



*An Interview with Peter Carruthers*

# Physics, Philosophy, Leadership, & Policy

by Leonard M. Simmons, Jr.  
and Geoffrey B. West

**P**eter Carruthers recently stepped down — or stepped up, as he puts it — from a seven-year tenure as Leader of the Los Alamos Theoretical Division to return to the main work of his professional life — research in pure physics.

*During these seven years we have seen a new side of Pete — a tough leader with vision, foresight, and an instinct for making things happen. He has changed the image of the Laboratory in the eyes of the scientific community, and has fought hard and successfully in Washington for support of basic research in physics.*

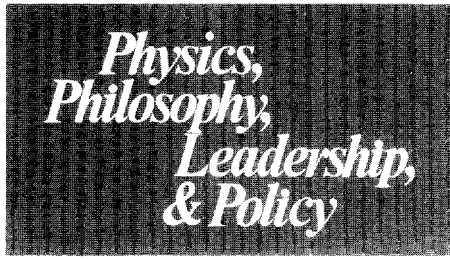
*The metamorphosis from a scholarly professor to tough Division Leader was indeed a shock to us. We had known him at Cornell University as something of a boy wonder, dedicated to his work and surrounded by graduate students not much younger than he. His breadth of experience in both solid-state and particle physics was rare among his contemporaries. His openness, encouragement, and enthusiasm for new ideas — his sardonic wit, good judgment, and appreciation for real talent attracted many students to him. With his horn-rimmed glasses, mild exterior, and office over-owing with books and papers, he appeared more like an old-fashioned scholar of classical manuscripts than a hard-driving physicist on his way to the top. He had and still has serious interests in music, bird watching, and trout fishing and, of course, an intense deep love of physics. Thus we were somewhat surprised when Harold Agnew, then Director of Los Alamos, invited Pete to become Theoretical Division Leader; even more surprising, however, was that Pete accepted!*

*Although Pete entered the Laboratory as a novice in administration, he used what leverage he had to accomplish a great deal. He restructured the Theoretical Division and established new groups in many areas (high-energy physics, theoretical biology, statistical physics and materials theory, theoretical molecular physics, applied mathematics, and detonation theory). He stimulated intellectual excitement and a strong sense of exploration; he hired talented people and left*

*them free to work. He brought eminent scientists from outside to participate in the life of the Laboratory. It was an uphill battle and for those who had known Pete in his previous life, quite something to see! To us, Pete was Clark Kent stepping into a telephone booth before important meetings, to emerge as Superman ready to fight for what he believed in.*

*Pete had not been part of the scientific establishment before coming to Los Alamos, but his new position gave him entree into the corridors of power and he quickly took full advantage of the opportunity. He became involved in national science policy, participating as Chairman of the National Science Foundation's Physics Advisory Panel, member of the National Academy of Sciences' Committee on U.S.-USSR Cooperation in Physics, member of the Department of Energy's High Energy Physics Advisory Panel, and Chairman of the Board of Trustees of the Aspen Center for Physics. He joined JASON, a group of U.S. scientists who work three months each year on scientific and policy aspects of the country's defense and energy problems. He still holds many of these positions today, and recently he has been appointed a Senior Fellow of the Laboratory.*

*We now know Peter as someone who cares deeply on a grand scale, but still remains a champion of the little guy, the bright young scientist who needs to find a job. He can be ruthless and uncompromising and act harshly when necessary (but not without the side effect of sleepless nights). As one of the few Division Leaders at Los Alamos to have been appointed directly from an academic position at a great university, he has fared well and served well. He has become a force in the world of science policy and will undoubtedly continue in that role.*



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SIMMONS: When you came to Los Alamos your experience in administration consisted of running an undergraduate course of maybe 50 people. What made you think you could run a big theoretical division?

CARRUTHERS: It never occurred to me that I couldn’t do a thing like that. I just thought it took good judgment. Of course, it takes mostly stamina.

SIMMONS: Do you think administrative experience is important for most administrative jobs in science?

CARRUTHERS: A little bit will make life easier, but I don’t think that’s as important as having the right instincts for finding good people and letting them do what you want done. The key issue is that the people who work for you respect you and respect your judgment and fairness. Of course an illustrious scientific reputation can get you respect, but you may be a wretched administrator, nevertheless. On the other hand, a good administrator *per se* may not have sharp scientific judgment. And neither may be able to judge the quality of people. Even if you’re a competent scientist, after a while you may lose the freshness of that competence and, since there’s no reason for a scientist to be an administrator in a scientific establishment unless he maintains scientific judgment and competence, we might be better off with good administrators. It’s a very complicated business.

One of the principal evils of the federal science establishment, both in Washington and outside, is the emergence to power of a permanent ruling class of rotating bureaucrats who don’t command the respect of the people that they control. There is an entropy death facing American science with its present trend

to expansion of titles, and functions, and memos, and the ever-present Xerox and Kodak machines that simply produce communications and the need for more communications.

Ever since I’ve been here there’s been an increasing trend, both externally and internally, towards the illusion that you can manage science, whereas all you can really do is to get good people who are interested in the subject you want to develop. This increasing accountability at all levels of the federal establishment exudes a cold air that drives out the kind of neurotic and creative people that you need to make a breakthrough. There has to be a feeling of freedom and reward. You can’t get good science out of people who recognize that they are being managed.

Of course you do have to deliver, but there is a way of getting to the answer by leadership, which I distinguish from management. Leadership is the king waving the flag in front of the army, saying “Let’s go get the bastards.” Then the captains will race along enthusiastically and fight the battle. With management the king sends a telegram from 50 miles behind the lines saying “Why aren’t you to latitude 42.54? According to our long-range plan, you were supposed to have been there last week. Kindly fly to Washington and explain why you are not yet at your milestone.” One thing that can be done to improve things is to encourage the few people who have both first-rate scientific talent and some aptitude or tolerance for leadership and responsibility to get in there and take their turns. That should mean that it’s not a lifelong sentence that will destroy their research careers. Another thing is to make sure that there aren’t so

many rewards for mere politicians and entrepreneurs. That requires the best leadership at the highest level.

WEST: Do you have any regrets, having left Cornell?

CARRUTHERS: I often feel quite nostalgic for Ithaca itself. But I've found that people in Los Alamos are really more fun. On the other hand, my responsibilities here have kept me from doing much serious research. I was at Cornell for 17 years altogether, as a graduate student and faculty member. It was extremely peaceful, and I never realized what an idyllic, quiet place it was and how conducive it was to doing flat-out scientific research with a minimum of distraction. Of course the long, gray, wet winter encourages work,

WEST: Why did you take the job here?

CARRUTHERS: I don't know what Faustian tendencies were growing in me but I began to realize that the Laboratory was an enormous resource, and that it might be possible to do something with it. In the area of physics I knew about, which was essentially pure academic-type physics, the Theoretical Division did not have very much to offer. Its areas of excellence were in the more applied areas that I would have to learn about later. But finally I decided I needed to change my life, and I became very eager to get the job.

One of the attractions was the sheer physical beauty of the Rocky Mountains. The other thing is that Los Alamos is a very dynamic place, however confused, in which all kinds of things are going on. There is much more traffic through Los Alamos of significant scientists involved in national affairs than there ever was at Cornell. I wanted to have a try at living in a different kind of

environment.

At Cornell, each department had a moat around it. There was not a community of scholarship in which humanists talked with scientists about significant issues. If there was, then I wasn't part of it. I think Los Alamos is much more integrated intellectually.

WEST: Had there been a disenchantment for you in university life?

CARRUTHERS: Yes. University life is entrenched and rigid, and it's very hard to transform the way a place is. I felt frustrated that there was no chance to influence the future. I felt this was an opportunity to change the world in some way beside writing yet another paper.

SIMMONS: Universities have the reputation of being bastions of liberalism. Don't you believe that's true?

CARRUTHERS: I certainly don't believe it's true. Especially after I testified for the students after a police riot.

University life has many virtues, and often I miss them. Especially I miss the students and the general mix of cultural opportunities. But it certainly has its limitations, and it's not clear that universities will always have the predominant role in scientific research that they have had in the past. In a way, the entire sociology of science may be undergoing a change—the same kind of change which makes it possible to have really good science at a national laboratory, presumably dedicated to giant projects and technology. It may mean something for the whole future of science in the country.

WEST: Do you see that as positive?

CARRUTHERS: If it creates excellence, then I would say it's positive; whether it's better than what went before, I don't know. It's just different,



and I'm not sure what it will be. It might be the case that in 10 or 20 years there will be more exciting, first-rate science done at institutes and laboratories than at universities. Universities are in a terrible financial situation. They are over-tenured. The age of the faculty keeps growing. The number of students is decreasing. There are very few job opportunities for young people. It's not clear that the sociology of the traditional university is going to allow the nurturing of science in the vigorous way that it was when we were coming through the system.

There are, of course, new pressures on our country which never existed before, and those pressures require that scientists pay attention to new issues. The institutes or laboratories may be the proper vehicle for that. Not to say that I don't have many criticisms of the way federally supported science is managed in this country.

WEST: Thinking about coming to Los Alamos, did you see that somehow you could play a role in national issues?

CARRUTHERS: Yes, that was a very conscious part of it. After World War II many scientists became involved in de-

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fense issues. They became plugged in to the right power circles and were turned to for advice. Now there is a new generation coming in, and I view myself as part of that new generation.

SIMMONS: Did you think you would be able to continue to do a significant amount of science after you came here?

CARRUTHERS: I foolishly felt I could spend half my time on research. In Ithaca, if I could spend 4 hours a day working on physics that would be a good day, by the time teaching, students, etc., were taken care of. When I talked to Harold Agnew he said sure, take as much of your time as you want. But gradually it wears you down.

WEST: Did you enjoy the taste of power that you had as Division Leader—the fact that the corridors of power in Washington opened up?

CARRUTHERS: Power comes from various sources, and some of it, much of it, comes *ex officio*, if the job is sufficiently high. For example, just being a Group Leader or Division Leader has intrinsically a certain amount of fiscal power and people immediately notice that. There are people who had never paid any attention to me until I had the fiscal power of being a Division Leader. Suddenly they noticed me and were extremely friendly. Now, I’m going to find out which of them are my real friends.

In Washington, unless you have either an enormous scientific reputation or some large responsibility, the doors of power are not available to you. Of course, once you get sufficiently known to all the people who move in this sphere of influence, the titles are not so necessary. I certainly found that being a Division Leader opened up many op-

portunities for me, *ex officio*, that wouldn’t have existed had I been just a professor at Cornell.

I enjoyed being in the center of the action very often. I suffer from the schizophrenia of wanting to be simultaneously a quiet scholar working in a corner and also making the right things happen—and making sure that the bad guys don’t get in there and make the wrong things happen. There’s no doubt that those things strongly motivate me.

WEST: Do you think you can return to being the quiet scholar sitting in the corner?

CARRUTHERS: That’s the big question. It’s not so much that I’m corrupted, but whether I’ll be left alone. In this general area (on the national scene) in which genuine talent is so scarce, if you open your mouth and say a few sensible words people may overestimate your intrinsic merit in this regard. Soon you’re serving on every committee that’s available, and spending all your time on airplanes.

To me one of the greatest hopes for science in this country is the informal collection of scientists who are sensitive to policy issues and willing to be advocates of science, not just for their own institutions but on the national scene. These people are always in touch with each other. They prevent lots of bad things from happening, and even occasionally cause a good thing to happen.

SIMMONS: As far as Los Alamos is concerned, there seems to be constant warfare between advocates of basic science and advocates of purely programmatic work. Is that healthy?

CARRUTHERS: The people who work on practical and applied things often feel that the basic-science people are para-

sites on the body of the parent organization, and that those people even have contempt for the people who are screwing screws into hardware. There is a natural suspicion in the two camps.

In the Laboratory at large, the problem is very real. In T-Division, we have integrated the spectrum of interests, so that the people doing basic work become aware of some of the long-range, applied problems and contribute to solving those problems, and the people doing nuts and bolts work may be helped by being close to people working at the frontiers of science. Both extremes profit from the existence of the other, if they will only agree to be friends.

I don't see how you can have a first-class multidisciplinary national facility like Los Alamos unless you have a strong team of people who are at the frontier of fundamental science. Without that you're not plugged in to the real intellectual life of the scientific community. At the same time, you cannot justify a large multihundred-million-dollar facility of the sort that we have without addressing some of the genuine technological issues of the country. This organism is not necessarily a freak invented by happenstance. It could be one of the strongest scientific organizations as we move out of this century. The universities may never recover from the demographics and the sociology of our culture, and our culture is not going to be able to subsidize the research activities of the professors on the basis of their teaching activities. I expect that there will be an increase in institutions, but not to the extent that the Soviets have institutionalized their whole scientific establishment, with very little first-class research done in universities. I

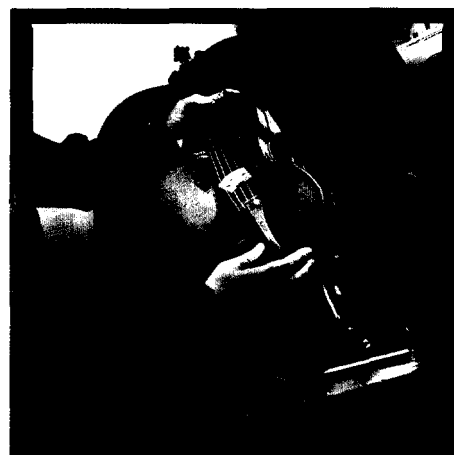
think that the national laboratories, or variants thereof, may be on the rise.

SIMMONS: Why did you choose a career in physics?

CARRUTHERS: When I was young I became interested in birds and fishing, all of the outdoors and the creatures in it. I became locally famous as a promising young biologist. It was claimed—though falsely—that I could identify a bird by listening to it walk on a branch. I became a Boy Scout nature counselor, and I was the court of appeal for many merit badges on biological subjects. After a while I began to realize that biology was very hard. I was looking at the stars, became interested in physics, and read books, some of the old-fashioned inspiring books on relativity and quantum mechanics. I also got books which were way beyond me. As a high school junior I couldn't understand Riemannian calculus and its implications for relativity, but I stared lovingly at the equations. I was very excited by these books, and at the same time I felt that biology was farther from the fundamental essence of exact science, although I wouldn't agree with that point of view any more. I had decided by the time I was 16 that I would become a physicist. In the meantime, I was playing a lot of music, but I was not in a serious musical community, and there weren't any influences on me which would have led me into that as a career.

WEST: You were pretty much self-motivated?

CARRUTHERS: In high school I was regarded as a freak. I was the smart, fat kid with the violin, the honorary Jew of Middletown, Ohio. When I first went to college I came under the spell of Bertrand Russell. I read all of his athe-



istic works and joined his cult. When you're 19 that can be just right. I even founded a Philosophy Club at Carnegie Tech. We met and seriously discussed all the major philosophical questions, no doubt in some naive way.

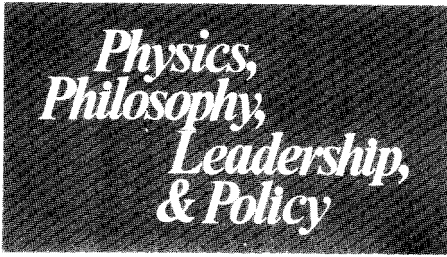
SIMMONS: Did you read Whitehead?

CARRUTHERS: Oh, yes, he was a very dull fellow, but I felt obliged to read him because he was considered so important—just like lots of contemporary theories.

WEST: What kind of an education did you have at Carnegie?

CARRUTHERS: During the first two years I found physics not very interesting, but I knew that the good stuff was just beyond. I spent a lot of time reading broadly. I soon became intoxicated with just learning. In my sophomore year, at one time I was taking 11 courses. I tried to learn everything I could.

At the end of my sophomore year I had reached the right level to really be interested in physics. I remember being inspired by Max Born's book on atomic physics. There is a chapter on wave-corpuscles or wave-particle duality, and the blinding insight of that remains with me. It was much more excit-



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ing than the austere beauties of relativity, which seem to me essentially kinematical, but don't have the awe-inspiring dynamical content of quantum mechanics.

When I first went to Cornell as a graduate student I was taking quantum field theory without having ever learned scattering theory—a slightly perverse way to do it. The instructor and my classmates seemed to have no interest in the pathological diseases which make that theory not a theory. I assumed that I was slow, because they all nodded their heads simultaneously in phase whenever some obscure mystery went past at the speed of light.

I was very much interested in statistical mechanics and the many-body problem and was turned off initially by the field theory problems. So I started doing research, and as soon as I learned Fermi's "Golden Rule," I was writing papers in solid-state physics. However, it happened that Feynman was taking a sabbatical at Cornell, and I found to my amazement that in his lectures he laughed at all the absurdities of field theory. It came as a liberating influence to find out that the things that bothered me were exactly the things that this great man considered absurd and which had to be removed from the correct theory eventually.

WEST: What are you referring to?

CARRUTHERS: The entire structure of infinities and mathematical sicknesses in an otherwise beautiful structure. I began thinking a bit about particle physics problems. I was so far advanced in writing solid-state research papers that I could have graduated after maybe a year and a half of graduate "school. but I did not want to be identified as some particu-

lar kind of theorist. I wanted to be a general theorist, and so I decided I would do a thesis in particle physics. I screwed up my courage and went to visit Bethe, which is a very scary thing for a student. He seemed to have heard of me and took me on. Then, of course, he went back to Geneva for some indefinite time for disarmament talks.

I came in very early in this period and saw him studying a *Physical Review Letters* article having to do with the one-pion exchange mechanism of producing an extra pion off a nucleon. It was written by Charlie Goeble. He said, "Well, Charlie Goeble is a very smart fellow, and I'm not sure that this is exactly right, but there is something very interesting here and why don't you look at it." So, I looked at it and decided that what was missing was the interaction of one of the final pions with the nucleon. I wrote a thesis on this subject.

After three years at Cornell, I was finished, got an NSF postdoctoral fellowship, and settled into Cornell. Then I was summoned to the Chairman's office and told that I was going to be an Assistant Professor, teaching quantum mechanics to the first-year graduate students. I said I wasn't ready to teach quantum mechanics, that I wasn't even ready to be a professor. I asked them to leave me alone and let me have my postdoc and do my research. (Of course, this situation is inconceivable nowadays.) But I joined the Cornell Physics Department, I piled my desk high with learned books on quantum mechanics, studying all of the old puzzles that quantum mechanics like to ponder.

SIMMONS: This was about 1960?

CARRUTHERS: January of 1961. For

several years I tried to keep up in both solid-state and high-energy physics but with teaching and graduate students and so on, I didn't have the stamina to do the research in two major fields. I chose to stay in particle physics and gradually stopped the other. However, that way of thinking about many-body systems has always influenced my particle physics. It's the way of thinking that has now taken over almost all of field theory and modern particle physics. So I've never regretted that experience.

WEST: Did you enjoy teaching?

CARRUTHERS: Oh, yes. That's the main thing I miss, being at Los Alamos, the students and their frisky ways. I remember being frustrated giving exams because the students always averaged 37%. I would decide to make the test easier the next time. It didn't work. I was learning at the same rate that I was making the tests easier. At one time I got a book on how to teach. I never read it, of course, and the students continued to get 37% on all exams.

WEST: Were you a good teacher?

CARRUTHERS: Well, you'll have to ask Mike Simmons, who only listened to the smutty remarks, I think. I thought I was well organized; I don't know if I was a good teacher.

SIMMONS: Did you do much preparation for your classes?

CARRUTHERS: I felt naked and defenseless if I didn't come with a complete army of notes with all derivations intact, though as you well know, there were times when I might improvise after an all-night graduate party. There were some subjects in which I felt so totally in control that I didn't need any preparation, but that was based on some early mastery of the subject.

WEST: Did you enjoy having graduate students?

CARRUTHERS: I especially enjoy *working* with graduate students. They keep you moving. I prefer *teaching* undergraduates. Graduate students are so much in awe of the professor that they don't really give him a hard enough time, whereas the undergraduate sits there and says, "Who are you? Why should I believe that? That doesn't sound very convincing to me." What are you going to do when a student tells you that?

WEST: What did you do?

CARRUTHERS: Well, I brought beer to the final exam. Along with Bob Dylan quotations to entitle each question.

SIMMONS: What was it like being Bethe's student?

CARRUTHERS: It was quite peaceful because he was spending a lot of his time in Geneva—disarming. These were the first serious disarmament talks, as I recall.

WEST: Do you see that, in any way, as a connection between what has become an interest of yours here at Los Alamos?

CARRUTHERS: It might be, but at that time it must have been very well suppressed because I was completely uninterested in politics. I couldn't imagine why he spent all his time going around the world dealing with what I considered insoluble problems. I valued him for his insight into physics, and I was sorry he wasn't around more.

SIMMONS: Did he have any particular influence on your style of research?

CARRUTHERS: He had a very strong influence on my general standards. Bethe is a hero, and a father figure to me, an intellectual father figure. After you're around him awhile you realize that it's got to be right, and there isn't any other

option. It's not half right, or almost right; it really has to be right. That's a lesson that is not very much in vogue these days. Also he was usually uninterested in very abstruse theories and I think that, for a while, had an impact on my own orientation in" theory, though I've drifted to becoming more abstract as the years go on.

WEST: You said that at the very beginning of your graduate career you chose particle physics because you wanted not to be identified as a specialist. Was that in any way related to Bethe?

CARRUTHERS: No, that was pre-Bethe. I saw that people got categorized and put in boxes and I didn't want to be one of those people.

SIMMONS: Let me ask you about a different kind of thing, something that I call the "Feigenbaum effect." There is a typical progression for a talented young person in science: from his Ph.D. to the postdoctoral appointment, during the first year of which he publishes a half a dozen papers, thereby acquiring his second appointment; during the first year of that, another half a dozen, thereby earning a permanent position. Some people violate all of that. Are there lots of them or only a few? Is there something wrong with this standard way of handling young scientists?

CARRUTHERS: Yes. There are quite a few sensitive people who can't stand the strain of competition with people equally good, or who have bad luck, don't have stamina, or have personal problems. Some of them are lost. This happens in every field. You lose people everywhere. I think you lose fewer people in physics than you do in most highly intellectual activities. But the "Feigenbaum effect" is real. Mitchell Feigenbaum did little to

preserve his career by publication. One of the best things I have done here was to pull somebody off the street whose career was endangered, but whose talent was unmistakably superior to almost anybody else that I had ever met of that age group. This is high-risk investment, and it rarely happens at a university.

Five years later, Feigenbaum has produced major advances\* in our understanding of turbulence, one of the outstanding scientific puzzles of our time. In the usual institutional context, if you were looking for somebody to study turbulence, you would advertise for an expert in fluid mechanics, preferably with computer skills. However, decisive advances usually come when talented people take a fresh look at a subject, The practice (at Los Alamos and elsewhere) of hiring "already qualified" people for specific tasks, often under the pressure of programmatic deadlines, is harmful to the long-term quality of science.

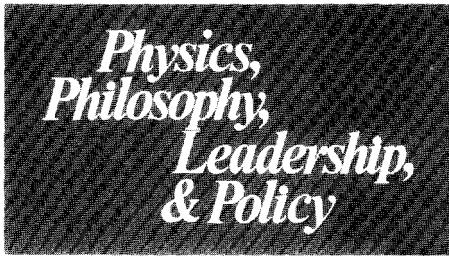
But the quality survives miraculously, despite all the human foibles that are translated into the way science is done. That's largely due to the experimentalists, I suppose. Somehow science is self-correcting. Even though credit often is assigned unfairly, the actual evolution goes on, you sort out the better ideas from the junk, and occasionally there are major insights.

SIMMONS: Let's talk about something different. The importance of music to you dates back to a very early age.

CARRUTHERS: Nine years old. I suppressed it pretty much during college and immediately after. The desire to do my own research overwhelmed every-

\*See "Universal Behavior in Nonlinear Systems," Los Alamos Science, No. 1 (Summer 1980).





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thing else in my life. Then I began to come back to it.

When I was about eight, I heard Jascha Heifitz play the Beethoven violin concerto on the radio, and I said, “I want to do that.” So my mother arranged for me to get a violin and a teacher.

There’s nothing which quite combines the sensuous with the intellectual as really first-rate music. It’s a daily part of my existence as you can tell by the dirty, gnarled fingertips.

SIMMONS: Did you ever have second thoughts about not following music as a career?

CARRUTHERS: There’s no doubt about that. What I had secretly wanted to do for a long time, and thought was the finest creative act, was to write first-rate classical music. Of course, you may not know that you’ve written first-rate music until much too late, but that seems to me to be one of the finest things.

WEST: What do you see as the relationship between the experience of physics and the creative process in physics and that of music?

CARRUTHERS: I’m just old enough now that everything seems to be merging. If you’re working on a Bach sonata, you may have to play a hundred times through a few lines before it really begins to work. If you’re doing physics you may have to bang your head on the wall for a couple of weeks before you finally see the way through to doing it right. There is a lot of hard work involved, but when it’s all in place you have great satisfaction.

I can’t account for the intellectual component of music, but I feel it extremely deeply. The structure and the

counterpoint, the development of the themes, are rooted in our mental structure in the same way, or related to the way, that I feel the texture of mathematical equations which express physical laws. There is a beauty to that.

WEST: Do you see or feel that as a spiritual experience?

CARRUTHERS: Both things are spiritual, but there is the question about truth. In science there is finally an experiment. You may have thought that your equations were extremely beautiful but, in fact, they are likely to be wrong. However beautiful, you have to throw them away.

If you’ve written a marvelous sonata, in what sense is it right or wrong? It may have internal consistency, structure, and beauty. There is always this extra constraint on scientific work, which at least superficially distinguishes science from the arts. The arts seem more free-wheeling and lacking of boundary conditions. In fact, the farther you are from the exact literal truth, the more insight you may give to the actual truth. You may value it more because it’s so bizarre that it reflects onto the truth in a way which illuminates the truth, whereas with the physical theories, if they are wrong, they are out, and you are ruined. But in the actual doing of science, you don’t proceed that way. You’re optimistic, and you don’t throw away ideas until you’ve had the fun of creating something.

SIMMONS: Do you see more of an analogy between art and mathematics?

CARRUTHERS: Yes, I certainly do. Mathematics can create its own structure of logic and beauty and doesn’t have to face an experiment.

WEST: Changing the subject, who are some of the physicists that you most

admire? The people outside physics? Whom do you see as wise men?

CARRUTHERS: I always especially admired Landau because of his universal scope and overwhelming intuition. It’s easy to admire some of the historically great mathematicians. You can’t imagine how they did what they did. Musicians, poets, writers, and so on too numerous to list. Montaigne’s essays are splendid examples of wisdom. I often read one at bedtime. The cynical old bastard has thought of everything and is gifted with the best touch. I love Russian literature; I find wisdom in all this madness, as in Kafka.

SIMMONS: This is the second time that Russia has come up in the conversation, and I want to ask your opinion of U.S.-Soviet scientific relations. What is a proper and profitable posture for individual scientists in the U.S. and for U.S. institutions dealing with Soviet colleagues?

CARRUTHERS: I’m somewhat of a moderate on this question. As in the U. S., there are many kinds of people in the Soviet Union. There are those who have used the system to promote themselves, and there are many highly principled, brilliant people who somehow manage to create in this system. I feel that you have to encourage the latter and maintain contact with them. Simply turning your back on the scientists because the government is repressive in regard to human rights is probably a mistake. On the other hand, they’re too outrageous in sending Sakharov off to live under house arrest in Gorki. Some official notice is necessary, not just for his sake but for the cause of these people in general. The cessation of official exchanges for a finite fixed time was a useful expression

of that, though the Soviet government does not seem to understand that this is felt by the majority of the American scientists. The August decision of the U.S. Academy to continue the ban on exchanges is destructive to the exchange program, however.

SIMMONS: A number of colleagues have said they cannot in good conscience go to meetings in the Soviet Union or to meetings where Soviet scientists were present. Is that dangerous?

CARRUTHERS: I think the self-righteousness is self-evident. It's very easy to be self-righteous in this world, and I don't see the point of that. It's very frustrating to realize that we have to work on a time scale which is so long that we may not live to see the outcome of it. By encouraging the moderate elements in the Soviet Union, perhaps in 50 years there will be noticeable change. I think we should do that. The fact that we are frustrated year after year and have all of these absurd and humiliating developments doesn't seem to me to be reason to give it up. I think a continuing, restrained and, above all, adult response to these problems is what we need.

SIMMONS: Acting as the representative of our Academy of Science, you've dealt with representatives of the Soviet Academy. Our academy has little impact on anything. Theirs runs almost everything in science. What's it like dealing with them in those circumstances? Do they realize the difference?

CARRUTHERS: My general impression is, they overestimate the political influence of the American academy. They find it hard to realize that it's not symmetrical. On the other hand, they don't really run everything, because

there is the State Committee on Atomic Energy, which is somewhat like DOE, and there are, of course, the KGB and the Communist Party to complicate things. For example, dealing individually with Russian physicists, I found them extremely cooperative and helpful, but by the time an agreement filters through the system to an actual exchange program, so many people have put in their two-cents worth that the resemblance to the original agreement is hard to see. Their internal politics is even more complex than ours.

WEST: Do you feel there should be more institutional guidance in U. S. science and technological programs than there is at present?

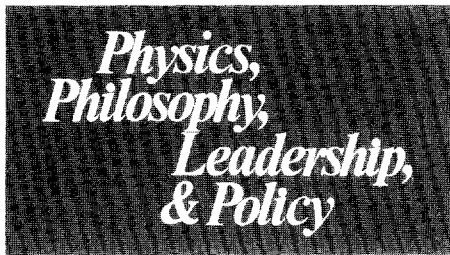
CARRUTHERS: I certainly do. The National Academy of Sciences occupies the turf but doesn't do anything much except issue reports.

SIMMONS: What about the Physical Society?

CARRUTHERS: The Physical Society is an ineffective organization. In the American Physical Society you have, of course, only a part of science. The officers are donating their time. Most of them don't take their gloves off and go in there and fight. You have to be willing to go into the arena these days to advocate your cause. It's not a matter of gentlemen discussing the future of science over tea. There are too many powerful interests in the country who will use up all the resources with no attention to the long-range health of the society. You have to be willing to represent your case in the most powerful possible way. That means confrontation.

SIMMONS: Is there a way of using the Physical Society, of changing it so that it can be an advocate for physics?

***“I can't account for the intellectual component of music, but I feel it extremely deeply. The structure and the counterpoint, the development of the themes are rooted in our mental structure in the same way or related to the way, that I feel the texture of mathematical equations which express physical laws. There is a beauty to that.”***



*"The greatest virtue is to survive, living and leading the life that you consider productive and decent. "*

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## INTERVIEW

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CARRUTHERS: It does advocate. The advocacy depends very much on who's President at any given moment. We have gentlemen Presidents who don't really work hard at it, and we have street fighters who get in there and stir things up.

SIMMONS: Street fighters?

CARRUTHERS: Well, you do it in hallways and corridors. It's not really street fighting—it's corridor fighting.

SIMMONS: What are the gut issues?

CARRUTHERS: Unfortunately, money is the bottom line. There is the partitioning of the money that science gets, and there is tremendous confusion about basic versus applied science. In the last few years, the Carter regime has been proudly announcing giant increases for basic science. For example, the National Science Foundation, a case with which I am familiar. Every time I look at one of those budgets closely I find that the increases are not at all in basic science. They are in engineering or something else, educational programs or things like that and at the end of the year basic sciences don't even get inflation. It's been true for several years.

WEST: What effect will this have?

CARRUTHERS: In the case of the Science Foundation, the deterioration of the university research base has been extremely serious: cuts in the number and size of grants to university professors, cuts in the number of postdocs and graduate students supported by these funds, dilapidated equipment, the U.S. falling behind competitively with respect to other countries in certain areas. Unfortunately, a similar trend is going on in the Department of Energy, which supports the national labs. The high-energy physics budget is quite a

ways down, and the Europeans are taking a very competitive run on our pre-eminence in that field.

My general attitude towards doing science is that if we don't try to be best we're going to be second-rate. We have to try to be best. We can address the case of basic science, and I mention high-energy physics, for example, where we've dominated in almost every sense for 20 to 30 years, partly due to the rejuvenation of American physics during the Second World War by European immigrants. Even on our native strength, it has been an extremely powerful showing. The obvious excitement of the subject has penetrated every part of the culture, and the very best young people are thereby attracted into science. First of all, it might be the most glamorous things of black holes, neutrinos and quarks and whatever—a bias toward science and technology attracts people who in earlier days might have done something different. As soon as we lose that glamorous image, it seems unlikely that the very best people will go in for these fields. It's extremely important to maintain an image of being first-class as a country in as many areas of basic science as we can. We can't maintain our position in the world unless a special effort is made to get the very best people into these areas. So we have to guide them towards working in science and technology in order to maintain the political, military and economic strength of the country in the face of a very hungry and aggressive outside world. People have to be in a high state of excitement to maintain any excellence.

SIMMONS: A lot of our colleagues see some immorality in weapons work or association with institutions that engage

in national defense work. Do you have anything to say on this issue?

CARRUTHERS: The greatest virtue is to survive, living and leading the life that you consider productive and decent.

WEST: Do you feel that scientists have a responsibility to work on problems of national security and defense? Would you say, for example, that if we're to support SLAC or Fermilab, the people there should spend some of their time thinking about these problems.

CARRUTHERS: I don't think you can apply a formula to these people. They are all quite different. I do think there is a responsibility, and adopting a holy attitude towards it is naive. But if you are bored or hate that kind of work, you shouldn't be made to do it. I think scientific activities can be defended on an intrinsic basis. There is a need for intelligent people to interact with the defense community, because that is a closed society where people speak a special language and often arrive at very peculiar conclusions.

SIMMONS: Do you disagree with advocates of phased disarmament who argue that an increase in armaments would be stabilizing in many instances?

CARRUTHERS: I know something about the arms race, and it's appalling. It's increasing all the time and it's very frightening. After a summer at JASON I get very depressed. I've heard all the generals and colonels, and I've heard what the rampaging technology can do next year that it couldn't do 10 years ago.

WEST: Do you believe in a kind of nuclear stalemate theory?

CARRUTHERS: I have believed in nuclear stalemate theory, but only between the U.S. and the Soviet Union. As soon

as every chief in the world has his own bomb, you can expect nuclear war. I think there are bad times ahead.

WEST: Within that context, what do you think of the morality of someone working on defense problems? What kind of stance should that person take?

CARRUTHERS: I've been accused of lending my own—as I hesitate to say—prestige to it. I think it's also immoral to turn your back on it and say that you're too fine to be involved in even discussing it.

SIMMONS: Should someone whose morality is too fine to work on defense problems be working on disarmament problems?

CARRUTHERS: If they're good enough in science, let them do whatever they want to do. They don't have to be intelligent in the ways of the world to advance the cause of knowledge.

WEST: What are your reactions to people who are highly critical of nuclear energy?

CARRUTHERS: I think they haven't thought through the awful alternatives. I consider it the least of the evils. The real evil is that the planet is overpopulated, and there's no sign of change in the pressures from that direction. Nobody has ever had the guts to face that evil. I hope we don't solve it by a nuclear war. Coal is much more dangerous than nuclear power. Slavery to OPEC is much more dangerous than nuclear power. What else is there?

Of course, the problems are very tough, and science may be silly to dash forth with a quick answer. On the other hand, what's happened in the political process is a paralysis in almost every sensible proposal. We have to live with uncertainty, and we might as well live on



a ten-year time span and not say, "Well, in a hundred years perhaps the salt mine will crack and there will be some horrible leak and my grandchildren will be rendered sterile or some other worse thing." There is a kind of self-righteousness in that kind of attitude which can be very counterproductive.

WEST: One of the reactions in the political sphere is that scientists have been self-righteous.

CARRUTHERS: It's almost required. You must have the self-confidence to carry you through the hostilities and criticisms. It may look like arrogance even if it's not.

WEST: Do you see that as a serious problem?

CARRUTHERS: Well, I don't know why the public trusts Walter Cronkite and not the scientists ■

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# Los Alamos

Los Alamos National Laboratory  
Los Alamos, New Mexico 87545