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Michael Nauenberg, Professor of Physics: Recollections of UCSC, 1966-1996

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Abstract:
Michael Nauenberg, Professor of Physics: Recollections of UCSC, 1966-1996, is the edited transcript of a single interview conducted by Randall Jarrell on July 12, 1994. Nauenberg received his B.S. from the Massachusetts Institute of Technology in 1955 and his Ph.D. from Cornell University in 1960. Prior to his appointment as a professor of physics at UC Santa Cruz in 1966, he was an assistant professor at Columbia University and a visiting associate professor at the Stanford Linear Accelerator Center and Stanford University.

At UCSC, Nauenberg served as department chairman of physics from 1970 to 1972, and again from 1983 to 1985. He was instrumental in developing both Stevenson and Crown Colleges, but in 1973 shifted his focus to building a graduate program in physics. He also founded and served as the director of the Institute of Nonlinear Sciences at UC Santa Cruz.

Nauenberg's primary research interests are in particle physics, condensed matter physics, and nonlinear dynamics, and he is the author of numerous publications in these areas. His most recent work is on a new quantum mechanical treatment of neutrino and neutral meson oscillations and on the dynamics of wave packets in weak external fields. He has had a long standing interest in the history of physics and mathematics, particularly during the seventeenth century, and published about a dozen articles on the works of Hooke, Newton and Huygens, and several reviews of recent books on Newton's *Principia*. He has a particular interest in the history of physics and has helped to bring historians of science and physicists together.

In this oral history narration, Nauenberg shares his impressions and critical evaluation of UCSC as an experiment in public higher education, particularly the tensions between the college-based model and the pressures of the faculty tenure system within the large research University of California system. He points out that the founders of UCSC appear to have overlooked or underestimated the demands building graduate programs would make on faculty members' time. He discusses faculty appointments in the physics department, as well as other key faculty members on campus. He provides a sweeping and cogent assessment of the strengths and


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achievements of the physics department, and describes the struggle to establish the very successful Santa Cruz Institute for Particle Physics, as well as his frustrating and ultimately unsuccessful political battle to attract the Institute for Theoretical Physics to UC Santa Cruz. This oral history is an invaluable and insightful historical contribution by a senior faculty member with an extensive and distinguished history on the Santa Cruz campus.

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Introduction

From 1991 through 1994, the University of California initiated three early retirement options for faculty and staff, known as VERIP (Voluntary Early Retirement Incentive Program), as a salary saving measure during a period of unprecedented budget cuts. The thinking was that many senior faculty with high salaries would retire and be replaced by young faculty at the lower end of the salary scale. At UC Santa Cruz, a number of pioneering senior faculty opted for early retirement. Since many of these faculty might leave the area, the Regional History Project initiated interviews with a group of them in order to document their recollections of early campus history and their participation in the development of various boards of studies (at that time UCSC's designation for departments) which over the years has led to the campus's national academic distinction in a number of disciplines, particularly in physics.

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Stevenson and Crown Colleges, but in 1973 shifted his focus to building a graduate program in physics. He also founded and served as the director of the Institute of Nonlinear Sciences at UC Santa Cruz.

Nauenberg's primary research interests are in particle physics, condensed matter physics, and nonlinear dynamics, and he is the author of numerous publications in these areas. His most recent work is on a new quantum mechanical treatment of neutrino and neutral meson oscillations and on the dynamics of wave packets in weak external fields. He has had a long standing interest in the history of physics and mathematics, particularly during the seventeenth century, and published about a dozen articles on the works of Hooke, Newton and Huygens, and several reviews of recent books on Newton's *Principia*. He has a particular interest in the history of physics and has helped to bring historians of science and physicists together. [1]

In this oral history narration, Nauenberg shares his impressions and critical evaluation of UCSC as an experiment in public higher education, particularly the tensions between the college-based model and the pressures of the faculty tenure system within the large research University of California system. He points out that the founders of UCSC appear to have overlooked or underestimated the demands building graduate programs would make on faculty members' time. He discusses faculty appointments in the physics department, as well as other key faculty members on campus. He provides a sweeping and cogent assessment of

1. Please see Nauenberg's web page for more details on his activities and publications http://mike.ucsc.edu/~michael/

the strengths and achievements of the physics department, and describes the struggle to establish the very successful Santa Cruz Institute for Particle Physics, as well as his frustrating and ultimately unsuccessful political battle to attract the Institute for Theoretical Physics to UC Santa Cruz. This oral history is an invaluable and insightful historical contribution by a senior faculty member with an extensive and distinguished history on the Santa Cruz campus.

As this oral history goes to press, Michael Nauenberg is now a professor emeritus at UC Santa Cruz and remains a very active member of the physics profession. He recently organized a forum of the history of physics session of the April 2000 meeting of the American Physical Society. The topic of this session was ""New Perspectives on the History of Ancient Astronomy."

Nauenberg kindly read the edited transcript and made numerous small changes and corrections, as well as adding a written narration on the physics department which appears on pages 34 to 42. Copies of this volume are available in the Bancroft Library at the University of California, Berkeley; and in Special Collections, McHenry Library, University of California, Santa Cruz. The Regional History Project is supported administratively by Christine Bunting, head of Special Collections and Archives, and Acting University Librarian, Robert White.

—Irene Reti

October 29, 2004

University of California, Santa Cruz

Appointment at UC Santa Cruz

Jarrell: To start out, what year did you come to Santa Cruz?

Nauenberg: 1966.

Jarrell: What attracted you to this campus?

Nauenberg: Well, the first thing naturally was the absolutely magnificent, beautiful setting. Having been on the East Coast in rather dreary institutions like Columbia University (laughter) it seemed unbelievable that a campus could be built here. So that was my first reaction. But actually I came to talk with [Chancellor Dean] McHenry in 1965, because of the ideas that he and others had about how to develop this institution, which seemed to me very exciting. The notion that one would not just work in one's discipline, but also have very close interactions with people in other disciplines, particularly for a scientist to have interactions with people in the humanities and the social sciences, seemed very attractive to me. This was possible because of the colleges, the unique feature here. Because in the past I had found that there was a complete separation between the sciences and the humanities. In institutions like Columbia and Stanford, where I was a faculty member, and at Cornell and MIT, where I was a graduate student, I never met people in philosophy or history, or anything else. So I thought, you know, it would be a more interesting career if I had opportunities for these types of interactions.

Jarrell: More collegial and more interdisciplinary?

Nauenberg: Well, not just interdisciplinary. I'm interested in other things besides my professional interests, and I wanted to be able to talk, discuss, to participate in seminars with people in other fields, which eventually I did manage to do. It was very exciting to me. It seemed a more fulfilling career than one in a normal institution, which meant working only with colleagues in one's own discipline. To give you an example, the only historian I ever met when I was at Columbia was Walter [P.] Metzger, who actually lived in the same apartment building. We met not at the university, but in the apartment elevator one day and we began to talk. He said, "Oh, you are at Columbia. I'm at Columbia, too." So we became friends, not because of the university setting, but because we happened to live in the same apartment. When I decided to go into academic life, as opposed to work in laboratories, it was because of my broader interest in other things than science. I was discouraged in my early career by the fact that when you start university life, you meet people in your own field, and your whole career seems to be centered around your department, your area of interest. So I thought this institution [UCSC], after talking with Dean McHenry and others, seemed to be on the right track, and the idea of colleges where people would mix and possibly teach together with people in other fields was a very attractive one.

Jarrell: How did you get connected up with UCSC? Who was involved in your appointment here?

Nauenberg: Well, I remember this very specifically. As I said, I came in 1966, but already a year earlier I was invited to come by Chancellor McHenry. I was

actually interviewed by Page Smith, who was at that time the first provost at Cowell College. It was this interview that decided me to postpone my coming to Santa Cruz. Page Smith told me what he thought a physics department should be like. I remember . . . at the end of the conversation I said to Page Smith, "Well, you mean, you want to have a history of physics department in Santa Cruz, not a physics department?" I didn't like what I heard, and I thought, well, I am also interested in research in my own field, and if this is what his ideas are. . . And of course at the time there were no scientists here. Nobody in the sciences had been recruited yet. [Francis H.] Clauser, and [Kenneth V.] Thimann had not yet come aboard, or were only being considered. I thought that potentially . . . it would have been difficult for me to develop my own ideas of what I thought a physics department should be like. I decided that with someone senior like Page Smith being in charge of the college, and being the provost, and having ideas so fundamentally different from what I thought should be done to develop the sciences here, that it was kind of risky. So I sent a long letter to McHenry and said that I was interested, but that I would postpone coming here until I saw more clearly where his institution was going.

It seemed to me that at the beginning people were floundering around as to how to develop the campus. There were general ideas. But specifics... You know, you can't ask someone in the history department to plan for a department in the sciences, particularly someone like Page Smith who was never very favorably inclined, as in later years it became clear to me, towards the sciences. So the result was that I postponed the decision. I told McHenry that I was interested, but I

didn't come. A year later, I was invited back, and by that time Francis Clauser was here as a vice chancellor for science and engineering, and Kenneth Thimann had accepted the position as chairman of biology and provost of Crown College. When I spoke with both of them, their ideas resonated with my own. They clearly understood the role of science; we had similar ideas. I told them that that was much more like what I wanted to hear. (laughter) They indicated we were also going to have a serious physics department. Above all, I was interested in graduate studies. For example, I remember that when I mentioned graduate school to Page Smith I got a blank stare back from him.

Jarrell: The idea that there would be a graduate program in physics?

Nauenberg: Yes. Without a graduate program in physics I would not have contemplated coming here. But they assured me that it would in fact be in the plan to have a graduate program, not just undergraduate teaching, which I was very much interested in too. I didn't want to be in a four-year college instead of a university, where you teach only undergraduates. That wasn't my plan.

Jarrell: Well, UC Santa Cruz couldn't turn out that way, because it's a UC campus, which is a research university.

Nauenberg: Yes, but you know all these things were not clear to outsiders like me at the time. UC Santa Cruz seemed as if it would be different. At the time that I came here, I also had an offer from UC Irvine. In fact, I was considering going to Irvine, which had started at the same time. But UCI had a much more

conventional format, and they already had a chairman of the physics department, Ken[neth W.] Ford. I felt if I went to Irvine, things would be easier, but it didn't have that feature [the colleges] that attracted me to Santa Cruz. As I said before, I wanted to be in an environment where one would interact with people in other disciplines.

Building Stevenson and Crown Colleges

Jarrell: Which leads me to my next question. Of course Santa Cruz at the time called its departments "boards of studies." Your appointment was jointly a college and a board of studies appointment. Tell me about your college affiliation. Maybe you can hit the high points in the early years, what your experience has been. Was there a conflict between these two affiliations?

Nauenberg: No, in fact I felt my own experience was just what I hoped that it would be in the early days. Actually, I was involved with the build-up of two colleges. When I came here Stevenson had just started, and Crown had not yet begun. Although I was interested in being in Crown, the planned science college, where Kenneth Thimann would be the provost, Charles Page had been selected as the provost of Stevenson and I was asked to join an existing college. So the first year I was involved with Stevenson. As I hoped, I made many friends in the humanities and social sciences. I liked Stevenson. I liked the atmosphere, the general ideas, and I participated actively in developing various programs.

At the same time I was also involved with the start of Crown College. Stevenson had already been built, and there existed offices there, but I didn't have an office

in Stevenson. I had an office in Thimann Labs, what is called now Thimann, Natural Sciences I, next to Burney [J.] Le Boeuf. And soon I learned a lot about the sex life of elephant seals, which was his area of expertise. On the other side was another biologist, so I had two biologists next to me during my first year in UCSC. At the same time that I was at Stevenson, I was participating with Kenneth Thimann in the design and the development of Crown College, because ultimately I wanted to be in a science college rather than in a social science college.

My early experiences there were extremely favorable. Through Crown College I met the biologist Cedric [I.] Davern. Eventually we teamed up and gave a college course on evolution, both molecular and astrophysical evolution. I got to know Cedric extremely well. I'm sure you're aware of him. He was one of the pioneering faculty here. What I had hoped for at UCSC had in fact materialized. Every week I was involved in to the college. We had regular faculty meetings. We discussed programs. I was on an executive committee, and I came regularly to discuss programs and other things. I got to know a lot of the faculty through the college; the college was the social environment through which we got to know each other. Eventually, I got to know all the members of my college quite well through our faculty meetings chaired by Kenneth Thimann. We had regular meetings of the faculty to discuss programs, students, social events, organize talks, meetings, etc. We also had events where we would invite faculty from other colleges. The environment on the campus that went beyond my usual

environment as part of the department was really through the college. And I thought about it very positively.

Developing Graduate Programs

Now, as far as time involvement, in the first four or five years we could spend a lot of time working to develop the college, particularly because at that time we still did not have a graduate program, so we didn't have to worry about graduate students. Our graduate program started in 1971 or 1972. Of course, with the advent of graduate programs, the involvement of faculty with colleges became limited. Since there are just twenty-four hours in a day, there's just so much that you can do. The campus had been developed without any careful thinking about how the faculty could develop both colleges and graduate programs. How would you get faculty more involved in the colleges after their graduate programs were mounted? I mean, if college involvement substituted for the lack of graduate programs, that was one thing, but when you then wanted to have both graduate program and college involvement, to do that with the same setting as the other campuses became a major problem. This was a problem that was totally overlooked. Perhaps I shouldn't say overlooked, but it was never discussed. I don't recall participating in any discussions as to how we were going to solve that problem.

Jarrell: Yes, I think nobody ever really thought of it as a problem.

Nauenberg: Well, let me put it this way. I thought that the original planners, Dean McHenry, and Clauser, and the senior faculty at the time did consider this

problem. Junior people like me were just beginning their careers. I was only six years out of my Ph.D., and I had no concept of these things, or wouldn't be thinking along those lines. But I can't believe that the senior faculty, i.e. Thimann, Clauser, and McHenry hadn't realized that things would change once we had graduate programs, and that these programs would require additional resources, the kind of resources that the colleges in England have—like Cambridge and Oxford, which were the models for UCSC. What they forgot was that . . .perhaps they didn't forget, but they didn't emphasize that these colleges in England have extra resources and some, e.g Trinity College in Cambridge, are extremely wealthy.

Jarrell: Oh, they're endowed.

Nauenberg: They are endowed, and we had nothing. So it's not a mystery that once the graduate programs began to flourish here, that had to come at the expense of faculty involvement in the colleges. Some faculty were able to maintain their college involvement, because they were not involved with graduate students or graduate courses so they could continue with their college involvement. But the rest of us were heavily committed to developing graduate programs. I had to devote an enormous amount of time and attention to develop graduate physics programs here, to hire graduate faculty and to create an atmosphere so that we would have graduate students. After the middle Seventies I had to literally give up my involvement with the college. I think after 1972 or 1973 my college involvement just fell off exponentially I came to meetings and

social events, but I never again taught a college course until last year, when I taught a course on the scientific revolution. That was inevitable, because no one offered any sort of course relief within the department. We had to teach our departmental courses, including our graduate courses. There was a structural mistake here at the very outset. There was some sort of a dream, as to how we would have both college instructions and graduate programs, without the corresponding resources to accomplish it. If I hadn't involved myself in the graduate program there would have been no Institute for Particle Physics (SCIPP), which I helped to found, or an Institute of Non-linear Science. We also attempted to create an institute for theoretical physics, but unfortunately we lost a competition for an NSF grant to UC Santa Barbara. We can talk about that later.

Jarrell: One thing you haven't mentioned in this division of labor. Mounting graduate programs would inevitably diminish your participation in college teaching, in college-building. Also, the whole issue of conducting your research, and having time for your research. Even before the graduate programs were mounted that seems to have been a problem.

Nauenberg: Well, I don't think so. Because I did manage to do my research, and I think I was prolific. I managed to write quite a few papers. If you look at my bio-bibliography during the period of the Sixties, I wrote a lot of research papers. I had postdoctoral fellows, and every year I had research grants. I worked mostly with these postdoctoral fellows. Doing basic research was not the problem, because people still can do that and have extra time because they don't have to

deal with program development and graduate programs, and taking care of graduate students. Graduate students, and supervising theses takes an enormous amount of time which went above and beyond what was required earlier. So I don't think research was a problem. To have this broader contact that otherwise would not have occurred broadened my horizons and gave me a better perspective on things. If you always focus narrowly, all a person sees is his own discipline, and this is not as healthy an environment, I think, as when you have the opportunity of seeing what other people do, and not just in a kind of superficial way, but to be involved with people in other disciplines.

Controversies over Tenure Criteria

Jarrell: I think one of the reasons I asked that question is . . . I'm sure you recall when Paul [A.] Lee was not given tenure here, that Page Smith shortly thereafter retired, and at least part of his reasoning was this whole publish or perish controversy, which did absorb some faculty members in that argument of—can we build a college, can we be devoted to undergraduate teaching and service to the colleges, and yes, it's at the expense of some people's research.[2] It seems that was not a problem for you.

Nauenberg: For me?

Jarrell: Yes.

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2. Page Smith resigned from the University in 1973 after his colleague and friend, Paul Lee, an Assistant Professor of Philosophy, was not given tenure at UCSC. Smith used that occasion as an symbolic protest against what he considered the rigidity of the "publish or perish" system governing promotion and tenure at UCSC—Editor.

Nauenberg: On the contrary, I became involved in that argument with Page, and I confess that I helped his early retirement. I felt profoundly frustrated with Page Smith. From the outset, even before I came here, as I mentioned earlier, I already disagreed with Page's views on academic issues. He had a reputation as an outstanding scholar, and had written books that are very highly regarded. But he seemed to me to have had a blind spot. Perhaps he thought that everyone was like him—self-motivated and willing to do scholarly work. He thought nobody should sit and judge junior people about their publications, or expect them to show what they could accomplish, and I can give you various examples of that.

At the time, we had a member of our department who was also a fellow in Cowell College. He was here for four years and was coming up for tenure, but he had not published one single paper. Both my colleague Bruce Rosenblum and I, as senior faculty members (at the time we were the only tenured physics faculty members) had the obligation to review his work and to decide whether to recommend him for tenure. We made efforts to get him to write at least one paper for publication without success. Therefore, we decided against forwarding a recommendation for tenure. Page Smith, however, gave a recommendation from Cowell College which was signed by everybody who was anybody on this campus. I have a copy of this letter saying that this young man was one of the great luminaries in the college, and therefore should be promoted.

Of course the University of California has standard procedures for promotion, and the appropriate committees were not persuaded to recommend tenure, and

neither was Chancellor McHenry. But if it had been left to Cowell College, he would have received tenure. To me this was an eye-opener on the dangerous attitude of some colleges' faculty who wanted to promote everybody. Page Smith said that once you knew somebody personally, how could you deny that person tenure? But this view contradicts the rules of the University of California, which consider personal relations to be irrelevant for tenure decisions. The University of California has systemwide rules governing tenure criteria, in which you have to accomplish certain teaching and research missions, and you have to persuade your peers of the quality of your teaching and your research. Tenure is awarded on those grounds, and on the promise of your future work. Some people manage to accomplish these things and they get tenure and that's the end of it. They never again do anything and coast along. That's one of the disgraces at the university. Unfortunately, we had a lot of that. I've been on various committees and I feel our University blew it in many respects. I mean, you can see how disparate the quality of the so called boards [departments] are. We have excellent boards, and I'm proud to say that physics is one of the boards that is highly regarded, primarily because we have adhered to the regulations. We may have made some mistakes, but we tried at every point to do things by accepted procedures. So have other boards that are very accomplished.

But there were boards that played around with the system and used college criteria for promotion. Today, some of them are even in receivership. More should be in receivership, because they recruited and promoted people who never again did serious scholarly work. I think Page would have destroyed this campus as a

serious institution if he had continued to be influential, because he was completely opposed to the UC University-wide regulations regulating tenure. He felt that too much was being published and a lot of it was garbage, all of which is completely true . . . but he had no alternative method to insure that somebody would do good work. Page did not provide an alternative other than "let's hope for the best." Humanities was supposed to be the centerpiece of this campus, with sciences only as the frosting on the cake, but that has not turned out to be the case.

Jarrell: Yes, it was quite surprising that the original vision stressed liberal arts, humanities, but it's been the sciences which have become preeminent.

Nauenberg: Yes, but I understand why because I saw it all happen with my very own eyes. You know, the first time I came here in 1965 I felt that the sciences were going to be sort of the frosting on the cake. We were just here to round things off. UCSC was going to shine as a humanities and arts institution. This was going to be the emphasis on this campus, and the sciences were going to be there because...

Jarrell: They have to be there?

Nauenberg: Because a university needs science. Of course that's one of the reasons I didn't come the first year, because I didn't feel that in the beginning McHenry had the sciences as much in mind as he had the other aspects. He started with the humanists, and Page Smith was one of the first recruits.

Jarrell: Yes.

Nauenberg: The year after that he recruited people of the caliber of Kenneth Thimann, Aaron [C.] Waters, [Francis] Clauser, and Joseph Bunnett, all of whom I respected. I said to myself, well, if those guys came here and they were senior people, I think that there's a chance for serious science here. Then I felt . . .

Jarrell: It wasn't so reckless to come here?

Nauenberg: Yes, that it wasn't so reckless. The scientists maintained strict standards in almost all the disciplines. For example, Aaron Waters did a great job in the earth sciences, and today we have one of the most respected earth sciences departments in the world. Kenneth Thimann did very well recruiting in biology, and Joe Bunnett likewise in chemistry. But I was not as happy with mathematics. The chair for mathematics, Ted [J.W.T.] Youngs, did not do as well, and the result is that even today this department is somewhat in trouble.

Jarrell: I'm not aware.

Nauenberg: Again, primarily because of the recruitment. Look at the respected departments here. Find out who were the pioneering faculty and who got things started. (laughter) The good departments had usually very good leadership at the very beginning. Some departments have now been turned around because of efforts made after [Robert L.] Sinsheimer became chancellor. He saw some departments going very poorly, and helped rebuild them. But some departments did very poorly because the initial people who were recruited were weak.

Often in the rush to build up it seemed as if nobody was watching. It's unlikely that some of the mature campuses like Berkeley or UCLA would have considered some of the incompetent people recruited at UCSC during the early days. It's extremely difficult to do anything once a faculty member has tenure if he/she is not performing according to academic expectations. Sometimes there were legal suits, trying to get rid of these people. (laughter) You probably know some of the most famous ones. To no avail. My notion is that if you recruit a faculty member and he/she turns out to be weak you just write it off as a business loss.

Jarrell: So what do you do? You just say it's a loss?

Nauenberg: Yes. So you keep him/her on with as low a profile as possible. Some people say you should burden them with a lot of teaching and so on, but I think that's throwing them at the very people we're trying to serve, the students.

Page Smith fueled the controversy with Paul Lee. Lee was in Crown College, brought here by Kenneth Thimann in religious studies. Like other people who came here, he bought into the Santa Cruz ethos.

Jarrell: In the counterculture?

Nauenberg: In the counterculture. We discussed Paul Lee's future, because I was a member of the college, and I understood that he had not done the minimal amount of expected scholarly work. Kenneth felt very protective because Lee was, after all, his protégé. But there was no way he could be promoted to tenure. Page Smith's defense of Paul Lee just underscored the extremes of Page's views.

He insisted in not following the systemwide regulations, but his own views about promoting faculty. At this point he was challenged by some of us. I sent him a letter and said . . . at this point he was actually on CAP, you know the committee for . . .

Jarrell: Academic promotion.

Nauenberg: I said to Page, if you're on that committee, but you do not believe in the systemwide regulations for recruitment and promotion, how can you explain this? I mean, you can't be on a committee and then not abide by its regulations. That was right in the middle of the Paul Lee thing. He wrote me a letter and again enunciated his principles, that friendship, etc. makes it impossible to deny someone tenure. Page threatened to leave UCSC if Lee was not granted tenure, and he retired soon afterwards.

Jarrell: You know, sometimes this argument, this issue, is reduced to some sort of conflict between humanists and scientists. I don't think that that's the case at all. You seem to be talking about standards, and being a part of a larger system.

Nauenberg: It's not a conflict between humanists and scientists at all. One of my closest friends on this campus was Joe [Joseph H.] Silverman, who was a senior professor of Spanish literature. I got to know him extremely well. We talked endlessly about these very same issues, and we saw eye to eye. In fact, Joe was more of a mentor and an advisor to me on academic issues than anyone else. There was rarely a conflict between our views. The same was the case with John

[M.] Ellis, who I also know very well. John was a senior professor in German literature, and he often said he wished the humanities would have the high standards that the sciences here have.

Early on, some of the top humanities faculty left in great disappointment. For example, Richard Olson, in the history of science, went to Harvey Mudd. William Lillyman, a professor in German literature, felt unappreciated and did not get the merit increase he expected. You know what happened to him? He went to UC Irvine and promptly was appointed the dean of humanities, where he served with great distinction for many years. Many of my friends in the humanities—Lillyman, Olson, Silverman, Ellis, and others who were accomplished scholars were running into difficulties because of the ambiance set up by people like Page Smith.

Jarrell: Right. There was an ambiance in those years in the late 1960s and 1970s of praxis and experiential learning.

Nauenberg: But this happened not only in the humanities. There were attempts to bring some of this into the sciences. You may remember that Bob [Robert S.] Edgar became the first provost of Kresge College. Edgar was a distinguished molecular scientist from Cal Tech who had done pioneering work, mapping genes of drosophila. There was no question about his stature as a scientist. But when he came, he became even more extreme than some of the people in the humanities. He brought with him a guru, Michael Cohen, and between the two of them they turned Kresge College into what we used to call the touchy-feely college. I went

to several meetings at Kresge when I was trying to hire people in the physics department. At that time every appointment in the department had also to have approval from one college. For example, my colleagues and I thought that Stanley Flatté would be a very good choice for the department, which turns out to have been the case. As you know, Stanley is a distinguished, well-established figure here. But at the time he was rejected by Kresge on completely spurious grounds. I went to the interview that Michael Cohen gave him, and I thought I was in never land. Another scientist who got sucked into this stuff was Matthew Sands, who came from SLAC to be the dean of natural sciences. I helped to bring him here. He joined Kresge, and evidently was delighted with this whole counterculture thing. I insisted on hiring Stanley, and I told Matt, who agreed with Kresge's recommendation and refused as dean of sciences to make the appointment, that I would go to Chancellor McHenry unless he reversed his decision. At the time Matt seemed more interested in Kresge than in the development of the sciences, and subsequently he was asked by McHenry to step down as dean of sciences.

Physics Board of Studies

Jarrell: I'd like to move on now to your assessment of the physics board. You were the first appointment?

Nauenberg: Not quite.

Jarrell: The second appointment?

Nauenberg: Well, it turns out I was probably the first person who was approached here. But because I turned the first offer down, in the meanwhile, there were several other appointments, of which only two actually were kept, namely Ron[ald H.] Ruby and Peter [L.] Scott. They were the first two people who accepted as junior faculty and who stayed here. There was another physicist, [John] Gillespie, who must have been a temporary appointment, because he wasn't here when I came. The other person who came the year I came was Bruce Rosenblum. So then we had four faculty, as best as I remember, in 1966.

Now, you ask me about my assessment of physics and so forth. As I saw the development, I felt that there was a unique opportunity here to develop a physics department and emphasize certain areas for which we would have the opportunity to attract good faculty. For example, the proximity to the high energy accelerator at Stanford suggested high energy particle physics, which was my primary interest at the time.

Jarrell: Right, SLAC. [Stanford Linear Accelerator Center]

Nauenberg: The linear accelerator, SLAC, was an ace card for UCSC. I came from there, and at the time my primary research interest was in particle physics. I also felt SLAC would have a great attraction for faculty who wanted to do research here. We were in competition with institutions all over the country to attract faculty, and you might ask—why should anybody want come here?

Jarrell: I was actually going to ask you that.

Nauenberg: In the 1960s there were no difficulties as there are today in getting faculty positions at good departments. So we needed to have some reason that would make UCSC attractive. I felt that we had to carefully choose our topic of interest. Now I happen to know from my own involvement in high energy physics that experimentalists like to be close to the laboratories. When you are at a great distance and are always in airports and on planes, that's not very conducive to happy family relations. And since wives also play a role in hiring people I played the card that said, "Look, here you are only forty-five minutes from SLAC. You can even live over the hill." And today many of our faculty live in Los Gatos. Of course, I kept up my connections with SLAC. I had an office there and I would go back and forth at least once a week and attend seminars there.

Jarrell: Really? Even after you were appointed here?

Nauenberg: Oh, yes. We had that advantage. Like everyone else in the field, one needs to have close contacts with other senior and expert colleagues. I wouldn't have come here if it were not for the proximity of SLAC so that I could continue to interact with my colleagues there. Every Friday I attended seminars at SLAC. I also would go to other colloquia and seminars either there or in Berkeley. The proximity to Stanford and Berkeley was essential to us in the early stages of development here.

Jarrell: It reminds me also of astronomy and astrophysics and Lick [Observatory], just in the sense of the proximity.

Nauenberg: Well, the importance of Lick in the development of science at UCSC cannot be underestimated. Let me say a couple of more things about SLAC. We focused on high energy physics as one of the major areas in which to develop physics research at UCSC. The other area was condensed matter physics. Bruce [Rosenblum] was the first person who was hired in this field. He was actually hired after I was hired here. After a couple of years Bruce decided to give up his research in superconductivity and gave up his laboratory. He never went back to it.

Jarrell: What does that mean?

Nauenberg: He just stopped doing research. He was recruited to develop condensed matter physics. At the beginning he had a very good lab, where he was doing research in superconductivity. But for reasons better known to himself he chose at one point to just stop doing research in physics.

Jarrell: Yes. And in a small department . . .

Nauenberg: This forced me to get deeply involved in the development of condensed matter as well as particle physics, which had been my field of expertise. At the time I was not involved in condensed matter physics at all. But after I realized that Bruce was no longer going to play a role in this, I decided to educate myself, and began to play a major role in the development of condensed matter. In fact, all the major hirings we made in condensed matter were people I've worked with, like George Gaspari, Joe Rudnick, and Peter Young.

Jarrell: And who were recruited for this area.

Nauenberg: Yes, by me. I felt there was no choice. Either I became knowledgeable in condensed matter, or that area would not be developed here, because without a scientist who is an expert you can't recruit properly. This is one of the biggest mistakes this campus made in the past. You can't recruit in areas where you're not knowledgeable. Because recruiting is more than just sending out for letters and then reading them and thinking that's the end of it. If you have no expertise in an area, you can't judge. You don't know how to read those letters. Quite often people are not entirely honest, and they want to recommend somebody because he's a friend or because they like him, and even . . . time and again you get a recommendation for somebody that someone wants to get rid of in their institution, and they make the person seem very good. I've seen this happen often enough to feel it's almost endemic in academia.

You have to be extremely knowledgeable in recruitment. When we have a full department and there are experts in all areas, as we have today, that's not a problem. But it was a big problem in the early period, not to make a mistake in hiring. So for these reasons I felt that I had to engage myself, which I did, to much more of an extent than I expected. So I became involved also in hiring our experimental condensed matter scientists, like David Belanger, and Bud [Frank G.] Bridges. Bud was hired at the time that Bruce was still involved in research. As you may imagine, it is very difficult to attract good people to a department where the senior faculty are not engaged in research.

We had few people turn us down in high energy physics, and we rapidly made great progress. Today everybody knows that that's one of our strongest groups, not only in the UC system, but anywhere in the world. So that went extremely well. We have a large, very accomplished group. Condensed matter physics has gone a lot slower, but we are establishing ourselves also in this field. In fact, yesterday I heard that we just hired a very good young man with strength in the field.

There were two important things we did that made a big difference here. One of these was to establish an institute, a research institute in particle physics.

Santa Cruz Institute for Particle Physics

Jarrell: The Santa Cruz Institute for Particle Physics.

Nauenberg: The development of the institute was a key idea initiated by my colleague Clem [Clemens A.] Heusch. I recruited Clem from Cal Tech. He and I and some of the junior faculty realized that to develop the high energy group it would be good for us to have a research institute to protect us against some of the problems that we saw coming up with the colleges, problems that we talked about earlier. We saw the institute as an umbrella that would help against the pressures for more college teaching at a time when what we wanted to do was to strengthen our research. So the Institute for Particle Physics played a key involvement in that.

Jarrell: What year was it established?

Nauenberg: Eventually the proposal for the institute went out in 1978. It took a year, and what is interesting is that it came very close to being destroyed. We did not get support from some of our colleagues. I still remember that one of the social scientists, Dennis McElrath, wrote a very negative report. He was on an important campus committee, and claimed that it was not a good idea to have an institute. So we had internal committees . . .

Jarrell: This was an ORU [Organized Research Unit]?

Nauenberg: This was to be an ORU in particle physics, but we had a strong negative report from a social scientist, Dennis McElrath. I happened at the time to be chairman of budget and planning, and I was very closely connected with the administration. Fortunately I got an early look at some of these reports, which were not sent to us directly. (They were sent only to the administration.) So I could fight some of this negativism. In the eleventh hour, the UC systemwide committee which reviews all the ORU proposals turned ours down. But I had learned how the system worked. I had gotten to know the key people there, and even knew the man who was in charge of the review. I requested all the documents, including the panel's report that turned down our proposal. It turned out that that this report was full of garbage. It was poorly argued against us. I promptly rebutted it, with copies to both [President] Saxon and his vice president, Bill [William B.] Fretter. Fortunately I knew Bill Fretter, who like Saxon was also a physicist. I sent him our rebuttal, and I said that the report of his committee was wrong on factual matters. They quoted McElrath. I knew that they would use

internal negative comments. Fortunately, Fretter and Saxon decided to ignore their own panel's recommendation, and accepted our proposal. In this way SCIPP was founded.

As everybody knows, today SCIPP is one of our strongest assets here. But it was a close call. I also asked Chancellor Sinsheimer to help, but I don't know whether he did anything. If SCIPP had not been approved, high energy physics here would have collapsed, and we would not have been able to attract some of the good people we have now. Instead of having one of the best experimental particle physics groups in the world today, we would have had nothing. It came close, because we had negative inputs from some faculty of our own campus, who were fearful that a strong science component would upset the balance.

Chancellorship of Mark N. Christensen

Nauenberg: I don't know if you are going to bring it up, but the worst disaster we suffered through was the Christensen period.

Jarrell: I was going to bring it up.

Nauenberg: I was going to say that that period made us veterans in fighting the system, because under Christensen's chancellorship we learned the depth of corruption that can occur in academia.

Jarrell: You had to get involved and get political?

Nauenberg: Well, we had to get political . . . we had to do more than that. We created the first revolution in the system. At no time in the past, in the history of UCSC, did a faculty revolt against the chancellor. That's sort of unheard of. But we created a group of senior faculty here and decided, eventually . . . in my mind it was an issue of Christensen goes or I'd better find myself another institution. Christensen was about to wreck this institution. I thought it was plain and clear to everybody. I learned the hard way that when the bureaucrats, especially in the president's office, close ranks, forget about it. Our battle with him was terrific right up to the end.

We actually had a group of people who met regularly and did everything we could to accomplish the task. To many of us, if Christensen stayed our institution would collapse. He was incredibly incompetent and careless. You know, in many cases there are mitigating circumstances but I never heard of any mitigating circumstance about Christensen. It was just a complete joke that they sent him to us as a chancellor. It shows the depravity of some people in the system, when they care so little about the institutions they're supposed to be caring for. That had nothing to do with Saxon. That all came under the previous administration. But I found it unbelievable when Saxon became president that he never really acted, never took any action against Christensen. That was unbelievable.

Jarrell: Well, we can get back to that. But you were talking about the Institute for Particle Physics. After Sinsheimer became chancellor, in the first year or two, there

was this theoretical physics institute and there was a competition among various institutions to develop it.

Institute for Theoretical Physics

Nauenberg: Well, we also tried to develop an institute for theoretical physics. The idea of this institute, by the way, was prior to the ORU for particle physics. My first idea was to develop an institute for theoretical physics. Given that there was a competition, we thought that there was a good chance of having the institute here, because the NSF wanted a neutral place. Even though we had competition from Cal Tech, Cornell, Illinois, and other well-established institutions, it was clear that there was a good chance that we would be able to get the institute here in Santa Cruz.

I went to Washington, D.C. to argue with others about this and I was very pleased to learn that in fact in the end it became a competition between us and UC Santa Barbara. All the other institutions were eliminated. Unfortunately Santa Barbara rolled out the red carpet. Sinsheimer was unwilling to compete. [Robert A.] Huttenback allocated four FTE's [full-time equivalent positions] for the institute: one for the director and three for permanent members. NSF would pay their salaries. It would be a freebie for the University. When I pointed out to Sinsheimer what was happening, he said that he would give us only one position, the director. He was not going to give us any additional FTE's. The irony is that at the time he had in his pocket something like twenty FTE's that he had not decided

how to allocate, and shortly afterwards there were cuts in the UC budget and he lost these positions.

When Sinsheimer came here he got a sort of dowry from systemwide of FTE's, so he had plenty of FTE's. But he wanted to sit there and figure out in his own good time what to do with them. So when we asked him to compete so that we would be on a par with UC Santa Barbara, he refused to do it. I told him that we were not in the competition. We would lose. Now there were other reasons. There were other things that we had to try to match. At that time Santa Barbara did not look more attractive than Santa Cruz. But there was just no way to get Sinsheimer to give us the same opportunity that their chancellor was willing to give them. Of course Huttenback ran into a lot of problems because of what he did. Various committees that he did not consult when he made his decision were very upset with him. He apparently made all his decisions without consultation. He paid for it later, I think. Many people think that there were a lot of axes to grind that brought him down. But of all the things that Huttenback did, the greatest contributions he made to the campus was to bring the institute.

Sinsheimer never acknowledged his mistake. In his autobiography (I particularly looked on what he had to say about this episode) he did not acknowledge his blunder.[3] We were left out of the competition for the theoretical physics institute when he did not match the kind of FTE support that UC Santa Barbara offered. I was in Washington, D.C., and there I talked with everybody, and I knew we had

3. See *The Strands of a Life: The Science of DNA and the Art of Education* (Berkeley: University of California Press, 1994).

to overcome problems and compete with Santa Barbara, which kept rolling out the red carpet. I think Sinsheimer's mistake was that he thought the NSF would not turn him down, given the choice between him, a world recognized figure in biology, and Huttenback, who Murray Gellmann (Physics Nobel Prize winner) refers to only as Cal Tech's former football coach.

A couple of years later I went to Santa Barbara and became deputy director of the institute and spent two years there, even considering the possibility that I would stay there, because I was profoundly disappointed with the lack of support here. I had been instrumental in getting the theoretical physics institute to UC, because knowing how the system works I figured out how to lower the overhead of the institute to eight percent. You see, the NSF wanted to get the biggest buck.

Jarrell: Bang for their buck?

Nauenberg: Bang for their buck, exactly. So I went and demanded that we would find out what was the basis of the overhead, which was a standard overhead of thirty or forty percent at the time for any grants. I asked how it would apply to the institute. That's one of the best kept secrets of the UC system—they never tell you that. They never told me when I was a principal investigator. But because systemwide wanted the institute at UC they gave me the information, and I discovered that most of it was inapplicable. In fact, there were only two items that were applicable: namely the accounting that would have to write the paychecks, and the use of the library. Otherwise the institute would function more or less independently. For those two items they figured out it would be eight percent. So

they granted what was a totally unique exception of eight percent to UC. I thought I had a really, really big card for UC Santa Cruz. But of course Santa Barbara got wind of this, and they also were granted the same.

Jarrell: Isn't that terribly low?

Nauenberg: As expected.

Jarrell: Considering all of the scandals we've heard about Stanford and all of the stuff in terms of funding and overhead and . . .

Nauenberg: Exactly. But it was reasonable in terms of what the University would be doing. Because the institute would still be renting the space, they would pay. But in fact, Santa Barbara offered for free a floor of a whole building. Of course you know that today they've built a whole new building for the institute. It was built by one of the most outstanding architects in the world, Michael Graves. I was at the inauguration just a few months ago. It has brought UC Santa Barbara rapidly into one of the foremost departments in physics in the country.

Jarrell: Right. People will want to come. Because it's there.

Nauenberg: Of course UC Santa Barbara became very attractive because of the institute. This could have happened here. It was certainly within our power. We still could have lost in the end, but what I haven't gotten over is the fact that we lost because we never really competed. As I said, the fact was that Sinsheimer said no to what was absolutely a key element. After that of course we were all

discouraged. The NSF was not going to give the institute to UC Santa Cruz where there was only one FTE, when UC Santa Barbara promised four. After we lost, I went to see Sinsheimer, and I said, "Bob, perhaps now you'll explain it to me." I remember vividly that he said: "Well Mike, you know, if I had given you the additional three FTE's, then Huttenback would have given another two FTE's to his people, and you would have come back . . ."

Jarrell: And said, I want more?

Nauenberg: Right then I said to him, "That's why we lost. Because that's not the way one should think about it." Sinsheimer had a lot of good qualities. But he didn't understand what any good rug dealer knows, the give and take when selling a rug.

Jarrell: You have to be flexible?

Nauenberg: You have to be flexible. McHenry had this seat-of-the-pants political sense. It was a disaster that he left too early, before our institution had time to flourish and settle down. It felt like he had abandoned us. After all, he was the person who was responsible for my coming here. He knew everybody. He was friendly and cared about what you did. Now that I have retired I got two letters from him saying nice things about me.

Jarrell: Isn't that lovely.

Nauenberg: You know, he was the only one.

Jarrell: It's a different place now.

Nauenberg: Oh, it's totally changed.

Jarrell: It's more impersonal, more bureaucratic?

Nauenberg: By design. Chancellor Pister has made it a point not to meet faculty. I tried to encourage him to meet with us regularly. He doesn't know half of our senior faculty.That's the new administration.

Jarrell: Do you mean it's a modus operandi?

Nauenberg: It's a new modus operandi. I joked when I heard about the bullet proof glass window in the chancellor's office. I was away and then I read about it. It's not bullet proof, it's sound-proof. He doesn't want to hear from us directly. The more you can block out anything that . . . you have to say on a personal level. All communication now goes through channels officially. Chancellor [Karl] Pister admitted that to me in the early days when I tried to talk to him. "Oh yeah," he said, "In Berkeley we had a faculty club. When I was dean everyone knew that the business of the campus was mainly carried through in informal discussions."

Jarrell: Right. Breaking bread together?

Nauenberg:. By the time you are on a committee you often are formalizing what you've discussed informally elsewhere. Yet, as a very successful dean, and knowing how things operate, he's chosen on this campus to do things otherwise. I know it's by design, because I said to him, "Look, I'll try to organize some lunches

so you could meet some faculty." "Oh," he said, "I'm delighted. Great! Please do that!" Then the first time I tried to organize a lunch he sent me a message and said he couldn't do it. Other faculty who've tried to organize lunch meetings have to clear it with their dean, because the dean might be offended if the chancellor talked with us unofficially.

Recruiting the Physics Faculty[4]

The most important task which I carried out during my tenure at UCSC was the recruitment of physics faculty. When I accepted my appointment as associate professor of physics in 1966, there were only two junior faculty members here who had been appointed previously, Ron Ruby, and Jim Currin. In the fall we were joined by Peter Scott from Stanford University, and Bruce Rosenblum from RCA, who had been recruited by Thimann or Clauser.

In the first few years I could not sit and wait for people to apply for positions in our department. I had to go out into the physics community and actively recruit. My first recruit in particle physics was David Dorfan, who at the time was a postdoc of Mel Schwartz (a Nobel Prize winner) working on an experiment in SLAC. During this early period Bruce and I also recruited George Gaspari, who is in theoretical condensed matter physics, and Bud Bridges, who is an experimentalist in this field. I had heard that a bright student of Richard Feynmann at Cal Tech, George Chapline, was looking for a position, and I was very glad that he also joined our department. George and I began to collaborate,

4. Nauenberg provided this section as written comments submitted in 2004 as this oral history went to press.

and eventually we published several papers together. Unfortunately, George was not very enthusiastic about teaching undergraduates, and after a few years he went to the Livermore National Laboratory.

One day Francis Clauser told me that another recent Cal Tech graduate, Bill Burke, who did his Ph.D. thesis under Kip Thorne, was also looking for a faculty position. We immediately invited Bill to give us a seminar here, and I remember that both Francis and I were very impressed at the difficult problem which Bill had brilliantly tackled in his thesis: the back reaction of gravitational waves on its source. Unfortunately, the physics allocation for faculty had been filled, but Francis managed to persuade the astronomers at Lick, who had recently joined UCSC, to provide half of an FTE for Bill to be shared with physics. Bill remained in a joint faculty position, and was a valued colleague until his untimely death in a car accident.

We also supplemented our faculty with postdoctoral fellows, some of which I supported on my various National Science Foundation grants. One of these early postdocs, who I had met during a 1968 sabbatical leave in Paris, was Fritjof Capra. He accepted my offer to come here, but soon he came under the spell of the Santa Cruz counterculture of the Sixties, and eventually stopped his physics research. Instead, with my encouragement and others, he wrote a book on particle physics and eastern philosophy, *The Tao of Physics*, which became an immensely popular book. This book made him famous, but today no one knows that his career got started here.

During visits to CERN, and international particle physics research in Geneva, Switzerland, I had interacted with David Jackson, the author of the famous book *Classical Electrodynamics*, who had previously offered me a position at the University of Illinois in Champagne, Urbana. I contacted David and suggested that he come to UCSC, and he appeared to be very interested and came for a visit. But shortly afterwards he also received an offer from UC Berkeley, and he chose to go there. Bruce and I had a similar disappointment when we offered a senior position in condensed matter to Leo Falicov. Soon thereafter UC Berkeley also decided to attract Leo, and he went to Berkeley as well. By then I realized that Berkeley was not interested in having us as a competing UC physics department in Santa Cruz, and that they would hinder rather than help our recruiting efforts.

I was also in touch with some experimental particle physicists at Cal Tech, and one of them, A. Tollestrup recommend that we offer a position to Clem Heush. Clem accepted our offer, and promptly began building up a group in experimental particle physics. He brought along one of his best students, Abe Seiden, who later obtained one of the first Ph.D. degrees in our department. Afterwards, Abe became an assistant professor, and we appointed him director of SCIPP, our particle physics organized research unit, which I discussed in an earlier part of this interview. In the early Seventies I also recruited two particle theorists, Richard Brower who received his degree from UC Berkeley, and Joel Primack, who was a Harvard Fellow at the time. We were in competition with Stanford who had also offered a position to Joel, but I persuaded him to come here. It may seem surprising that a bright upcoming particle physicist like Joel

would have preferred to come to UCSC rather than Stanford, but I persuaded Joel to come here because our position was "tenure track," and I was convinced that he would meet UC standards and receive tenure. Today, Joel is one of the leading cosmologists in the world.

About this time I also helped recruit Stanley Flatté, who I had met many years earlier while I was at SLAC. Stan came here as an experimental particle physicist, but he became interested in sound propagation in random materials and fluids, and he is now an internationally recognized expert in this field. Earlier during this interview I described some serious difficulties we had in getting an appointment for him here, because of the nefarious role of the colleges in the recruitment process. In the early days I wanted to offer a position to Tini Veltman, a particle physicist from Utrecht, the Netherlands who I knew very well (he was the best man at my wedding), only to be turned down by the then provost of Merrill College, John Marcum. In 1998 Tini received the Nobel Prize in Physics.

In condensed matter theory I succeeded in attracting Joe Rudnick to our department. Joe has become a leading physicist in his field. Unfortunately, in the early Eighties Joe was recruited by UCLA. Our particle physics group had expanded with the establishment of SCIPP, which I discussed earlier. SCIPP allowed us to bring a large number of research physicists to our department, including Terry Schalk, Hartmunt Sadrozinski, and Donald Coyne, but due to the departure of Joe Rudnick, our efforts to establish a condensed matter group faltered.

When Bob Sinsheimer became chancellor, I asked him that we be allowed to offer three simultaneous positions in condensed matter, and he agreed. For a senior position to replace Joe Rudnick I went after Peter Young, who I had met during my stay at the UC Santa Barbara Institute for Theoretical Physics in the early Eighties. At the time Peter was only a "reader" at Imperial College, and I had started to collaborate with him. Eventually, I persuaded him to come to UCSC, with the promise of two further positions in condensed matter. My high expectations in Peter were soon realized. A year after he joined our department, Peter received the Maxwell Medal, which is the highest award given to a promising young physicist in England, and meanwhile he has become one of the most prominent leaders in statistical mechanics. With the two additional positions which we received from Sinsheimer, Peter and I recruited David Belanger, who was a postdoc working on experiments in magnetic phase transition with Vincent Jaccarino at UC Santa Barbara, and Josh Deutsch who works in theoretical statistical mechanics.

In the early Eighties, Brower became interested in designing and building a supercomputer to carry out calculations in quantum chromodynamics. We could not support such an effort here, and he left UCSC for a position in the engineering department of Boston University. But then the dean of sciences at that time, Frank Drake, refused to allow us to recruit a replacement. At the time a well-known particle theorist, Tom Banks, had expressed interest in coming here, and he had received also offers from other West Coast campuses, including UC San Diego. In our efforts to attract Tom, we were supported by the astrophysics community, and

when Drake rejected our request for a position for him, we sent him a high power delegation, which included Sandy Faber. This delegation promptly changed Drake's mind, but then I met very strong opposition for this appointment from the chair of the Committee on Budget and Planning, Geoff Pullum, who is a professor of linguistics. Pullum insisted that physics needed to hire a woman in the department, and he would not budge from his position. At the time I was chair of the physics department, and I was then called by Julia Armstrong, the head of Human Resources, to explain why we did not have any women faculty in our department. The problem is that highly qualified women scientists were limited in numbers, and those that would qualify were snatched up by more competitive institutions. Ultimately, I went to Bob Sinsheimer, who fortunately overruled Pullum and Armstrong, and allowed us to make an offer to Tom Banks. Tom became one of the bright stars of our physics department. Two years later he accepted another offer from Rutgers University, but recently he returned and now has a joint position with UCSC and Rutgers.

Meanwhile, after Tom left, we made an offer to another excellent particle theorist, Michael Dine, who initially rejected our offer. Then several members of our department, including Joel Primack, wanted to make an offer to another candidate who in my opinion ranked well below Michael Dine. I succeeded, however, in persuading the department to hold the position open, and a year later Dine accepted our offer.

Eventually we were able to hire a woman to our faculty, Pat Burchat, who was an experimental particle physicist. She was an excellent teacher and researcher, but unfortunately we lost her to Stanford University. I am very pleased that today our physics department is highly regarded, indicating that my recruiting efforts were successful.

Teaching and Research During the First Decade

During my first few years here I taught practically in all the physics courses, and although I am a theoretical physicists by training, I also participate in laboratory instruction. Actually, as a student I always enjoyed performing experiments, and this turned out to be an important asset in my work here. Let me give one example. One day in the early Seventies, Peter Scott asked me to check an experimental setup which he had devised for our students. It consisted of a sawtooth blade, fixed at one end, which was put into forced harmonic motion by a sine wave oscillator. According to the standard theory in physics textbooks, the amplitude of the motion of the blade should increase smoothly with the frequency of the oscillator, reach a maximum when this frequency corresponded to the resonance frequency of the blade, and then decrease smoothly at higher frequencies. But some students in the class had found that under certain circumstances their experiment gave a different result. Peter wanted to know if I could explain this discrepancy between experiment and theory. I went to the lab to repeat the experiment, and to this day I remember very well my enormous surprise to find that for large values of the oscillator signal, there was a discontinuous jump in the response of the blade as the frequency of the oscillator

exceeded some critical value. By reproducing the experiment several times, I verified that this phenomenon was real. Moreover, I found that if I decreased the frequency of the oscillator, the jump in the response amplitude of the blade occurred at a different frequency. I could not understand this phenomena, but I suspected that it had something to do with the large amplitude of the oscillation which violated the condition for the linear response that was at the basis of the theory. I then went to the Science Library, and found a book on non-linear oscillations, which showed that the response of a nonlinear oscillator was actually in accordance with our experiment. This was a great eye opener, because none of the courses or textbooks which we had on those days discussed such phenomena. I immediately began to educate myself on the physics of nonlinear oscillators, and then I gave a course on this subject. The brightest student in my class was Rob Shaw, who at my urging solved one of the equations for nonlinear oscillators, the van der Pol equation, by using an analog computer.

The reason why we had access to such a computer is an interesting story. Francis Clauser had come to UCSC to start an engineering school, but in the early Seventies UC systemwide decided against the establishment of such a school. As a consequence, Francis left to become dean of engineering at Cal Tech. He donated the equipment which he had acquired for his planned engineering school, and asked me whether I wanted the analog computer, which I gladly accepted. In my course Rob not only learned about nonlinear oscillators, which at the time was not taught in any physics department in the United States, but he also learned how to solve the equation for such oscillators with the analog

computer. A couple of years later, my colleague Bill Burke heard about the strange behavior of the equations of another nonlinear system, the Lorenz equations, which behaved in a seemingly chaotic fashion, and he asked Rob whether he could solve these equations on the analog computer. Rob promptly succeeded, showing for the first time the intricate behavior that is now well-known as the Lorenz butterfly. Together with two other graduate students, Doyne Farmer and Norman Packard, Rob started a very exciting pioneering research enterprise in nonlinear dynamics, which became well known under the generic name of chaos theory. I helped to support this group, which was later joined by a bright undergraduate student, Jim Crutchfiel. Later, Joe Rudnick and I also contributed to the theoretical development in this field, and we supported further research by establishing an organized research unit in nonlinear science.

In the mid-Seventies I also became interested in a new development in theoretical physics, the renormalization group, which was being applied very successfully to the problem of phase transitions in statistical mechanics. During my 1973 to 1974 sabbatical leave at the university of Utrecht in the Netherlands, I was asked to mentor a graduate student, and I had the good fortune to get Bernard Nienhuis, who turned out to be one of the best students I have supervised. Bernard learned quickly the new developments in statistical mechanics, and soon we were doing state of the art calculations in what became known as the real space renormalization group. We could not complete our research during my sabbatical leave, and therefore I invited Bernard to join me at UCSC during the 1974-1975 academic year. Eventually our research led to six papers published in *Physical*

Review, including two PRL letters, and to Bernard's Ph.D. thesis, which he completed after he returned to Utrecht. Today Bernard is a distinguished professor of physics at the University of Amsterdam, and recently he became a member of the Dutch Royal Academy of Sciences.

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