question of the driving force for plate motions. One of his prime contributions during this period was an analysis of how stress diffuses across tectonic plates. This work explained the phenomenon of postseismic deformation observed in seismically active plate boundary regions such as the San Andreas fault zone following large earthquakes. As in other areas of physical science, Walter's central contributions to plate tectonics continue to influence modern thinking to this day.

WORLD WAR II AND THE POSTWAR YEARS

While Walter was working at the Blue Hill Observatory and before he was ordered to Fort Monmouth, he completed his monograph on infrared atmospheric radiation. When he received the orders to move, Charles Brooks, director of the observatory, offered to read the proofs, so Walter, his wife, and six-month-old daughter could depart for Fort Monmouth. There he was given a civil service classification as a meteorologist and a salary corresponding to his age, education, and experience, which lifted him to a comfortable level considerably higher than the marginal one he had before. He soon found, however, that the Signal Corps no longer had much interest in meteorology, since the subject had been taken over by the Air Corps. He was told he was to become an electronics specialist, thus ending his career as a meteorologist. Walter protested, mainly against the military way of doing things, but was told this was war and he had to obey orders.

He was assigned to the quartz crystal division, one of the largest and busiest branches of the Signal Corps Laboratories. He found himself in charge of a small group of younger people. Individualist that he was he did not relish the bureaucratic functions the job entailed. This resulted in an awkward situation one day when he was surprised while holding a soldering iron in his hand by an inspector from the Civil Service who severely rebuked him for stooping to such manual efforts, when the government could hire people to do it for half what he was being paid. Nonetheless, he rapidly learned electronics in this work and carried out technical supervision and inspection of industries that built quartz crystals into radio transmitters.

About a year later, an emergency arose that required the help of men versed in meteorology. There had been trouble with anomalous propagation, in which radio waves are bent by moisture in the air. This interferes with the ability of radar to measure distances of targets and causes artillery that is guided by radar to be directed to a wrong distance. Walter wrestled with the problem for some time without success, when he was told it was to be transferred to the Radio Wave Propagation Committee of the National Defense Research Committee. He was then released to serve with the committee, which had set up an office in one of the upper floors of the Empire State Building in New York City. He spent the rest of the war there with a half dozen other technical people. During this time he wrote a simply worded pamphlet for technical personnel explaining the meteorological origins of anomalous propagation, how to recognize it, and how to make the best use of radar sets that one could under such conditions. It was illustrated by a very capable artist, was printed in tens of thousands of copies, and was distributed to the technical branches of the Allied armed services. This constituted Walter's basic contribution to the war effort.

After the end of the war, his office was disbanded. He had lost his connections with atomic physicists many years before and had no desire to return to meteorology. Through a contact he had made on the Radio Wave Propagation Committee, Walter obtained a position in the RCA laboratories at Princeton testing antennas of new design. Since this did not appeal to him, he managed to get himself transferred to a section supervised by Dwight O. North, a theoretical physicist. The group was studying the properties of solids used in electronic solid-state devices. Walter found a very congenial group among his collaborators but decided it was too late in life for him to switch to industry.

After two years at RCA, he found a position as associate professor of physics at the University of Pennsylvania in Philadelphia. He taught a graduate course in mechanics that later became his mainstay in teaching. He wrote a review for physicists, "The Interior of the Earth and Geomagnetism," in 1950 that made him known to his American colleagues. He had been warned by friends that there was an ingrained political constellation in Philadelphia that would make an extended stay unpleasant. He found this to be true, so when he received an offer from the University of Utah's physics department to join the faculty and develop a graduate program he was definitely interested. Oppenheimer, who was then director of the Institute for Advanced Study in Princeton, told him that the University of Utah had a very high scholastic level judging from the excellent students he got from there when he was still in Berkeley. Considering Oppenheimer's critical acumen, this seemed a very high recommendation and Walter accepted the offer in 1950. At Utah he gradually wound up teaching a comprehensive set of courses in theoretical physics, with the exception of quantum mechanics, which was taught by the most distinguished member of the faculty, the theoretical chemist Henry Eyring.

In an earlier discussion with John von Neumann, Walter had expressed the desire for a small inexpensive computer. He was told that the first such machine had just come out. The Air Force wanted such machines tested by competent academic people, so they bought one and loaned it to him in 1951, indifferent about the scientific problem he proposed to solve. Walter became intimately familiar with the design of the machine since it had been hastily constructed and the diodes frequently burned out and had to be replaced. As a result he was unable to carry out any lengthy calculation, but he did teach himself Boolean algebra, the formal logic of computing machines. This experience with early electronic computers gave Walter a sense of their capacities and limits, which was to prove useful in comparing them with living organisms. He gained first-hand knowledge of what was called cybernetics, now called systems theory, and he gave some thought to using the computer as a model of the brain. He decided to write a book in which the first two chapters were a survey of the basic ideas and techniques of cybernetics, followed by more philosophical notions. This book, *The Physical Foundation of Biology* (1958), was the first of four books he wrote in biology.

In 1955 after he wrote the first draft of the book, Walter decided to get an opinion about it from the university's biomedical community. When he asked several members of that community whom they recommended to read the book, they all named a young professor of biophysics who had an interest in theory. This man took it home and brought it back a few days later. He said, "I have read this. It is thought provoking, in fact extremely thought provoking—but so far as I am concerned, I do not think, I observe."

This was Walter's first encounter with a kind of mentality he found widespread among biologists who, living among the most gigantic accumulation of data the world had ever seen, proclaimed that salvation lay in more data. This was, so to speak, a shattering incident for Walter since it denied science as a creative activity. He had always considered scientific research in which observation was related by a reciprocal interaction with thought; observational results tended to modify thought, which in turn engendered suggestions for more observations until significant knowledge had come close to its boundaries. This dynamic process embodied the advance in understanding in physical science, but it seemed that in the life sciences the prevailing approach, the philosophy as it were, had assumed a quite different form. This discrepancy became increasingly clear in Walter's later years and sharpened his desire to pursue the philosophical interpretation of biological matters.

Walter decided early during his tenure in Salt Lake City that he had risen as high as he could in an academic career short of moving to a more prestigious university. To do so would require that he specialize in only one of the several scientific fields in which he had become competent, in order to remain on top in one of the fields. Instead, he decided to remain a generalist or, as he preferred to call it, a "natural philosopher." This decision was reinforced by the tragic loss in 1954 of his beloved wife Margaret. Since he did not want to become a purely speculative philosopher, he invented a technique in which he divided his time into periods of several weeks to several months in which he concentrated on only one of the subjects in which he was interested. He found periods of this size long enough to permit strong concentration and continued the scheme for the rest of his life.

The loss of his wife left Walter alone with their children. He had developed cordial relations with his neighbors, who were mainly Mormons, and his children felt at home in the neighborhood. He had become fond of the Mormons with their simple way of life combined with their appreciation of higher education, but he did not want to see himself or his children dissolved in the Mormon collectivity. Therefore, he accepted an offer from Roger Revelle to become the first professor at the newly developing University of California at San Diego, which was actually located in La Jolla.

Not long after he arrived in 1956 at La Jolla, the university committed itself to develop the San Diego branch into a major center of graduate study equivalent to that of the

Berkeley and Los Angeles campuses. A building to accommodate the nascent physics department was completed, and a number of highly competent young physicists were recruited. Walter felt that these men were completely steeped in technology and did not share his philosophical interests. He decided he did not fit into such a department, and Revelle created a niche for him at the Scripps Institution of Oceanography to do research in geophysics.

However, about this time Walter heard from Harry Hess, chairman of the Geology Department at Princeton, with an offer of a professorship in geophysics. Hess was the individual among geologists who above all took Alfred Wegener's theory of continental drift seriously. Walter's interest in geodynamics therefore fit with Hess's interest. The Geology Department was in one wing of a building built early in the century when evolution excited the academic world, with much of the research centered on paleontology. The Geology Department was in one wing of the building and biology in the other, and the libraries for both were in one large common room in the center. These seemed ideal conditions for Walter to continue his dual existence in geophysics and the philosophy of biology, and he accepted the offer in 1962.

Moving to Princeton was relatively easy for Walter since his children were now grown. Two years after his arrival he married again, this time to a cousin, Susanne, whom he had known from childhood. Although he met several distinguished biologists during his five years at Princeton, he found that his philosophical probings into the foundations of biology made them uncomfortable, as might be expected for one who aims to solve a specific question and does not want to be distracted from it. He concluded that biology had always been like what physics had become only late in its existence, during his lifetime. It was only by stripping off the practical implications and complexities that physicists as natural philosophers could develop the methods that led to the major experimental discoveries and the great unifying mathematical schemes that constitute the grand edifice of modern physical science. The situation in biology was far more difficult. There was no grand edifice, but there was evidence of a general unity of pattern in organic nature. Walter felt that the situation in our age is uniquely propitious for developing a basic theory of organisms because there was, for the first time, an altogether coherent abstract scheme, quantum mechanics, for representing the physical basis of life. It needed only the assumption that biology is the realm of the utterly complex to realize that the simple black and white world of mathematics might no longer apply, and the whole conceptual system of scientific analysis might have to be reconstructed. This task was to occupy most of the rest of Walter's life. In 1967 he left Princeton for a research professorship at the University of Maryland, which relieved him of all teaching obligations. While he lost the prestige of Princeton, he gained the freedom to do whatever research he pleased. In the summer of 1974, having reached his seventieth birthday, he was duly retired from the University of Maryland. A few months later he received an offer of a suitable postretirement affiliation as adjunct professor in the Department of Earth and Planetary Sciences at Johns Hopkins University, which entailed no formal obligations. There he wrote the memoirs that form the backbone of this article up to this point.

BIOLOGICAL WORK

Walter's specific interest in biology as a scientific discipline began in Paris in the early 1930s as a result of his discussions with Theophile Kahn, but his particular approach to the problems of biology grew out of his earlier experience with depth psychology in Frankfurt. He felt there was a fundamental distinction between the living and nonliving states, but it had to be one that did not violate quantum mechanics. Von Neumann's 1932 book *Mathematical Foundations of Quantum Mechanics* demonstrated that all ensemble averages of physical quantities obey differential equations of a simple kind; that is, their change in time is causally determined. Since Walter was convinced that a distinguishing characteristic of organisms was that their long-term behavior was not causally determined, he set himself the task of finding a way around von Neumann's completeness proof and found it in the concept that the members of any biological class are heterogeneous; that is, they share some but not all characteristics, while the members within any physical class such as electrons, photons, atoms, and molecules are rigorously identical to one another. The heterogeneity of biological classes arises from the unfathomable complexity of living things. His aim was to construct a formal scientific logic that is suitable for organisms. His first publication explicitly in biology was in 1951, although he lists a 1937 paper on quantum measurements and generalized complementarity among his biological publications. Altogether there were about thirty published papers in biology through 1984 plus at least as many drafts of papers extending through 1989, which did not get published but are in the Elsasser collection in the Milton Eisenhower Library at Johns Hopkins. Seven of the latter were written in 1988 and 1989, indicative of his continued interest and activity in the area. Walter also wrote four books on theoretical biology, the first appearing in 1958 and the last in 1987.

I first became aware of Walter's biological thought in the early 1960s through his first book and his articles in the *Journal of Theoretical Biology*. The book, *The Physical Foundation of Biology*, was heavy going for an experimental biologist, for the first half dealt with unfamiliar physical feedback control systems and information theory while the second half was more abstract and philosophical than most biologists are accustomed to. Some twenty years went by during which I became increasingly aware from my daily work that conventional ideas of causality were inadequate to deal with the often unpredictable behavior of cells in culture. One day in 1981 upon reading Volume 1 of Leslie Foulds's classic book on cancer, *Neoplastic Development,* I came upon a brief account of Walter's biological thought that seemed to anticipate the types of problem I was encountering. Rather than go back to his earlier papers, I wrote a letter to Walter asking about further development of his concepts, half expecting my request to be consigned to the wastebasket. To my delight he answered promptly in a friendly manner that seemed to invite further correspondence. That began an intense intellectual exchange that lasted through the 1980s and grew to some 500 pages. As it began, Walter was writing a draft of his last book, *Reflections on a Theory of Organisms* (1987), which he sent me in stages. The manuscript went through many revisions and several privately printed editions, paid for out of Walter's pocket before he found a one-woman publishing company in Canada to bring the book out. Through all this time he was unbelievably generous with his time, writing extensive expositions of his biological thought and general philosophy, often in response to naive questions of a pragmatically trained biologist.

While at Princeton Walter wrote a second book, *Atom and Organism* (1965). After it appeared, he decided he had fallen under the sway of the establishment. This made him feel as though he had been flying with clipped wings, a situation he was determined to correct. The third book, *The Chief Abstractions of Biology* (1975), was written while he was at the University of Maryland, where he was free to do whatever research he chose. It formalizes many of the thoughts of the earlier books in a few abstract concepts. The fourth and final book is a distillation and refinement of the concepts formulated in the previous books. The following summarizes some of the main ideas expressed in his biological *oeuvre.*

All of Walter's biological writings refer to the immense complexity of the organism based on the number and types of atoms in a cell and the number of possible bonds connecting the atoms in organic molecules. Complexity is taken as an intrinsic aspect of the living state. Another important consideration is the near reversibility of most biochemical reactions as expressed in a well-known textbook by Conant and Blatt.[1] "Biochemical reactions liberate or absorb small amounts of energy. . . . Apparently the necessity for reversible reactions with relatively small energy changes is characteristic of biochemical reactions." Walter recognized that the heat of biochemical reactions is close to that of thermal noise and therefore almost the direct opposite in its properties for maintaining and transmitting information from those of computing machinery, which maximizes the difference between signal and noise. The input by noise in biology is therefore removed from empirical control, and adequate results can no longer be obtained by a purely mechanistic model. The closeness of energy exchange in biochemical reactions to thermal noise is necessary for the decision-making ability of the organism, allowing it to choose between available states without need for more than a minimal supply of energy. This condition contributes to what Walter called the fragility of the living state, defined as the capacity of the system to respond with large-amplitude changes to small perturbations. In that sense fragility may also be characteristic of the processes involved in development and differentiation of the organism. At the same time, the maintenance of information is so powerful that many species do not change their species-specific characteristics for millions of generations.

These thoughts led Walter to formulate a holistic set of principles to represent the living state. These principles are not scientific laws in the usual sense since they are not derivable from the mathematics of quantum mechanics. They define that which is in the form of regularities but not determined by atomic and molecular physics. The basic assumption in his holistic interpretation is that "an organism [or a cell] is a source (or sometimes a sink) of causal chains which cannot be traced beyond a terminal point because they are lost in the unfathomable complexity of the organism [or cell]." The basic principles of organisms as listed in his 1987 book are the following:

1. **The first principle is ordered heterogeneity.** Combinatorial analysis shows that the number of structural arrangements of atoms in a cell is immense; that is, much greater that 10^{100}, a number that is itself much larger than the number of elementary particles in the universe (10^{80}). But biology shows us there is regularity in the large where there is heterogeneity in the small, hence order above heterogeneity. This concept of ordered heterogeneity was first introduced by the molecular biologist Rollin Hotchkiss, systematized by the embryologist Paul Weiss, but given quantitative definition and set in a general theory by Walter.

2. **The second principle is creative selection.** A choice is made in nature among the immense number of possible patterns inferred in the first principle. The availability of such a choice is considered the basic and irreplaceable criterion of holistic or nonmechanistic biology. The term "creative" refers to phenomena that, like everything in biology, are compatible with the laws of physics but are not uniquely

determined by them. No mechanism can be specified by whose operation those selected differ from those not selected. He points out that the number of different patterns is also immense in the physical science of statistical mechanics, but in that case the variation of structure from pattern to pattern averages out. The patterns of inorganic systems repeat themselves over and over again ad infinitum, while those of each organism are unique. The selection of a relatively small number of organisms from the immense number of possibilities allowed by quantum mechanics is a primary expression of biological order and is the scientific counterpart of the term “creativity” used in ordinary language.

3. **The third principle is holistic memory.** It provides the criterion for choice not expressed in the second principle. That criterion is information stability. The term “memory” in a generalized sense indicates stability of information in time or, as in the case of heredity, the reproduction of information in an empirical sense, that is, without our knowing the full mechanism of reproduction. The creative selection of the second principle means the organism has many more states to choose from than are actually needed. The third principle says the organism uses this freedom to create a pattern that resembles earlier patterns. Walter borrowed the term “memory without storage” from the philosopher Henri Bergson, who was considering the memory function of the brain in his book *Matter and Memory*. Walter considered holistic memory an epistemological innovation that was the touchstone of his theoretical scheme but realized that it might seem like black magic to many of his readers. However, he noted that the concept is free from internal contradiction while it obviously runs counter to habitual thought. In that formal sense it is no different from the concept of the antipodes, which would have been

inconceivable before Newton since the people in Australia should have fallen off the earth. Memory without storage is considered as transmission of morphological features through time without a material memory device, just as relativity is based on the transmission of signals through space without a material carrier.

4. Holistic memory requires a **fourth principle, operative symbolism**, to indicate that a material carrier of information is needed, namely DNA, but this acts as a releaser or operative symbol for the capacity of the whole organism to reconstruct a complete message that characterizes the adult of the next generation. Walter was sketchy and superficial about the fourth principle and considered it in the nature of a specific detail. In other words, operative symbolism is not necessary to the development of the postulational system of the first three principles that can do away with the conceptual difficulties and internal contradictions that always appear in any purely mechanistic interpretation of organic life. The informational system of organisms is therefore postulated to be dualistic; on one level it is mechanistic in the operation of the genetic code; on the other level it is holistic, involving the entire cell or organism.

Walter's epistemological revision for the life sciences has been ignored by most biologists and attacked by some. The cool and sometimes downright hostile response of the biologists is probably related to the challenge presented to the basic preconceptions, often subconscious, that underlie their present modus operandi. The most pervasive of these preconceptions is that biology is ultimately an extension of physics and chemistry and can be studied in an analogous manner. Walter's theoretical innovations require a novel experimental approach that is just beginning to take shape to deal with the holistic aspects of cell and organismic behavior. Despite the difficulties, his thought has found strong support from a few outstanding biologists such as Leslie Foulds and Paul Weiss. It has also met with approval from some notables among theoretical physicists, including Pauli and Wigner, and from the information theorist L. Brillouin. Perhaps his strongest support has come from Frederick Seitz, a student of Wigner's in the early 1930s and a founder of modern solid-state physics. Seitz spent a decade as president of the Rockefeller University, where he was in continuous contact with many of the most creatively active individuals in molecular and cell biology and was impressed with their ingenuity. However, he was struck by the comparative rigidity of their molecular concepts and their enormous confidence (or overconfidence) that reductionism would lead to an understanding of all aspects of living systems. Flying in the face of these attitudes was the fact that the picture of such systems that was evolving at the molecular level was becoming ever more complex with each new major phase of development. Seitz felt that the outlook of the molecular biologists was somewhat reminiscent of the attitude of some nineteenth-century physicists who believed that the universe was a gigantic clockwork governed by the laws of classical physics. Ironically, Seitz's own work provides the theoretical foundation for the currently fashionable field of structural biology. While musing on the situation in biology he came upon Walter's work, which he considered a "profound analysis of the status of biological systems in the physical world." He felt that the biological community had "to a substantial degree lost sight of the forest for the trees and presumably will continue to do so until it is forced to reexamine its own foundations either through the appearance of obvious paradoxes or because it becomes enmeshed in unresolvable complexity—or both." When that time comes, he is "certain that the profoundness of Walter's work will be appreciated and will form a significant part of the cornerstone of understanding of living systems by the biological community."

Walter's work has already formed the cornerstone of my own understanding of living systems through its effect on my day-to-day work with cells in culture. A major feature of the behavior of cells dissociated from the organism and from one another is their radical heterogeneity in a large variety of behavioral and physico-chemical properties. This was anticipated in Walter's principle of ordered heterogeneity but appeared experimentally at the cellular level rather than the molecular level which most concerned him. Another feature of these cells in Walter's terms is their fragility, so they change their growth behavior in a striking and enduring fashion in response to small physiological differences in their environment. These responses are foreshadowed in Walter's principle of creative selection, which I modified to progressive state selection to image cellular behavior. Paradoxically, the behavior of some cells, depending on their initial state, is extremely stable, so that both fragility and stability are subsumed in the same system, as full consideration of Walter's theory would suggest. This goes along with his insight that there are no "yes/no" or purely arithmetic answers in the behavior of living systems. All depends on the initial state of the cells and the perturbations to which they are subjected. On a personal note, his philosophical analysis liberated me from the reductionist strictures that dominate biological thought and allowed me to acknowledge and organize the actual behavior of cells as seen every day before my own eyes rather than sweep the frequently inconvenient behavior under the rug. There is no doubt in my mind that Walter was correct in the evaluation he left with his own collected papers in the Johns Hopkins library that, although he was best known for his work in geophysics, his controversial ideas in theoretical biology were what historians would want to study. I believe his ideas will play a central role in the future development of biology.

WALTER ELSASSER'S *Memoirs of a Physicist in the Atomic Age* was the major source of information used here in describing his life up to 1974. His sister, Maria Lindberg, and Eugene Parker of the University of Chicago provided some personal insights. Peter Olson of Johns Hopkins provided a description of Walter's work on geomagnetism and plate tectonics. Frederick Seitz, formerly president of Rockefeller University, contributed his thoughts on Walter's biological work. Most of the section on that work was derived from Walter's published biological writings and from his extensive correspondence with me between 1981 and 1991. My wife, Dorothy Rubin, helped in every phase of preparing this memoir.

NOTE

1. *The Chemistry of Organic Compounds,* 2nd ed., Ch. 20. New York: MacMillan, 1947.

HONORS

1932 Research Prize of the German Physical Society
1957 Member, National Academy of Sciences
1971 John A. Fleming Medal, American Geophysical Union
1972 Fellow, American Academy of Arts and Sciences
1977 Gauss Medal, Braunschweig, Germany, (200th Anniversary of Gauss's birth)
1979 Penrose Medal (USA)
1987 National Medal of Science

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