Elly and Jim Langer above Lake O'Hara in the Canadian Rockies, 2013.

James Langer

My Career as a Theoretical Physicist—So Far

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Annu. Rev. Condens. Matter Phys. 2017. 8:1–11

First published online as a Review in Advance on December 7, 2016

The *Annual Review of Condensed Matter Physics* is online at conmatphys.annualreviews.org

https://doi.org/10.1146/annurev-conmatphys-031016-025141

Keywords

physics institutes, multidisciplinary research, nonequilibrium phenomena

Abstract

Theoretical physics and the institutions that support it have changed greatly during my career. In this article, I recount some of my most memorable experiences as a physicist, first as a graduate student with Rudolf Peierls at the University of Birmingham in England and later as a colleague of Walter Kohn at the Institute for Theoretical Physics in Santa Barbara, California. I use this account to illustrate some of the changes that have occurred in my field and also as a rationale for asserting that theoretical physics has an increasingly vital role to play in modern science.

INTRODUCTION

Theoretical physics has evolved and matured since it emerged as a distinct scientific specialty just over a century ago. I've had the extraordinary good luck to be directly involved in this field for about half of that time. My teachers and my teachers' teachers were among its legendary founders, and I've shared responsibility for helping younger generations build on those foundations in ever more remarkable directions. What follows is an account of some of my experiences and adventures. I'll start with Rudolf Peierls's Department of Mathematical Physics in Birmingham, England, and I'll talk about Walter Kohn and the Institute for Theoretical Physics (ITP) at the University of California, Santa Barbara (UCSB)—which I see as related chapters in this story. Then I'll discuss more recent developments.

BIRMINGHAM, 1955–58

In 1954, I was a senior physics major at Carnegie Tech [Carnegie Institute of Technology, now Carnegie Mellon University (CMU)] in Pittsburgh. By this time, my enthusiasm for math and science had completely overshadowed my disappointment about not being talented enough for a career in painting or performing in Gilbert and Sullivan operettas. Having been born in Pittsburgh, and having stayed there during my high school and college years, I wanted to see more of the world, so I applied for and won a Marshall Scholarship that would take me to any university in Britain that would accept me. Walter Kohn, then a junior member of the Carnegie Tech faculty, advised me to go to Birmingham and become a thesis student with Rudolf Peierls, instead of choosing the supposedly more glamorous option of going to Oxford or Cambridge. Walter even supervised a reading course in quantum mechanics for me, so that I could go directly into a British research program.

It took me a day or so in October 1955 to be sure that I'd made the right choice. Our group of Marshall Scholars had enjoyed a leisurely boat trip across the Atlantic and two days of being wined and dined in London before being sent off by train to our respective universities. Birmingham at first seemed utterly dull and provincial. I was greeted at Chad Hill, the old hotel that then served as the graduate men's hall of residence, by a plate of shepherd's pie (mashed potatoes with reconstituted dried peas and a layer of beef gristle) that had thoughtfully been kept warm for me because there was no other place to find dinner at that time in the evening. But I also was greeted by Stanley Mandelstam, the other Peierls student in residence at Chad Hill, who welcomed me in his shy, quiet way. Stanley already was working toward his mathematical representation of elementary-particle scattering theory that was to become one of the standard analytic techniques in the field for the next decade or so.

The Birmingham Department of Mathematical Physics was a scientific community unlike any other, before or since. It was housed in a one-story temporary wooden building, completely separate from the big red brick structure that housed the Physics Department. All of us—senior staff, postdocs, visitors, and graduate students—had desks in that building. The group was small enough that, encouraged by "Prof" (Peierls) and his outspoken wife Genia, we all came quickly to know each other. Even we lowly graduate students were expected to share ideas and questions with everyone else and to participate in the seminars and discussion groups.

Peierls had grown up in Berlin, where he began his studies of mathematics and physics. His research training started in 1926 with Arnold Sommerfeld at the University of Munich, where he met his lifelong friend and scientific colleague Hans Bethe. Then in 1928 Peierls moved to Leipzig University to work with Werner Heisenberg and a year later to ETH Zurich to work with Wolfgang Pauli. These were the years in which the principles of quantum mechanics were being discovered along with the beautiful mathematical techniques that have become the tools of our trade. Peierls's early research efforts were applications of those tools to theories of electrons in crystals and of various vibrational and magnetic phenomena. These were also the years in which he acquired his life-long love of traveling. [His autobiography is aptly titled *Bird of Passage* (1).] He met Genia on his first trip to the Soviet Union in 1930 and brought her back to Zurich in 1931. With the rise of Hitler, the Peierlses moved to England in 1933, settling in Birmingham in 1937.

In 1940, Peierls and Otto Robert Frisch (a nephew of Lise Meitner) wrote a famous two-part memorandum addressed to the British government (2). The first part presented what was then an unexpected theoretical result—that it would be possible to produce a nuclear explosion by assembling a feasibly small mass of the purified uranium isotope U^{235}. The second described the horrible consequences of the use of such a weapon. This memorandum triggered the joint British-American Manhattan Project and brought the Peierlses to Los Alamos in 1944. In 1946, they returned to Birmingham where Peierls became a professor and the chair of the Department of Mathematical Physics.

My memories of life as a graduate student at Birmingham are mostly consistent with Peierls's account in *Bird of Passage*. As stated there, Peierls insisted that we be interested in all of theoretical physics, by which I think he meant absolutely any research that could be pursued quantitatively and honestly. He made this point clearly during my first year. He gave us what he called a qualifying exam in which we were asked to solve a certain number of problems selected from a larger list. The catch was that, for each of us, the problems in our own areas of research were crossed off the list. I guess that this goal of scientific diversity was too ambitious even in 1956. So far as I know, Peierls never repeated the exam, nor did any of us ever find out how we did on it. But the event impressed me greatly.

In addition to emphasizing the importance of breadth in physics, Peierls insisted that we learn new things by doing them. We did have regular, usually excellent lecture courses given by the senior staff, including Peierls on solid-state physics and Dick Dalitz on elementary particles. But these were just supplements to the main activity of doing research. In my first year Peierls suggested that I learn about quantum electrodynamics by working with Gerry Brown to compute the Lamb shift in heavy elements. In my second year, under the watchful eyes of Sam Edwards and Paul Matthews, I learned about particle field theory by estimating the effects of the newly discovered strange particles in pion-nucleon scattering. Both of these efforts produced publishable results. The second became a single-author paper that mercifully sank quickly beneath a wave of much more important literature in that field.

In my third year I learned about nuclear reactions and wrote a PhD thesis on that subject. I was moving in a direction started by Gerry Brown and Cyrano de Dominicis. But because Gerry was in Copenhagen most of that year, I discussed my work regularly with Peierls himself. That was a learning experience in a way that surprised me at the time. I remember showing Prof various bits and pieces of my research project and getting just the opposite of the responses I had expected. When I was proud of myself because I had carried through some calculation successfully, Peierls saw the point immediately. He was not particularly interested in the details and seemed disappointingly unenthusiastic. However, when I needed help with something that wasn't working well or—better yet—had an idea that was likely to be wrong, then his eyes lit up and the discussion became animated and rewarding. In retrospect, I think that the most important lesson here was that scientists have not just a right, but an obligation to be wrong—and, of course, to recognize their mistakes. If we're not making mistakes, we're not making progress.

There was an historical lesson to be learned from those experiences. A huge amount of important new research was done in Birmingham during the 1950s, and Peierls was at the center of essentially all of it. Bob Schrieffer came as a postdoc just after writing his famous thesis on superconductivity. Paul Martin was also a postdoc, in effect, on leave from Harvard. (Both

Bob and Paul appear later in this story.) New ideas—especially the emerging field theories in elementary-particle, nuclear, and solid-state physics, were constantly being discussed in seminars and discussion groups. Peierls always sat in the front asking the hard questions, no matter whether the speaker was Bethe or Julian Schwinger or a lowly graduate student like me. Peierls had a big influence on all of these research projects. The opportunity to interact with him was the main reason why so many of the world's leading physicists came to Birmingham in those days. However, he was almost never a coauthor of any of the papers written in his department. He seems to have had a firm rule that he would not put his name on a paper unless the original idea had been his own and then only if he had done an appreciable part of the calculations and most of the writing. Although Gerry Brown and I reported regularly to Peierls about our work, I don't think it ever occurred to us to include him as a coauthor on any of our papers. His role was to encourage people and ideas.

PITTSBURGH, 1958–1982

I finished my thesis in the spring of 1958. Having satisfied my desire to see some of the world with trips to France, Italy, Spain, Greece, and other parts of Europe, I accepted a job back in Pittsburgh at Carnegie Tech. Walter Kohn was still there, and I decided to use my growing expertise in many-body theory to start learning from him about solid-state phenomena. I also wanted to marry Elinor Aaron. Elly and I had known each other since our days at Taylor Allderdice High School. We dated occasionally during our college years and corresponded increasingly often by mail while I was in Birmingham. We're still having wonderful adventures together, including lots of travel.

The next decade went by in a great rush. By the end of it, Elly and I had produced three children. I had gone from being an instructor to a full professor and chair of the faculty senate. Carnegie Tech had become Carnegie Mellon University. My first teaching assignment in 1958 was a graduate course in classical mechanics and special relativity. I may have been the youngest kid in the class, and this was the first course I'd ever taught. I worked very hard—so successfully that my teaching evaluations went steadily downward from then on. And somehow during that decade I also did some research.

The faculty senate experience was exciting because anti-Vietnam War sentiment was at its peak, as was campus outrage about racially segregated construction unions then at work on the new physical sciences building. During the Kent State–Cambodia protests in 1969, the students asked me to join them when they occupied the administration building. I negotiated their withdrawal by promising that I would come with them the next day on a march to the Federal Building downtown and that I would bring some senior faculty with us. So I hastily recruited Bill Mullins, the distinguished materials scientist who was then the dean of the College of Engineering and Science, and Dick Cyert, the dean of the Mellon Graduate School of Industrial Administration who was later to become the president of CMU. The three of us led a rag-tag line of students chanting "up the ass with the ruling class" until we were stopped by the Tactical Police Force, which had forcibly broken up a similar march a day or so before. Luckily, the civilian director of public safety arrived before the situation became seriously ugly. He and I knew each other largely because of Elly's already growing visibility in community affairs. After a brief conversation, we were allowed to proceed on the condition that we didn't obstruct traffic. We arrived safely at the Federal Building and then Cyert, Mullins, and I took a taxi back to CMU, where Cyert treated us to stiff drinks from an emergency supply in his office.

My research in those years was every bit as engrossing as the politics. Kohn was the leader of an active group working on many-electron theory, until he left for the University of California, San Diego, in La Jolla, in 1960. John Ward was there briefly, and Quin Luttinger visited. Michel Baranger, Dick Cutkosky, and Lincoln Wolfenstein were doing related work in particle and nuclear field theories. Bob Griffiths arrived at CMU in 1964. I focused first on many-body transport problems, which I guess was the beginning of my longstanding interest in nonequilibrium phenomena. I also became very interested in nonperturbative methods. My field theoretic approach to nucleation problems eventually became known, without my consent, as instanton theory. During a year's leave at Cornell University, Vinay Ambegaokar and I used related ideas to compute the decay of supercurrents in superconducting wires, and Michael Fisher and I did an analogous calculation for flow in superfluid helium, thus bringing superflows back within the constraints of the second law of thermodynamics.

In the 1970s, I flirted with administrative responsibilities by serving for three years as the associate dean of the newly formed Mellon Institute of Science, but I learned "by doing" that I didn't want to go in that direction. I made three trips to Moscow to participate in the Refusnik seminars. On two of those trips, I traveled with Joel Lebowitz, whose extraordinary life story needs to be told on its own. The Moscow trips were less nerve-racking than managing Elly's first campaign for election to the Pittsburgh School Board. She won, only by the skin of her teeth, and she never trusted me with that job again. In 1981, she was reelected easily after having led Pittsburgh's far-reaching school desegregation effort as president of the board. My main research during those years was in theories of nonequilibrium pattern formation, especially dendritic crystal growth, in collaboration with Bob Sekerka and Heiner Muller-Krumbhaar. That project continued through the 1980s in Santa Barbara.

SANTA BARBARA, 1982–1995

My story brings me now to the ITP at UCSB. The idea for such an institute started with Boris Kayser at the National Science Foundation, who thought that our field needed some analog of a national laboratory. He solicited proposals, brought finalists to Washington to make presentations, and ultimately awarded the first (and, so far, only!) such grant to UCSB. The authors of the UCSB proposal were Jim Hartle, Ray Sawyer, Doug Scalapino, and Bob Sugar. Their plan was that the ITP would host about four different research programs per year, two in the fall and two in the spring, each focusing on a specific set of emerging research problems, and each involving ten or twenty visiting participants at any given time. To provide expertise and continuity for these activities, the ITP would have a director and a small group of permanent members, all of whom would have tenured appointments in the UCSB Physics Department. There would be postdoctoral associates in residence for two years or more. And there would be a broadly representative external advisory board to recommend programs and help recruit the senior scientists. The then incoming UCSB Chancellor, Bob Huttenback, played a key role by promising the resources needed to make this plan feasible.

Kohn agreed to move up the coast from La Jolla to be the first director of the ITP. I was much intrigued when he asked whether I'd be interested in becoming one of the permanent members. CMU had been very good to me; but its priorities were understandably more in computer science than in physics. Elly assured me that she'd had her fill of electoral politics. (Nevertheless, in Santa Barbara, she was elected twice to the City Council and then lost an election for mayor.) Most importantly, the proposed scheme for the ITP had captured what, for me, had been the essential features of Peierls's Department of Mathematical Physics in Birmingham. Walter had been to Birmingham before he sent me there; Bob Schrieffer was to be a permanent member at ITP and played a role in recruiting me; and the first chair of the ITP Advisory Board was Paul Martin, another Birmingham friend. It seemed to all four of us that the main difference between the ITP and Birmingham should be that at ITP we would have a small group of senior scientists trying to play the role of one Peierls.

Walter Kohn had grown up in Vienna. As a teenager, just as World War II was about to begin, he escaped from Austria on one of the last Kindertransport trains that was allowed by the Germans to take Jewish children to foster homes in England. Then, as an "enemy alien," he survived a series of internment camps in England and Canada, served in the Canadian army, and earned a physics degree at the University of Toronto and a PhD with Schwinger at Harvard. When I first met Walter at Carnegie Tech in 1954, he was emerging as one of the new leaders in solid-state physics. His density-functional theory of interacting electrons, for which he won the Nobel Prize in 1998, was developed in the 1960s primarily in Paris and La Jolla.

Walter led the ITP to a strong start. With Bob Sugar as the first deputy director and with a small but remarkably effective administrative staff led by Bonnie Sivers (and later Deborah Storm), the ITP quickly became a destination of choice for both program participants and postdocs. Its activities had a multidisciplinary flavor from the beginning. There were research programs in neural networks and nonequilibrium dynamics as well as activities in particle and nuclear physics, astrophysics, and condensed matter. Early program organizers included Sam Edwards, Bruno Zumino, John Cahn, Ernie Moniz, and John Hopfield. In a few years, the ITP became a major asset for the university as a whole when recruiting new faculty in physics and engineering and in related disciplines. The rise of UCSB as an internationally prominent research university was due in large part to the growing reputation of the ITP.

For me, life at the ITP was ideal. My main responsibility was to participate in identifying the areas of modern science that seemed most promising, both for my own research and for the ITP in general. The choice was guided by Peierls's all-encompassing definition of theoretical physics. To help me in this, I enjoyed a steady stream of the world's most talented and imaginative students and postdocs plus the steady stream of ITP program participants. I made a weakly correlated random walk through theoretical physics, drifting away from the world of quantum phenomena and more toward problems in statistical physics and materials science that seemed to me to be every bit as deep and challenging. In this mode and in constant interaction with junior collaborators, I moved from dendritic solidification patterns (snowflakes), to various kinds of propagating interfaces including fracture surfaces, to earthquake propagation, and to the dynamics of plastic deformation. Later, I'll say more about some steps along this walk, but first I want to continue with the story of the ITP.

Walter Kohn served as ITP director from 1979 to 1984; Bob Schrieffer was director for the next five years, from 1984 to 1989; and I served from 1989 to 1995. Each of us faced different challenges in our efforts to keep the ITP focused on its primary mission in the face of changing circumstances. Walter had to start from the beginning to establish operating procedures. With the help of Bob Sugar, he/we learned quickly how to run this new kind of institute, and there were no major catastrophes that I can remember. Walter's most serious miscalculation was that he underestimated the speed at which computers were growing in power and becoming essential tools for research. The ITP started solving this problem by getting a supplementary grant from NASA sufficient for buying work stations and paying someone to be systems manager. Unfortunately, without consulting me or anyone else, NASA decided to call us a "center of excellence in solidification processing." In retrospect, this designation was an early symptom of later difficulties in maintaining funding consistent with our basic mission.

During Bob Schrieffer's tenure the ITP's visibility was growing while competition for resources was becoming stiffer. More people wanted to be involved in our programs, but fewer of them were able to be away from their home institutions for long enough times. That meant that fewer participants would be in residence for the whole months-long programs and more visitors came for just a few weeks or less. With the growing ease of electronic communication, this was not a disaster; in fact, it increased the total number of scientists who were participating in our events. But it also substantially increased administrative costs. At the same time, it was becoming ever clearer that our facilities were inadequate. Since the beginning, we had been pleasantly crowded on the top floor of UCSB's Ellison Hall. But we had only a very small seminar room; so all conferences associated with our programs had to be held off-site, sometimes at local hotels; and we had no flexibility to adjust our program schedules in response to important scientific developments. Bob made a major effort to convince the UCSB administration about our need for a new building and laid much of the groundwork for that project. Like Walter, he set a tone of modest, warm, and thoughtful leadership for the ITP, especially in guiding talented young scientists at the beginnings of their careers.

Both the financial problems and the facilities issues came to a head as I was taking over as director in 1989. By this time, what I like to think of as our "Peierlsian" style of operation was being imitated around the world. The National Science Foundation (NSF) started mathematics institutes in Berkeley and Minnesota, the Department of Energy (DOE) started a nuclear theory institute in Seattle, and Cambridge University in England established its Newton Institute. We were happy to welcome visitors from each of these places who wanted to learn first-hand what we were doing—and also, I'm sure, to see what mistakes to avoid. So far as I could tell, however, neither the UCSB administration nor the US funding agencies quite appreciated what was happening. We badly needed new resources, both for operating expenses and a new building. Huttenback had left UCSB in 1986. Without him, the ITP was not a priority for the UCSB Development Office, and I was not supposed to compete with the University in soliciting private donors. The National Aeronautics and Space Administration (NASA) dropped our grant without even telling us that they had done so. DOE was willing to entertain proposals for special projects, but told me explicitly that we should remain primarily an NSF operation. Luckily, Boris Kayser was still at the NSF. After we jointly carried out a detailed analysis of the situation, Boris was able to help. Luckily as well, the State of California had made available to the University of California system some bond funds predicated on the assumption that the investments would be repaid by increased research income; and some of those funds, along with the wonderful site overlooking the ocean, were allocated to the ITP.

Much of my term as ITP director was devoted to the design and construction of the new building. Several world-famous architects were interested enough in the project to visit campus to talk with us about it. My first job was usually to dissuade them from designing a monastery where scholars could think deeply without distractions. We wanted just the opposite—a building with lots of discussion areas and with open-door offices along the main corridors so that interactions would occur as naturally as possible. It is visually obvious to people who know about contemporary architecture that we chose Michael Graves. I do not remember it being a simple job to work with Graves in designing this building; but I do remember having lots of fun doing it. And I think that the building has worked exactly as intended for our special kind of institution.

SANTA BARBARA, 1996–PRESENT

I stayed in the ITP director's office long enough to move into the new building; but, after fifteen years with the ITP, it was time to move out. I was followed by Jim Hartle, David Gross, and now Lars Bildsten, who have expanded the level of activity in ways that I think are exciting and appropriate. I also admire the ways in which they have succeeded in raising private funds and thus providing the ITP with a degree of financial independence. Our building, now called Kohn Hall, has been expanded with funds from Fred Kavli; accordingly, ITP has become "KITP." As I write, a privately funded residence for our visitors is under construction. I've purposely stayed out of the loop in these developments, but I continue to enjoy coming to talks at the KITP, and occasionally "visiting" as a program participant.

Since leaving the ITP, I've followed what I think is a Peierlsian corollary—that scientists should be active in public affairs, especially when those affairs are related to one's scientific competence. After his experiences in World War II, Peierls became much involved in efforts to curb the spread of nuclear weapons. He was one of the key participants in the Pugwash Conferences. I especially admired his colleague Hans Bethe's activities on US governmental advisory committees, where he was meticulously honest in reporting scientific facts even when those facts were not the ones that he wished were true. Having grown up with the civil rights and anti-Vietnam War movements, I naturally accepted the opportunities that came to me later. I was pleased that, in the 1990s, the American Physical Society (APS) had opted to play a more politically active role than most other professional societies. When I was APS president in 2000, we convened a panel of scientists and former government officials who recommended organizational changes in the DOE Office of Science. We also started a nonclassified study of the boost-phase ballistic-missile defense system that was being considered at that time. Both studies were responsible APS efforts that ultimately turned out to be influential. In the same spirit, I was pleased to serve for four years as the vice president of the National Academy of Sciences (NAS), where I played a variety of roles, mostly having to do with the public understanding of science and the processes by which the Academy provides advice to the US Government.

When I finished at the APS and NAS, I retired from my teaching position at UCSB and since then have devoted most of my time to research. I also agreed to be the inaugural editor of the *Annual Review of Condensed Matter Physics* (ARCMP), a position that gave me yet other ways to explore the multidisciplinary nature of the modern physical sciences. Starting the ARCMP was a remarkably good job for me; but, as a matter of personal principle, I stepped down from it after six years, well before I might have stopped enjoying it.

THEORETICAL PHYSICS: 2016

It seems to me that theoretical physics—and fundamental science in general—is intellectually more prosperous now than it has been at any other time in my career. Recent advances in instrumentation and computational power are mind-boggling. We can watch what is happening inside complex materials atom by atom, and we can watch galaxies form at the outer edges of the Universe. We can simulate such behaviors on modern supercomputers, and I can solve complicated equations and analyze huge amounts of data on my laptop while traveling on an airplane. Each time we solve one problem, we find we have opened new fields of inquiry that are every bit as important and challenging as the ones we have just conquered. The quantum theory of gravity becomes more mysterious the more my colleagues inquire into it. Biology seems to be turning into physics: Whenever we use our physical insight to look more deeply into biological systems, the more amazing they appear.

The other side of this picture, however, is that the sociology of basic research is entirely different from what it was in the 1920s when Peierls began his career or in the 1950s when Walter Kohn and I began ours. The scale of the research enterprise is orders of magnitude larger than it was in those days. Distinctions between basic and applied research have become less distinct and yet, paradoxically, more important for funding decisions. Parts of theoretical physics that Peierls would have argued should be indistinguishable from each other have evolved into separate disciplines competing counterproductively for recognition and financial support. I argued in a *Science* editorial (October 12, 2012) that, despite the best efforts of US funding agencies to promote innovative research, our scientific community has not yet found a sensible way to enable multidisciplinary innovation. Our failure to do so has harmed all of us.

To explain these simultaneously optimistic and pessimistic remarks, I'll return to my random walk through theoretical physics and use my own experiences to illustrate what I mean. The underlying theme of my research has been nonequilibrium physics in various parts of materials science such as solidification processing and strength of materials. In each of these cases, basic understanding is essential for developing predictive theories, and predictive capabilities are essential for useful applications. At bottom, all of these cases are ones in which energy and entropy are caused to flow through materials, moving them away from states of thermal and/or mechanical equilibrium. However, the basic principles that govern such flows have been surprisingly elusive since the days of Ludwig Boltzmann and Josiah Willard Gibbs.

In retrospect, the problem of predicting dendritic solidification patterns was easy. Here, heat flows from the hot to the cold part of a system containing a liquid-solid interface, and latent heat is released at that interface as the solid grows. The challenge is to figure out how these thermal mechanisms determine the interfacial behavior. In this case, the accepted theories of heat transfer and phase transitions are automatically consistent with the laws of thermodynamics. The Mullins-Sekerka instability had provided a crucial starting point in 1963. A decade or so later, when my colleagues and I started to think about this problem, it became clear that surface tension is a mathematically singular perturbation that controls the instability and thus determines what patterns are formed. The phase-field method combined these insights with a scheme for tracking interface motions; but, when we first thought of it at CMU in 1978, this method seemed to pose a prohibitively difficult numerical challenge. With the advent of modern computers, and with the leadership of people like Alain Karma and Jim Warren, two of my UCSB graduate students, it has become one of our most powerful tools for research in solidification processing—a clear example of an important bridge between basic and applied research.

The situation is entirely different in solid mechanics, where a fundamental goal should be to predict the strength and deformability of engineering materials. For decades, leaders of this field have insisted that the basic phenomenon of strain hardening in polycrystalline solids poses an intractably complicated problem and that only phenomenological curve-fitting strategies are feasible. At the same time, they have asserted that statistical concepts such as entropy are irrelevant. Their equations of motion for the density of dislocations, or for the density of flow defects in amorphous materials, have made no use of energy conservation. Nor have their equations been constrained by the second law of thermodynamics so that complex chaotic systems always move toward states of higher probability. I think that these strategies are wrong and that, because of them, this vitally important field has made far too little progress for a very long time.

My own adventures in this area started in the 1990s, when my colleagues and I erroneously thought that if we could understand dendritic instabilities in solidification we ought to be able to understand fracture dynamics. We may have been right in a sense, but we had to make a large number of important mistakes before we began to see what was happening. Now, in 2016, we may finally be making some progress, but I warn the reader that my views remain utterly heretical among many of my colleagues.

In the late 1990s, having concluded that the conventional cohesive-zone models of propagating cracks were mathematically ill posed for the study of crack-bending or tip-splitting instabilities, Michael Falk and I decided to use molecular dynamics simulations to look at fracture in simple models of amorphous—rather than crystalline—materials. Michael quickly noticed that crack advance in these systems was determined by extended plastic deformation in the neighborhood of the crack tip. We also realized that we had no first-principles theory to describe that kind of plasticity, so we forgot briefly about fracture and looked instead at uniform glassy materials flowing in simple shear. Then, with very little help from me, Michael wrote his prize-winning PhD thesis in which he introduced his "D-squared-minimum" method to identify spontaneously occurring, localized, shear-transformation zones (STZs) and showed that his statistical equations of motion for these ephemeral flow defects predicted the observed plastic behaviors, including yielding transitions.

Falk's results triggered a series of advances that are continuing to this day. Eran Bouchbinder and I reformulated the nonequilibrium statistical physics of plastically deforming systems, focusing on the roles played by internal state variables such as the density of STZs. We also proposed a thermodynamic definition of an effective temperature that measures the degree of disorder of the atomic configurations of such systems. This effective temperature differs from the ambient temperature when the configurational states are forced out of equilibrium with the thermal fluctuations. The coupled equations of motion for the STZ density, the effective temperature, and the elastic stress fields are roughly analogous to the phase-field equations in solidification. Both describe spatially varying flows of energy and entropy. Lisa Manning has shown that these STZ equations predict shear-banding instabilities. More recently, Chris Rycroft and Eran Bouchbinder have used these equations to predict the fracture toughness of bulk metallic glasses, which exhibit an experimentally verified brittle-to-ductile transition as a function of annealing time. There is much more to do along these lines, but it seems that we are now making progress.

As I write this account, I think I am seeing the beginnings of a parallel set of developments for dislocation-driven deformations of polycrystalline solids. Again, the theory starts with equations of motion for flows of energy and entropy, and these equations are supplemented by an equation of motion for the effective temperature of the disordered crystal within which the dislocations are moving. It appears that I can use these equations to solve some long standing puzzles in strain hardening observations, and perhaps also to understand strain localization and fracture. But it is too early to be sure.

One of my main reasons for mentioning the dislocations is that they bring me back to my original theme. Dislocation theory once was a part of a multidisciplinary effort to understand the mechanical properties of materials. Peierls published a paper on dislocations in 1940 at the same time that he was thinking about the possibility of nuclear weapons. He never returned to dislocations, and I don't remember him ever mentioning them in Birmingham or on his visits to Santa Barbara later in his career. He claimed in his autobiography that he was surprised to learn, decades after doing the work, that the "Peierls stress" had become a common term in the literature. Nevertheless, he did consider dislocation dynamics to be a part of theoretical physics. Another prominent condensed matter theorist who made major contributions to dislocation theory was Jacques Friedel, an old friend of Walter Kohn who visited the ITP several times. But somehow—unlike in solidification theory or the condensed matter theories that are relevant to quantum devices—the bridges between theoretical physics and applied solid mechanics have mostly disappeared in 2016. At the moment, I am doing the best I can to fix this problem.

Although I'm concerned about connections between some related scientific specialties, I'm pleased by the spirit of courageous innovation that I see in many other areas of theoretical physics these days. Here are three examples. Perhaps the most wildly ambitious of these is the joint effort by cosmologists and quantum theorists to understand the fundamental nature of our Universe using pure logic—e.g., Einsteinian thought experiments and symmetry principles. This effort is inspired by the amazing flood of astronomical information that has suddenly become available to us. I'm similarly impressed by my colleagues who are trying to understand human thought processes starting from experimental information about neurons. It seems to be the height of *chutzpah* to think that we human beings might be smart enough to understand ourselves in such a fundamental way, but I'd be disappointed if some of us weren't probing possibilities in that direction. Finally, I have a special reason for mentioning the attempt by Nigel Goldenfeld and the late Carl Woese to use statistical mechanics to understand the evolutionary origin of the genetic code. Nigel was one of the first postdocs who came to work with me when I moved to Santa Barbara. He had been recommended to me by Sam Edwards, whom I had known since our days with Peierls in Birmingham. Nigel has made important contributions in theories of pattern formation and high-temperature superconductivity as well as in evolutionary biology. He and the others I've mentioned must certainly be making mistakes and may be off on entirely wrong tracks. But they are carrying on in the open-minded, multidisciplinary, and risk-taking tradition that I believe is essential for theoretical physics.

AFTERWORD

Among the greatest pleasures of my career has been the opportunity to work with many wonderful people—senior and junior colleagues, and especially postdoctoral and graduate students. Many of the latter have gone on to become versatile and influential scientists and community leaders. But there are far too many such people for me to talk about each of them individually without changing the nature and purpose of this article. Maybe, at some time in the future, I'll write a different kind of memoir.

DISCLOSURE STATEMENT

The author is unaware of any affiliations, memberships, funding, or financial holdings that might affect the objectivity of this review.

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Finally, in support of my contention that we are now seeing progress in predicting the strength of solid materials, I refer the reader to several recent publications.

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Annual Review of Condensed Matter Physics

Volume 8, 2017

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Errata

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