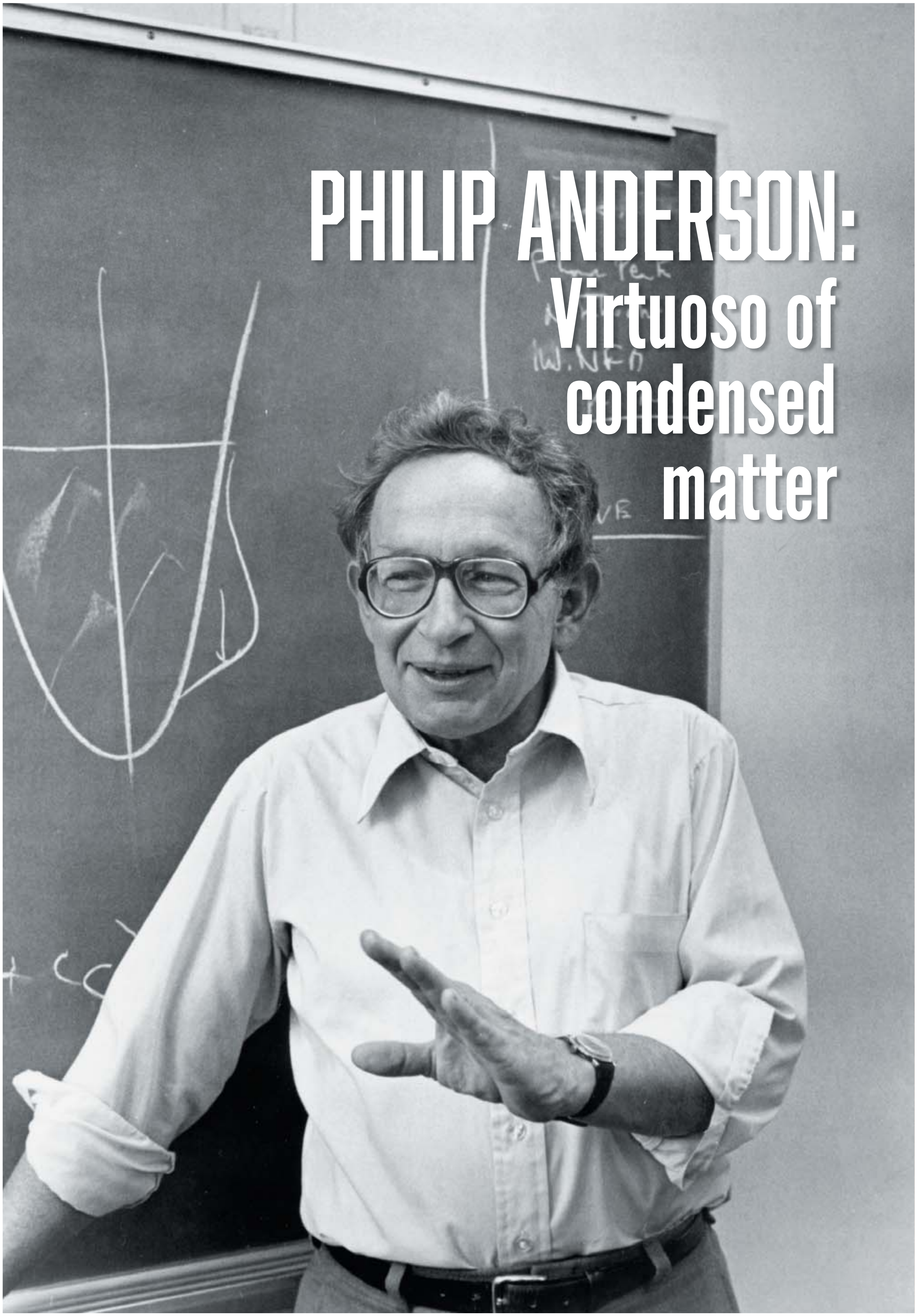
Philip Anderson: Virtuoso of condensed matter

Andrew Zangwill

Citation: Physics Today **75**, 3, 28 (2022); doi: 10.1063/PT.3.4960
View online: https://doi.org/10.1063/PT.3.4960
View Table of Contents: https://physicstoday.scitation.org/toc/pto/75/3
Published by the American Institute of Physics

PHILIP ANDERSON: Virtuoso of condensed matter



Philip Anderson in October 1977. (Courtesy of the AIP Emilio Segrè Visual Archives, PHYSICS TODAY Collection.)

Andrew Zangwill is a professor of physics at Georgia Tech in Atlanta. This article is based on his book *A Mind Over Matter: Philip Anderson and the Physics of the Very Many*, published by Oxford University Press in 2021.

Andrew Zangwill

The theorist's work on disordered and magnetic solids earned him a Nobel Prize, but it was his profound influence on the condensed-matter community—and well beyond—that set him apart.

Philip Warren Anderson (1923–2020) was one of the most accomplished and important physicists of the second half of the 20th century. Over a 50-year career at Bell Labs, Cambridge University, and Princeton University, he demonstrated superb taste, profound intuition, and remarkable creativity in the effort to understand the way nature works.

More than any other person, Anderson helped combine many-body physics with the patchwork of topics once called solid-state physics into the intellectually coherent field known today as condensed-matter physics. In his 1984 monograph *Basic Notions of Condensed Matter Physics*, he argued that the construction and application of model Hamiltonians was a far better way to understand a system of 10^{23} particles than solving the many-body Schrödinger equation. Textbooks of condensed-matter physics written in the past few decades show that his view has prevailed.

The late Nobel laureate Pierre-Gilles de Gennes greatly admired Anderson and once described him as "the pope of solid-state physics."[1] The nickname is apt because Anderson tried to establish doctrine for his subject. The faithful paid close attention to his every utterance, and many made special efforts to seek his views and approval. By his own reckoning, Anderson was a rebel, a curmudgeon, and a person with an insatiable curiosity about why things in nature behave the way they do. In this article I survey Anderson's life and science with an eye toward understanding his enormous impact.[2]

Son of the heartland

Anderson's ancestors on both sides of his family fought against the British in the American Revolutionary War. Later generations of those Scottish and Irish immigrants established farmsteads in the rich soil of western Indiana. Farming did not appeal to everyone, and Anderson's maternal grandfather and uncle enjoyed long careers teaching Latin, mathematics, and English at Wabash College in Crawfordsville, Indiana. A similar attitude led his father and paternal uncle to become plant pathologists. Anderson grew up in the Urbana–Champaign area because his father was a professor at the University of Illinois. Frequent visits back to Crawfordsville kept him in close touch with his family, shown in figure 1, and with the traditional Hoosier character traits of pugnacity, skepticism, patriotism, and sensitivity.

In high school, Anderson excelled in both academics and athletics—track, tennis, and speed skating. He acted in the school play every year, wrote and read the senior class history at commencement, and participated in the biology and chess clubs. His senior yearbook photograph was labeled *The Importance of Being Earnest*, after the title of an Oscar Wilde play.

During those years, Anderson often accompanied his father and a group of University of Illinois faculty members, known as the Saturday Hikers, on outings that featured hiking, swimming, softball, and left-wing political talk. The latter instilled in the boy what became a lifelong commitment to social justice. One Saturday Hiker, F. Wheeler Loomis, chaired the university's physics department, and his recommendation helped Anderson win a scholarship to attend college at Harvard University.

The US entered World War II when Anderson was a sophomore. Eager to contribute to the national effort, he switched from physics to an accelerated degree program in electronics physics created by Harvard specifically to prepare students for war work. After graduation he served for two years as a microwave engineer at the US Naval Research Laboratory in Washington, DC. That experience convinced him that his talents lay in theoretical physics. When the war ended, Anderson returned to his alma mater to pursue a PhD. He felt that Harvard still owed him a proper physics education because his electronics-physics courses never mentioned quantum theory.

Like Anderson, many wartime college graduates had gone into war work or military service. Peacetime thus brought a pent-up supply of applicants to graduate programs. As a result, a large group of theoretically minded graduate students arrived at Harvard at the same time that Anderson did. Eleven of them chose to work in nuclear physics with the university's newly hired superstar Julian Schwinger. For his part, Anderson exhibited a contrarianism that would become familiar to later observers when he found a reason to dislike nuclear physics. Instead, he worked with department chair John Van Vleck and did the first fully quantum mechanical calculations of the microwave absorption spectrum of small molecules.

Anderson graduated in January 1949 with a PhD thesis that is still widely cited today. Job hunting was difficult because interviewers showed little interest in a person trained in molecular physics; they were looking for experts in nuclear physics. He had already accepted his only offer—at an academic institution with no graduate program—when Van Vleck arranged an interview at Bell Labs. A few weeks later, Anderson began working at Bell as a theoretical physicist in William Shockley's solid-state-physics group. At the time it was the only group in the US devoted to the subject.

FIGURE 1. A FAMILY PORTRAIT. Philip Anderson stands front and center in 1934 at age 10, with his immediate family and some of his Crawfordsville, Indiana, relatives. Directly behind Anderson is his mother, Elsie. His sister, Eleanor Grace, stands at the far left. His father, Harry, stands third from the left. (Courtesy of Susan Anderson.)

Bell Labs

For 50 years in the middle of the 20th century, Bell Labs was arguably the greatest R&D organization in the world. Anderson benefited greatly by being there, and the labs benefited greatly from his presence. In his first few months, he consumed Frederick Seitz's 1940 monograph *The Modern Theory of Solids*, confirmed a speculation of Shockley's about the origin of ferroelectricity in the ceramic oxide barium titanate, and conducted a journal club discussion of a paper in which Linus Pauling proposed what he called a resonating valence bond approach to metallic cohesion.

Like many before him, Anderson soon grew frustrated with Shockley's imperious manner. He turned for guidance to three other outstanding Bell Labs theorists: Gregory Wannier, Conyers Herring, and Charles Kittel, all shown in figure 2. Wannier taught him to love statistical mechanics. Herring taught him solid-state physics and shared his encyclopedic knowledge of the literature. Kittel taught him magnetism and specifically proposed that Anderson work on antiferromagnetism, a topic that was newly accessible experimentally by using magnetic neutron scattering.

In January 1952 Anderson submitted to the *Physical Review* an approximate quantum theory of antiferromagnetism.[3] The paper is significant historically because it includes the first discussion of spontaneous symmetry breaking, the phenomenon whereby a system adopts one particular configuration from among a set of degenerate and symmetry-connected configurations, despite the invariance of the system's Hamiltonian to that symmetry. Among other things, Anderson discussed what is today called a Goldstone mode in connection with the collective rotation of the direction of the spins of an antiferromagnet. It took a decade before any other physicist took special note of Anderson's ideas about symmetry breaking.

An encounter with the Japanese theorist Ryogo Kubo led to an invitation for Anderson to attend what was the first International Conference of Theoretical Physics in Tokyo and then to spend six months visiting Kubo's research group. Bell Labs gave Anderson an unpaid leave of absence—the Fulbright Foundation paid his salary—and he, his wife Joyce, and his daughter Susan arrived in Japan in September 1953 (see figure 3).

At the conference, Anderson spoke up in a half-dozen sessions and discovered that he could talk comfortably with such senior, first-rank theorists as Felix Bloch, Lars Onsager, and Nevill Mott. Afterward, the positive reaction Anderson got from Kubo and other young Japanese theorists to a lecture series he presented on contemporary magnetism boosted his confidence even more. He realized on the trip home that he was no longer a neophyte solid-state physicist. He felt secure in his abilities, confident in his scientific taste, and certain that he could strike out independently as a theoretical physicist.

Most of Anderson's single-authored papers from his first 15 years at Bell Labs combined intuitive arguments with detailed analytic calculations. Examples include his incorporation of Coulomb effects into a self-consistent treatment of the Bardeen-Cooper-Schrieffer model of superconductivity and two papers cited by the Nobel Committee for Physics of the Royal Swedish Academy of Sciences when it awarded Anderson a share of the 1977 Nobel Prize in Physics.

The Nobel committee drew attention to Anderson's discovery that a propagating wave can be trapped and localized by a disordered medium.[4] Perplexing spin resonance data obtained from doped silicon crystals by his Bell Labs colleague George Feher led Anderson to construct and analyze a simple model for the motion of electrons in a spatially disordered lattice. He guessed and then proved that such disorder could suppress quantum mechanical tunneling enough to localize otherwise freely propagating electron wavefunctions. Like spontaneous symmetry breaking, disorder-induced wave trapping—now called Anderson localization—was not appreciated (or even believed) by many of his colleagues until well after the paper appeared.

The Nobel committee also cited Anderson's analysis of the persistence (or not) of a magnetic moment when an atom with unpaired spins is immersed in a nonmagnetic host metal.[5] He tackled that problem after spending weeks studying pertinent data obtained by another of his Bell Labs colleagues, Bernd Matthias. The paper Anderson wrote on magnetic moments is one of the best written of all his scientific publications. He summarizes the experimental situation, discusses previous theory on the subject, develops a model Hamiltonian, gives a qualitative discussion of special cases, performs a Hartree–Fock analysis, extracts the important conclusions, and points out the limitations of his approximations.

Anderson enjoyed talking to experimenters, and he was eager to learn the technical details of their work. He took the time to understand their motivations and laboratory strategies, and he relished grappling with the raw data himself. In a 1999 oral history interview with the American Institute of Physics (publisher of PHYSICS TODAY), he went so far as to characterize himself as "six tenths theorist and four tenths experimentalist," despite never having performed an experiment himself.

Cambridge

Anderson spent a sabbatical year (1961–62) at the University of Cambridge. He published only one minor paper there, but his influence led directly to Nobel prizes for two other physicists. The first went to Brian Josephson, who learned about symmetry breaking from a graduate class Anderson taught. Outside of class, Josephson and Anderson spent hours discussing the meaning of the phase of the macroscopic superconducting wavefunction. Less than a year later, Josephson published the short paper in which he predicted the DC and AC effects that today bear his name (see the article by Anderson, PHYSICS TODAY, November 1970, page 23). For that work, he earned a share of the 1973 Nobel Prize in Physics.

Anderson played a similarly important role when the Nobel committee awarded a share of its 2013 physics prize to Peter Higgs (see PHYSICS TODAY, December 2013, page 10). Anderson had learned at daily tea with Cambridge particle physicists that existing gauge field theories failed to produce a mass for the carriers of the weak nuclear force. In a flash of insight, he realized that with a suitable change of variables, his earlier analysis of Coulomb effects in the Bardeen-Cooper-Schrieffer model for superconductivity was relevant to the elementary particle's mass. In 1963 Anderson wrote a *Physical Review* article aimed at particle physicists outlining his idea,[6] and Higgs realized that a relativistic version of Anderson's discussion was all that was needed.

The sabbatical year confirmed a long-standing Anglophilia in Anderson and Joyce. Anderson thus was happy to accept a job offer from Mott, chair of physics at Cambridge and a longtime champion of Anderson localization, for a half-time professorship in the department's solid-state theory group. Bell Labs reduced Anderson's commitment to half time as well. From 1967 to 1975, that schedule allowed him to teach and supervise research students at Cambridge from October to March.

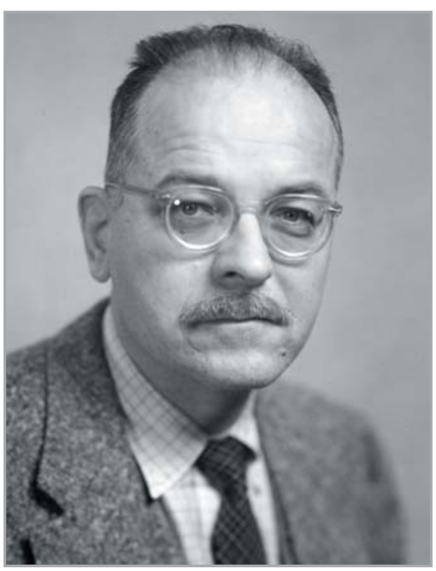


FIGURE 2. ANDERSON'S MENTORS at Bell Labs (from left): Gregory Wannier, Conyers Herring, and Charles Kittel. (Wannier portrait courtesy of the AIP Emilio Segrè Visual Archives, PHYSICS TODAY Collection; Herring and Kittel portraits courtesy of the AIP Emilio Segrè Visual Archives.)

Some of the issues he addressed during that period are listed in figure 4.

A particular triumph of Anderson's involved what's known as the Kondo effect. To explain that phenomenon, the task of theory was to characterize the ground-state spin configuration for a class of magnetic alloys in which the electrical resistance showed a minimum as the temperature decreased toward zero. That task turned out to be the most challenging many-electron problem of the 1960s. Anderson presented his final solution in 1970—first, in a difficult and equation-rich paper written with two junior collaborators and then in a masterful and elegant single-author paper.[7] In both, one finds the invention of the renormalization group method a full year before Kenneth Wilson's magisterial formulation of that technique in its full generality.

A few years later, Anderson and the distinguished Welsh physicist Sam Edwards invented a model to describe the magnetic behavior of an exotic class of metal alloys called spin glasses.[8] Their solution for the ground-state configuration of spins was approximate, but attempts to do better soon revealed a huge problem. The simultaneous presence of disorder and conflicting constraints implied that the number of computations required to obtain a solution increased exponentially with the number of spins in the system.

The same computational problem occurs when one tries to solve the celebrated traveling-salesperson problem. Notwithstanding the difficulty, the Edwards–Anderson model has enjoyed steady popularity over many years. That happened because, with a change of variables, the model applies to a host of nonphysics problems, such as airplane scheduling, mail delivery, pattern recognition, integrated circuit wiring, and message encoding.

More is different

In 1972 Anderson published an article called "More is different: Broken symmetry and the nature of the hierarchical structure of science."[9] Its purpose was to rebut an often-stated claim by some high-energy physicists that their research into the physics of the very small was somehow more fundamental than the research conducted by solid-state physicists into the physics of the very many. That fundamentality argument had been used for decades to enhance high-energy physicists' prestige and to justify the large claim they made on government funds to plan, build, and maintain the large particle accelerators needed for their work.

Anderson accepted the reductionist view that all things seen in nature must be consistent with the known properties of elementary particles. What he denied was the claim that the behavior of complex many-particle systems could somehow be derived from the rules of particle physics. To the contrary, considerations no less fundamental than those used by particle physicists are required to discover laws and properties present at, say, the micron scale. That's because, like symmetry breaking, those laws and properties emerge for reasons that are not at all apparent if one's analysis begins at the nanometer scale.

Wetness is an example. That property of a liquid would be quite unfathomable and would never be predicted by someone familiar with only the properties of individual molecules and their interactions. One must experience wetness to be able to formulate a language to understand it.

Anderson's emergence arguments in "More is different" resonated not only with condensed-matter physicists and chemists but also with physiologists, ecologists, and other

FIGURE 3. CHATTING OVER TEA in Japan in January 1954 are, from left, Susan Anderson, Masao Kotani, Philip Anderson, Ryogo Kubo, Takahiko Yamanouchi, and Joyce Anderson. (Courtesy of Hiroto Kono and the Kubo family.)

"macroscopic" biologists who felt marginalized by molecular biologists who claimed a unique fundamentality for their own work. Other people responded to Anderson's statement in the article suggesting that when the size of a system becomes large enough, one should stop thinking about decreasing symmetry and start thinking about increasing complexity.

A decade later Anderson and a small group of scientists launched the Sante Fe Institute, a think tank dedicated to the study of complex systems. There, ideas about complexity dovetailed with developments in nonlinear dynamics and found fertile ground among experts in fields as diverse as economics, neuroscience, computer science, and operations research.

Princeton

In 1975 Anderson swapped his half-professorship at Cambridge for a half-professorship at Princeton. As was the case at Cambridge, Anderson was often disorganized as a lecturer, but the classes he taught to advanced students permitted him to hone the ideas that, nearly a decade later, formed the basis for his book *Basic Notions of Condensed Matter Physics.* The publication of that grand synthesis coincided with his retirement from Bell Labs and the expansion of his professorship from half-time to full-time.

Anderson's research style at Princeton remained what it had always been: Engage deeply with experimental data; look for "anomalies," cases where experiment and current theory do not agree; and construct a model Hamiltonian—90% of the task, Anderson said—to explicate the physics. His remarkable intuition often told him the answer he was seeking. But he relied increasingly on others to supply the supporting mathematics. That was the case when he recruited three colleagues and prodded them to construct a scaling theory of disorder-induced wave localization.[10]

The final results of the so-called Gang of Four collaboration elegantly reproduced Anderson's previous wave-localization results in three dimensions and extended them to one and two dimensions. An avalanche of work on localization by others ensued (see the articles by Ad Lagendijk, Bart van Tiggelen, and Diederik Wiersma and by Alain Aspect and Massimo Inguscio, PHYSICS TODAY, August 2009, pages 24 and 30, respectively).

It was not easy to be an Anderson research student because he rarely provided guidance about how to proceed with calculations. More than a few students have characterized his supervision of their PhD theses as "oracular." They left meetings with him having no idea what he was trying to communicate, only to realize weeks or months later what he had meant. Many senior physicists had the same problem, a situation summarized by the Russian theorist Anatoly Larkin when he said, "God speaks to us through Phil Anderson. The only mystery is why He chose a vessel that is so difficult to understand."[11]

Superconducting Super Collider

In 1970 Anderson learned from a panel at an American Physical Society meeting that financial commitments needed to build the National Accelerator Laboratory (later Fermilab) might disrupt funding for "small science" projects across the country. He responded with an article in *New Scientist* magazine that was critical of Big Science as practiced by the high-energy physics community.[12] Years later he reiterated those views when he assumed the role of the most outstanding public opponent of the Superconducting Super Collider (SSC), a giant machine being built by the US to test the standard model of particle physics.

On 4 August 1993, Anderson and the theoretical physicist Steven Weinberg, a principal architect of the standard model, testified back-to-back at a congressional hearing about the project. Weinberg defended the SSC on the grounds of fundamentality. Anderson argued that the truth or falsity of the standard model did not justify the cost of the SSC if the funds needed to maintain its operation diverted funds from projects in other scientific fields, where equally important questions—many with more practical import—remained to be answered. Although a great deal of money had already been spent, Congress pulled the plug on the SSC two months later.

Historians of science have concluded that testimony by scientists played almost no role in the decision to discontinue the SSC. Ever-increasing cost estimates, poor project management, and political expediency were the main reasons for its demise (see the article by Michael Riordan, PHYSICS TODAY, October 2016, page 48). Nevertheless, to this day, some people blame Anderson for the debacle.

High-temperature superconductivity

In 1986 the world of condensed-matter physics was turned upside down by the discovery of superconductivity at unprecedentedly high temperatures in a class of ceramic copper oxides. Anderson had long been fascinated by superconductivity, and he was the first theoretical physicist to discuss the new superconductors in print.[13] The paper was groundbreaking because it dismissed the relevance of the electron–phonon interaction—the well-understood mechanism for superconductivity in conventional metals and alloys—in the new materials and instead emphasized the short-range Coulomb repulsion between electrons.

Anderson's paper suggested that the oxide superconductors were best studied using a Hamiltonian introduced years earlier by John Hubbard as a model for ferromagnetism. An aside: Anderson often claimed invention of the Hubbard model for himself, which is almost true. An exact solution of the Hubbard model was (and remains) unknown, so he outlined a guess for the ground-state many-body wavefunction that was related to the resonating valence bond state that Linus Pauling had studied 40 years earlier (see the Reference Frame by Anderson, PHYSICS TODAY, April 2008, page 8).

At the 1987 March Meeting of the American Physical Society, Anderson was the first theorist to speak at the famous all-night "Woodstock of physics" session devoted to high-temperature superconductivity. He was also the only theorist to sit on the dais at a news conference the next morning to discuss the issue. Other theorists had different ideas about the new superconductors, and a 20-year period began during which Anderson was unable to convince the majority of his colleagues to accept his views. The fact that his ideas kept changing—mostly in response to new experimental results—did not help.

Anderson was fiercely competitive as a physicist. He had a good relationship with almost all experimenters, but he could be quite abrasive in the heat of debate with other theorists. Unfortunately, he became possessive about the theory of high-temperature superconductivity (even as his ideas changed), and he dismissed the work of other theorists as wrongheaded or worse.

A handful of people responded in kind, and the field began to resemble a combat sport. For that reason, more than a few young people declined to enter the field. Today, with the rancor of the early years long past, no single theory can account for all the behavior seen in the oxide superconductors. Probably the only universally accepted idea is one that Anderson fully embraced: Subtle many-body physics lays at the heart of the matter.

A man in full

Later in life, Anderson became interested in reaching audiences beyond the physics community. He did so by publishing essays and book reviews in journals, magazines, and newspapers. Topics he discussed include arms control, complexity, religion, science politics, futurology, the culture wars, and the meaning of science.[14] He engaged philosophers of science by reckoning that the structure of science was more like a highly interconnected web than an evolutionary tree or a pyramid.[15] A provocative 1994 essay he wrote for the British newspaper *Daily Telegraph* offered "four facts everyone ought to know about science." Anderson identified those as: science is not democratic, computers will not replace scientists, statistics are sometimes misused and often misunderstood, and good science has aesthetic qualities.[16]

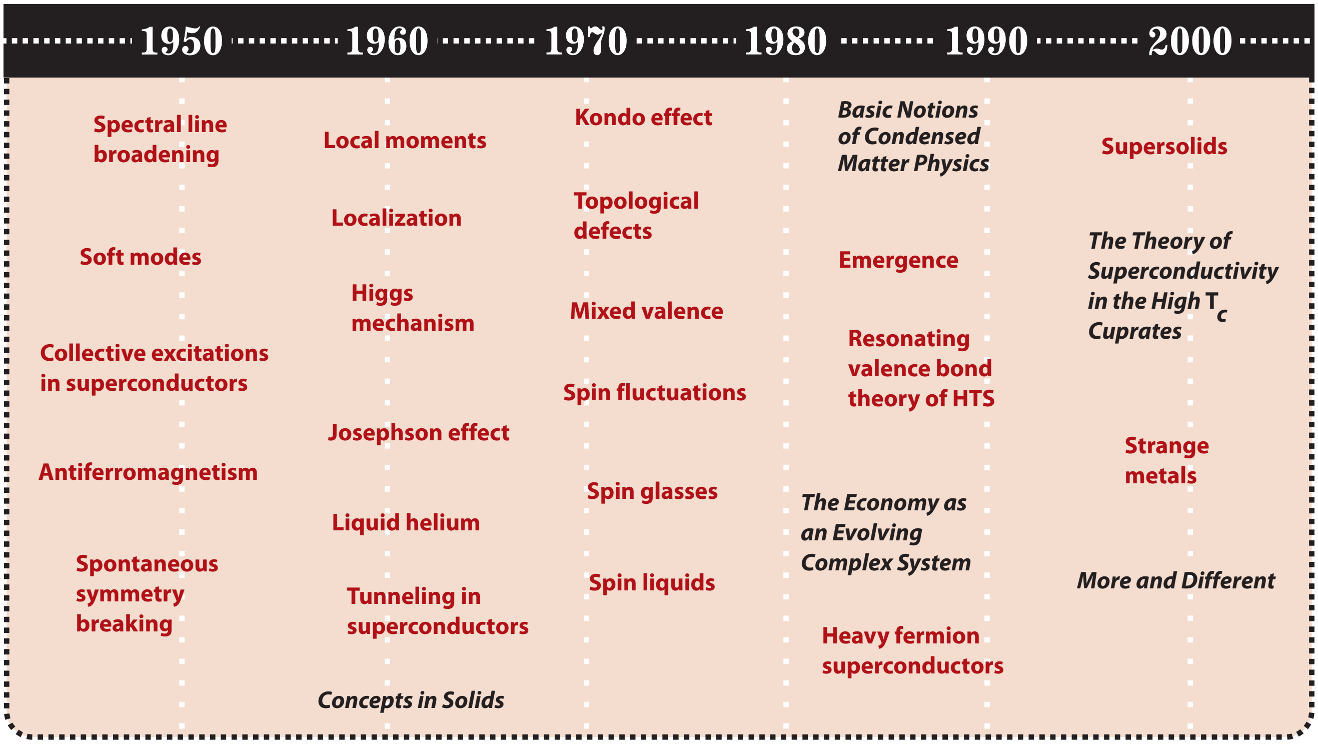


FIGURE 4. TIMELINE of some of Philip Anderson's research activities. The list is not comprehensive. Book titles are in italics. The acronym HTS stands for high-temperature superconductivity.

The second "fact" reflects Anderson's peculiar attitude about the use of computers in theoretical physics. On the one hand, he admired the computational work of personal friends, such as William McMillan and Volker Heine. On the other hand, much more than most scientists of his generation, he quite unfairly identified the least creative practitioners as typical of the field. That tendency led him, for example, to disparage numerical calculations of the electronic structure of matter without bothering to familiarize himself with the state of the subject. It is ironic, then, that some of the greatest progress in understanding the origins of high-temperature superconductivity in recent years has come from extensive computer simulations of the Hubbard model and its variants.

Anderson was a lover of knowledge, rationality, culture, and nature. Outside of physics, his main passions were hiking, politics, gardening, the game of Go, and Romanesque architecture. His close friends knew him to be warm, generous, and loyal—particularly to those in need. On more than one occasion, he made it possible for a struggling former student or postdoc to spend time at Princeton so he could help as they got their lives in order. He was witty and a charming storyteller, but not a joke teller. Several years after receiving the Nobel Prize, he used an assumed name and wore big black glasses and a fake moustache to present a poster at a conference where 10% of the presented talks included the words "Anderson model" in their titles.

Anderson's wife and life partner, Joyce, played an essential role in his professional success. Particularly during the full-time Bell Labs years, she provided discipline and structure and worked hard to ensure that he behaved in the manner expected of a rising star in the organization. As a former English major, she later made a point of editing all his nontechnical writing for clarity and precision. Over more than 70 years of marriage, Anderson rarely remained in the office after 5:00pm because he knew his wife was waiting for him at home.

Philip Anderson was one of the brightest stars in the firmament of theoretical physics for half a century. Bell Labs launched and sustained him for many years, but he only rarely involved himself with applied problems. Nevertheless, his conceptual formulations profoundly influenced a broad swath of the physics world. Future historians will count him as one of the world's greatest scientists.

REFERENCES

1. L. Plévert, *Pierre-Gilles de Gennes: A Life in Science*, World Scientific (2011), p. 287.
2. A. Zangwill, *A Mind Over Matter: Philip Anderson and the Physics of the Very Many*, Oxford U. Press (2021).
3. P. W. Anderson, *Phys. Rev.* **86**, 694 (1952).
4. P. W. Anderson, *Phys. Rev.* **109**, 1492 (1958).
5. P. W. Anderson, *Phys. Rev.* **124**, 41 (1961).
6. P. W. Anderson, *Phys. Rev.* **130**, 439 (1963).
7. P. W. Anderson, G. Yuval, D. R. Hamann, *Phys. Rev. B* **1**, 4464 (1970); P. W. Anderson, *J. Phys. C: Solid State Phys.* **3**, 2436 (1970).
8. S. F. Edwards, P. W. Anderson, *J. Phys. F: Met. Phys.* **5**, 965 (1975).
9. P. W. Anderson, *Science* **177**, 393 (1972).
10. E. Abrahams et al., *Phys. Rev. Lett.* **42**, 673 (1979).
11. Ref. 2, p. 139.
12. P. W. Anderson, *New Sci.* **51**, 510 (1971).
13. P. W. Anderson, *Science* **235**, 1196 (1987).
14. For essays on these topics, see P. W. Anderson, *More and Different: Notes from a Thoughtful Curmudgeon*, World Scientific (2011).
15. P. W. Anderson, *Stud. Hist. Philos. Mod. Phys.* **32**, 487 (2001).
16. A. Zhang, A. Zangwill, *Phys. Perspect.* **20**, 342 (2018).

PT