Quarks & Quirks among friends

"What could be worse than a bunch of physicists gathering in a corner at a cocktail party to discuss physics?" asks Pete Carruthers. We at *Los Alamos Science* frankly didn't know what could be worse. . .or better, for that matter. However we did find the idea of "a bunch of physicists gathering in a corner to discuss physics" quite intriguing. We felt we might gain some insight and, at the same time, provide them with an opportunity to say things that are never printed in technical journals. So we gathered together a small bunch of four, Pete Carruthers, Stuart Raby, Richard Slansky, and Geoffrey West, found them a corner in the home of physicist and neurobiologist George Zweig and turned them loose. We knew it would be informative; we didn't know it would be this entertaining.

WEST: I have here a sort of "fractalized" table of discussion, the first topic being, "What is particle physics, and what are its origins?" Perhaps the older gentlemen among us might want to answer that.

CARRUTHERS: Everyone knows that older gentlemen don't know what particle physics is.

ZWEIG: Particle physics deals with the structure of matter. From the time people began wondering what everything was made of, whether it was particulate or continuous, from that time on we had particle physics.

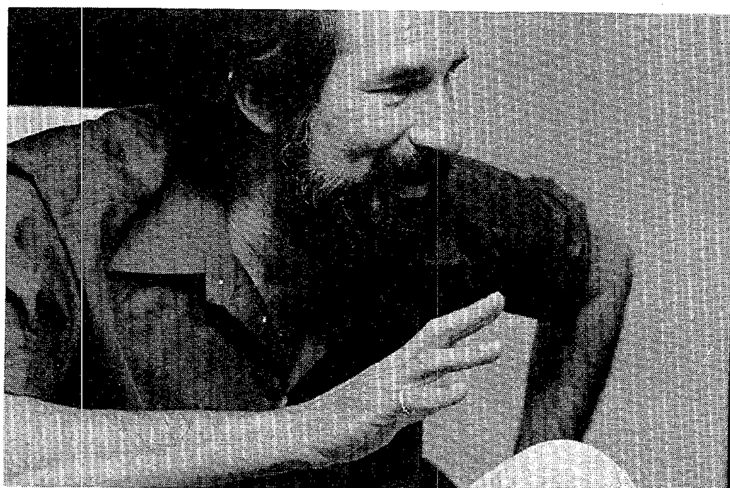
WEST: In that sense of wondering about the nature of matter, particle physics started with the Greeks, if not observationally, at least philosophically.

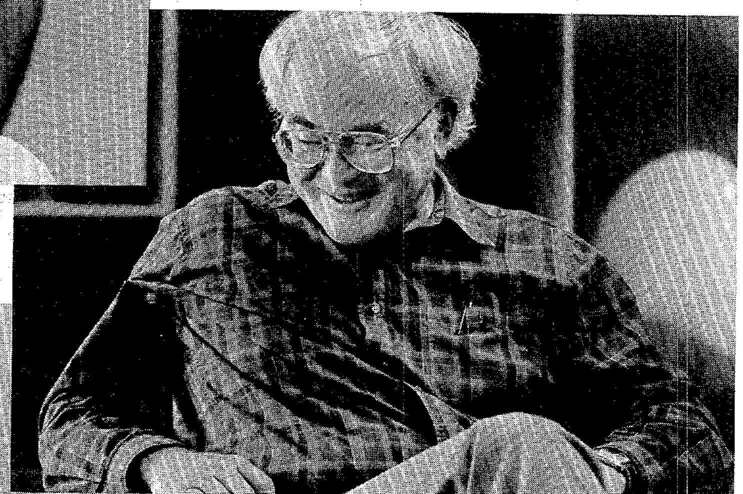
ZWEIG: I think one of the first experimental contributions to particle physics came around 1830 with Faraday's electroplating experiments, where he showed that it would take certain quantities of electricity that were integral multiples of each other to plate a mole of one element or another onto his electrodes.

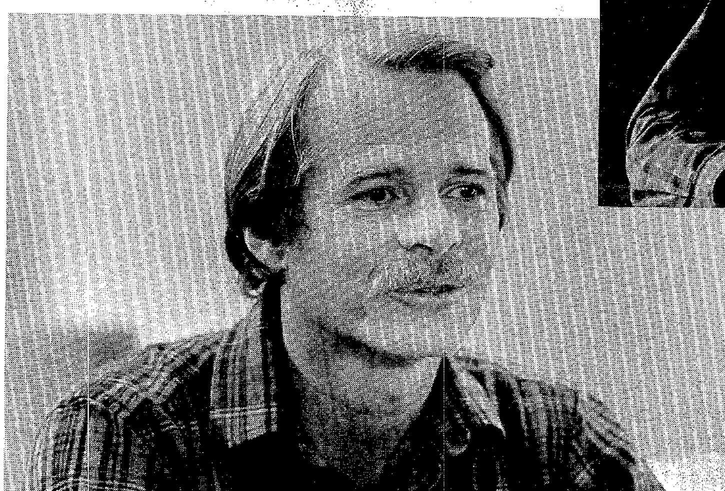
An even earlier contribution was Brown's observation of the motion of minute particles suspended in liquid. We now know the chaotic motion he observed was caused by the random collision of these particles with liquid molecules.

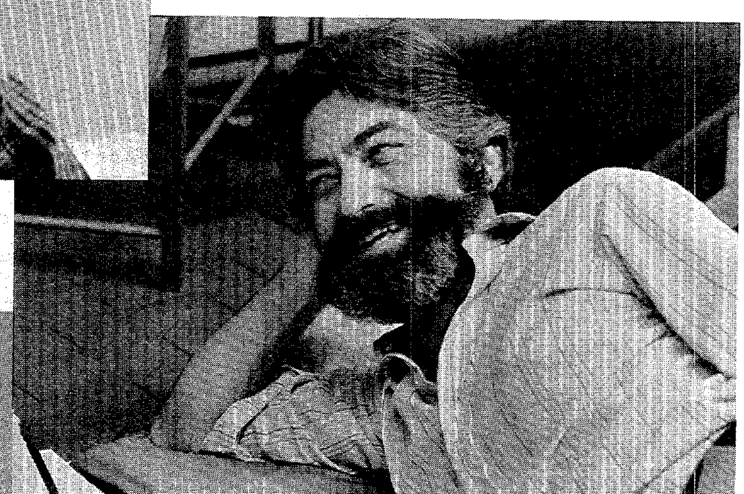
RABY: So Einstein's study of Brownian motion is an instance of somebody doing particle physics?

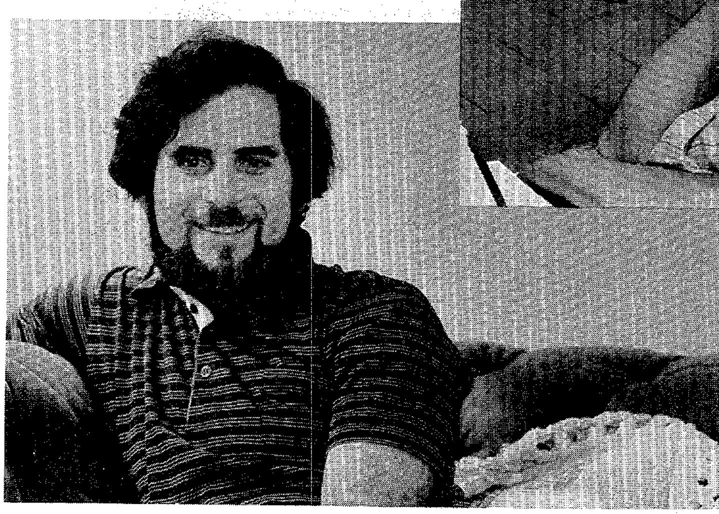
ZWEIG: Absolutely. There's a remarkable description of Brown's work by Darwin, who was a friend of his. It's interesting that Darwin, incredible observer of nature though he was, didn't recognize the chaotic nature of the movement under Brown's microscope; instead, he assumed he was see-











ing "the marvelous currents of protoplasm in some vegetable cell." When he asked Brown what he was looking at, Brown said, "That is my little secret."

SLANSKY: Quite a bit before Brown, Newton explained the sharp shadows created by light as being due to its particulate nature. That's really not the explanation from our present viewpoint, but it was based on what he saw.

CARRUTHERS: Newton was only half wrong. Light, like everything else, does have its particulate aspect. Newton just didn't have a way of explaining its wave-like behavior. That brings us to the critical concept of field, which Faraday put forward so clearly. You can speak of particulate structure, but when you bring in the field concept, you have a much richer, more subtle structure: fields are things that propagate like waves but materialize themselves in terms of quanta. And that is the current wisdom of what particle physics is, namely, quantized fields.

Quantum field theory is the only conceptual framework that pieces together the concepts of special relativity and quantum theory, as well as the observed group structure of the elementary particle spectrum. All these things live in this framework, and there's nothing to disprove its structure. Nature looks like a transformation process in the framework of quantum field theory. Matter is not just pointy little particles; it involves the more ethereal substance that people sometimes call waves, which in this theory are subsumed into one unruly construct, the quantized field.

ZWEIG: Particle physics wasn't always quantized field theory. When I was a graduate student, a different philosophy governed: S-matrix theory and the bootstrap hypothesis.

CARRUTHERS: That was a temporary aberration.

ZWEIG: But a big aberration in our lives! S-matrix theory was not wrong, just largely irrelevant.

RABY: If particle physics is the attempt to understand the basic building blocks of nature, then it's not a static thing. Atomic physics at one point was particle physics, but once you understood the atom, then you moved down a level to the nucleus, and so forth.

WEST: Let's bring it up to date, then. When would you say particle physics turned into high-energy physics?

ZWEIG: With accelerators.

SLANSKY: Well, it really began around 1910 with the use of the cloud chambers to detect cosmic rays; that is how Anderson detected the positron in 1932. His discovery straightened out a basic concept in quantized field theory, namely, what the antiparticle is.

CARRUTHERS: Yes, in 1926 Dirac had quantized the electromagnetic field and had given wave/particle duality a respectable mathematical framework. That framework predicted the positron because the electron had to have a positively charged partner. Actually, it was Oppenheimer who predicted the positron. Dirac wanted to interpret the positive solution of his equation as a proton, since there were spare protons sitting around in the world. To make this interpretation plausible, he had to invoke all that hanky-panky about the negative energy sea being filled—you could imagine that something was screwy.

SLANSKY: Say what you will, Dirac's idea was a wonderful unification of all nature, much more wonderful than we can envisage today.

ZWEIG: Ignorance is bliss.

SLANSKY: There were two particles, the proton and the electron, and they were the basic structure of all matter, and they were, in fact, manifestations of the same thing in field theory. We have nothing on the horizon that promises such a magnificent unification as that.

RABY: Weren't the proton and the electron supposed to have the same mass according to the equation?

WEST: No, the negative energy sea was supposed to take care of that.

CARRUTHERS: It was not unlike the present trick of explaining particle masses through spontaneous symmetry breaking. Dirac's idea of viewing the proton and the electron as two different charge states of the same object was a nice idea that satisfied all the desires for symmetries that lurk in the hearts of theorists, but it was wrong. And the reason it was wrong, of course, is that the proton is the wrong object to compare with the electron. It's the quark and the electron that may turn out to be different states of a single field, a hypothesis we call grand unification.

WEST: Well, it is certainly true that high-energy particle physics now is cloaked in the language of quantized field theory, so much so that we call these theories the standard model.

CARRUTHERS: But I think we're overlooking the critical role of Rutherford in inventing particle physics.

WEST: The experiments of alpha scattering on gold foils to discern the structure of the atom.

ZWEIG: Rutherford established the paradigm we still use for probing the structure of matter: you just bounce one particle off another and see what happens.

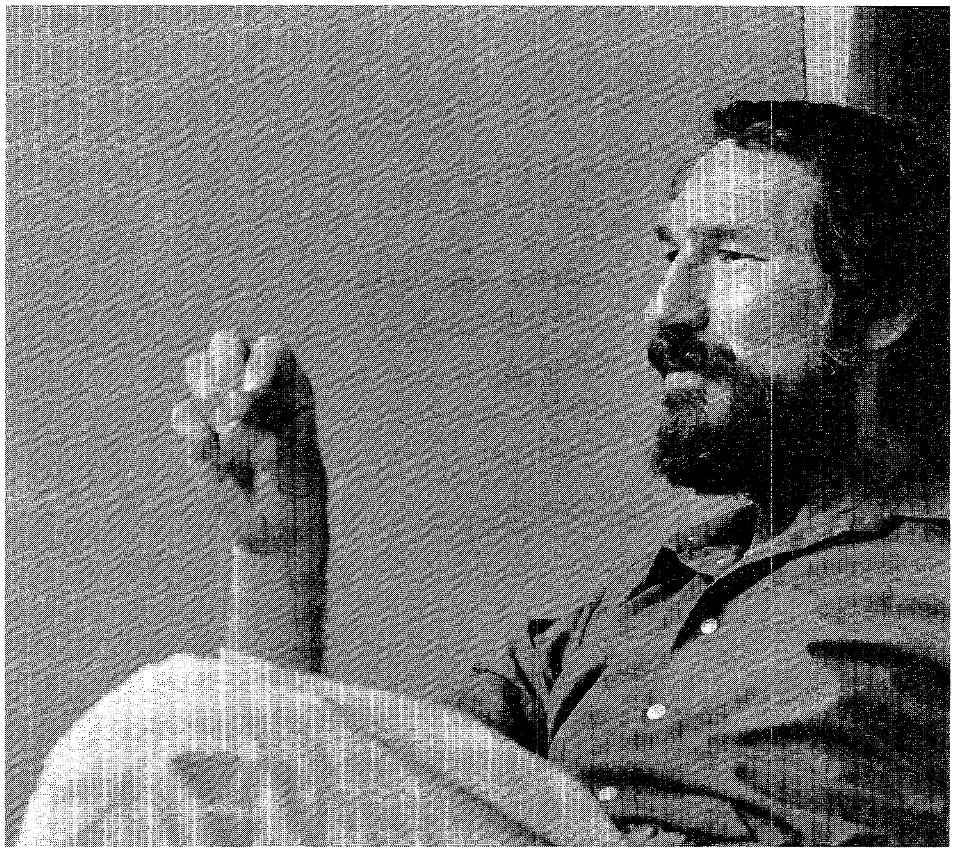
CARRUTHERS: In fact, particle physics is a continuing dialogue (not always friendly) between experimentalists and theorists. Sometimes theorists come up with something that is interesting but that experimentalists suspect is wrong, even though they will win a Nobel prize if they can find the thing. And what the experimentalists do discover is frequently rather different from what the theorists thought, which makes the theorists go back and work some more. This is the way the field grows. We make lots of mistakes, we build the wrong machines, committees decide to do the wrong experiments, and journals refuse to publish the right theories. The process only works because there are so many objective entrepreneurs in the world who are trying to find out how matter behaves under these rather extreme conditions. It is marvelous to have great synthetic minds like those of Newton and Galileo, but they build not only on the work of unnamed thousands of theorists but also on these countless experiments.

"To understand the universe that we feel and touch, even down to its minutiae, you don't have to know a damn thing about quarks."

WEST: Perhaps we should tell how we personally got involved in physics, what drives us, why we stay with it. Because it is an awfully difficult field and a very frustrating field. How do we find the reality of it compared to our early romantic images? Let's start with Pete, who's been interviewed many times and should be in practice.

CARRUTHERS: I was enormously interested in biology as a child, but I decided that it was too hard, too formless. So I thought I'd do something easy like physics. Our town library didn't even have modern quantum mechanics books. But I read the old quantum mechanics, and I read Jeans and Eddington and other inspirational books filled with flowery prose. I was very excited about the mysteries of the atom. It was ten years before I realized that I had been tricked. I had imagined I would go out and learn about the absolute truth, but after a little bit of experience I saw that the "absolute truth" of this year is replaced next year by something that may not even resemble it, leaving you with only some small residue of value. Eventually I came to feel that science, despite its experimental foundation and reference frame, shares much with other intellectual disciplines like music, art, and literature.
WEST: Dick, what about you?
SLANSKY: In college I listed myself as a physics major, but I gave my heart to philosophy and writing fiction. I had quite a hard time with them, too, but physics and mathematics remained easy. However, since I didn't see physics as very deep, I decided after I graduated to look at other fields. I spent a year in the Harvard Divinity School, where I found myself inadvertently a spokesman for science. I took Ed Purcell's quantum mechanics course in order to be able to answer people's questions, and it was there that I found myself, for the first time, absolutely fascinated by physics.

During that year I had been accepted at Berkeley as a graduate student in philosophy, but in May I asked them whether I could switch to physics. They wrote back saying it

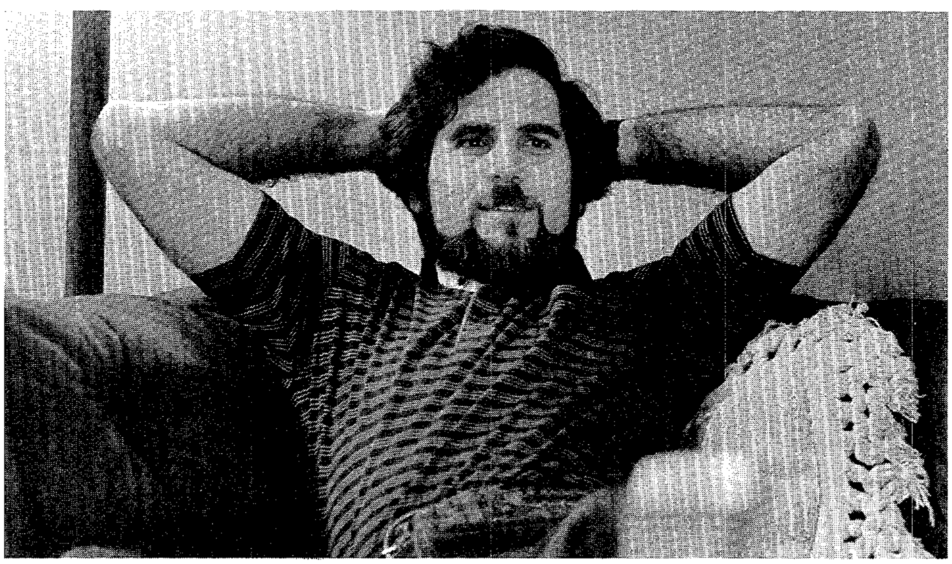


GEOFFREY B. WEST: "One of the great things that has happened in particle physics is that some of . . . the wonderful, deep questions . . . are being asked again . . . Somehow we have to understand why there is a weak scale, why there is an electromagnetic scale, why a strong scale, and ultimately why a grand scale."

would be fine. I don't know that one can be such a dilettante these days.
SCIENCE: Why were people in Divinity School asking about quantum mechanics?
SLANSKY: People hoped to gain some insight into the roles of theology and philosophy from the intellectual framework of science. In the past certain philosophical systems have been based on physical theories. People were wondering what had really happened with quantum mechanics, since no philosophical system had been built upon it. Efforts have been made, but none so successful as Kant's with Newtonian physics, for example.
CARRUTHERS: Particle physics doesn't stand still for philosophy. The subject is such that as soon as you understand something, you move on. I think restlessness characterizes this particular branch of science, in fact.
SLANSKY: I never looked at science as something I wanted to learn that would be absolutely permanent for all the rest of the history of mankind. I simply enjoy the doing of the physics, and I enjoy cheering on other people who are doing it. It is the intellectual excitement of particle physics that draws me to it.
ZWEIG: Dick, was there some connection, in your own mind, between religion and physics?

"The real problem was that you had a zoo of particles, with none seemingly more fundamental than any other."

SLANSKY: Some. One of the issues that concerned me was the referential mechanisms of theological language. How we refer to things. In science we also have that concern, very much so.
ZWEIG: What do you mean by "How we refer to things"?
SLANSKY: When we use a word to refer to *God* or to refer to great generalizations in our experience, how does the word work to refer beyond the language? Language is just a sound. How does the word refer beyond just the mere word to the total experience? I've never really solved that problem in my own mind.
CARRUTHERS: When you mention the word *God,* isn't there a pattern of signals in your mind that corresponds to the pattern of sound? Doesn't *God* have a peculiar pattern?
SLANSKY: The referential mechanisms of theological language became a major concern around 1966, after I'd left Harvard Divinity. Before that the school was under the influence of the two great theologians Paul Tillich and Reinhold Niebuhr. Their concern was with the eighteenth and nineteenth century efforts to put into some sort of theoretical or logical framework all of man and his nature. I found myself swept up much more into theological and philosophical issues than into the study of ethics.
ZWEIG: Do you think these issues lie in the domain of science now? Questions about what man is, what his role in nature is, and what nature itself is, are being framed and answered by biologists and physicists.
SLANSKY: I don't view what I am trying to do in particle physics as finding man's place in nature. I think of it as a puzzle made of a lot of experimental data, and we are trying to assemble the pieces.
CARRUTHERS: But the attitudes are very theological, and often they tend to be dogmatic.
SLANSKY: I would like to make a personal statement here. That is, when I go out for a walk in the mountains, enjoying the beauties of nature with a capital *N*, I don't feel that that has any very direct relationship to formulating a theory of nature. While my personal experience may set my mind in motion, may provide some inspiration, I don't feel that seeing the Truchas peaks or seeing wild flowers in the springtime is very closely related to my efforts to build a theory.
WEST: Along that line I have an apocryphal story about Hans and Rose Bethe. One summer's evening when the stars were shining and the sky was spectacular, Rose was exclaiming over their beauty. Allegedly Hans replied, "Yes, but you know, I think I am the only man alive that knows why they shine." There you have the difference between the romantic and the scientific views.
RABY: Particle physics to me is a unique marriage of philosophy and reality. In high school I read the philosopher George Berkeley, who discusses space and time and tries to imagine what space would be like were there nothing in it. Could there be a force on a particle were there nothing else in space? Obviously a particle couldn't move because it would have nothing to move with respect to. Particle physics has the beauty of philosophy constrained by the fact you are working with observable reality. For a science fair in high school I built a cloud chamber and tried to observe some alpha particles and beta particles. That's the reality part: you can actually build an experiment and actually see some of these fundamental objects. And there are people who are brilliant enough, like Einstein, to relate ideas and thought to reality and then make predictions about how the world must be. Special relativity and all the Gedanken experiments, which are basically philosophical, say how the world is. To me what particle physics means is that you can have an idea, based on some physical fact, that leads to some experimental prediction. That is beautiful, and I don't know how you define beauty except to say that it's in the eye of the beholder.
ZWEIG: How was science viewed in your family?
RABY: No one understood science in my family.
ZWEIG: Well, did they respect it even if they



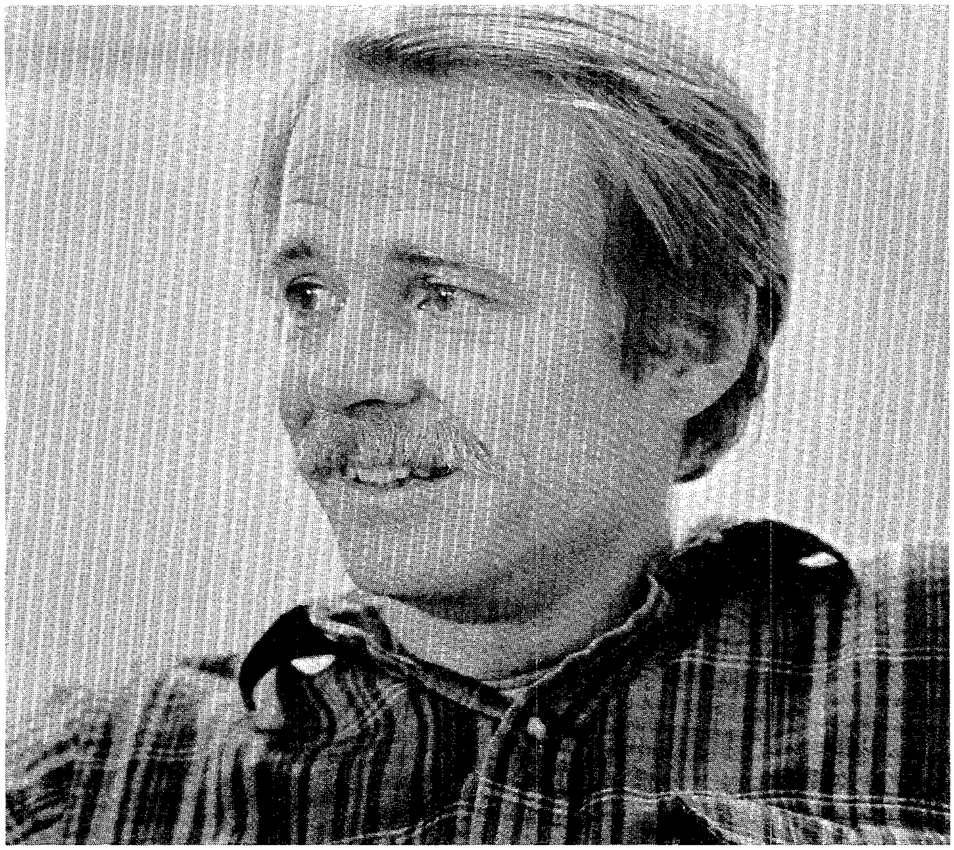
STUART A. RABY: "I think what particle physics means to me is this unique intermarriage of philosophy and reality. . . . Particle physics has the beauty of philosophy constrained by the fact that you are working with observable reality. . . . If you have a beautiful idea and it leads to a prediction that, in fact, comes true, that would be the most amazing thing. That you can understand something on such a fundamental level!"

". . . . one thing that distinguishes physics from philosophy is predictive power. The quark model had a lot of predictive power."

didn't understand it?
RABY: I guess they accepted the fact that I would pursue what interested me. I'm the first one in my family to finish college, and that in itself is something big to them. My grandfather, who does understand a little, has read about Einstein. My grandfather's interest in science doesn't come from any particular training, but from the fact that he is very inventive and intuitive and puts radios together and learns everything by himself.
ZWEIG: Was he respected for it?
RABY: By whom? My grandfather owned a chicken market, so he did these things in his spare time.
WEST: That's interesting. I have to admit I am another person who got into physics in spite of himself. I was facile in mathematics but more keen on literature. I turned to natural sciences when I went to Cambridge only because I had begun reading Jeans and Eddington and all those early twentieth century visionaries. They were describing that wonderful time of the birth of quantum mechanics, the birth of relativity, the beginning of thinking about cosmology and the origin of the universe. Wonderful questions! Really important questions that dovetailed into the big questions raised by literature. What is it all really about, this mysterious universe?

The other crucial reason that I went into science was that I could not stand the world of business, the world of the wheelerdealer, that whole materialistic world. Somehow I had an image of the scientist as removed from that, judged only by his work, his only criteria being proof, knowledge, and wisdom. I still hold that romantic image. And that has been my biggest disappointment, because, of course, science, like everything else that involves millions of dollars, has its own wheeler-dealers and salesmen and all the rest of it.

My undergraduate experience at Cambridge was something of a disaster in terms of physics education, and I was determined to leave the field. I had become very interested in West Coast jazz and managed to obtain a fellowship to Stanford where, for a year, I could be near San Francisco, North Beach, and that whole scene. Although at first I hated Palo Alto, my physics courses were on so much more a professional level, so much more an exciting level, that my attitude eventually changed. Somehow the whole world opened up. But even in graduate school I would go back to reading Eddington, whether he were right or not, because his language and way of thinking were inspirational, as of course, were Einstein's.
CARRUTHERS: Do you think our visions have become muddied in these modern times?
WEST: I don't think so at all. One of the great things that has happened in particle physics is that some of the deep questions are being asked again. Not that I like the proposed answers, particularly, but the questions are being asked. George, what do you say to all this? You often have a different slant.
ZWEIG: My parents came from eastern Europe—they fled just before the second World War. I was born in Moscow and came to this country when I was less than two years old. Most of my family perished in the war, probably in concentration camps. I learned



RICHARD A. SLANSKY: "It is the intellectual excitement of particle physics that draws me to it, really . . . . I find particle physics an intriguing effort to try to explain and understand, in a very special way, what goes on in nature . . . . I enjoy the effort . . . . I enjoy cheering on other people who are trying . . . . I think of it as a puzzle made of a lot of experimental data, and we are trying to assemble all the pieces."

"It is an old Jewish belief that ideas are what really matter. If you want to create things that will endure, you create them in the mind of man."

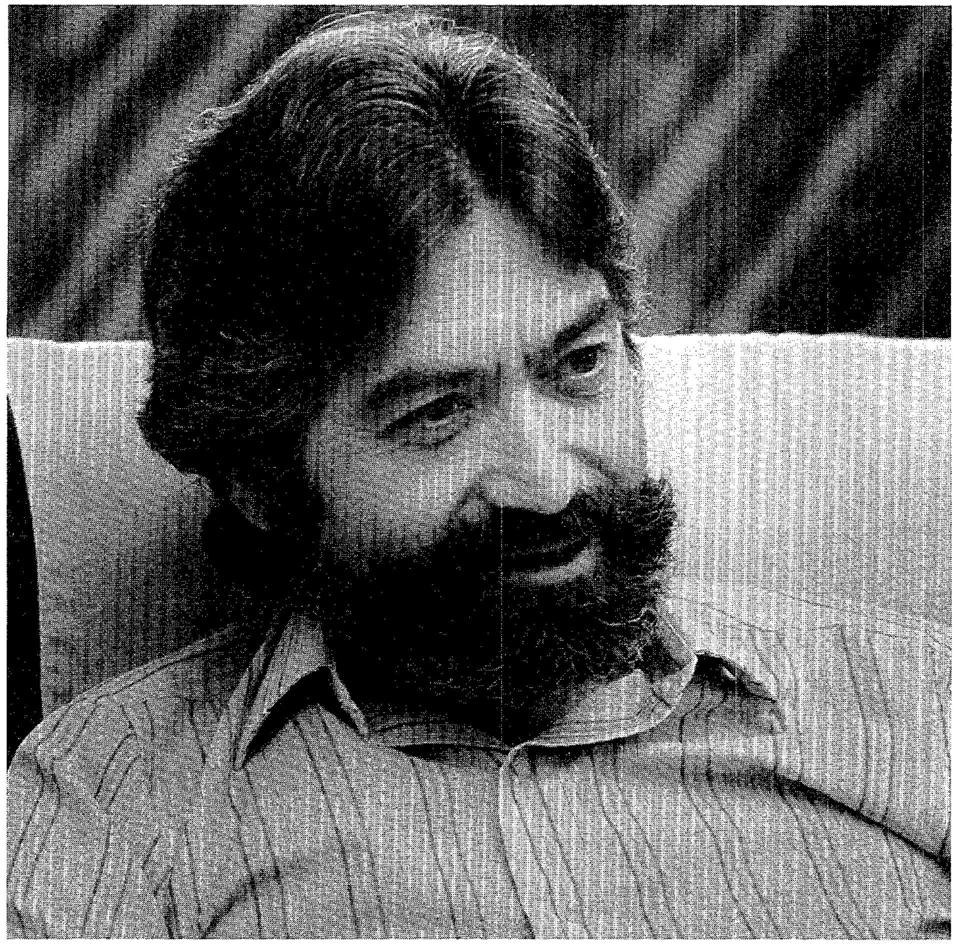
at a very early age from the example of my father, who was wise enough to see the situation in Germany for what it really was, that it is very important to understand reality. Reality is the bottom line. Science deals with reality, and psychology with our ability to accept it.

I grew up in a rough, integrated neighborhood in Detroit. Much of it subsequently burned down in the 1967 riot. I hated school and at first did very poorly. I was placed in a "slower" non-college preparatory class and took a lot of shop courses. Although I did not like being viewed as a second class citizen, I thought that operating machines was a hell of a lot more interesting than discussing social relations with my classmates and teachers.

Eventually I was able to do everything that was asked of me very quickly, but the teachers were not knowledgeable, and classes were boring. In order to get along I kept my mouth shut. Occasionally I acted as an expediter, asking questions to help my classmates.

At that time science and magic were really one and the same in my mind, and what child isn't fascinated by magic? At home I did all sorts of tinkering. I built rockets that flew and developed my own rocket fuels. The ultimate in magic was my tesla coil with a six foot corona emanating from a door knob.

College was a revelation to me. I went to the University of Michigan and majored in mathematics. For the first time I met teachers who were smart. And then I went to Caltech, a place I had never even heard of six months before I arrived. At Caltech I was very fortunate to work with Alvin Tollestrup, an experimentalist who later designed the superconducting magnets that are used at Fermilab. And I was exposed to Feynman and Gell-Mann, who were unbelievable individuals in their own distinctive ways. That was an exciting time. Shelly Glashow was a postdoc. Ken Wilson, Hung Cheng, Roger Dashen, and Sidney Coleman were graduate students. Rudy Mössbauer was down the hall. He was still a research fellow one month before he got the Nobel prize. The board of trustees called a



GEORGE ZWEIG: "I learned at a very early age. . .that reality is the bottom line. Science deals with reality, and psychology with our ability to accept it."

crash meeting and promoted him to full professor just before the announcement. I remember pleading with Dan Kevles in the history department to come over to the physics department and record the progress, because science history was in the making, but he wouldn't budge. "You can never tell what is important until many years later," he said.

CARRUTHERS: I've forgotten whether I first met all of you at Cal Tech or at Aspen.

ZWEIG: Wherever Pete met us, I know we're all here because of him. He was always very gently asking me, "How about coming to Los Alamos?" Eventually I took him up on his offer.

WEST: Before we leave this more personal side of the interview, I want to ask a question or two about families. Is it true that physicists generally come from middle class and lower backgrounds? Dick, what about your family?

SLANSKY: My father came from a farming family. Since he weighed only ninety-seven pounds when he graduated from high school, farm work was a little heavy for him. He entered a local college and eventually earned a graduate degree from Berkeley as a physical chemist. My mother wanted to attend medical school and was admitted, but back in those days it was more important to have children. So I am the result rather than her becoming a doctor.

CARRUTHERS: My father grew up on a farm in Indiana, was identified as a bright kid, and was sent off to Purdue, where he became an engineer. So I at least had somebody who believed in a technical world. However, when I finally became a professor at Cornell, my parents were a bit disappointed because in their experience only those who couldn't make it in the business world became faculty members.

WEST: What about your parents, George?

ZWEIG: Both my parents are intellectuals, people very much concerned with ideas. To me one of the virtues of doing science is that you contribute to the construction of ideas, which last in ways that material monuments don't. It is an old Jewish belief that ideas are what really matter. If you want to create things that will endure, you create them in the mind of man.

WEST: What did your parents do?

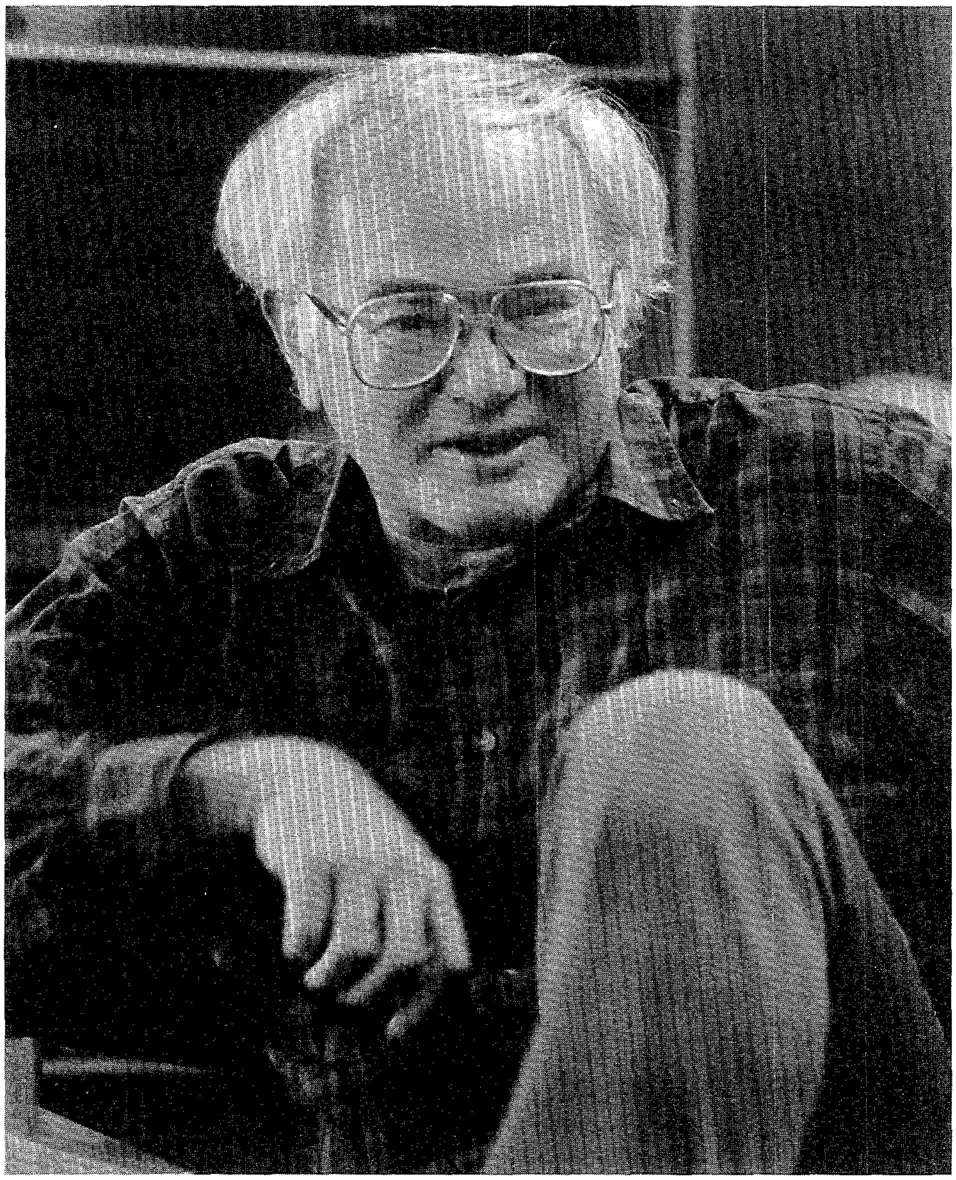
ZWEIG: My mother was a nursery school teacher. She studied in Vienna in the '20s, an exciting time. Montessori was there; Freud was there. My father was a structural engineer. He chose his profession for political reasons, because engineering was a useful thing to do.

WEST: Then all three of you have scientific or engineering backgrounds. My mother is a dressmaker, and my father was a professional gambler. But he was an intellectual in many ways, even though he left school at fifteen. He read profusely, knew everything superficially very well, and was brilliant in languages. He wasted his life gambling, but it was an interesting life. I think I became facile in mathematics at a young age just because he was so quick at working out odds, odds on dogs and horses, how to do triples and doubles, and so on.

CARRUTHERS: Are we all firstborn sons? I think we are, and that's an often quoted statistic about scientists.

WEST: Have we all retreated into science for solace?

RABY: It's more than that. At one time I felt divided between going into social work in



PETER A. CARRUTHERS: "There's no point in a full-blown essay on quantum field theory because it's probably wrong anyway. That's what fundamental science is all about—whatever you're doing is probably wrong. That's how you know when you're doing it. Once in a while you're right, and then you're a great man, or woman nowadays. I've tried to explain this before to people, but they're very slow to understand. What you have to do is look back and find what has been filtered out as correct by experiments and a lot of subsequent restructuring. Right? But when you're actually doing it, almost every time you're wrong. Everybody thinks you sit on a mountaintop communing with Jung's collective unconscious, right? Well you try, but the collective unconscious isn't any smarter than you are."

"Why do the forces in nature have different strengths . . . That's one of those wonderful deep questions that has come back to haunt us."

order to be involved with people or going into science and being involved with ideas. It was continually on my mind, and when I graduated from college, I took a year off to do social work. I worked in a youth house in the South Bronx as a counselor for kids between the ages of seven and seventeen. They were all there waiting to be sentenced, and they were very self-destructive kids. The best thing you could do was to show them that they should have goals and that they shouldn't destroy themselves when the goals seemed out of reach. For example, a typical goal was to get out of the place, and a typical reaction was to end up a suicide. I kept trying to tell these kids, "Do what you enjoy doing and set a goal for yourself and try to fulfill that goal in positive ways." In the end I was convinced by my own logic that I should return to physics.

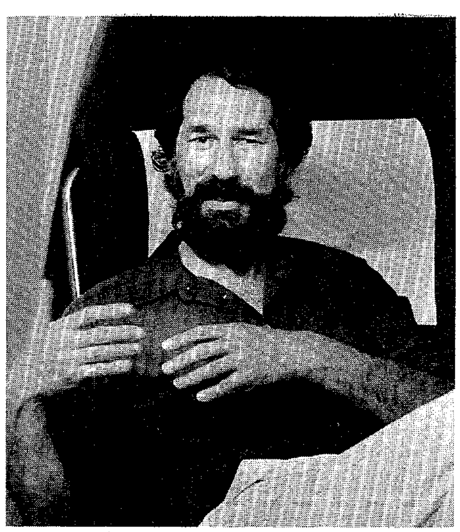
WEST: Let's discuss the way physics affects our personal lives now that we are grown men. Suppose you are at a cocktail party, and someone asks, "What do you do?" "I am a physicist," you say, "High-energy physics," or "Particle physics." Then there is a silence and it is very awkward. That is one response, and here is the other. "Oh, you do particle physics? My God, that's exciting stuff! I read about quarks and couldn't understand a word of it. But then I read this great book, *The Tao of Physics.* Can you tell me what you do?" I groan inwardly and sadly reflect on how great the communication gap is between scientists such as ourselves and the general public that supports us. We seem to have shirked our responsibility in communicating the fantastic ideas and concepts involved in our enterprise to the masses. It is a sobering thought that Capra's book, which most of us don't particularly like because it represents neither particle physics nor Zen accurately, is probably unique in turning on the layman to some aspects of particle physics. Whatever your views of that book may be, you've certainly got to appreciate what he's done for the publicity of the field. As for me, I find it difficult to talk about this life that I love in two-line sentences.

Now, the cocktail party is just a superficial aspect of my social life, but the problem enters in a more crucial way in my relationship with my family, the people dear to me. Here is this work which I love, which I spend a majority of my time in doing, and from which a large number of the frustrations and disappointments and joys in my life come, and I cannot communicate it to my family except in an incredibly superficial way.

SLANSKY: The cocktail party experiences that Geoffrey describes are absolutely perfect, and I know what he means about the family. Now that my children are older, they are into science, and sometimes they ask me questions at the dinner table. I try to give clear explanations, but I'm never sure I've succeeded even superficially. And my wife, who is very bright but has no science background, doesn't hesitate to say that science in more than twenty-five words is boring. Sometimes, in fact, I feel that my doing physics is viewed by them as a hobby.

CARRUTHERS: Socially, what could be worse than a bunch of physicists gathering in a corner at a cocktail party to discuss physics?

RABY: I find there are two types of people. There are people who ask you a question just to be polite and who don't really want an answer. Those people you ignore. Then there are people who are genuinely interested, and you talk to them. If they don't understand what a quark is, you ask them if they understand what a proton or an electron is. If they don't understand those, then you ask them if they know what an atom is. You describe an atom as electrons and a nucleus of protons and neutrons. You go down from there, and you eventually get to what you are studying—particle physics.



WEST: Does particle physics affect your relationship with your wife?

RABY: My wife is occasionally interested in all this. My son, however, is genuinely interested in all forms of physical phenomena and is constantly asking questions. He likes to hear about gravity, that the gravity that pulls objects to the earth also pulls the moon around the earth. I have to admit that I find his interest very rewarding.

WEST: Maybe, since we've been given the opportunity today, we should start talking about physics. Particle physics has gone through a minirevolution since the discovery of the psi/J particle at SLAC and at Brookhaven ten years ago. Although not important in itself, that discovery confirmed a whole way of thinking in terms of quarks, symmetry principles, gauge theories, and unification. It was a bolt out of the blue at a time when the direction of particle physics was uncertain. From then on, it became clear that non-Abelian gauge theories and unification were going to form the fundamental principles for research. Sociologically, there developed a unanimity in the field, a unanimity that has remained. This has led us to the standard model, which incorporates the strong, weak, and electromagnetic interactions.

SLANSKY: Yes, the standard model is a marvelous synthesis of ideas that have been around for a long time. It derives all interactions from one elegant principle, the principle of local symmetry, which has its origin in the structure of electromagnetism. In the 1950s Yang and Mills generalized this structure to the so-called non-Abelian gauge the-

ories and then through the '60s and '70s we learned enough about these field theories to feel confident describing all the forces of nature in terms of them.
RABY: I think we feel confident with Yang-Mills theories because they are just a sophisticated version of our old concept of force. The idea is that all of matter is made up of quarks and leptons (electrons, muons, etc.) and that the forces or interactions between them arise from the exchange of special kinds of particles called gauge particles: the photon in electromagnetic interactions, the $W^{\pm}$ and Z^0 in the weak interactions responsible for radioactive decay, and the gluons in strong interactions that bind the nucleus. (It is believed that the graviton plays a similar role in gravity.) [The local gauge theory of the strong forces is called quantum chromodynamics (QCD). The local gauge theory that unifies the weak force and the electromagnetic force is the electroweak theory that predicted the existence of the $W^{\pm}$ and Z^0.]
ZWEIG: You are talking about a very limited aspect of what high-energy physics has been. Our present understanding did not develop in an orderly manner. In fact, what took place in the early '60s was the first revolution we have had in physics since quantum mechanics. At that time, if you were at Berkeley studying physics, you studied S-matrix, not field theory, and when I went to Caltech, I was also taught that field theory was not important.
SLANSKY: Yes, the few people that were focusing on Yang-Mills theories in the '50s and early '60s were more or less ignored. Perhaps the most impressive of those early papers was one by Julian Schwinger in which he tried to use the isotopic spin group as a local symmetry group for the weak, not the strong, interactions. (Schwinger's approach turned out to be correct. The Nobel prize-winning SU(2) $\times$ U(1) electroweak theory that predicted the $W^{\pm}$ and Z^0 vector mesons to mediate the weak interactions is an expanded version of Schwinger's SU(2) model.)
WEST: In retrospect Schwinger is a real hero in the sense that he kept the faith and made some remarkable discoveries in field theory during a period when everybody was basically giving him the finger. He was completely ignored and, in fact, felt left out of the field because no one would pay any attention.
SCIENCE: Why was field theory dropped?
CARRUTHERS: Now we reach a curious sociological phenomenon.
RABY: Sociological? I thought the theory was just too hard to understand. There were all those infinities that cropped up in the calculations and had to be renormalized away.
CARRUTHERS: I am afraid there is a phase transition that occurs in groups of people of whatever IQ who feverishly follow each new promising trend in science. They go to a conference, where a guru raises his hands up and waves his baton; everyone sits there, their heads going in unison, and the few heretics sitting out there are mostly intimidated into keeping their heresies to themselves. After a while some new religion comes along, and a new faith replaces the old. This is a curious thing, which you often see at football games and the like.
WEST: The myth perpetrated about field theory was, as Stuart said, that the problems were too hard. But if you look at Yang-Mills and Julian Schwinger's paper, for example, there was still serious work that could have been done. Instead, when I was at Stanford, Sidney Drell taught advanced quantum mechanics and gave a whole lecture on why you didn't need field theory. All you needed were Feynman graphs. That was the theory.
RABY: The real problem was that you had a zoo of particles, with none seemingly more fundamental than any other. Before people knew about quarks, you didn't feel that you were writing down the fundamental fields.
WEST: In 1954 we had all the machinery necessary to write down the standard model. We had the renormalization group. We had local gauge theories.
SLANSKY: But nobody knew what to apply them to.
SCIENCE: George, in 1963, when you came up with the idea that quarks were the constituents of the strongly interacting particles, did you think at all about field theory?
ZWEIG: No. The history I remember is quite different. The physics community responded to this proliferation of particles by embracing the bootstrap hypothesis. No particle was viewed as fundamental; instead, there was a nuclear democracy in which all particles were made out of one another. The idea had its origins in Heisenberg's S-matrix theory. Heisenberg published a paper in 1943 reiterating the philosophy that underlies quantum mechanics, namely, that you should only deal with observables. In the case of quantum mechanics, you deal with spectral lines, the frequencies of light emitted from atoms. In the case of particle physics, you go back to the ideas of Rutherford. Operationally, you study the structure of matter by scattering one particle off another and observing what happens. The experimental results can be organized in a kind of a matrix that gives the amplitudes for the incoming particles to scatter into the outgoing ones. Measuring the elements of this scattering, or S-matrix, was the goal of experimentalists. The work of theorists was to write down relationships that these S-matrix elements had to obey. The idea that there was another hidden layer of reality, that there were objects inside protons and neutrons that hadn't been observed but were responsible for the properties of these particles, was an idea that was just totally foreign to the S-matrix philosophy; so the proposal that the hadrons were composed of more fundamental constituents was vigorously resisted. Not until ten years later, with the discovery of the psi/J particle, did the quark hypothesis become generally accepted. By then the evidence was so dramatic that you didn't have to be an expert to see the underlying structure.
RABY: The philosophy of the bootstrap, from what I have read of it, is a very beautiful philosophy. There is no fundamental particle, but there are fundamental rules of how particles interact to produce the whole spectrum. But one thing that distinguishes physics from philosophy is predictive power. The

"It's important to pick one fundamental question, push on it, and get the right answer . . ."

quark model had a lot of predictive power. It predicted the whole spectrum of hadrons observed in high energy experiments. It is not because of sociology that the bootstrap went out; it was the experimental evidence of J/psi that made people believe there really are objects called quarks that are the building blocks of all the hadrons that we see. It is this reality that turned people in the direction they follow today.

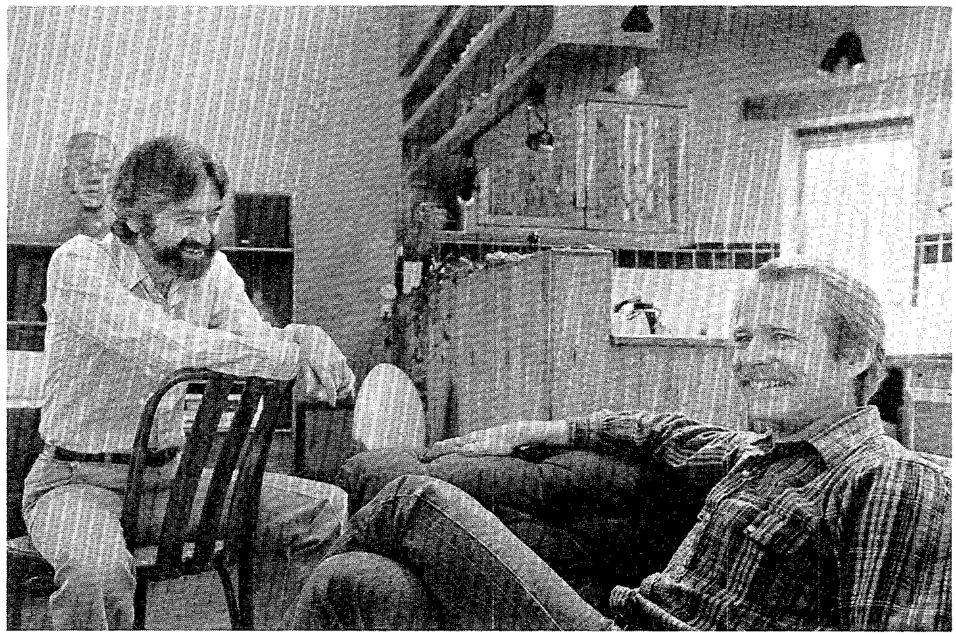
CARRUTHERS: And because of the very intense proliferation of unknowns, it is unlikely that the search for fundamental constituents will stop here. In the standard model you have dozens of parameters that are beyond any experimental reach.

SCIENCE: But you have fewer coordinates now than you had originally, right?

CARRUTHERS: If you are saying the coordinates have all been coordinated by group symmetry, then of course there are many fewer.

WEST: I think the deep inelastic scattering experiments at SLAC played an absolutely crucial role in convincing people that quarks are real. It was quite clear from the scaling behavior of the scattering amplitudes that you were doing a classic Rutherford type scattering experiment and that you were literally seeing the constituents of the nucleon. I think that was something that was extremely convincing. Not only was it qualitatively correct, but quantitatively numbers were coming out that could only come about if you believed the scattering was taking place from quarks, even though they weren't actually being isolated. But let me say one other thing about the S-matrix approach. That approach is really quantum mechanics in action. Everything is connected with everything else by this principle of unitarity or conservation of probability. It is a very curious state of affairs that the quark model, which requires less quantum mechanics to predict, say, the spectrum of particles, has proven to be much more useful.

SLANSKY: Remember, though, there were some important things missing in the bootstrap approach. There was no natural way to incorporate the weak and the electromagnetic forces.



WEST: That picks up another important point; the S-matrix theory could not cope with the problem of scale. And that brings us back to the standard model and then into grand unification. The deep inelastic scattering experiments focused attention on the idea that physical theories exhibit a scale invariance similar to ordinary dimensional analysis.

One of the wonderful things that happened as a result was that all of us began to accept renormalization (the infinite rescaling of field theories to make the answers come out finite) as more than just hocus-pocus. Any graduate student first learning the renormalization procedure must have thought that a trick was being pulled and that the procedure for getting finite answers by subtracting one infinity from another really couldn't be right. An element of hocus-pocus may still remain, but the understanding that renormalization was just an exploitation of scale invariance in the very complicated context of field theory has raised the procedure to the level of a principle.

The focus on scale also led to the feeling that somehow we have to understand why the forces in nature have different strengths and become strong at different energies, why there are different energy scales for the weak, for the electromagnetic, and for the strong interactions, and ultimately whether there may be a grand scale, that is, an energy at which all the forces look alike. That's one of those wonderful deep questions that has come back to haunt us.

RABY: I guess we think of quantum electrodynamics (QED) as being such a successful theory because calculations have been done to an incredibly high degree of accuracy. But it is hard to imagine that we will ever do that well for the quark interactions. The whole method of doing computations in QED is perturbative. You can treat the electromagnetic interaction as a small perturbation on the free theory. But, in order to understand what is going on in the strong interactions of quantum chromodynamics (QCD), you have to use nonperturbative methods, and then you get a whole new feeling about the content of field theory. Field theory is much richer than a perturbative analysis might lead one to believe. The study of scaling by M. Fisher, L. Kadanoff, and K. Wilson emphasized the interrelation of statistical mechanics and field theory. For example, it is now understood that a given field theoretic model may, as in statistical mechanics systems, exist in several qualitatively different phases. Statistical mechanical methods have also been applied to field theoretic systems. For example, gauge theories are now being studied on discrete space-time lattices, using Monte Carlo computer simulations or analog high temperature expansions to investigate the complicated phase structure. There has now emerged a fruitful interdisciplinary focus on the non-linear dynamics inherent in the subjects of field theory, statistical mechanics, and classical turbulence.

ZWEIG: Isn't it true to say that the number of things you can actually compute with QCD is far less than you could compute with S-matrix theory many years ago?

WEST: I wouldn't say that.

ZWEIG: What numbers can be experimentally measured that have been computed cleanly from QCD?

RABY: What is your definition of clean?

ZWEIG: A clean calculation is one whose assumptions are only those of the theory. Let me give you an example. I certainly will accept the numerical results obtained from lattice gauge calculations of QCD as definitive if you can demonstrate that they follow directly from QCD. When you approximate space-time as a discrete set of points lying in a box instead of an infinite continuum, as you do in lattice calculations, you have to show that these approximations are legitimate. For example, you have to show that the effects of the finite lattice size have been properly taken into account.

RABY: To return to the question, this is the first time you can imagine calculating the spectrum of strongly interacting particles from first principles.

ZWEIG: The spectrum of strongly interacting particles has not yet been calculated in QCD. In principle it should be possible, and much progress has been made, but operationally the situation is not much better than it was in the early '60s when the bootstrap was gospel.

SLANSKY: Yes, but that was a very dirty calculation. The agreement got worse as the calculations became more cleverly done.

WEST: The numbers from lattice gauge theory calculations of QCD are not necessarily meaningful at present. There is a serious question whether the lattice gauge theory, as formulated, is a real theory. When you take the lattice spacing to zero and go to the continuum limit, does that give you the theory you thought you had?

RABY: That's the devil's advocate point of view, the view coming from the mathematical physicists. On the other hand, people have made approximations, and what you can say is that any approximation scheme that you use has given the same results. First, there are hadrons that are bound states of quarks, and these bound states have finite size. Second, there is no scale in the theory, but everything, all the masses, for example, can be defined in terms of one fundamental scale. You can get rough estimates of the whole particle spectrum.

WEST: You can predict that from the old quark model, without knowing anything about the local color symmetry and the eight colored gluons that are the gauge particles of the theory. There is only one clean calculation that can be done in QCD. That is the calculation of scattering amplitudes at very high energies. Renormalization group analysis tells us the theory is asymptotically free at high energies, that is, at very high energies quarks behave as free point-like particles so the scattering amplitudes should scale with energy. The calculations predict logarithmic corrections to perfect scaling. These have been observed and they seem to be unique to QCD. Another feature unique to quantum chromodynamics is the coupling of the gluon to itself which should predict the existence of glueballs. These exotic objects would provide another clean test of QCD.

ZWEIG: I agree. The most dramatic and interesting tests of quantum chromodynamics follow from those aspects of the theory that have nothing to do with quarks directly. The theory presumably does predict the existence of bound states of gluons, and furthermore, some of those bound states should have quantum numbers that are not the same as those of particles made out of quark-antiquark pairs. The bound states that I would like to see studied are these "oddballs," particles that don't appear in the simple quark model. The theory should predict quantum numbers and masses for these objects. These would be among the most exciting predictions of QCD.

RABY: People who are calculating the hadronic spectrum are doing those sorts of calculations too.

ZWEIG: It's important to pick one fundamental question, push on it, and get the right answer. You may differ as to whether you want to use the existence of oddballs as a crucial test or something else, but you should accept responsibility for performing calculations that are clean enough to provide meaningful comparison between theory and experiment. The spirit of empiricism does not seem to be as prevalent now as it was when people were trying different approaches in particle physics, that is, S-matrix theory, field theory, and the quark model. The development of the field was much more Darwinian then. People explored many different ideas, and natural selection picked the winner. Now evolution has changed; it is Lamarckian. People think they know what the right answer is, and they focus and build on one another's views. The value of actually testing what they believe has been substantially diminished.

SLANSKY: I don't think that is true. The technical problems of solving QCD have proved to be harder than any other technical problems faced in physics before. People have had to back off and try to sharpen their technical tools. I think, in fact, that most do have open minds as to whether it is going to be right or wrong.

WEST: What do you think about the rest of the standard model? Do we think the electroweak unification is a closed book, especially now that $W^{\pm}$ and Z^0 vector bosons have been discovered?

SLANSKY: It is to a certain level of accuracy, but the theory itself is just a

"It may be that all this matter is looped together in some complex topological web and that if you tear apart the Gordian knot with your sword of Damocles, something really strange will happen."

phenomenology with some twenty or so free parameters floating around. So it is clearly not the final answer.

SCIENCE: What are these numbers?

RABY: All the masses of the quarks and leptons are put into the theory by hand. Also, the mixing angle, the so-called Cabibbo angle, which describes how the charmed quark decays into a strange quark and a little bit of the down quark, is not understood at all.

ZWEIG: Operationally, the electroweak theory is solid. It predicted that the $W^{\pm}$ and Z^0 vector bosons would exist at certain masses, and they actually do exist at those masses.

SLANSKY: The theory also predicted the coupling of the Z^0 to the weak neutral current. People didn't want to have to live with neutral currents because, to a very high degree of experimental accuracy, there was no evidence for strangeness-changing weak neutral currents. The analysis through local symmetry seemed to force on you the existence of weak neutral currents, and when they were observed in '73 or whenever, it was a tremendous victory for the model. The electron has a weak neutral current, too, and this current has a very special form in the standard model. (It is an almost purely axial current.) This form of the current was established in polarized electron experiments at SLAC. Very shortly after those experimental results, Glashow, Weinberg, and Salam received the Nobel prize for their work on the standard model of electroweak interactions. I think that was the appropriate time to give the Nobel prize, although a lot of my colleagues felt it was a little bit premature.

RABY: However, the Higgs boson required for the consistency of the theory hasn't been seen yet.

SLANSKY: A little over a year ago there were four particles that needed to be seen—now there is only one. The standard model theory has had some rather impressive successes.

WEST: Can we use this as a point of departure to talk about grand unification? Unification of the weak and electromagnetic interactions, which had appeared to be quite separate forces, has become the prototype for attempts to unify those two with the strong interactions.

RABY: In the standard model of the weak interactions, the quarks and the leptons are totally separate even though phenomenologically they seem to come in families. For example, the up and the down quarks seem to form a family with the electron and its neutrino. Grand unification is an attempt to unify quarks and leptons, that is, to describe them as different aspects of the same object. In other words, there is a large symmetry group within which quarks and leptons can transform into each other. The larger group includes the local symmetry groups of the strong and electroweak interaction and thereby unifies all the forces. These grand unified theories also predict new interactions that take quarks into leptons and vice versa. One prediction of these grand unified theories is proton decay.

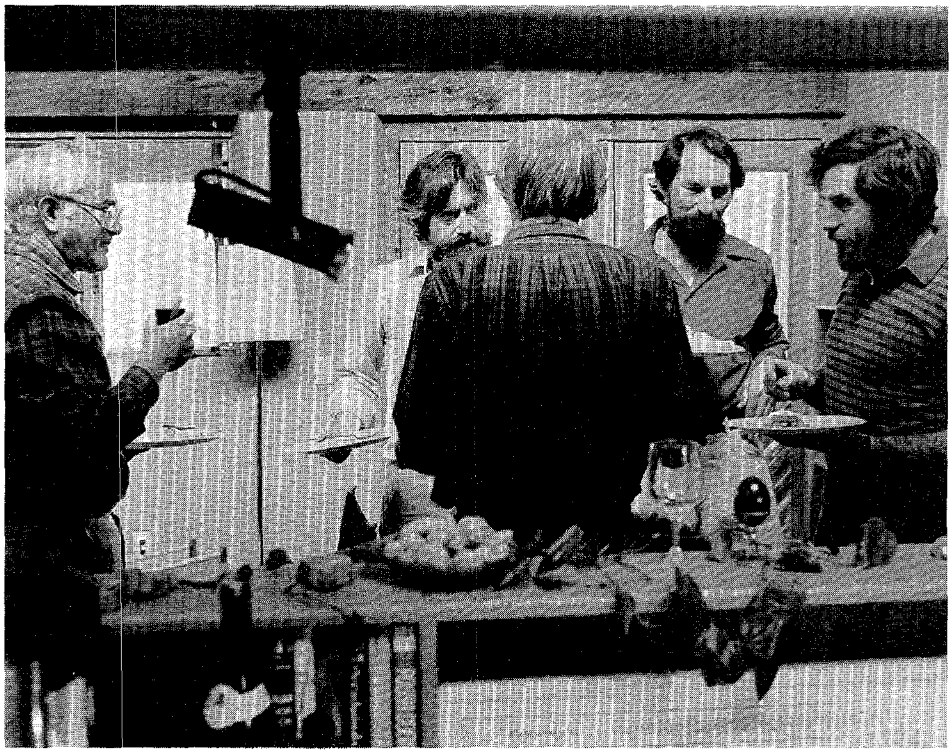
WEST: The two most crucial predictions of grand unified theories are, first, that protons are not perfectly stable and can decay and, second, that magnetic monopoles exist. Neither of these has been seen so far. Suppose they are never seen. Does that mean the question of grand unification becomes merely philosophical? Also, how does that bear on the idea of building a very high-energy accelerator like the SSC (superconducting super collider) that will cost the taxpayer $3 billion?

CARRUTHERS: Why should we build this giant accelerator? Because in our theoretical work we don't have a secure world view; we need answers to many critical questions raised by the evidence from the lower energies. Even though I know that as soon as you do these new experiments, the number of questions is likely to multiply. This is part of my negative curvature view of the progress of science. But there are some rather primitive questions which can be answered and which don't require any kind of sophistication. For instance, are there any new particles of well-defined mass of the old-fashioned type or new particles with different properties, perhaps? Will we see the Higgs particle that people stick into theories just to make the clock work? If you talk to people who make models, they will give you a panorama of predictions, and those predictions will become quite vulnerable to proof if we increase the amount of accelerator energy by a factor of 10 to 20. Those people are either going to be right, or they're going to have to retract their predictions and admit, "Gee, it didn't work out, did it?"

There is a second issue to be addressed, and that is the question of what the fundamental constituents of matter are. We messed up thirty years ago when we thought protons and neutrons were fundamental. We know now that they're structured objects, like atoms: they're messy and squishy and all kinds of things are buzzing around inside. Then we discovered that there are quarks and that the quarks must be held together by glue. But some wise guy comes along and says, "How do you know those quarks and gluons and leptons are not just as messy as those old protons were?" We need to test whether or not the quark itself has some composite structure by delivering to the quarks within the nucleons enough energy and momentum transfer. The accelerator acts like a microscope to resolve some fuzziness in the localization of that quark, and a whole new level of substructure may be discovered. It may be that all this matter is looped together in some complex topological web and that if you tear apart the Gordian knot with your sword of Damocles, something really strange will happen. A genie may pop out of the bottle and say, "Master, you have three wishes."

A third issue to explore at the SSC is the dynamics of how fundamental constituents interact with one another. This takes you into the much more technical area of analyzing numbers to learn whether the world view you've constructed from evidence and theory makes any sense. At the moment we have no idea why the masses of anything are what they are. You have a theory which is attractive, suggestive, and can explain many, many things. In the end, it has twenty or thirty

"If we can get people to agree on why we should be doing high-energy physics, then I think we can solve the problem of price."



parameters. You can't be very content that you've understood the structure of matter.
SLANSKY: To make any real progress both in unification of the known forces and in understanding anything about how to go beyond the interactions known today, a machine of something like 20 to 40 TeV center of mass energy from proton-proton collisions absolutely must be built.
SCIENCE: Will these new machines test QCD at the same time they test questions of unification?
SLANSKY: The pertinent energy scale in QCD is on the order of GeV, not TeV, so it is not clear exactly what you could test at very high energies in terms of the very nonlinear structure of QCD. Pete feels differently.
CARRUTHERS: All I say is that you may be looking at things you don't think you are looking at.
WEST: Obviously all this is highly speculative. A question you are obligated to ask is at what stage do you stop the financing. I think we have to put the answer in terms of a realistic scientific budget for the United States, or for the world for that matter.
CARRUTHERS: Is there a good reason why the world can't unify its efforts to go to higher energies?
WEST: Countries are mostly at war with one another. They couldn't stop to have the Olympic games together, so certainly not for a bloody machine.
SLANSKY: The Europeans themselves have gotten together in probably one of the most remarkable examples of international collaboration that has ever happened.
WEST: Yes, I think the existence of CERN is one of the greatest contributions of particle physics to the world.
RABY: But the next step is going to have to be some collaborative effort of CERN, the U.S., and Japan.
WEST: Our SSC is going to be the next step. But you are still not answering the question. Should we expect the government to support this sort of project at the $3 billion level?
SLANSKY: That's $3 billion over ten years.
RABY: You can ask that same question of any fundamental research that has no direct application to technology or national security, and you will get two different answers. The "practical" person will say that you do only what you conceive to have some benefits five or ten years down the line, whereas the person who has learned from history will say that all fundamental research leads eventually either to new intellectual understanding or to new technology. Whether technology has always benefited mankind is debatable, but it has certainly revolutionized the way people live. I think we should be funded purely on those grounds.
WEST: Where do you stop? If you decide that $3 billion is okay or $10 billion, then do you ask for $100 billion?
ZWEIG: This is a difficult question, but if we can get people to agree on why we should be doing high-energy physics, then I think we can solve the problem of price. Although what we have been talking about may sound very obscure and possibly very ugly to an outside observer (quantum chromodynamics, grand unification, and twenty or thirty arbitrary parameters), the bottom line is that all of this really deals with a fundamental question, "What is everything made of?"

It has been our historical experience that answers to fundamental questions always lead to applications. But the time scale for those applications to come forward is very, very long. For example, we talked about Faraday's experiments which pointed to the quantal nature of electricity in the early 1800s; well, it was another half century before the quantum of electricity, the electron, was named and it was another ten years before electrons were observed directly as cathode rays; and another quarter century passed before the quantum of electric charge was accurately measured. Only recently has the quantum mechanics of the electron found application in transistors and other solid state devices.

Fundamental laws have always had application, and there's no reason to believe

"I groan inwardly and sadly reflect on how great the communication gap is between scientists such as ourselves and the general public that supports us."

this will not hold in the future. We need to insist that our field be supported on that basis. We need ongoing commitment to this potential for new technology, even though technology's future returns to society are difficult to assess.

CARRUTHERS: Whenever support has to be ongoing, that's just when there seems to be a tendency to put it off.

WEST: What's another few years, right? Now I would like to play devil's advocate. One of the unique things about being at Los Alamos is that you are constantly being asked to justify yourself. In the past, science has dealt with macroscopic phenomena and natural phenomena. (I am a little bit on dangerous ground here.) Even when it dealt with the quantum effects, the effects were macroscopic: spectroscopic lines, for example, and the electroplating phenomena. The crucial difference in high-energy physics is that what we do is artificial. We create rare states of matter: they don't exist except possibly in some rare cosmic event, and they have little impact on our lives. To understand the universe that we feel and touch, even down to its minutiae, you don't have to know a damn thing about quarks.

ZWEIG: Maybe our experience is limited. Let me give you an example. Suppose we had stable heavy negatively-charged leptons, that is, heavy electrons. Then this new form of matter would revolutionize our technology because it would provide a sure means of catalyzing fusion at room temperature. So it is not true that the consequences of our work are necessarily abstract, beyond our experience, something we can't touch.

WEST: This discussion reminds me of something I believe Robert Wilson said during his first years as director of Fermilab. He was before a committee in Congress and was asked by some aggressive Congressman, "What good does the work do that goes on at your lab? What good is it for the military defense of this country?" Wilson replied something to the effect that he wasn't sure it helped directly in the defense of the country, but it made the country worth defending. Certainly, finding applications isn't predominantly what drives people in this field. People don't sit there trying to do grand unification, saying to themselves that in a hundred years' time there are going to be transmission lines of Higgs particles. When I was a kid, electricity was going to be so cheap it wouldn't be metered. And that was the kind of attitude the AEC took toward science. I, at least, can't work that way.

SCIENCE: George, do you work that way?

ZWEIG: I was brought up, like Pete, at a time when the funding for high-energy physics was growing exponentially. Every few years the budget doubled. It was absolutely fabulous. As a graduate student I just watched this in amazement. Then I saw it turn off, overnight. In 1965, two years after I got my degree from Caltech, I was in Washington and met Peter Franken. Peter said, "It's all over. High-energy physics is dead." I looked at him like he was crazy. A year later I knew that, in a very real sense, he was absolutely right.

It became apparent to me that if I were going to get support for the kind of research I was interested in doing, I would have to convince the people that would pay for it that it really was worthwhile. The only common ground we had was the conviction that basic research eventually will have profound applications.

The same argument I make in high-energy physics, I also make in neurobiology. If you understand how people think, then you will be able to make machines that think. That, in turn, will transform society. It is very important to insist on funding basic research on this basis. It is an argument you can win.

There are complications, as Pete says; if applications are fifty years off, why don't we think about funding twenty-five years from now? In fact, that is what we have just heard: they have told us that we can have another accelerator, maybe, but it is ten or fifteen years down the road.

SLANSKY: We really can't build the SSC any faster than that.

ZWEIG: They could have built the machine at Brookhaven.

WEST: Let's talk about that. How can you explain why a community who agreed that building the Isabelle machine was such a great and wonderful thing decided, five years later, that it was not worth doing.

SLANSKY: It is easy to answer that in very few words. The Europeans scooped the U.S. when they got spectacular experimental data confirming the electroweak unification. That had been one of our main purposes for building Isabelle.

CARRUTHERS: If you want to stay on the frontier, you have to go to the energies where the frontier is going to be.

ZWEIG: Some interesting experiments were made at energies that were not quite what you would call frontier at the time. CP violation was discovered at an embarrassingly low energy.

SLANSKY: The Europeans already have the possibility of building a hadron collider in a tunnel already being dug, the large electron-positron collider at CERN. It is clear that the U.S., to get back into the effort, has to make a big jump. Last spring the High Energy Physics Advisory Panel recommended cutting off Isabelle so the U.S. could go ahead in a timely fashion with the building of the SSC.

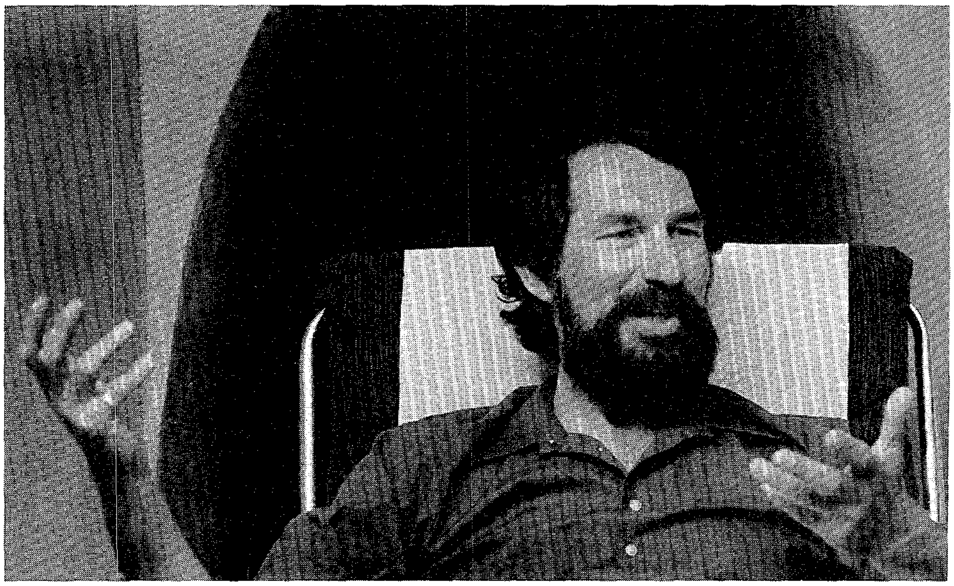
WEST: If you were a bright young scientist, would you go into high-energy physics now? I think you could still say there is a glamour in doing theory and that great cosmic questions are being addressed. But what is the attraction for an experimentalist, whose talents are possibly more highly rewarded in Silicon Valley?

RABY: It will become more and more difficult to get people to go into high-energy physics as the time scale for doing experiments grows an order of magnitude equal to a person's lifetime.

ZWEIG: Going to the moon was a successful enterprise even though it took a long time and required a different state of mind for the participating scientists.

WEST: Many of the great creative efforts of medieval life went into projects that lasted more than one generation. Building a great cathedral lasted a hundred, sometimes two hundred years. Some of the great craftsmen, the great architects, didn't live to see their

"I consider doing physics something that causes me an enormous amount of emotional energy. I get upset. I get depressed. I get joyful."



work completed.

As for going to high energies, I see us following Fermi's fantasy: we will find the hydrogen atom of hadronic physics and things will become simpler. It is a sort of Neanderthal approach. You hit as hard as you can and hope that things break down into something incredibly small. Somewhere in those fragments will be the "hydrogen" atom. That's the standard model. Some people may decide to back off from that paradigm. Lower energies are actually amenable.

CARRUTHERS: I think that people have already backed off. Wasn't Glashow going around the country saying we should do low-energy experiments?

WEST: Just to bring it home, the raison d'etre for LAMPF II is to have a low-energy, high-intensity machine to look for interesting phenomena. It is again this curious thing. We are looking at quantum effects by using a classical mode—hitting harder. The idea of high accuracy still uses quantum mechanics. I suppose it is conceivable that one would reorient the paradigm toward using the quantum mechanical nature of things to learn about the structure of matter.

SLANSKY: Both directions are very important.

SCIENCE: Is high-energy physics still attracting the brightest and the best?

SLANSKY: Some of the young guys coming out are certainly smart.

CARRUTHERS: I think there is an increasing array of very exciting intellectual challenges and new scientific areas that can be equally interesting. Given a limited pool of intellectual talent, it is inevitable that many will be attracted to the newer disciplines as they emerge.

ZWEIG: Computation, for example. Stephen Wolfram is a great example of someone who was trained in high-energy physics but then turned his interest elsewhere, and profitably so.

CARRUTHERS: Everything to do with conceptualization—computers or theory of the mind, nonlinear dynamics advances. All of these things are defining new fields that are very exciting—and that may in turn help us solve some of the problems in particle physics.

ZWEIG: That's optimistic. What would physics have been like without your two or three favorite physicists? I think we would all agree that the field would have been much the poorer. The losses of the kind we are talking about can have a profound effect on a field. Theoretical physics isn't just the cumulative efforts of many trolls pushing blocks to build the pyramids.

WEST: But my impression is that the work is much less individualized than it ever was. The fact that the electroweak unification was shared by three people, and there were others who could have been added to that list, is an indication. If you look at QCD and the standard model, it is impossible to write a name, and it is probably impossible to write ten names, without ignoring large numbers of people who have contributed. The grand unified theory, if there ever is one, will be more the result of many people interacting than of one Einstein, the traditional one brilliant man sitting in an armchair.

SCIENCE: Was that idea ever really correct?

WEST: It was correct for Einstein. It was correct for Dirac.

SCIENCE: Was their thinking really a total departure?

ZWEIG: The theory of general relativity is a great example, and almost a singular example, of someone developing a correct theoretical idea in the absence of experimental information, merely on the basis of intuition. I think that is what people are trying to do now. This is very dangerous.

RABY: Another point is that Einstein in his later years was trying to develop the grand unified theory of all known interactions, and he was way off base. All the interactions weren't even known then.

WEST: Theorizing in the absence of supportive data is still dangerous.

CARRUTHERS: Particle physics, despite all of its problems, remains one of the principal frontiers of modern science. As such it combines a ferment of ideas and speculative thoughts that constantly works to reassess the principles with which we try to understand some of the most basic problems in nature. If you take away this frothy area in which there's an enormous interface between the academic community and all kinds of visitors interacting with the laboratory, giving lectures on what is the latest excitement in physics, then you won't have much left in the way of an exciting place to work, and people here won't be so good after awhile. ■

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