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Whatever Happened to Solid State Physics?

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Abstract

Subfields of physics are born, expand, and develop in intellectual scope, then can spawn new offspring by subdividing, can disappear by being absorbed in new definitions of the fields of physics, or may merely decline in vigor and membership. Textbooks, seminar programs, graduate courses, and the chosen structure of industrial laboratories all contributed to making solid state physics a vibrant subfield for 30 years, to ultimately disappear into regroupings with names such as condensed matter, materials science, biological physics, complexity, and quantum optics. This review traces the trajectory of the subfield solid state physics through the experiences of the author in relationship to major university departments and Bell Labs, with digressions into how he became a physicist, physics education, and choosing research problems.

What is physics? To me—growing up with a father and mother who were both physicists—physics was not subject matter. The atom, the troposphere, the nucleus, a piece of glass, the washing machine, my bicycle, the phonograph, a magnet—these were all incidentally the subject matter. The central idea was that the world is understandable, that you should be able to take anything apart, understand the relationships between its constituents, do experiments, and on that basis be able to develop a quantitative understanding of its behavior. Physics was a point of view that the world around us is, with effort, ingenuity, and adequate resources, understandable in a predictive and reasonably quantitative fashion. Being a physicist is a dedication to the quest for this kind of understanding.

So I grew up taking things apart, seeing how they worked, repairing bicycles, exploring chemistry in the kitchen (or better, out of sight in the cellar), building flyable model airplanes, crystal sets, and simple radios, playing with batteries and coils of wire, and learning to think with my hands and manipulate real objects. One of my earliest memories is of a small screwdriver that was kept in a drawer of the treadle-operated sewing machine my mother used. It was for minor sewing machine adjustments, but I was allowed to use it on anything in the house—as long as I put it back in the drawer. And if occasionally I could not reassemble the object I had attacked, my father would patiently do so in the evening. My early concept of what it would be like to be a physicist was a somewhat mystical idea of carrying on such playful explorations at a more abstract level.

In my father's generation, physics certainly was not associated with being well paid. My father went to Berlin on a Guggenheim Fellowship in 1929, planning to return to the United States in 1930 to take an academic position. The Wall Street crash of 1929 intervened, eliminating all university hiring. Instead, he took a sequence of temporary positions, including setting up the physics exhibit at the 1933 World's Fair in Chicago, the city where I was born. He ultimately took a position with the Libby Owens Ford glass company, where he worked on tempered glass and thermopane, both of which turned out to be materials science problems. Heedless of the financial consequences of being a physicist, I only briefly considered an alternative career. My high school chemistry teacher had been superb, whereas my physics teacher (in hindsight) lacked an understanding of even the rudiments of mechanics and electricity. Thus influenced, when listing potential majors on college applications I wrote "physics or chemistry." Upon my arrival to Swarthmore, I was assigned William Elmore as my advisor. Bill knew my father and my background. I still remember going into his office for the first time, when he picked up the card describing me and my interests, took out a pencil, and crossed out two words, remarking, "I don't believe we need to consider chemistry."

Ultimately, one must choose something particular on which to do research. So late in my Swarthmore career, I began looking at graduate schools in physics with an eye to what I might specialize in. My upbringing had focused my interest particularly on the physics of the world around me: not the physics of the nucleus or the cosmos, but rather the physics of the daily world and its technologies. I eventually went to graduate school at Cornell, rather than Princeton, because in 1954 it seemed to have a segment of the department interested in a field called solid state physics. It was reflected in a few courses with solid state in their titles, a seminar, two faculty doing theoretical research in the area, and about four experimental thrusts. Experimental problems being worked on included low-temperature thermal conductivity, color centers in the alkali halides, the UV spectroscopy of insulators, and X-ray absorption. The one person doing research on superfluity of ^{4}He was certainly not within the solid state physics community as defined at the time. Donald Holcomb had arrived recently from the University of Illinois, had a Varian NMR rig, and was on the fringe. The other established interests of the department were nuclear and particle physics, X-rays, and cosmology/astrophysics.

Over the next three years, I took three wonderful solid state physics courses given by Albert W. Overhauser. The first of these was based on the first edition of Charles Kittel's *Introduction to Solid State Physics* (1), published in 1953. Overhauser had done his PhD thesis under Kittel's

guidance at UC Berkeley, and had heard lectures on which the text was based. The other two courses might have been called Special Topics in the Quantum Theory of Solids. These required a much broader base in useful specialized treatises, and prominent among them was Seitz's *The Modern Theory of Solids* (2).

Good textbooks can be very important to the definition of a field. The breezy and original *Molecular Biology of the Gene*, by James Watson (3), defined a new field for a generation of biology students. *The Modern Theory of Solids* (2) was almost such a book (the adjective breezy has never been associated with Seitz), describing the field of solid state physics in 1940 in a fashion that was both sweeping and unifying. Such books also define the field boundaries by what they omit or downplay. Thus, for example, ^{4}He low-temperature physics and fluids in general are omitted from *The Modern Theory of Solids*. Statistical physics is chiefly neglected. Glasses don't exist. Superconductivity is given two pages and no data or equations in this 600-page book. The physics of the electronic technologies of the time is represented by a brief account of thermionic emission at a metal surface, a terse account of the photographic process, and a footnote pointing to a recent paper on the theory of point-contact rectification. Piezoelectricity is not even in the index, although long-distance telephony and AT&T were completely dependent on piezoelectric crystals for frequency stability in multiplexing communication channels.

The Second World War took place between the writing of these two books, bringing with its tragedies an enormous upheaval in physics research and technology applications. This revolution took place chiefly at US government and industrial laboratories, and the reification of solid state physics that resulted was due in great part to this influence. In the summer of 1945, Mervin Kelly, the executive vice president of the AT&T Bell Telephone Laboratories, signed off on a directive (see Reference 4, p. 80): "[A]ll the research activity in the area of solids is now being consolidated in order to achieve a unified approach to theoretical and experimental work in the solid state area… [for] the obtaining of new knowledge that can be used in the development of completely new and improved components of communications systems." This directive created the world's first major industrial solid state physics laboratory, and in passing defined what was to be meant by that subject in an industrial context. The transistor was an early success of that laboratory.

Kittel spent four years at Bell Labs prior to moving to UCB and writing his text; the choice of topics reflects that experience. I still treasure my copy of the first edition of *Introduction to Solid State Physics* (1) (although it was rapidly replaced by a much better second edition). There were other much more specific and deeper books available, such as A.H. Wilson's *Theory of Metals* (5) and Shockley's *Electrons and Holes in Semiconductors* (6). To me, the genius of Kittel's book was its simplicity. It made you able to understand an immense variety of phenomena without encumbering the explanations by obfuscating mathematics. The weakness of the book was that it left you (as a theorist) with no idea of where to start to develop a deeper understanding of any of the topics covered.

In the middle of my second year at Cornell, I approached Albert W. Overhauser to ask if he would supervise my thesis and help me find a thesis problem. Through courses and doing problems, I was rapidly acquiring the tools of theoretical physics, but I had no idea how to find an appropriate research problem. Finding a good problem was not (and is not even today) a subject discussed in classes or seminars. Happily, Al had a long list of interesting puzzles. These were often in the form of a paradox, such as "elementary theoretical analysis of a particular phenomenon in solid state physics gives result A, while experiments give an entirely different result B." For most of them, Overhauser himself had no idea what was producing the conflict between a common-sense theoretical approach to an answer and the reality coming from experiments. I picked one having to do with the radiative lifetime of an exciton in a crystal, where the conflict was within theory itself. Naive theory yielded either zero or infinity depending on how it was applied, neither of which seemed

to make sense. It became my problem, and Overhauser never worked on it at all. He was enormously supportive as a listener and critic when I went to see him, but finding direction and resolving technical theoretical issues was entirely my problem. The great gift he gave me was ownership of an interesting question, and total responsibility for research and progress. One day he simply told me that I had better start writing up my understanding, and that was the end of my PhD research. The polariton, a new solid state physics particle, was invented to resolve the paradoxical situation. The single paper written from the 1958 thesis is still highly cited (as is the single author) thanks to the existence of lasers, the polariton condensate, and modern photonics. Thank you, Al. I have done my best to repay you through similarly nurturing another generation of independent students.

With a theoretical solid state thesis in hand, I went into the job market. Academia or industry? The answer was clear from the Solid State Seminar speakers I had heard. AT&T's Bell Labs (Murray Hill) and General Electric (Schenectady) had broader, more vigorous research programs in solid state physics than did any university. I came home from visits to these laboratories positively euphoric about both the field as it was pursued in industry and the relatively unfettered research climate of both laboratories. I would ultimately go to Bell Labs chiefly because of its laboratory administrative structure, which had been reworked so that there was a small theoretical physics group not directly assigned to a subfield such as magnetism or semiconductors. By contrast, GE had a couple theorists in each of its solid state physics topical subgroups. And of course, joining Bell Labs in solid state theory was a little like joining the Yankees as a pitcher.

The theorists were all working on problems similar to those that had motivated Overhauser. P.W. Anderson had just written his "Absence of Diffusion in Certain Random Lattices" paper. It would form the basis of his Nobel Prize and was motivated by an effort to explain some paradoxical electron spin relaxation results in Feher's experiments in doped silicon. Melvin Lax was trying to formulate the noise problem in semiconductor diodes in such a way that it did not violate the second law of thermodynamics. (It is very difficult to write a theory in which the diode rectification characteristic results in zero average charge on a capacitor in a simple diode-resistor circuit.) Conyers Herring was inventing phonon drag to explain an anomalously huge thermo-electric power in doped semiconductors. Gregory Wannier was working on Stark ladders and their possible observation at high electric fields. What strikes me, looking back at the enterprise, was that the subjects of study were generic problems of solid state physics. The theoretical arguments were often based on detailed experiments, but they were not specifically driven by the materials science and device needs of AT&T. The bulk of Bell Labs' effort was of course problems of the latter kind, but the theoretical group (subdepartment 1111) was different.

Herring read all the solid state literature, and even did some of the translations from the Soviet literature himself. He ran a monthly journal club, selecting the most interesting of his readings for presentations that he assigned to relevant Labs scientists. Few declined. These highly argumenta-tive meetings were marvelous for my extended education and as an introduction to the extremely diverse society that comprised Bell Labs.

Conyers was also the head of the theory group and saw me struggling to identify a next problem to work on. He suggested visiting with experimentalists to get my own sense of where interesting puzzles lay, and I still remember many of those visits. Arthur Schawlow explained the 1958 Townes-Schawlow theoretical paper on the possibility of an optical maser. He then took from his desk drawer a pink rod about three inches long and described the wonderful intricacies of the spectroscopy of Cr^{3+} impurities, which made Al_2O_3 into a ruby. He finished his discourse by remarking that unfortunately the R1 and R2 spectral lines were totally unsuitable for making an optical maser. (Two years later, after Maimen had shown lasing action in a flash-lamp pumped ruby, the crystal was put to use in the first Bell Labs–built laser. The significance of making a pulsed laser had not occurred to Schawlow.) Bill (Willard) Boyle was measuring the low-temperature magneto-

thermal properties of the semimetal bismuth. George Feher had mapped out the electronic wave function of phosphorous donors in silicon with the ENDOR technique that he had recently developed. Bernd Matthias was telling all who would listen that superconductivity could not really be BCS-like because of the absence of an isotope effect in some materials. Robert J. Collins introduced me to edge-emission luminescence in CdS, which provided a topic for my second paper.

During a visit with Jim Lander's subdepartment, which was totally devoted to ZnO, I was introduced to David G. Thomas, who was to provide me with the necessary puzzles for the next few years. The mystique of the golden days of Bell Labs and the wonderful foresight of its administrators and scientists is the reason I tell this story. Prior to 1960, vacuum tubes were the basic amplifiers in all telephone electronics. Vacuum tubes required thermionic emission from hot cathodes, but for long life and low power consumption, the temperature needed to be as low as possible. BaO-coated cathodes worked well in this regard, so Bell Labs formed a group to investigate BaO. By the mid-1950s it was apparent that the days of vacuum tubes were coming to an end. In spite of this fact, the first transatlantic cable carrying telephone conversations (1956) had vacuum tube amplifiers every 43 miles. (The cable was taken out of service because of technical obsolescence after 22 years, with all the vacuum tubes still working.) With BaO no longer a material of future technological interest to AT&T, and because the group had experience with oxide materials, it changed its focus to zinc oxide. ZnO was known to be a semiconducting material and therefore possibly related to electronics technology. Crystals were relatively easy to grow, and ZnO crystals are not hydroscopic, avoiding a problem that had plagued BaO research. Thus, Lander's group was able to preserve itself within the Bell Labs structure by contriving a somewhat plausible alternative rationale for the group's existence.

This was the environment within which I met chemist D.G. Thomas and formed a working theory-experiment alliance that ultimately ranged over many different compound semiconductors. It was to provide me with unanticipated problems and paradoxes for years, to provide AT&T with a major knowledge base in compound semiconductors and semiconductor optics, and to garner a joint Oliver E. Buckley Solid State Physics Prize from the American Physical Society (1969). But at the time, no one would have guessed that the combination of light and compound semiconductors would have a major technological future.

In what sense was solid state physics a separate field of study in 1958? It was pursued chiefly by specialists who did not contribute to other branches of physics. For example, theorists like Shockley, Bardeen, Anderson, and Kittel, whatever their training, were finding their problems from within solid state physics and separated themselves from nuclear, particle, or astrophysics. By contrast, an earlier generation of theorists, like Bethe, Block, Wigner, and Pauli, had worked across a broader span of undifferentiated physics and, when solid state physics emerged as a field (and some detail began to matter), ceased to contribute to it.

There were evident boundaries between solid state physics and other subfields of physics. Low-temperature physics had been chiefly thought of as a separate field of physics, studying the properties of ^{3}He and ^{4}He, and on a perpetual quest for lower and lower temperatures. Bell Labs, after much debate, finally bought a Collins machine (helium liquefier), but its intended purpose was to study solids at low temperatures, where fundamental properties became simpler, and to characterize materials that might have technological significance in communication. Photonics had not yet been invented—there were no lasers. Liquid-crystal physics was in its infancy and lay outside the span of solid state physics. Spin physics had its own set of experimental practitioners, such as Purcell, Townes, Abragam, and Hahn, who found solids a useful experimental medium but were not fundamentally motivated by a desire to understand solids. A boundary was quickly growing in meetings of the American Physical Society. The March meeting had become a focus for papers in solid state physics, and the preeminent January (New York) and April (Washington, DC)

meetings were quickly reducing their fraction of solid state physics papers, and, more slowly, the attendance of solid state physicists. In sum, educational programs, textbooks, seminar programs, professional meetings, the definitions of industrial programs, and the behaviors of individual scientists jointly attest to the existence of solid state physics as a separate subfield of physics in 1960.

Comparing the content of the March APS meeting in 1960 and 2010 is a way of seeing what has happened to solid state physics. The 2010 meeting contained much that would be at home in the 1960 meeting and by this criterion can be classified as solid state physics. Graphene physics is a new addition but clearly to be classified as solid state physics. Experiments on lattices of optically trapped atoms push the envelope a bit but might with a little stretch of the imagination be called solid state experiments. But what about photonics, avalanches and self-organization, quantum computation, the crumpling of newspaper, liquid crystals, ^{3}He low-temperature physics, complex systems, and biological physics? A meeting that originally had well-defined boundaries has agglomerated into the much broader subject of condensed matter physics. Why did this happen, rather than a Balkanization of the subject matter at the APS meetings? Why didn't this diverse subject matter result in severely separate communities? By asking such a question, we are asking why the historical evolution has been such, and we cannot expect great precision in an answer. I see at least three strong influences, namely the role of theory, the particular psychology of those who pursue physics, and accidents in the evolution of each subsubject that has agglomerated into condensed matter physics. I briefly comment on each in turn.

First, the role of theory and theorists: Although solid state theorists in 1957 were gradually becoming a separate group pursuing separate and somewhat specialized problems, this separation has never been complete. Some problems of solid state physics are so general in their mathematical structure or in their applicability that they are physics problems, not solid state physics problems. Nowhere was this more true than in superconductivity.

It is difficult from a modern perspective to grasp what a paradigm-breaking viewpoint the BCS theory was in 1957. A substantial fraction of talented theorists had worked with little success on the problem. The frustration of such theorists is expressed in Feynman's talk on superfluidity and superconductivity, given at a general conference on problems in theoretical physics at the University of Washington in September 1956. After giving his excellent account of a physical understanding of superfluid ^{4}He, he discusses the problem of superconductivity without success, concluding in frustration:

> We should not even have to look at the experiments. Every time we look at another experiment, we make the problem easier. It is like looking in the back of the book for the answer, which is slowly being unveiled by the details of the various experiments. There is no reason to require the experiments. The only reason that we cannot do this problem of superconductivity is that we haven't got enough imagination.[1]

Yet within a year, the BCS theory was published, and accepted as correct, bringing with it a complex order parameter and a new kind of broken symmetry, a new kind of pairing, and interesting coherence factors that were quickly verified in experiments.

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[1]The talks from this conference were written up and published in the *Reviews of Modern Physics*, in 1957 (7), from which this quote was taken. In the introduction to that issue by E.U. Condon, J.R. Schrieffer is one of those thanked for help with manuscripts. Years ago, Bob confided to me that he had ghost-written most of Feynman's paper on the basis of the lecture, but the conclusion was pure Feynman.

Decades later, I asked Feynman what he had thought about BCS theory when it was published in 1957. I expected him to say something like, "I looked at the paper for ten minutes, saw the pairing trick, and knew it was right and that everything else would trivially follow." Instead he said that he had been psychologically unable to look at the paper for six months, and did so only because his physicist sister Joan had shamed him into it.

Feynman was not the only prominent theorist who found it emotionally difficult to embrace BCS theory. At a Princeton University physics colloquium circa 1966, E.P. Wigner asked the unfortunate speaker, "I see that you analyze your results using the BCS theory. You experiment on granular aluminum, with particle sizes less than 1,000 Å. There is an uncertainty of 10,000 V in the electrical potential of such a particle due to the fluctuations of the number of electrons in the particle, as described in BCS theory. Can you please explain to me what effect these fluctuations have on your electrical measurements?" It was so clearly a prepared trick question to be sprung on the unwary, designed to sow mistrust of a theory that Wigner did not like because it was not his.

BCS theory had effects on the structure of physics because it rose above the particularities of solid state physics and, with its sophisticated example of broken symmetry and its applications to neutron stars, was an influence in maintaining the connection between theoretical solid state physics and elementary particle theory and astrophysics. Getting the theoretical underpinnings of superconductivity and gauge invariance correct led P.W. Anderson to write a paper describing (in a nonrelativistic context) the Higgs phenomenon as a way of accounting for mass, which was deliberately published in the "Particles and Fields" section of the *Physical Review*.

During the following two decades, there was much in theoretical solid state physics that was to connect it with other subfields of physics. Scaling and critical exponents tied solid state phenomena to the statistical physics community. Liquid-crystal physics was developed, with its diverse connections to topological states and symmetry. The Kondo problem became surprisingly linked to scaling phenomena. More sophisticated models of strongly interacting electrons in solid state theory became interesting to particle physicists. An interest in disordered systems and glasses was kindled, which was to provide through spin glasses a link to complexity physics. Once again, as solid state physics broadened into such areas, the physicists involved wanted to meet with like-minded scientists, and the simple way to do so was to develop the broader framework we now designate as condensed matter physics.

The motivations and backgrounds of those who were attracted to solid state physics is a second factor important for solid state physics to evolve into a much broader field of condensed matter physics. Solid state physics was one haven for physicists who were drawn to physics by a desire to understand the world around them on a personal level. Why is iron ferromagnetic? Why does a wire point pressed to a galena (PbS) surface result in a rectification contact? (Such point-contact diodes were an essential feature of crystal set radios.) Why is ZnS a phosphor? Why do transparent NaCl crystals turn colored when exposed to ultraviolet light? Why is copper a good conductor of electricity and iron a poor one? Why do pencil marks conduct electricity? School-age pre-physicists of my era tended to do simple experiments that raised such questions, whether in the basement or in science classes. Solid state physics was the subject that centered on finding the answers. For students of physics, each such question based on simple observations implies a research program that could lead to the answers. Each line of experiment designed to answer one of these easily posed questions tended to produce data that in turn posed questions at a deeper level. The field of solid state physics was an endless frontier.

Solid state physics in 1960 was in greatest part table-top physics, requiring neither large groups of researchers nor elaborate experimental equipment. It grew up in an era when research funding for experimental physics in universities was quite limited, and the typical experiment was run by

one graduate student working with a professor who had perhaps two or three students. Even at Bell Labs, IBM, or GE, where the funding for experimental solid state physics was rather more generous, solid state research was mostly an individual enterprise, with each researcher having a "group" comprising one, a single laboratory technician.

Many solid state physicists were more connected to the endless-frontier aspect of doing science than to the particular question they were pursuing at the moment. The small-group, small-laboratory aspect produced an ability to rapidly redeploy research effort in a quite new direction. This agility spread solid state researchers into adjacent areas. For example, when lasing action in ruby was discovered in 1960, many solid state researchers whose mode of study involved light rapidly turned their efforts to laser physics and nonlinear optics, creating new fields of study. As this began, it was natural for these people to attend the same scientific meetings they had always attended, meetings populated with their scientific and social friends. The March APS solid state meeting thus began to agglomerate these two new fields. Simultaneously, the development of lasers gave new ways to study some of the previous questions of solid state physics, further unifying this technically diversifying research community. Other examples of expansion and agglomeration include the relationship between the statistical mechanics of phase transitions, self-organization, and complexity physics.

The third factor determining the breadth of the diverse subject matter now part of condensed matter physics is the particular history behind the accidental development of each subarea. Each is different. I will give an admittedly biased but firsthand account of how biological physics wound up being a part of condensed matter physics meetings. Other stories with which I am less familiar describe how complexity physics, information physics, and the statistical physics of fluids and phase transitions amalgamated with the broader umbrella of condensed matter. In 1960, it had not yet happened. Ideas about and measures of information were somehow only a part of communication engineering. I attended a meeting in Utrecht on many-body physics in 1960, and most of the attendees were not card-carrying solid state physicists. The category condensed matter had yet to be described.

Since the era in which physics became a separate subject within science, some physicists have made significant contributions to biology. Nineteenth-century icons like Helmholz and Galvani come to mind, particularly notable because their contributions were so closely coupled to the frontiers of physics at the time.

In the more modern era, physicists Max Delbruck, Seymour Benzer, Francis Crick, and Wally Gilbert were determined to contribute to mainstream biology. And they did so in major ways. However, if you read their major contributions, you will find nothing specific that marks these researchers as physics-educated. Although their inventiveness and clarity of thought are exemplary, the same can be said for many of their biology- or chemistry-educated contemporaries who contributed to the development of molecular biology. No physics journal presented their results, and their accomplishments in biology were not presented at APS meetings. In short, such scientists left the intellectual community of physics and affiliated with the biological community. This crossover is significant, for community consensus defines what questions are important, what interpretational frameworks are valid, and what facts are beyond dispute.[2]

Biological physics is now linked with condensed matter physics. Biological physics began when well-known physicists who had become interested in biology (defined very broadly) began pursuing this interest while keeping in strong touch with their physics roots and the physics community. There is of course a long tradition of physics in biology falling under such diverse labels as radiation biology, membrane physics, biophysics, biological instrumentation, etc., within physics

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[2]Apropos of this, I pass on the comment made to me by Francis Crick, who remarked concerning a somewhat problematic theory I was advocating, "[I]n biology no theory should fit all the facts, because not all of the facts are true."

departments and medical schools. But prior to ~1965, no physics department that counted (no major physics department) had such activities as an appreciable, integral, and respected part of its intellectual and academic endeavors. An occasional maverick could be tolerated with wry amusement, but even that maverick was generally working within the context of tackling problems defined by biologists. No industrial research laboratory or government laboratory had made the broad interface between physics and biology a major technological focus in the fashion that Bell Labs had done with solid state physics.

But times were changing. The success of X-ray determinations of the structure of DNA and a few proteins generated an increased interest in exploring life processes at the atomic level, of understanding the functional properties of biomolecules the way we were able to understand the properties of Si crystals or p-n junctions or laser physics. The applicability of the rapidly developing experimental tools of EPR, NMR, and lasers/modern optics to biology created opportunities in biology for experimental physicists. And the very success of solid state physics in identifying and understanding so many of the first-order simple and obvious questions and problems of solids generated a set of physicists looking for new problems. Some turned to further, deeper questions in solids; some turned to other developing fields with obvious physics content (such as laser physics). A few turned to biological physics, where most of the zero-order questions were not yet posed, let alone answered.

In the 1960s, when a physical scientist looked at a biological process it appeared purposeful and almost miraculous. How could such a process as cell division or thinking be merely the simple laws of classical physics in a system without design? For the physicists' explanation of the mysteries and seeming miracles of life processes will not involve Planck's constant in any profound fashion.[3] The profound mysteries of biology lie in "more is different" (8). There are two timescales (at least), one the timescale of evolution, and the other the timescale of the life processes of a single living organism. On both timescales, what is seen looks utterly unlike a result that we have seen in any physical system. When a physicist really understands something, he can explain it to another physicist in such a way that the other physicist will feel the result is obvious. For the budding field of biological physics, such understanding is the ultimate holy grail.

A physicist attracted to biological puzzles must start the quest for this kind of understanding with modest biological goals, chosen to be particularly amenable to the techniques and attitudes of experimental physics. George Feher left Bell Labs to take a professorship at the nascent UCSD's Physics Department and began EPR-based studies of the micromachine responsible for bacterial photosynthesis. Ivar Giaever at the GE Research Laboratory turned his understanding of metallic films to developing more sensitive viral assays. Leon Cooper turned from superconductivity theory to the theory of learning in neurobiology, interacting with experimental neurobiologists. I mention these three as examples of very successful solid state physicists who already in the mid-1960s were finding their next problems in biological systems, but whose papers remained in the quantitative and modeling world of physics and readable by the solid physics community. Biological physics grew by agglomerization at the periphery as well. In the early 1970s, Hans Frauenfelder turned from using nuclear physics as a probe of local solid state environments to the study of local field

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[3]The role of quantum mechanics in biology is central but trivial. Making and breaking covalent chemical bonds between molecules immersed in a solvent is intrinsically a quantum mechanical problem involving electrons. Yet for most purposes, the fast motions of electrons and many fast vibrational motions can be integrated out (adiabatic), producing force fields that drive the slower, more collective coordinates that are the useful descriptors of biological processes. Although there have been elegant philosophical discussions of issues such as whether consciousness is necessary for quantum mechanical measurement and wave-packet collapse, they contribute little to our physical understanding of the biological core processes such as self-replication, processes billions of years older than consciousness in animals with highly developed nervous systems.

environments in myoglobin. He was a particularly effective advocate of what he named biological physics, believing that biological matter was so unusual that its properties should be studied for their own sake, and that such studies were unfettered by questions of biological relevance. Of course, in the long run truly unusual properties will often be present as a result of evolutionary importance, bringing biology back in by the rear door. The insistence that physicists should ask their own questions of biological systems, and should be writing for physicists not for biologists, became part of the intellectual divide between biological physics and the older discipline of biophysics.

Choosing problems is the primary determinant of what one accomplishes in science. A supportive environment also helps. I therefore take the liberty, finally, to digress about my own intellectual history in the physics-biology interface. I have generally had a relatively short attention span in science problems (note the fine line between the polymath and the dilettante, where I am often offside). Thus, I have always been on the lookout for more interesting questions either as my present ones get worked out, or as they get classified (by me) as intractable. In 1968 I had run out of problems in condensed matter to which my particular talents seemed useful. (I once heard Linus Pauling remark on this topic in an interview: "I ask myself, 'Is this a problem to which I am likely to make a contribution?'" Acknowledging one's own abilities, style, and weaknesses is ever so useful.) I went to the Cavendish Laboratory on a Guggenheim Fellowship for half a year hoping to find new interesting avenues, but found little for me. Returning to Princeton and my consultancy in the semiconductor group at Bell Labs, I ran into Robert G. Shulman, a chemist who was doing high-resolution NMR on hemoglobin. He described the cooperative oxygen binding of the four widely separated iron atoms in the centers of their heme groups. An incredible panoply of physics techniques was being used to study the molecule. NMR, EPR, optical spectroscopy, resonance Raman scattering, X-ray structure studies, Mossbauer spectroscopy—all the clever experimental techniques of solid state physics—seemed relevant to Hb. It was for a while the physicist's hydrogen atom for understanding how proteins function. Shulman wanted theoretical company, so he made a strong effort to interest me in the problem and the future of such studies for turning biology into a "hard" science.

So hemoglobin provided me a simple entry from condensed matter physics to biological matter physics. How structure and low-lying excitations begat experimental physical properties was still the name of the game, as was the case in much of solid state physics. The one singular conceptual addition to the science was the notion of function: that there is a small subset of properties that is of great importance to biology, and that evolutionary choices have shaped biological systems so that they function well. The term function is peculiarly biological, occurring in biology and in applied sciences/engineering, which are pursued to benefit humans, but not relevant to pure physics, pure chemistry, astronomy, or geology.

I worked with Shulman's group for a couple years trying to understand the interaction energy that caused the cooperativity observed in the equilibrium oxygen binding at the four distant heme groups. Bell Labs was reasonably sympathetic to this venture. My consulting was moved from a semiconductor group to the biophysics group, with only the wry remark that they could scarcely raise my consulting fee that year since I was moving from an area where I was an expert to an area where I knew nothing. It was a fair comment. There was some success in interpreting diverse experiments in a common framework. [One of the other biologically unheralded members of this group was Seiji Ogawa, who 20 years later became famous (while still at Bell Labs!) by using his expertise in NMR and Hb to invent functional magnetic resonance imaging of the brain.]

The Bell Labs group next turned its efforts from Hb to tRNA, where they could determine aspects of secondary structure from NMR. Lacking any way to relate such experiments to functional questions, I somewhat lost interest. I did however attend *many* seminars from outside

speakers, who were describing the functional biological aspects of tRNA without knowing much about its structure. The one that still sticks out in my memory 45 years later was by Herbert Weissbach on protein synthesis. Filled with far too many details for any physicist to remember and including a funky movie of students playing the parts of amino acids, RNA, proteins, etc., culminating with a linked chain of amino students being generated while phosphate and tRNA students reeled off into oblivion, the two-hour talk was my penance for joining the biophysics group at Bell. The only overall impression I got was that there seemed to be an extravagant waste of high-energy molecules in the processes of protein synthesis, very much a physicist's view and not mentioned by the speaker, who was intent on describing a linear biochemical pathway for assembling a protein.

At the same time, I was giving my first biophysics course. I spent an unreasonable amount of time on hemoglobin. Unfortunately, hemoglobin is a poor introduction to problems in biology because its most obvious physics problem is a question of equilibrium. The essence of biology is the dynamics of a driven system often far from equilibrium. About four weeks into the term, I sat down one evening determined to develop a theoretical treatment of *any* problem of biological dynamics. The sole precondition was that it needed to be handled at a level and fashion that required a knowledge of only elementary quantum mechanics and rudimentary solid state physics. I quickly realized that from a physics viewpoint the simplest chemical reactions in biology were electron transfers with little nuclear motion and no rearrangements of chemical bonds. Early stages of photosynthesis and some of the important processes in oxidative phosphorylation were of this nature. So that evening I identified my topic for the next week and roughed out the solution to the electron–transfer rate problem. It was trivial to do because the problem was so similar to the problem of trapped electron-hole recombination I had studied in semiconductors ten years earlier. Then, finding the simple problem not appropriately treated in the literature, I wrote up the classroom exercise for publication in *PNAS*. (There I described the theory in a highly simplified form, stripping out most of the physics sophistication, in hopes that biochemists might read it.) In short, this problem was identified only out of my pedagogical need. Happily, it turned out to be of interest to others and provided a starting point for many theoretical and experimental studies.

I then needed a second problem of biological dynamics for my class. I had heard enough in seminars about protein synthesis and the chemical problems surrounding it that I turned to the kinetic problems of *accurately* making proteins. Most of the biochemistry surrounding protein synthesis was based on a lock-and-key description with an incorrect reaction not possible because "the wrong amino acid doesn't fit." Indeed, understanding biochemistry was generally viewed as a problem of mapping out "what happens." What does *not* usually happen was not much thought about. From a physics perspective, most chemical reactions are possible at room temperature. There will be only different energies associated with similar but different reactions and thus different Boltzmann factors. Discriminations are actually based on energy differences. "A happens and B does not happen" should be replaced by "A happens at a rate $\sim\exp(-E_A/\kappa T)$ and B at rate $\sim\exp(-E_B/\kappa T)$." The ratio of bad rates to good rates must be $\exp[-(E_A - E_B)/\kappa T]$, where $E_A - E_B$ is a discrimination energy. I managed to put together a few lectures showing that for accurate biosynthesis, a network of chemical reactions should not be pushed to operate too quickly. In the course of lecture preparation, I made a solid-state-type estimate of the discrimination energy between two very similar amino acids, valine and isoleucine, that differ in only a single methyl group. I calculated 1/50 for the ability of an isoleucine binding site to discriminate against valine. Unfortunately, the experimental number in biological protein synthesis (from the work of physicist R. Loftfield) is about 1/3,000. So much for my ability to carry estimation techniques from one field to another!

The issue nagged me. After a month, I realized that the way out of this paradox was that Loftfield and I were both correct, and that biology must have found a way to proofread at the molecular level, thus bootstrapping an accuracy of $(1/50)^2$ from an intrinsic fundamental accuracy of ~1/50. I quickly found two ways to think about proofreading, both of which required a dissipation of free energy. I suddenly remembered the Weissbach seminar and the profligate use of energy in protein syntheses it implied. The straightforward solution, using an energy source to drive kinetic proofreading, is left to the reader and is experimentally well established in molecular biology and immunology.

These two problems and papers [both published in *PNAS* in 1974 (9, 10)] were to make my brief career in molecular biology and were the basis on which I was offered a professorship with the chemistry and biology divisions at Caltech. What was their effect on the Princeton Physics Department? I never gave a physics colloquium or solid state seminar on either work. The general attitude was that I was probably doing something interesting, but it involved too many details for the Princeton Physics Department. When in 1979 I went to see Physics Chair Val Fitch to tell him about the offer from Caltech, there was no counteroffer. Val said it would be best for both of us for me to leave Princeton. Sadly, he was right, and I left the nonsupportive safety of a community I loved.

My entry into biological information processing at the level of the nervous system was entirely accidental. I had spent the winter of 1977 at the Bohr Institute/Nordita in Copenhagen, as part of their sporadic but continuing outreach toward biology. I arranged many broadening interfacial seminars, but found no new problem for myself. Shortly after my return to Princeton, Francis O. Schmidt descended on me. He ran an entity called the Neuroscience Research Program, which chiefly held small meetings attended by 20 regular members of the program and 20 outsiders chosen for the special topic under consideration at that particular meeting. Schmidt invited me to talk at the next meeting. I told him I knew nothing of neuroscience. He said that it didn't matter, "just speak on what interests you," so I talked about biomolecular accuracy. The audience—neurologists, neuroendocrinologists, psychologists, immunologists, electrophysiologists, neuroanatomists, biochemists—understood little of what I said. It didn't matter. It was a put-up job. Frank wanted to add a physicist to the group, hoping to bring someone who would interact with his subject and perhaps help it to become more complete as a science. He had gotten my name from relativist John A. Wheeler, who (for reasons that I have never grasped) had always been one of my staunch supporters.

I was captivated. How mind emerges from brain is to me the deepest question posed by our humanity. It was being pursued by this NRP club of diverse talents and great enthusiasm. But this group of scientists would never possibly solve the problem because the solution can be expressed only in an appropriate mathematical language and structure. None involved with the NRP moved easily in this sphere. So I joined the group. My basic education in neurobiology was through attending the semiannual NRP meetings, sitting next to world experts in their fields, who would patiently explain to me what was going on.

In the fall of 1979, I had begun playing with the computational dynamics of binary neurons in simple nets. Unfortunately, neither the Princeton Physics Department nor the Bell Labs Biophysics Department had the kind of computing environment that was friendly to simulating a diversity of simple ideas whose consequences were not easily explored in mathematics, so I made little progress. I did give an NRP talk on the idea of computing with attractors. Mostly it was ignored, though one Young Turk came up to me afterward to tell me it was a beautiful talk but unfortunately had nothing to do with neurobiology. As I head off for the Society for Neuroscience meeting to receive its 2012 Swartz Prize (in computational neurobiology, a subject that did not exist 30 years ago) for my work expanding from that basic idea, I am thankful for experiences that have blunted my sensitivity to some forms of criticism.

Eventually, my knowledge of spin-glass lore (thanks to a lifetime of interaction with P.W. Anderson), Caltech chemistry computing facilities, and a little neurobiology led to the first paper in which I used the word neuron. It was to provide an entryway to working on neuroscience for many physicists and is the most cited paper I have ever written (11). Even AT&T was pleased, for it also generated a very frequently referenced patent for their patent pool, as well as strengthened links between neural biophysics and condensed matter physics at the Labs.

In 1997, I returned to Princeton—in the Molecular Biology Department, which was interested in expanding into neurobiology. Although no one in that department thought of me as anything but a physicist, there was a grudging realization that biology could use an infusion of physics attitudes and viewpoints. I had by then strayed too far from conventional physics to be courted for a position in any physics department. So I was quite astonished in 2003 to be asked by the American Physical Society to be a candidate for vice president. And, I was very happy to be elected and ultimately to serve as the APS president. I had consistently felt that the research I was doing was entirely in the spirit and paradigms of physics, even when disowned by university physics departments. I saw my election primarily as a symbolic act by the membership, saying "this too is physics"—or perhaps "this too is solid state physics."

Physics many times has had to make a choice between striving to keep a new component, a teenage child as it were, within the fold, or to send it out into the wilderness as a separate discipline. I am gratified that many—perhaps most—physicists now view the physics of complex systems in general, and biological physics in particular, as members of the family. Physics *is* a point of view about the world.

DISCLOSURE STATEMENT

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