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University of California
Berkeley, California

Joseph Cerny: A Career in Nuclear Chemistry and University Administration

Interviews conducted by
Paul Burnett
in 2014

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Joseph Cerny

Joseph Cerny is a nuclear chemist who spent his career at UC Berkeley. A graduate of Berkeley chemistry, Cerny went on to an illustrious career in both science and university administration. Along with his colleagues, he is the discoverer of direct two-proton emission, the fourth type of radioactive decay after the known alpha, beta, and gamma radiation. As Associate Director of Lawrence Berkeley Laboratory (LBL) and head of the Nuclear Science Division, he helped facilitate the opening of that institution to a wider range of research beyond physics and chemistry, including the life sciences and materials science. Beginning with his time as chair of the chemistry department, Cerny developed an interest in the needs and professional outcomes of graduate students and postdoctoral fellows that would become the fulcrum of his administrative career at UC Berkeley. From 1985 until 2000, Cerny was the Dean of the Graduate Division and Vice Chancellor of Research at UC Berkeley, where he oversaw the consolidation of research units and helped the university retain a high standard of research in a time of budgetary challenges. Most importantly, he spearheaded long-run data-gathering and analysis of the career outcomes of students and postdoctoral researchers that have been emulated around the world. During the latter part of his time in these positions, Cerny and his team at LBL developed a fruitful method of joining the beams from two particle accelerators to produce new research in the light elements. In 1999, BEARS (Berkeley Experiments in Accelerated Radioactive Species) inaugurated its first experiments. In addition to numerous awards in science, honorary degrees, and membership in elite academies of science, Cerny received the Berkeley Citation in 2013 for his contributions to the university.

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Interview #1: February 28, 2014
[Begin Audio File 1]

01-00:00:10
Burnett: This is Paul Burnett, of the Regional Oral History Office, interviewing Dr. Joseph Cerny for the University History series. It's Friday afternoon, February 28, 2014. This is tape one of our first interview. So, this set of interviews is going to be a life history, but I'm also hoping to use your life as a way to probe questions about changes in scientific practice and changes in scientific institutions, the discipline of nuclear chemistry, national laboratories, graduate education at public universities in the United States, and shifting relationships among universities, the federal and state governments, and private industry. So, we have a lot on our plate, but as with all life histories, we need to start at the beginning. There's an old saying, "May you live in interesting times," and I think I'd paraphrase that and suggest that you've lived in both interesting times and interesting places. I want to explore your family background a little bit and explore some of those interesting times and places. Can you tell me about your immediate family?

01-00:01:35
Cerny: Sure. So, my father was born on a farm in Kansas, and his mother had come from Bohemia, and his father had come from Moravia, and they met there. He was born in June 1906, and grew up on this farm outside a small town called Rossville. Rossville's about thirty or forty miles from Topeka. It was a reasonable farming opportunity, and then he had a younger brother by four years named Frank. My father was named Joseph. The Czechs only knew how to name men Joe or Frank, and women were Mary. That was it. [laughter] Unfortunately, his father died in an accident when he was eight because he also did small bridge building on the side, and the bridge he was building collapsed with him and some other people underneath it. So, they'd gone from it being a very prosperous beginning farm to a widow and these two boys. I think my father didn't like farming, and at one point, then, he just walked off the farm and went to University of Kansas, which wasn't very far away, and worked his way through. Everybody at that time took ROTC—he was going to Kansas in the late twenties, early thirties—because it paid for your junior and senior years, and that was really important as you were working your way through. He then started actually teaching high school at various places and ended up going to get a master's degree in accounting at the University of Illinois, Champaign-Urbana.

Meanwhile, my mother's family were of English heritage and had been in the country some time, and she grew up in a tiny town, Dover, Illinois, which was slightly outside Princeton, Illinois, but this town had, like, 250 or 300 people in it. It had been a very prosperous town before the railroad came through, and when the railroad came through, it didn't go through Dover. It went through a neighboring town and destroyed the economy. So, my mother's father actually sold cream separators for a living and traveled around doing that. Her mother was a nurse. I don't think she went to college, but she went to nursing school. My mother was sent to Knox College, which is a college in North Central Illinois, and there she majored in English and later taught high school. Then, she ended up going to University of Illinois at Urbana-Champaign, and somehow, she and my father met while she was getting her English master's degree and he was getting his master's and working towards a Ph.D., in accounting which he never finished. So, about then, of course, by the time I'm born, it's April 24, 1936. They got married in 1935. The Depression was roaring away.

01-00:04:39
Burnett: In full swing.

01-00:04:40
Cerny: So, my father got a job in Montgomery, Alabama, as an accountant. It probably lasted sixteen months, a little longer, but I was born there. Actually, I've never been back to Montgomery, Alabama—I keep thinking I should go once, but I haven't been back. We moved ten places or so before I ultimately landed in Mississippi, but making it simpler, at some point I think they had retreated to Dover because they didn't have any money, and then he got a college teaching job in the Chicago area. We actually lived in a suburb of Chicago called Villa Park, where I went to preschool. By then, it was clear there was going to be a Second World War, and since he'd gone through Army ROTC and hadn't yet gone in, he was pulled in on the early side.

He was actually in coast artillery, and so we were sent in 1941 to Port Townsend in Washington, where there's an Army base, and of course there's a lot of Navy bases there. So, that's where I went to first and second grade, and by then, my sister, Mary, was born in 1938, so she was two years younger than I am, and ultimately my younger brother, Jan, was born in 1944. We'll get them all born so they're established. So, I remember Port Townsend was a really nice place to visit and to go to school, and everybody was friendly. I think my father was sent to a huge, three months course somewhere, in the East, and my mother said, "Well, if you're going to do that," after he left, "I'm just going to go home and stay with my mother in Dover, Illinois." So, we went to Dover by bus. I remember everybody I met on the bus had started off reading comic books, so I had tons of comic books to read. We got to Dover and then a few months later, we turned around and took a train back to Port Townsend.

Then, this is third grade. I did the first part of third grade in Dover, Illinois, the second part in Port Townsend, then my father, they decided actually he was wasting his time as a coast artillery officer. They needed accountants much more, they needed people to audit post exchanges and things like that, so he started the job of auditing post exchanges. In that year, he was sent down to Riverside, so we went to Riverside, California for three months, which is where I did the third part of third grade. Then, apparently, he was helping do the books for Patton to leave for Europe, so he finished up the books on Patton. We had the summer, and then he was sent overseas. In summer 1944, he was sent to Hawaii, and then to Okinawa, and Korea, and he was gone two years, nearly, and got back very late in '45. So, we retreated to Dover, Illinois again. It's a really nice, lovely little town, everybody was friendly, and my mother had been there for years. It was a nice environment, but it was very sleepy. So, I went to a two-room schoolhouse, and I did fourth grade with first through fourth, and then I did fifth grade with the fifth through eighth. My guess is they probably put me in the fifth through eighth anyway because I was already a year ahead, so it was really nice. I really liked the area.

01-00:08:31
Burnett: You're not really an Army Brat. There are a bunch of reasons why you were moving around, some of which had to do with the war and the Army. The classic story is the difficulty adjusting or the trauma of making friends and having to move around. Is your story a bit different?

01-00:09:00
Cerny: Yeah, right, because I think the Army Brats also went places two or three years and then moved, and this wasn't that many years. First and second grade seemed to be fine, and then in this little town, I remember no fights or anything like that. A bigger town, Princeton, was about five miles away, and my mother thought it was safe so that I and friends could take a bus five miles. In fourth and fifth grade, I'd go to Princeton, and go to a movie and get back. Yes, the country part of it, I really liked. There were some streams outside town I remember that were just interesting to me. I just always thought streams would be good. But anyway, it was very small. I guess my mother's parents must have inherited the house because they couldn't have made any money selling cream separators. The house actually had been one of the main houses—it was large enough to actually have been one of the houses hosting people settling the area, in the 1850s or 1860s.

Then after the war, my father came back and taught for six more months in the Chicago area, and then he had this job offer at the University of Mississippi in Oxford. So, we went there without, I think, ever visiting. My father was a professor of accounting. We got there in the summer of '46, and the university had housing for faculty. During these years, my parents had really gotten used to living in houses with other people, so even when we were in Port Townsend, we had a beautiful Victorian, and we had the ground floor. Kids didn't care and it didn't seem to bother my parents, and we had picture windows looking out at two of the big mountains. In Oxford, we had the ground floor of a large house, and there was a big apartment upstairs where the wife of the deceased chancellor was spending her last days. Then, ultimately, we moved up into the upper other side of that. So, it had a good location. When I went there, I started sixth grade in the local first through sixth, and then University High School was a high school that taught everybody—that was white—in seventh through twelfth grade, and it was associated somewhat with the university but not too much. And, it was near the university. It was virtually next door to where we lived.

01-00:11:48
Burnett: Did it strike you immediately as a very different kind of place, even as an eleven and twelve-year-old?

01-00:11:57
Cerny: Yes. Well, it was not as friendly, certainly, initially, but there were other Yankees because there were lots of faculty who had come from the North. So, the high school wasn't just native-born Mississippians at all. Some of the competition in the high school was two girls in my class, both were from the North. So, that was more where the intellectual competition occurred.

01-00:12:49
Burnett: Were you stimulated or challenged there? This is middle school getting to being high school. Was it your only competition in the sense that there wasn't much going on academically there?

01-00:13:08
Cerny: Yes, there wasn't much going on academically there, and particularly when you think back about it. One exception was the high school mathematics teacher, Miss Teague, who was outstanding! Well, my father also didn't believe in giving allowances to boys, and so I had to have a paper route. So, I had an afternoon paper route for several years, and then I got the franchise and did it some, and had somebody work on it some, when I was gone. Whereas other kids were playing sports, I was spending my afternoon newspapering, and walking it on foot or on bicycle. Oxford was a Southern town, the historic, university had been there since 1848, right next door, the Confederate soldier was in the square town, facing south. It was really poor, I thought, coming there from this little town in Illinois, it looked poor to me. I remember, in '46, watching someone coming into town even with a mule. Even Dover didn't have that. I guess what I got more interested in was I decided to start playing clarinet, and then I switched to saxophone, and I got very interested in band. So, instead of doing sports, I did band. A wonderful band director came in about when I started the seventh grade, and within a couple of years, he'd actually whipped the group up enough to go to the state competitions and come back with As in most of the fields. Not the first year, but it did really well. Then, I helped co-create a dance band that I was in, while I was at high school, and then several of us were hired by a more professional person who was playing in Northern Mississippi, so I did that for a couple of years. I really enjoyed that. I wasn't an outstanding musician, but I was good enough to play second or third.

01-00:15:14
Burnett: If it's a dance band, what kind of dancing? What kind of music?

01-00:15:23
Cerny: When I went to the University of Mississippi—and I'll answer both questions—then this friend from high school who was also in the other band, said, "We should start our own band." He was going to get the music and I was going to be the manager. So, we actually had a fifteen-piece dance band, and I would book it in the Mississippi Delta or in Memphis, you know, a fifteen-piece band's pretty big. We got $170, everybody got $10 for four hours of work, we got $10 for gas, and he got $5 for being the director and I got $5 for being the business manager.

01-00:16:01
Burnett: Not bad.

01-00:16:03
Cerny: I loved it, and we played really nice formal balls in Memphis and in the Mississippi Delta, quite big, fancy things.

01-00:16:10
Burnett: So, these were what would be ballroom dancing songs, like? Or swing?

01-00:16:19
Cerny: More swing. Not formal ballroom. We preferred better music, and we had some better music, but we couldn't play much of this repertoire because the audience wasn't interested in it. So, we would play stuff they'd like to dance to. To hear really good music, we could go out. Every so often, Stan Kenton would come through Memphis, which was only 70 miles away and we could go to Memphis. Stan Kenton was playing the kind of music we all liked. Then, by my junior year in college, big bands were out and combos were in, and I couldn't automatically play that music, so I dropped out.

01-00:17:08
Burnett: Switch to that.

01-00:17:09
Cerny: Yes, and so I just stopped doing that, at that point, and was on the dance committee for the University of Mississippi, and we helped bring in big bands.

01-00:17:19
Burnett: Did you listen to recorded music? Did you have records that you listened to?

01-00:17:24
Cerny: A fair amount.

01-00:17:26
Burnett: Were you inspired by some of that? That got you going about starting your own thing? Or did that follow it?

01-00:17:32
Cerny: Just sort of somehow, we fell into it. I remember evenings working with one of the other people that played in the high school band, we were writing songs

for *Songs for Sale*, and you could send them to New York and they'd buy them, supposedly. Of course, they didn't. [laughter]

01-00:17:46
Burnett: Did they steal them?

01-00:17:48
Cerny: No, they didn't steal them, but it was something to do to keep you interested in the process. So, mainly, I was an outsider in the high school, generally speaking, and my sister was, too. My brother, being much younger—Mary was only two years younger—he was eight years younger and he'd gone there when he was two, but he certainly didn't become a southerner. But both my sister and my brother Jan became English literature majors because they were more interested in what my mother had been doing. After Jan was old enough to go to school, my mother taught high school English in a neighboring town. Her mother came to live with us in Oxford when we did, so she could help at home.

01-00:18:34
Burnett: And sort of continued in that line. Did you have an identifiable interest in science at this time? At, say, near the end of high school? Was this something that was interesting to you and that you had set yourself apart from the interests that your brother and sister had?

01-00:18:58
Cerny: Even though I'm an experimentalist, I'm not tinkering all the time. I sort of was focused on what I wanted to do, and that was it. Well, there was no money to go anywhere, else for college it was perfectly clear, Mississippi paid very badly and we had a really nice house to stay in because that came with it. I knew I couldn't go anywhere but Ole Miss. I didn't have anybody who'd given me a trust fund to go, unlike one of these two girls who was very good, actually her father was the head of the music department at Ole Miss. She had some kind of trust funds, and she actually got into Wellesley and went to Wellesley. The other girl didn't have the resources, and so she went to Ole Miss. Our high school had a poor physics class and poor general science. It had no chemistry class, but I decided anyway, I was going to be a chemical engineer, because I must have read it was a good opportunity. I decided it would get me out of Mississippi. So, my goal was to get out.

01-00:20:09
Burnett: So, you don't remember a point at which you either read that this was an opportunity, or is it something you picked up in the general culture?

01-00:20:17
Cerny: I must have read it.

01-00:20:19
Burnett: Right, the scientist, it's a good career to have and this is something that people are promoting.

01-00:20:23
Cerny: Probably people were promoting chemical engineering at the time, for all I know.

01-00:20:35
Burnett: So, you applied and obviously got into Ole Miss, and you decided to major in chemical engineering. There's a whole bunch of pre-reqs to get into that.

01-00:20:48
Cerny: Well, of course, this was Mississippi. Oxford had about 3,000-3,500 people, and Ole Miss had the same number of students. I did get into the Sigma Nu Fraternity. I stayed at home, which I really liked, and I was my own person. My parents didn't care when I came home and what I did, so that was really good. I had a very good friend named Dale Jones, who lived out in the dorms, who came from Indianola. So, I had friends and he joined Sigma Nu, too. Anybody could go to Ole Miss with a C average. There were a lot of freshman who wanted to be pre-meds, and the faculty flunked out about half of them in freshman chemistry, right away.

The chemical engineering curriculum was pretty tough. The physical chemistry and organic chemistry teachers were really good, and they had absolute standards, and that was it. So, I had to get up to their standards and the organic chemistry standard was very high. So, I think overall, the engineering curriculum was very chock full of things. They couldn't rely that much on math that people didn't have. In fact, in our high school, they hadn't even taught trig. Miss Teague was perfectly capable of teaching trig—she taught trig to our class for the first time—but the university started off teaching people college algebra, and then I had to re-take trig again. I suppose I could have gotten out of it, but I figured I'd get a ninety-nine really easily. I didn't make a wave, I guess. [laughter] So, that was the problem, and the problem in the physics side at Ole Miss was that the curriculum was weak. It wasn't that the faculty weren't good, but particularly the ones teaching sophomore physics were mainly teaching pre-meds and it was really watered down. It was physics with trig.

Before I left, I'd actually taken an advanced course in mathematical physics from a professor who had a Ph.D. and who was very well-known and had been around forever, and it was allegedly a first-year graduate course at Ole Miss. I figured it'd be lucky if it was a junior course at a decent school. But I took it, and it was good for me. I also took ROTC, which everybody had to take, so you had to take ROTC but you didn't have to continue, you had to do two years. Then, I decided—my father decided, too—that I should probably do the four years in ROTC, and so I did that. By the time you had a chemical engineering load and an ROTC program, which was extensive in the junior and senior years, you were pretty busy all the time.

01-00:24:05
Burnett: How much time are we talking about with ROTC? I'm Canadian, and so I'm not as familiar with ROTC, and of course, it's kind of in the past in respect to a presence on campus, except in these [public] universities [in the South].

01-00:24:22
Cerny: Well, it must have been four-unit classes by the time they were junior and senior, and then you had to do some kind of drill, so there's one drill afternoon. In addition, of course, you got all the engineering labs and everything else. By my senior year, I couldn't even get them all to fit, so the faculty just said, "Okay, go to as much of these two as you can, and we'll worry about it later." You had to go to Fort Benning, Georgia, for eight weeks in the summer after your junior year to do infantry camp. That was an extensive experience. After your sophmore year, the engineering was so complicated, you had to go to two six-week summer sessions, two courses each, with 100-degree heat and no air conditioning, to just get through the basic curriculum.

01-00:25:14
Burnett: So, you're trying to juggle those summer sessions and the summer in Fort Benning?

01-00:25:21
Cerny: They had it all planned—after the sophomore year, you did summer sessions, after the junior year—

01-00:25:25
Burnett: That's what you mean by keeping people busy?

01-00:25:28
Cerny: Right. The trouble was, even by the late fifties and early sixties, the people in the North were getting deferments. The people in the South were all going in the Army. Either they went through ROTC, they joined the Guard, or they got drafted. They just did it, and this woman I mentioned who went to Wellesley and met and married somebody at Harvard who was from Arkansas, and he came back to go to the University of Arkansas Law School because they wanted to live in Arkansas, either he got drafted while he was in law school or right afterwards. He got drafted. He had to go in the Army for two years as a private. So, nobody got out of it.

01-00:26:10
Burnett: No, they were pretty serious. David Pierpont Gardner, who was an erstwhile president of the UC system, he was Mormon and he wanted to go on a mission overseas, and they said, "If you go on mission, there's a good chance we'll draft you right off of your mission."

01-00:26:30
Cerny: Oh, really!

01-00:26:34
Burnett: That's a whole other story and we have a whole oral history on that, but yeah, they were very serious about this during this time. So, that made things doubly challenging for you. You wanted to excel, you wanted to get the grades, so you could get out.

01-00:26:52
Cerny: Well, I didn't excel at ROTC. I didn't excel at Fort Benning. I excelled okay at Ole Miss.

01-00:27:02
Burnett: Because it's officer training, it's more than boot camp. It's also leadership training, presumably? Training about the organizational structures, that kind of thing?

01-00:27:15
Cerny: Yeah, you had a lot of lectures on that, but the infiltration course there wasn't as serious as the one I had to do later on. You did a lot of hiking around, and they brought out an infantry brigade to show you what firepower really would look like. You'd sit in the stands and you could hardly believe it. So, they tried to impress us all. I actually didn't do very well from the point of view of the people there, though, because I guess they didn't see I had leadership potential for the Army. They even recommended I be dropped from the distinguished part of that, and perhaps I should be dropped from everything and grow up to be a master sergeant. Luckily, however, the head of the program at the University of Mississippi was final on these decisions, and so he left me in the ROTC program as only a cadet lieutenant, which is the bottom thing you can be—you know, a fake lieutenant rather than one of the majors who ran a brigade or something. Well, that was fine with me.

01-00:28:27
Burnett: But this gives you some support, as well, and this is very important?

01-00:28:32
Cerny: Yes, it gave you $27 a month. By then, my dance band money had run out because I wasn't doing that anymore. Now, my father was very nice to me. He had a rule and he said, "You can't drive at fifteen, even though the State of Mississippi will let you drive at fifteen." I didn't care. At sixteen, he gave me a '41 Nash. By then, he was in the barter business, he did a lot of income tax on the side or whatever. So, it wasn't much of a car, but it was a car.

01-00:29:03
Burnett: It was a car, that's what mattered. Yeah, absolutely. Well, I do want to talk about getting the whole process of the Fulbright, and your launching out of Mississippi. You were a sophomore, I guess, when *Brown* v. *Board of Education* was decided? Or junior? In 1954, so you started in '53. So, I guess to sort of set the climate, in 1950, I believe, there was an editor of the Ole Miss student newspaper who wrote an article for the student newspaper suggesting that desegregation might be a good idea. Within hours, there were

200 students protesting outside his house and they burnt a cross on his lawn. So, several years before you attended, this tension surrounding this kind of racial order in Mississippi was already beginning to bubble up a little bit. Did you know about this Krebs case at all? Did people talk about this kind of stuff, or was it kind of isolated and swept under the carpet?

01-00:30:43
Cerny: I think *Brown* v. *Board of Education* didn't get acted on in Mississippi till the late fifties because my mother was teaching and they didn't integrate the high schools till in the sixties sometime.

01-00:30:56
Burnett: Well, from what I've read, the acting on it was resistance, so organizing the Citizens Council's started, right, in direct response to it, and in response to the NAACP organizing in the state. So, there are battle lines being drawn while you're there.

01-00:31:17
Cerny: Yes, but Oxford, wasn't a Delta town, right? It was somewhat a university town, and you didn't really have that core of die-hard southerners, let's call them, so I don't think there was a Citizens Council in Oxford. They were out in the Mississippi Delta or other places. In reading this book about Meredith integrating Ole Miss, there was certainly an Ole Miss editor, student newspaper, that wrote some positive things and got into troubling circumstances. But earlier than that, I'd say you couldn't detect it. The Memphis newspapers were hopeless, and the news was definitely filtered and boring. I didn't become a *New Republic* liberal for a while, getting into all this stuff. I don't think so. It was just accepted, as my brother comments, Mississippi and Alabama were like apartheid in South Africa. It was just that way. There were the Freedom Rides, the Southern Freedom Rides hadn't started. So, really, the things that started to crack into it were when the Ole Miss sports teams weren't allowed to go to national competitions by the state government because there were blacks on the opposing teams. So, that was beginning to affect the morale of the people on the sports teams.

01-00:33:20
Burnett: Were the chemical engineering students in ROTC?

01-00:33:22
Cerny: Chemical engineering classes only had seven or eight students because there were so few majors in that. Some of the better chemical engineering students were called Navy (ROTC) contract students, who had a four-year way paid, and so they were quite good and had come to Ole Miss. Then, taking regular ROTC was the whole football team, and I was in a lot of classes with them. Nobody was being racist because it was a perfectly understood situation. You didn't talk about it because you didn't need to. I wasn't going to talk about it, knowing the difference in my attitude, so really—

01-00:34:13
Burnett: So, it was such thoroughgoing system that it rendered itself invisible in a lot of ways?

01-00:34:25
Cerny: In a way. I mean we had a woman who worked for my mother and father. This woman actually had a white father and a black mother, but of course, she was black. She worked for us for years, and we had perfectly open conversations and a fine relationship, and we also had another woman who worked for us for many years. Father would help, actually, some black people to get social security because a lot of these people didn't have social security, and they should have, and he was able, particularly later on, to get them through all the red tape to get it.

01-00:35:19
Burnett: You're saying these are African Americans?

01-00:35:20
Cerny: Yes. But I'd say it was just invisible. By the time I left in '57, and then I only came back to visit, and you certainly began to read about the Citizen Councils and all that, I agree, but it wasn't going on there until Meredith integrated Ole Miss, which was '62.

01-00:35:45
Burnett: Right, and that's, of course, much later. Were there moments when the veil was lifted, I suppose? You were valedictorian at Ole Miss, is that right?

01-00:36:01
Cerny: Yes. I was the valedictorian for the entire senior class, but my name was not on the commencement program because there was a left-wing group which was anti-segregation and I knew the people in it, and my sister was seriously in it, and so one of my colleagues in chem-e tried to turn me in to the dean of men, which I figured wasn't going to go very far. Which it didn't, but they did remove my name, or they just somehow didn't have a valedictorian that year. I got the chemistry award at another ceremony.

01-00:36:53
Burnett: So, how did you feel about that?

01-00:36:57
Cerny: Bemused. [laughter]

01-00:37:01
Burnett: If you had been a native son, I imagine that that would have been not so subtle, right? So, if you were planning on hanging around Mississippi, right, that was a way of saying, "We've got your number?" In the case of Krebs, he was, I think, from Massachusetts [ed. Note: he was in fact from Pascagoula]. He was a bit of a northerner, too, but was more settled. I think he'd grown up in Mississippi the whole time, and he was kind of hoping for a job as a journalist.

He could find nothing, and he had to leave. They had marked him in no uncertain terms.

01-00:37:47
Cerny: Well, I was told that the provost was actually a serious racist, and so he's the one that probably removed my name from the program. Then, they included it the next year.

01-00:38:00
Burnett: You were temporarily declared a non-person.

01-00:38:06
Cerny: I guess I was more worried at Ole Miss that I wouldn't get out. But I had almost all As—I didn't have perfect scores—and I applied to six graduate schools, not knowing whether I was going to get in. I got in all of them. Harvard, Chicago, Berkeley. When I was applying, I was certainly uneasy. Just worried about how they would evaluate the quality of the education even though you were doing well. I had received a three-year National Science Foundation pre-doctoral fellowship.

01-00:38:46
Burnett: Right, so you were worried about being marked externally in another sense. So, that would have been a concern because the reputation of Ole Miss, I think in science it would have been different, but the law faculty, for example, was basically buttressing the entire racial order in Mississippi. So, 90 percent of the lawyers in Mississippi were from Ole Miss. They did not have certain books and certain journals and certain articles in their library. They erased a whole other set of conversations about what could be just or legal. I guess you were spared that, in a number of ways, by being an outsider, by being an outsider academic who's kind of an interloper in some ways. Also because you did science, and so there's not really a racial politics to chemistry in this context.

01-00:39:53
Cerny: In that period, though, there were a number of women and some men coming down to work in the student church groups, and they were very liberal. My sister really gravitated toward that. None of us were particularly religious, but we would go to religious things for that kind of a community. So, they'd come down with completely different attitudes, but they knew they weren't going to make giant waves, but they were going to make friends. I should bring you in *The Price of Resistance* because the first chapter really quite well describes the University of Mississippi and the students. He had the right word for it; I should come back to it. They were just not intellectually engaged, let's say. They were just there. The girls were there looking for MRS degrees, and at this time, Ole Miss was having two Miss Americas in a row. We had gone to the Sugar Bowl in '53 and '55. I didn't mention it, but even though in 1953 I was a senior in high school, they wanted to buttress the Ole Miss band so they invited all the seniors from the high school to go with the Ole Miss band, so

we got to go to New Orleans and took the City of New Orleans from Memphis, un-chaperoned, for the males. That was really nice, particularly since it is going to be a theme in my comments that I felt so starved for travel after moving all over and then going nowhere. Anyway, we did make the trip and it was really great, to New Year's in New Orleans. There was this giant Mississippi battle flag that we marched out from underneath at half-time.

01-00:41:51
Burnett: Yeah, that's awkward, a little. Just to backtrack, this left-wing organization, was this a self-proclaimed left-wing organization that your sister was briefly associated with? Or is it something that was tarred with, "You are a left-wing organization," you're like a communist, in other words, a communist organization.

01-00:42:13
Cerny: They didn't get into that. A good friend of mine who lives out here was also like that, and he was born in Mississippi. They were just left wing, they would make jokes about it, and how we needed to do something—you know, make jokes about the Southern attitudes about things. They just sort of knew it, but they didn't do any big demonstrations or anything.

01-00:42:37
Burnett: Right, right, they weren't highly visible or targeted as much as organizations would be later. I guess just to cap that off, very recently a statue of James Meredith was vandalized at Ole Miss. What did you make of that when you read that? Did you feel that it's kind of the same old, same old?

01-00:43:19
Cerny: Two years ago, there also was a demonstration at Ole Miss for the fiftieth anniversary of Meredith integrating it, and they had to quell a disturbance. Just two years ago. Now, Chancellor Khayat did an outstanding job for fourteen years, but he's been retired since '09, I think. The new chancellor doesn't have his charisma, but he's a fine person. So, well, I thought demonstration was pretty bad. The university clamped down really hard. Yet it's just there. It's latent. It keeps resonating with just even that little bit my brother wrote. You had to go so far back to see what the mode was in the fifties of what it was really like. The only war they were talking about, as he says there, it's not the First World War or the Second World War or the Korean War—it's the Civil War. Oxford was burnt twice by Grant, but a few houses were saved.

01-00:44:58
Burnett: It's the war that changed everything for them.

01-00:45:00
Cerny: It changed *everything* for them. Science was okay, but it was hard work. We luckily had some smart professor come in, a physics professor, and he was going to teach a quantum mechanics course, and so I audited it anyway, just to see what was going on.

01-00:45:31
Burnett: So, there were so many push factors for you in your youth that you almost didn't have an opportunity to catch the spark or the fever of science, which is a common narrative, right? We just talked to another scientist who was tinkering with microscopes and Petri dishes at grade seven, grade six. It sounds like you had a slightly different experience that makes a lot of sense. So, you do very well, though, and you are one of ten people ever to win the Fulbright from Ole Miss. There are ten Fulbright winners from Ole Miss in its history. So, that was something that helped, I imagine, secure a way out. How did that come about?

01-00:46:34
Cerny: Well, okay, the recurrent theme of what we're going to talk about is I like to travel. [laughter] Sort of some excellent little travel things, asides, to put in. Well, I applied for a Rhodes, I applied for Fulbright, I applied for another one. I got interviewed by the Rhodes Committee in Jackson, and didn't do very well. They did tell me if I came back the next year, they would guarantee that I got to the finals. Anyway, I suppose it was nice they said that. Well, I'd gotten a summer job as a chemical engineer at Oak Ridge National Laboratory, outside Knoxville, in the reactor division. I had a really great summer there. I'm going to answer your question.

01-00:47:30
Burnett: No, this is great.

01-00:47:31
Cerny: Really great summer there, working on testing a fancy kind of liquid-liquid separator, that the chief scientist of the lab had thought up. So, they gave me that project, and I figured out how to do it and put it in a pilot plant. At some point, plant management called me in and said, "What are you doing? It's costing a lot of money here." I said, "Well, I was told to check the separator out: it was a kind of spinning device using the densities of the fluids that you sent in so that what you wanted came out the top and the residue out the bottom, and how are you going to test it but run it in a pilot plant?" So, I did. Other engineers looked at my early results and said, "Okay," and we finished it. It didn't work. They suppressed the report for a year. Luckily, I went back the next year and forced it out. While I was there, I was preparing to be a physical chemist at Harvard. However, the Fulbright came in, so I must have been the first alternate.

Everybody wants to go to London, Oxford, Cambridge, and I thought I'd like to go to Cambridge. I didn't know anything, so I went and looked up the Cambridge guidebook to learn what the research programs were. They were doing low temperature research. I said, "That sounds interesting," so I said, "That's what I'd like to do." Well, they were actually going to send me to the University of Manchester and they found me a low temperature professor at Manchester who was an expert. By the time I got to Manchester, I'd converted to being a nuclear chemist/physicist because I'd taken this evening class at

Oak Ridge and discovered radiochemistry, and I loved it. So, instead of going to Harvard and being a physical chemist, then I did this and I went to England, Manchester, and then politely told this professor I didn't want to do low temperature work and knew I wanted to go to Berkeley then. My main professor at Ole Miss had said, "Well, if you want to be a nuclear chemist, you go to Berkeley."

01-00:49:45
Burnett: Can we go to that evening course and loving radiochemistry—what was going on there? Was it partly the total Oak Ridge experience, or was there something kind of sweet about the science?

01-00:50:05
Cerny: Just intellectually, it was a science I knew nothing at all about. The radioactivity part was related to the projects in this division, which was a pilot plant to separate uranium from irradiated thorium isotopes for another way of doing fission, so you're going to have Uranium-233, so I had to learn some of that background. They just assigned me when I went to Oak Ridge. Then, this evening class, I said, "Wow, there's a lot I don't know here, and this looks really exciting to me."

01-00:50:36
Burnett: When I think about, all of the security concerns about these kinds of laboratories, and Oak Ridge was a national lab, right, so the way you explain it is you just got this summer job at Oak Ridge. How did that happen?

01-00:51:00
Cerny: Well, they were keen to recruit really good people, right? They wanted good engineers and this and that, and so the way to do it is to grab people when they're young and prove to them that Oak Ridge is a great place to be. So, when you get your Ph.D., you might come back.

01-00:51:14
Burnett: I guess because you had done ROTC, you're kind of pre-vetted, right?

01-00:51:20
Cerny: Maybe. Yeah, probably.

01-00:51:21
Burnett: There's a sense in which, like, this guy's okay. I'm thinking about the security concerns.

01-00:51:28
Cerny: There were three big sections, at least, at Oak Ridge. One of them had the gaseous diffusion plants and one of them had all these cyclotrons that were separating uranium isotopes, and this was more of the basic isotope production. This was just a pilot plant, and they were doing chemical separation processes and trying to see what was really going to work. You can understand the fundamentals, but they weren't making weapons there or anything. That was Los Alamos and Livermore. I don't know what there

was—the lab seemed pretty open to me because the year I came back, I got a job for two months after I came back from my Fulbright, and they couldn't find enough for me to do. They had a major test reactor project along the far end of the grounds, and it was a homogeneous reactor where they pump uranium slurry around in this big sort of circle, and then it would go into a spherical vessel. It would react then and give you the heat, and there'd be another set of pipes outside it, so you'd heat the water outside it, and ultimately generate electricity.

01-00:52:45
Burnett: Get a steam turbine going, basically.

01-00:52:47
Cerny: Yeah, and I'd actually read in England when I was there that they were having trouble at Oak Ridge because they thought they had a pinhole between the inner and the outer section, and that maybe some of the uranium might be going to leak in the outer section, and then maybe you'd get a lot of it collected and that wouldn't be a good idea. I was assigned to monitor the data on the two different ways of deciding how the reactor was running and what was really probably going on in this pinhole. I remember the lab director coming out each week that summer, who was a famous guy, and worrying about this and going to those meetings. You could sit there and look up the fallout patterns for Eastern Tennessee if something went wrong.

01-00:53:39
Burnett: Wow, that is some interesting work. Was there any sense as it's 1957, '58—

01-00:53:55
Cerny: Well, this is '57 and '58, yes.

01-00:53:58
Burnett: This is the summer of '58, I guess, and I'm thinking this is after Sputnik, it's the National Defense Education Act, and this is the Atom Age. So, I guess it might be a dumb question as to why you were interested in radiochemistry. This is something that was so much a part of popular culture, American identity, this was the shield keeping back the Soviets. So, it makes perfect sense—did you think of it in those terms, or you were just like, "This is really cool?"

01-00:54:44
Cerny: I remember driving around Mississippi to go see my friend in Indianola, let's say, and we were all thinking about some weapons attack. I'd say, "Well, you know, Memphis, they're not going to bomb Memphis. Even if they did, it's eighty miles away, so I'm not going to worry about this particular fall-out." But you knew it was going on. I think that was part of it. We didn't have great laboratory facilities or anything for chemical engineering. The chemistry was fine and had really good people, and then you went to a big, functioning lab. A lot was going on. It was exciting, this guy gives a talk at night, it's

something I don't know anything about and it interests me. You'd heard of some of these famous physicists. And I thought "this is the place for me."

Burnett: And you saw where the action was.

Cerny: Yes, and you knew the other plants had been heavily involved in the action, because that's where the separated isotopes were coming from. And I'm sure I read some more on it when I was at Manchester.

[End Audio File 1]

[Begin Audio File 2]

02-00:00:09
Burnett: This is interviewer Paul Burnett, interviewing Dr. Joseph Cerny. This is interview one, tape number two. So, we were talking about your early childhood and your time in Oxford, Mississippi, and your early education. Before we leave that altogether to talk about your launching out of Oxford, I want to ask you about the factors that helped or hindered your excelling in school. Family support, a cadre of friends who helped, what were the obstacles and what were the paths that helped you get where you are?

02-00:01:08
Cerny: Well, clearly it was an educated family. There were books all over the place. I think that they'd sort of struggled to get where they were, and I remember actually thinking I would try to apply to go to Harvard, which would have been a horrible mistake for me.

02-00:01:29
Burnett: Why do you say that?

02-00:01:31
Cerny: Oh, because I'd have been wiped out if I'd gotten in. I was too immature. I started to apply, there was no support at home for that, and of course, they were probably saying, "Where the hell are we going to find any money to do this?" I just dropped that idea. Also, I matured older. I actually grew, my senior year in high school and my first year in college, a lot, ending at 6 feet ¾ inches. I don't know where my motivation came from. I was motivated to do fine in high school. I didn't overdo it. At Ole Miss, the few classes I got Bs in were drafting, something else like that. [laughter] There was some horrible chem-e [chemical engineering] early junior level lab that was very ill-defined, and the equipment was a joke. A really nice professor ran it, and we wrote up what we could write up, and everyone got a B. Later on, he told me, "I've looked at your grades and I didn't realize your grades were virtually all As. If you want, I'll change that B to an A." He told me that twice over the course of my experience there, and I said, "No," to myself, "I barely deserved a B, and

let's let it go." When this other professor was actually doing the first quantum mechanics course at Miss Ole, I audited it and squeezed it in just because I wanted to learn something, but I had all this required stuff I had to do. Then, I also knew, from my summer school experience, that I needed more math. So, I took for credit this first-year graduate level course I mentioned earlier on advance math from this physics professor, just so I knew I was getting a getting a step up towards whatever I needed.

02-00:03:52
Burnett: You knew that there was something out there that you wanted to get towards. You weren't quite sure what it looked like yet, but it had to be better than where you were.

02-00:04:02
Cerny: Right, I knew that, and I didn't have any idea what research was at all, and I wanted to be a university professor. I didn't know what research was till I got to Berkeley.

02-00:04:14
Burnett: So, you were familiar with the academic environment, and your father was a professor, and your mother was educated, and you had a house full of books. So, there was conversation that involved curiosity at the dinner table, I imagine.

02-00:04:29
Cerny: Sure. My mother taught high school English and was really good. She was a great resource when I couldn't think of what essay to write about when I was in the eighth grade.

02-00:04:40
Burnett: She was your Wikipedia.

02-00:04:41
Cerny: Right. I think that's what it was. I lucked out in Oxford, but Mississippi took a huge toll on my sister because she was very smart, too, but the girls were just really mean to her. My brother escaped that in his way, but I think not entirely.

02-00:05:08
Burnett: Did she leave as well? Did she go to graduate school?

02-00:05:11
Cerny: No, she went to Ole Miss and then, well, for a disastrous year, my parents decided since I had had this Fulbright, it'd be really nice if I went to Berkeley, she should come out with me while I'm doing my first year at Berkeley and go to school there also, as a junior. So, we got along, but she couldn't handle Berkeley. I couldn't take care of her. We had to be independent operators living in the same lodging.

02-00:05:44
Burnett: Right, right, and what did she not handle about it, if you don't mind my asking?

02-00:05:48
Cerny: Oh, she couldn't make any friends.

02-00:05:51
Burnett: It was a different environment.

02-00:05:53
Cerny: She kind of dropped out at Berkeley and worked for the Red Cross, to help out. Things like that. I just couldn't come out here and compete with a new group of people and take care of my sister. It doesn't sound very generous, but I couldn't. She then finished up at Ole Miss.

02-00:06:15
Burnett: That's too much of a challenge.

02-00:06:18
Cerny: Late in life, both she and my mother separately apologized for that.

02-00:06:26
Burnett: That's interesting.

02-00:06:29
Cerny: I wasn't against it. I just couldn't—

02-00:06:32
Burnett: It was too much.

02-00:06:32
Cerny: It was too much.

02-00:06:37
Burnett: This is kind of a theme that I'll probably try to come back to because you become not only an expert in nuclear chemistry, later, you also cultivate expertise in reforming graduate education, and you're thinking about higher education, and today we are thinking a lot about reforming science education in the public school system. What is it that makes somebody track and what is it that makes someone come off the track? So, there's maybe other ways to make that part of the conversation later in our interview. Let's talk about the Fulbright and Manchester, and getting out. So, now you're out.

02-00:07:22
Cerny: It was great. I'd only flown once, didn't like flying (yet), so I took the train to New York and we all met up there. Never been to New York before.

02-00:07:35
Burnett: That in itself would have been—

02-00:07:38
Cerny: I'd barely gone anywhere after coming to Oxford! We drove to the Kansas farm every summer for several weeks because my father felt he had to help his mother and his brother, and his brother was shy and never married and they

had this farm. Grandmother certainly had had a really tough life, but for grandchildren, it was not a great place to be. But that's where we went, and things had gotten so bad when her husband died that she was going to make sure she had enough money to make it with those two kids. So, the electricity ran right by the house, it was fifty feet away. She didn't have electricity. Wouldn't, till she died. After that Frank stayed there and lived by himself. He didn't have it, either. So, there was no electricity, there was a short-wave radio that played the Czech stations because she spoke more Czech than English, and he spoke more Czech than English.

02-00:09:17
Burnett: Well, yeah. I come from an agrarian background as well, and my great-grandfather, my grandparents came to California in the 1920s, and they had to go back in 1934 because my great-grandfather was pulled under a tractor. So, it happened all the time. It happened all the time, so I hear where you're coming from. So, there were difficulties, and so what you're saying is that let's say you're a kind of middle class in the sense that your father was a professor.

02-00:09:56
Cerny: Well, we were lower middle class, financially.

02-00:09:58
Burnett: Right, and so any traveling you did was family-related, and usually for some kind of strong-arm labor that you can be sent out there to help out.

02-00:10:11
Cerny: Well, we children didn't have to do that, but he would go work. We weren't old enough to do it. Anyway, so then the Fulbrights were all gotten together, and we went over on the *SS United States*, as a group that was going to England, and maybe a few onto the Continent. So, we got to meet one another and got along fine, and I learned how to play bridge.

02-00:10:39
Burnett: It must have been incredibly exciting because not only are you on this adventure, you're on this adventure with other bright young people. You suggested that's the design of the program, to have them spend time with each other?

02-00:10:53
Cerny: Well, yes! We got to London, and then we got oriented a little bit in London. Then, they sent us all off to our universities, so we could get lodging and registered. Then, they got us together for another orientation, out at Harlech Castle, in Wales, which was beautiful. There were four or five Fulbrights in Manchester, anyway, so we knew one another. I got into lodgings with a very smart guy named Arnold Goldman, who was Jewish, from Massachusetts, and had gone to Harvard in English literature, and was a magna cum laude graduate. He stayed in the same rooming house that I did. The rooming house had a front and a back place you'd eat, and the back place is where the land

lady put the English students, and she put the American students, who were paying more, in the front. We were there in front with the visiting faculty or lecturers, too. So, we were there, in the front, by paying half a pound more a week. During the year, Arnold gave me an education in English literature. I read a lot of the modern novels and he could criticize it and tell me more. So, when I got to Manchester, I said, “Okay, I have a Fulbright and I will do serious science and math forty hours a week. They’re going to get forty hours a week when term is in, and that’s it. And I’m going to use the rest of the time to work on broader aspects of my education.”

02-00:12:30
Burnett: You spent it reading modern literature…

02-00:12:33
Cerny: Yes, and reading *The Manchester Guardian* and exploring the local environment and a lot of traveling. The Fulbright program encouraged us to interact and Arnold and I did interact, so we actually proposed to the main student newspaper that we’d write a column called “Invasion.” So, we wrote a weekly column and this was right after the Suez crisis, so it wasn’t quite so clear to me –

02-00:12:55
Burnett: It’s a little cheeky. [laughter]

02-00:12:57
Cerny: - that it was going to work out, but it worked out fine. So, we did that, and then he and I traveled. There was enough money with the Fulbright for students. It wasn’t a lot, it was £50 a month, but—

02-00:13:13
Burnett: But in war-ravaged Europe, it went a long way.

02-00:13:20
Cerny: His father had paid for him to have a small Anglia car of some type, so that winter, we motored across to Paris, where he had friends and we could hang out with these friends from Harvard in Paris. We then went down and went to the Riviera. We did Pisa and Rome and Venice and Florence.

02-00:13:39
Burnett: Oh, that sounds fantastic. That is fantastic.

02-00:13:41
Cerny: Yes, it was fantastic, and Arnold knew all about the museum and the churches etc., so you could have a running lecture.

02-00:13:46
Burnett: Right, right, exactly. Yeah, it’s wonderful to get a kind of guided tour from a person of letters who’s got all the stuff in there. Did you have English friends, British friends?

02-00:14:03
Cerny: Yes. We had some British friends. Arnold and I also traveled to London and Oxford several times, staying with Fulbright colleagues. We also took a long weekend trip to Dublin and environs.

02-00:14:06
Burnett: Was there a bit of tension that they were sort of backstairs to the Yanks?

02-00:14:12
Cerny: No, no, we fit in fine. We didn't have any super close friends, but we definitely had friends. Then, I met my future wife because one English guy in a science class said, "You know there's an American girl in this other math class. Why don't you go see who she is?" So, I did, and had Barbara Nadelka grown up in New Jersey and had started going to Pembroke, which was Brown's girls' school, and I guess she didn't like it. She'd gotten her major professor in the applied math program to arrange with a very distinguished math professor at Manchester to place her in the Manchester program as a sophomore. So, she came over as an applied math major, but as a sophomore.

02-00:14:56
Burnett: Bright.

02-00:14:59
Cerny: Then, I spent a fair amount of time with her after a while. In the spring, I actually traveled with her and we saw lots of Germany. The U.S. encouraged it, so we could actually fly from Hamburg, West Germany into West Berlin on an American plane, and at student rates, and we could then also get a bus tour of East Berlin.

02-00:15:33
Burnett: Wow.

02-00:15:34
Cerny: "Wow" was right, and just still all burnt out, most of it. There were a lot of eye-opening things.

02-00:15:43
Burnett: So, there was a lot of Marshall Plan rebuilding in West Berlin, there was a lot of investment in trying to clear the rubble. I imagine there was still rubble even in West Berlin, at that point.

02-00:15:54
Cerny: Yes, but nothing like East Berlin.

02-00:15:56
Burnett: And East was just still—

02-00:15:57
Cerny: Mainly rubble, but they'd fixed up a few buildings. There was a huge monument to Stalin, I remember, in a large cleared park area. They let us get off the bus in the park.

02-00:16:05
Burnett: So, did people live in half of a building?

02-00:16:09
Cerny: They only took us on this tour, to the better things, I imagine. This was a sponsored tour. But I remember in Mainz, you could still see the broken concrete on the banks of the river.

02-00:16:27
Burnett: Almost fifteen years later, yeah, that's astonishing.

02-00:16:29
Cerny: Then, we flew out, I think, to Munich; one of us had a friend in Munich that we briefly stayed with and he told me: "Have you read this new book that's just come out, called *Brighter Than A Thousand Suns*, by this Swiss journalist, the first real book on the atomic bomb?" I had not heard of it, and it was still only available in German, but I bought a copy and started reading it immediately, since I knew some German. Luckily, two-thirds of the way through, somebody had translated it, so I sped up. The book was first class and really fascinating to me. We then went back to Manchester from Germany by way of Brussels' World Fair, which started in April 1958 and was the first major fair after World War II. It was very impressive! I never missed any classes at Manchester due to these travels, but English terms were fairly short by US standards, with long Christmas and mid-spring breaks.

02-00:17:27
Burnett: So, you got a lot done and you were able to see a lot.

02-00:17:33
Cerny: The science classes were terrific and taught at a high level.

02-00:17:39
Burnett: But it wasn't the field that you were interested in? It's low temperature.

02-00:17:41
Cerny: Well, sure, because I'm interested in physical chemistry and I teach physical and nuclear chemistry, so I was taking their physical chemistry classes—thermodynamics and statistical mechanics and other math comes into it.

02-00:17:57
Burnett: Was that good preparation for you when you went to Berkeley?

02-00:18:01
Cerny: Yes, it was good background preparation.

02-00:18:03
Burnett: You got up to speed, do you feel?

02-00:18:04
Cerny: Right.

02-00:18:13
Burnett: Had you already applied to Berkeley when you left? Or you got deferred, right?

02-00:18:19
Cerny: No, they didn't do that back then.

02-00:18:22
Burnett: Not deferred—you got into six schools?

02-00:18:26
Cerny: Yeah, five or six. Right.

02-00:18:28
Burnett: The Fulbright allowed you to—

02-00:18:31
Cerny: No, you had to reapply for everything all over again! I had to apply for the National Science Foundation Fellowship all over again. I had to take the Graduate Record Exam all over again. Had to go down to London to do it. You just had to start fresh. They wouldn't let you take a year off back then. So, then I had to reapply to Berkeley and Harvard and Chicago and wherever else I'd applied.

02-00:19:01
Burnett: So, that wasn't daunting? I guess once you get a Fulbright, though, when you said that, I just thought, that's a little terrifying to be accepted to all these schools and then have to reapply and perhaps not get in. But I suppose doing the Fulbright only brings you up, and so you're probably not too scared of being rejected the second time around.

02-00:19:29
Cerny: Yes, I didn't think I'd get rejected but then having to take the tests to get a National Science Foundation Fellowship again—to retake the general GRE, and then you have to do a Graduate Record Exam in chemistry and also score well enough to get a fellowship.

02-00:19:48
Burnett: So, you reapplied and you find out near the end of your time in Manchester?

02-00:19:54
Cerny: I knew where I was going before I came back to the U.S. I also knew that I had a NSF Fellowship again.

02-00:20:05
Burnett: I've been thinking about being embarrassed to ask this question, "Why was Berkeley your first choice?" So, once you decided nuclear chemistry was it, that was the place to be, I would think.

02-00:20:22
Cerny: Yes, that was the place to be. Berkeley fights toe to toe with Harvard in physical chemistry, but in nuclear chemistry, the faculty and facilities at Berkeley.

02-00:20:37
Burnett: Nobody had better equipment, right.

02-00:20:38
Cerny: The facilities were here. Where else would you go?

02-00:20:42
Burnett: So, that was clear, and you're beginning to get excited about that.

02-00:20:48
Cerny: Yes, and at that time, it was a really popular field and Berkeley was very attractive. There were a lot of excellent nuclear chemistry graduate students from the late fifties and through the sixties, a lot. Then, it began to phase down.

02-00:21:04
Burnett: In the history of science, the story is that nuclear chemistry was kind of the golden child in the mid-fifties. The discovery of the transuranic elements and high energy physics becomes, right at that moment, it starts to really take off. Like, post-'58, the high energy physics becomes the true sort of queen at that point. But in the mid-fifties, nuclear chemistry was this incredibly exciting frontier field, and you're coming right at that moment where there's perhaps not even a discernible shift. I wanted to ask you about that. I'm getting ahead of myself, I think. So, you finish up at Manchester and you go directly to Berkeley?

02-00:22:08
Cerny: No, I came back to the US for the summer and go to Oak Ridge again for two months.

02-00:22:12
Burnett: Two additional months at Oak Ridge?

02-00:22:13
Cerny: To keep intellectually active and to earn some money.

02-00:22:16
Burnett: Were there any kind of nods at Oak Ridge to suggest that if, when you were done, "Give us a call," or it was not like that?

02-00:22:32
Cerny: I think I just wrote a letter and said, "Can I come back?"

02-00:22:35
Burnett: And they said "Sure?"

02-00:22:36
Cerny: Yes, I had a good working relationship with them. You want to go there, they've been nice to you, right?

02-00:22:41
Burnett: Right. So, you do two months there, and then you're—

02-00:22:47
Cerny: Drive out here.

02-00:22:48
Burnett: You're there and your sister's also there for a year, but that doesn't work out very well for her. So, when you arrive at Berkeley, what are your first impressions of the lab? Who do you meet with first?

02-00:23:06
Cerny: Well, I didn't know what to expect at Berkeley. I didn't know several things. One is that Berkeley chemistry had the most research-focused, loosest extra-curricular learning of probably any graduate chemistry program in the United States. So, at Harvard, when you go there, you have to take placement exams in three or four different kinds of chemistry, which you are going to forget, but you have to do it. I remember the first summer at Oak Ridge, I was already studying inorganic chemistry, of which I'd had none in my chemical engineering program, to pass that exam. Out here, there weren't any placement exams. The first thing that scared me was they wanted you to immediately find a research director. You should have probably found a research director before you got to Berkeley, but if you were foolish enough not to have done that, you had better go talk to people and find one. So, like crazy, I interviewed with all the nuclear chemistry faculty and found a great one. We'll return to that. So, I signed up with him mainly because he was a nice guy and I figured that it would all work out.

02-00:24:35
Burnett: That's a good reason.

02-00:24:38
Cerny: Well, when you don't know anything much. It used to be the department wanted you to find a research director in four weeks, and when I was department chair, I fought that. I said, "Come on, guys, let's make it be six or eight." I think I finally forced it maybe to eight, and then they didn't like that, and ultimately got back to six.

02-00:25:05
Burnett: So, it's a very durable culture. You're talking way ahead to when you were department chair, in the seventies, and you're saying that there was cultural resistance then, almost twenty years later, to what was the norm at other places.

02-00:25:26
Cerny: Berkeley's a research Ph.D. degree in chemistry. That's what it is, period. That's what G. N. Lewis liked, he probably didn't like anybody taking graduate classes, and that's the way we're going to do it. Of course, people coming from the good schools probably have a good idea of what they want to do, but people coming from poor schools… I didn't know that was even going to happen to me, or I might have tried to read up on it more. None of the literature said, "Oh, well, by the way, we hope you find a research director in your first four weeks." So, I didn't know that until I was told that by my first faculty advisor. I think that's way too short. The program really starts focusing on research and what you need for your research. When I did it, there were just a few "extra-curricular" courses. A lot of other places, you have to take all the beginning graduate-level organic, inorganic, whatever courses. We don't do that. But back then, they did force us to take an inorganic class, but not the lab, and you had to take a whole year of physical chemistry. Well, that makes sense because physical and nuclear are very close. No problem with that, you have a whole year of physical chemistry, good, that's in your discipline. The inorganic was kind of strange, that we needed to do that. Even an organic class, and I told the advisor in charge of all this, I said, "Look, I read a whole book on organic chemistry when I was going to Manchester, and I think that ought to count to get me out of this class," and he agreed. [laughter] So, we're very different from physics here in that way because in physics, you don't get near a research group, you don't even see one—you come here, you take classes, you have to do some kind of a placement exam, you have to go through a year of physics courses or so. Then, you have to take some serious written exams, and then you can go find a research director.

02-00:27:28
Burnett: So, it's not just Berkeley because Berkeley physics has much more of a training and pedagogical approach?

02-00:27:35
Cerny: It's Berkeley *Chemistry*.

02-00:27:36
Burnett: It's Berkeley Chemistry that has this totally, well, not totally unique—I was thinking of parallels, these are other disciplines, mind you, but it made me think of Chicago economics in the fifties. The emphasis was you're going to be formed, but you're almost immediately a research colleague when you go to graduate school there. Definitely, it's hierarchal, you're definitely nobody there, you are a nothing, but you are a nothing *partner*. So, there's a sense in which you're no longer quite a student and you are expected to be producing almost from the get-go and working with research projects that the faculty have going, and being expected also to initiate them. So, it sounds very similar. The other thing that was part of it, too, was that they had this seminar system, and they would have these seminars where everyone, the whole faculty—sound familiar?—would meet with all of the graduate students and there would be presentations of the work. It was this culture that they were hard on

each other, but there was this culture of a kind of hierarchal and differentiated collegiality. Does that ring any bells for you?

02-00:29:17
Cerny: It sounds really good. It's just chemistry and physics programs that I know about. I think it's a good approach, but you're not being forced into choosing one faculty member, to get started.

02-00:29:39
Burnett: It was pretty quick, if I recall. There was a lot of pressure. So, there's this very rapid baptism as a researcher: you're now a researcher, four weeks in.

02-00:29:53
Cerny: It's possible that Chicago Chemistry could be like that. I've never been there, and I do think they have a different ethic at Chicago, so that could well be. Here, it's just that physics is trapped in this other mode.

02-00:30:07
Burnett: I was wondering if it was maybe because of the apparatus, the fact that you were working with these expensive machines and there's this work culture that's tied to the machines? Is that part of it? But that would be true of physics.

02-00:30:23
Cerny: No, because of the theorists, right. I think our culture is that G. N. Lewis really just liked physical chemistry and he liked doing research, and that's why Berkeley was so great. We didn't even do organic chemistry, if you could help it.

02-00:30:43
Burnett: It was a focused research program, but I could generalize and say it was less scholastic and pedagogical, and much more of an experimental research program. When you go to graduate school, you're an experimentalist, right? There's theoretical work as well, but it's absolutely bound up in the experiments.

02-00:31:13
Cerny: We had a few theoretical chemists and you could join their groups, right, that don't do any experiments.

02-00:31:20
Burnett: So, you had to choose an advisor, choose a research director?

02-00:31:25
Cerny: Right, and I chose this Dr. Bernard Harvey, who was British and had gotten his degree at Oxford and then worked on the atomic bomb project in England, and then he was sent to Chalk River, in Canada where he worked on the bomb project, and then he ultimately came to LBL. He had been doing different kinds of experiments, but he actually was, right, just before I ended up signing up with him, he'd actually been a co-discoverer of Mendelevium. So, he did that with the sixty-inch cyclotron. So, I just chose him because he looked like

a nice person to work with, and then since he wasn't a faculty member, I had to have a co-director, who was Isadore Perlman, who was a faculty member and he was the head of the Nuclear Chemistry Division at LBL. I mainly did my work with Bernard Harvey, and that worked out perfectly for me.

02-00:32:36
Burnett: So, there's a question I have about how you transitioned. Maybe Manchester was this confidence-building year that you had because the Fulbright had that function. I'm curious as to how you go from feeling like you're under-prepared and under-resourced, and you're doing what you can to sort of catch up, but you feel like you're outside, you're on the periphery, you're in the boonies, and a year later, you're working with someone who worked on the Manhattan Project. How did that make you feel? At what point did this strike you as normal, I guess, is my question?

02-00:33:23
Cerny: It's really more that now you're in a class with thirty highly picked nuclear chemistry first-year students, and you're taking the nuclear chemistry sequence and the physical chemistry sequence. My first semester, I thought I didn't do too well for a while, but then I did.

02-00:33:51
Burnett: What changed?

02-00:33:53
Cerny: Well, I think I must have settled down a little bit. The nuclear chemistry part was easy, and so I was easily an A student. The physical chemistry faculty member had a Nobel Prize, but he hadn't learned anything new since about 1938. He was an expert on thermodynamics, but he didn't do modern statistical mechanics, and I knew about modern statistical mechanics because I'd been at Manchester. So, you're sitting there, saying to yourself, "Wait a minute, I'm learning old stuff here." He hadn't changed his lecture notes. I ended up getting all As that first semester, and thereafter. So, I discovered that I could handle the peer group.

02-00:34:43
Burnett: So, Manchester really was crucial for you.

02-00:34:44
Cerny: Must have been.

02-00:34:47
Burnett: I don't see how you could have gone from Ole Miss to Berkeley and had as smooth an adjustment. Manchester must have been very, very important for you. So, I didn't mean to cut you off, there, but tell me a little bit about Bernard Harvey and working with him and how you got oriented towards what your first project ended up being.

02-00:35:13
Cerny: He had decided to switch to on-line experiments using a cyclotron. He'd done other kinds of experiments that involved chemistry, but he wanted to make a change. He wanted to change into what was becoming modern experimental nuclear physics. He inherited some equipment that two of the physics faculty at Berkeley had at the 60-inch cyclotron on the Berkeley campus, but they didn't need it anymore, as they went on to higher energies elsewhere. One of them was getting older and the other actually left and became a dean in Oregon. So, it's not like you're making a new element, such as Mendelevium where you have a target, you shove it in the middle of the cyclotron, you bombard it enough, you pull it out, dump it somewhere, run up the hill, analyze the chemistry. In our experiments you take the beam out of the cyclotron and run it through vacuum pipes, and you put it in this huge, evacuated "scattering chamber" where you'll put a target. The beam will come in and hit the target, and then you're going to have detectors on a wheel, so that you can then see the reactions after the beam hits the target, what gets scattered at all the angles from zero to ninety degrees. You could go all the way to 180°, but most people don't. So, then, depending on the sophistication of your experiment, your detectors tell you what nuclear reactions are going on.

So, then, he started with that, and we had inherited a set-up where at the time, you were using vacuum tubes for the electronics and you were using crystals for the detectors. So, you were using sodium iodide crystals as your main energy detectors because you can put them on photomultiplier tubes and get a signal. If you then also used a cesium iodide detector, which you could cut really pretty thin, you could actually arrange it so a particle from a nuclear reaction in the target which was scattered at twenty degrees could go through the cesium iodide and get a signal off its photomultiplier, and then slam into the sodium iodide and get a signal off it, so you get two signals. By taking these two signals and plotting them one against the other, you could actually get data that told you the nature of the outgoing particle. So, if you came in with a helium nucleus, which is two protons and two neutrons, and it smashed and you wanted to look at just protons, you could see just protons in a band, or just deuterons, (a proton and a neutron), in another band, and then scattered alpha particles way out in another band. So, you could actually separate all the different outgoing particles. Having done that, you could add up the energies in the two detectors and get their total energy, and you've got all you want.

02-00:38:26
Burnett: You've got the entire event recorded.

02-00:38:29
Cerny: Right. You've got the thing recorded, so what my experiment would be, was to bombard a carbon target with an alpha particle, have a deuteron come out, and, see the energy levels in nitrogen. Look at energies of the deuterons that are coming out, and look at them as a function of angle, and see what the angular distribution looks like.

02-00:38:28
Burnett: Can we just characterize the kinds of information that you get? One is the angle after it strikes the target.

02-00:39:00
Cerny: Yes, you've set your detectors at an angle, so you will then move them, so you're going to do fifteen degrees and then twenty and then twenty-five and then thirty, so you've set them at an angle, and then you do your measurements at that angle. Then, you move it.

02-00:39:13
Burnett: Oh, that's what you mean by plotting it; you have to plot the results?

02-00:39:15
Cerny: Yes, as a function of the angle of the detectors.

02-00:39:49
Burnett: So, we've gone right into an experiment, here. Can we back up a little and talk about the broader nature of this work? You're interested in understanding light nuclei, you're interested in understanding nuclear structure.

02-00:40:27
Cerny: Yes, we were really trying to do "nuclear spectroscopy" and using "direct" nuclear reactions, and we're taking a particular type of nuclear reaction to let us look at the energy levels of these nuclei. You can look into what all the energy levels are, and then try to see if you can put them into certain kinds of patterns or understand the model that you would want to describe these light nuclei. So, in light nuclei, you would test a shell model like the shell model for electrons in an atom. If we were up in heavy nuclei, you wouldn't be able to really separate the levels out much because after the first one or two, they all get bunched up. So, ultimately, all my work has stayed in the light nuclei, more or less getting heavier, and we have been trying to find levels in nuclei as they become more and more unstable and interesting, by using the same nuclear reaction or different nuclear reactions to try to figure out their properties.

02-00:41:38
Burnett: What makes them interesting to you? Or to all nuclear chemists?

02-00:41:46
Cerny: Well, you really want to see if your nuclear models are working. There are the "compound nucleus" reactions, which occur a certain way, and direct reactions, which occur another way. So, the compound nucleus reaction would be that the carbon-12 and the alpha particle would just coalesce and become oxygen-16 for a while, and then it would break up. So, it would sit there for 10^{-16}seconds and break up. Okay. The theory for that says the angular distribution for a compound nucleus model would actually be boring. It would be either independent of angle after you corrected between the laboratory system and the center of mass system, where you have to do your theory, or it would look like this, go down a little and come back up. The direct reactions

can have major oscillations and changes, and you can have a theory, several different theories, then, to fit these oscillations to try to tell you what are the actual properties of these levels. So, you can't get the properties from the compound nucleus reaction, but you can get them from the direct reaction. So, by putting the theory to it, then you can learn the angular momentum and parity of all these states, and then you can say, "Well, what's the best nuclear model to really treat this?"

So, in our case, nuclear scientists hadn't done much of this kind of work, and we were just trying to look at what was the behavior of the states we were making—let's say in nitrogen-14—and actually, there were selection rules, and you couldn't make some of these states. So, I really had to try to understand these selection rules. Part of my thesis was involved in trying to understand them, and there was a particular conflict between two different sets of selection rules giving kind of the same results. The question was: were they really the same thing being said two different ways, or were there two different things?

We also wanted improved energy resolution in the detectors.

The problem with the cesium iodide and the sodium iodide is they had poor energy resolution. You couldn't resolve the peaks. So, instead of getting a bunch of closely spaced peaks, you had blobs. So, once you got the new silicon semiconductor detectors, you were much better off. When you substituted cesium iodide with a silicon—and luckily by then, LBL was pioneering in doing this—I was one of the first people to actually have some of these ΔE detectors. They would let you separate out the different species really well. That was one of the things, to really separate the protons from the deuterons and the tritons.

02-00:46:14
Burnett: So, when you were talking earlier about the tube detectors, you were talking about tubes as in tube amplifier, like a valve?

02-00:46:23
Cerny: Yeah, vacuum tube electronics.

02-00:46:27
Burnett: And you could not get the resolution from them because they were too simple or because they would distort?

02-00:46:35
Cerny: Well, no. The vacuum tube probably did pretty well. It was mainly that the cesium iodide didn't have as good intrinsic resolution as the semiconductors. In fact, when we first used the silicon detectors, we were still using the vacuum tube electronics, and the resolution got much better. It was only later on, when you also had transistors, it took a while, but by '64, then you had a lot of these fancy transistors coming in, and then you could get really good

electronic resolution and detector resolution both. So, then, combining the two, you were in good shape.

02-00:47:22
Burnett: So, there's a story there of changing instrumentation. This is right at the time when Alvarez in physics is introducing the bubble chamber, and he ushers in a whole new way of imaging in particle physics. There's a corresponding shift in nuclear chemistry, in detectors. Is it as dramatic?

02-00:47:47
Cerny: On a small scale, it was very dramatic for what you wanted to do. I'm doing nuclear physics in a chemistry department, right? So, all these experiments are essentially nuclear physics experiments, now with state of the art detectors. You originally started off only getting really thin silicon detectors, which gave you great resolution, which was fine because you only used them as a transmission detector in these experiments. Later on, you were able to make the silicon thick enough that you could get rid of the sodium iodide detector, and then you were in really good shape. So, that actually happened by mid 1963. It's a revolution in how you can do nuclear physics.

02-00:48:35
Burnett: This is the time when you are starting to collaborate with physicists. You mentioned earlier that when you were starting to work towards your thesis topic, that there were a couple of physicists who helped you think about—

02-00:48:57
Cerny: Well, they helped us get started in using this scattering chamber and special electronics I will mention, but they were doing different experiments. But really, I worked with Harvey in terms of those experiments.

02-00:49:49
Burnett: They're showing you how to use some of this equipment—

02-00:49:52
Cerny: I've simplified it because actually to separate nuclear reaction products, you had to have a special electronic tube that gave you a parabolic signal, and you needed this parabolic signal to actually let you separate the protons from the deuterons from the alpha particles. So, we didn't have to buy our own; it came along with the other apparatus.

02-00:51:04
Burnett: I do want to return to talking about detectors. So, tell me a little bit about how that happens and what that does. What other interesting events happened in this period.

02-00:51:30
Cerny: Shall I tell you about a social event?

02-00:51:32
Burnett: Well, yeah, there's a significant social event, isn't there, around this time? [laughter]

02-00:52:10
Cerny: Before I discuss that, let me mention a really good tradition in the Nuclear Chemistry Division, actually, at the time which was this mandatory seminar from 8:30 to 10:00 on Thursday mornings, where all the graduate students, everyone was supposed to go that had anything to do with science—there really weren't any postdocs to speak of—would present their research to a critical but supportive audience. That was really good because it gave you the real opportunity to present your work. I ended up presenting my work a lot, which is one of the things that helped me get on the faculty, I'm sure. That was really important. Unfortunately, that seminar seemed to go away in the same 16 months that I was gone to the Army. Maybe was the end of an era.

Now Bernard Harvey, the minute we found something new, he'd have me give a talk. Normally, they told me, but one time, Bernard just left me a note and a colleague covered the note up. So, I'm sitting there happily in the audience, and Professor Perlman, the head of the seminar, said, "Okay, Joe, it's your turn to talk." So, I went down to Perlman and said, "Nobody told me I was talking today." He said, "That's okay, you can figure out your talk while you listen to the twenty-minute talk of somebody else." So, I did that, but I managed to do so very well. I wrote everything on the blackboard and showed what Harvey and I had just been doing. Most of the nucleus chemistry faculty were there.

02-00:53:57
Burnett: Their reaction was one of support, or did they have questions and comments that helped you think about it? Or was it more a kind of almost an emotional support?

02-00:54:11
Cerny: I knew what I was doing—I put up a number of very complex equations that I had been solving to fit the angular distribution data that we had. I also was clearly very enthusiastic about my research.

02-00:55:08
Burnett: Were you married in Berkeley?

02-00:55:09
Cerny: No. Barbara Nedelka and I got married in Massachusetts in June 1959. She was born and grew up in New Jersey: she was 100% Czech—her mother was born in the US and her father came over as a two year old. Her parents had moved to Massachusetts, so we got married near Andover. (Those were certainly the "olden" days—I was 23 and she was 20.) Then we drove out to Berkeley. After her sophomore year at Manchester she had done her junior year back at Brown and then finished her BA with a senior year at Cal as a physics major. This may be amazing in today's world, but one of the famous

physics professors who was her initial course advisor said to her, “Why would woman ever want to do physics?”

We lived a half block west of Telegraph now. It was a nice street, clean and safe. There were fancy clothing and furniture—there always have been books stores. It was a street that adults shopped on. Ultimately the Free Speech Movement and its many aftermaths destroyed most of that. Our location was perfect—very close to campus. And, a major bonus was that—only one and a half block away—was the Cinema Guild and Studio (two theatres) playing “all” the important old (and some recent) movies with Pauline Kael in charge. She had been running the theatres since 1953, in repertory. We could take a break from studying in the evening, see a great movie, and come back and finish our homework. She provided written critical commentary on the movies and so it was a great movie education.

02-00:57:15
Burnett: There was a calendar?

02-00:57:16
Cerny: Yes a month or so in advance, you knew the shows that were playing.

02-00:57:22
Burnett: Did she curate it?

02-00:57:24
Cerny: Yes her fairly eSxtensive write-ups told you why you wanted to see that movie.

02-00:57:31
Burnett: And got you excited about it.

02-00:57:32
Cerny: Exactly, and the opportunity was just fantastic.

02-00:57:41
Burnett: That’s just prior to Free Speech.

02-00:57:49
Cerny: This is when I’m a graduate student, so this is still Fall ’59 through summer ’61.

02-00:57:52
Burnett: Beginning of the sixties, yeah.

02-00:57:55
Cerny: She ran up until the early sixties. I saw an obituary of hers, that said that her husband owned the theater— and then they stopped when they got divorced in the early sixties.

02-00:58:07
Burnett: Oh, that’s really interesting.

02-00:58:10
Cerny: And then she made a real reputation for herself as the film critic for the New Yorker all those years.

02-00:58:23
Burnett: And so your work was up on the hill? Was there a shuttle that went up there?

02-00:58:33
Cerny: I was very close to campus. All the experiments were at the 60-inch cyclotron on the Berkeley campus. So, you could walk late at night and not worry you'd be accosted. It was completely safe. It was just amazing how that changed over time. Otherwise, right, you could walk up the hill, or there was a shuttle during the day. My office was in Building 70A on the hill.

02-00:58:57
Burnett: Did you feel, after the Free Speech, like immediately after, did you feel like the tone of the campus changed and you felt less safe and secure?

02-00:59:07
Cerny: Yes. Well, the scene never ended until the mid-1970s.

02-00:59:13
Burnett: We will definitely talk about that.

02-00:59:15
Cerny: An event that could have gotten me into trouble occurred when the House Un-American Activities Committee decided to come to San Francisco in May of 1960. HUAC decided to use City Hall for some hearings. Many people wanted to attend including Berkeley students (I wasn't going) and matrons from Lafayette and environs. Separate lines were formed for these groups, and when the hearings opened, the guards only let in the matrons. Chaos then ensued and many students were washed down the city hall steps by the police and some were hurt. I had two very good female friends who were sociology graduate students and had been in the line. One of them had gotten hit, and the other one took her to a hospital and the police came through arresting many people in the hospital. The next day I was in SF picketing the HUAC with a number of upset friends and other professors, wondering if my security clearance would survive.

02-01:00:25
Burnett: Right, right, well, that was a concern.

02-01:00:28
Cerny: There was a hired goon taking all of our photographs. It was a really sad occasion. One of the girls was English, and she got deported, the other girl, who was waiting for a very famous scholarship in sociology to be awarded, and then they never gave it to her. This didn't prevent me from getting a security clearance, but I worried about it.

02-01:01:03
Burnett: There is so much we need to cover in those years, and that about the science, and then we'll backtrack to talk about in part. We're running out of tape here, so let's stop it now and then we'll pick up with the next session.

[End Audio File 2]

Interview #2: March 14, 2014
[Begin Audio File 3]

03-00:00:11
Burnett: This is Paul Burnett, interviewing Dr. Joseph Cerny for the University History series. We're in Berkeley, California. It's March 14, 2014. This is interview two, tape three. So, Dr. Cerny, we spoke last time about the beginnings of your research careers, beginnings of your graduate research, and so could you continue to tell us about the Ph.D. work that you were doing?

03-00:00:44
Cerny: Bernard Harvey was my main research director, and he decided to do a year-long sabbatical in Grenoble, France, during all of 1961. He left me to run his research group. So, the research group was myself and two younger Ph.D. students. I was doing all the experiments on the sixty-inch cyclotron, and there was plenty of accelerator time. We had a lot of good luck so we did a lot of things, but it was my first opportunity to be the real group leader and make sure that what these other students—of course, I was working with them anyway. So, that worked out really well. For help, there were a couple of other senior physicists in the group, and every so often, I could get experimental help from them, but they were really doing different kinds of experiments from what I was.

03-00:01:37
Burnett: But they had a lot of experience working – ?

03-00:01:41
Cerny: Right, because it was only my third year in doing things. So, that was really good. Also, Bernard Harvey really thought that the Nuclear Chemistry Division in general needed more theoretical support, and so he convinced the management that was true. A Canadian theorist named Norman Glendenning, who got his Ph.D. in, I think, Indiana, came out and joined the Nuclear Chemistry Division. So, I used him for theoretical support, and we also became very good friends because he was just a little older than I was. So, that all went along, and I finished my thesis by May, 1961, and the title was "Two Nucleon Transfer Reactions in the Light Elements." Really then, reactions in, like, carbon and nitrogen and staying below about mass twenty for almost all this work.

So, I actually got a job offer from Berkeley fairly early in the spring. I also, following Bernard Harvey's advice, had done a lot of free grading of the nuclear chemistry courses for the professors and things like that, which didn't hurt, I guess. I'd been recommended actually to Yale. Most East Coast colleges, then and now, don't have tenure-track assistant professorships, but Yale had one, had set one up in nuclear chemistry because they realize they weren't going to get anybody who was really pretty good any other way. So, we flew east to a Yale interview. Since my wife had grown up in New Jersey, and by then her parents were in Massachusetts, she came along, too. So, actually, it was our first jet trip east. I hadn't even thought about it. We flew from San Francisco to Chicago, and the reason I can remember the date to be exactly February 23, 1961, was that as we landed, there was a last major airline strike, and seven of the major airlines struck. We were there, watching the last plane come in from Mexico City, and then we ran into some other man who had to go east, and then we got in a car and drove all the way to Newhaven and New England.

03-00:04:14
Burnett: So much for the glamorous jet age, as it were.

03-00:04:18
Cerny: It was just fun. It was American, Eastern, Flying Tiger, National, Pan-Am, TWA, and Western all struck. So, I figured I could look that up, and I did. The interview from Yale went very well. Actually, a physicist named Allan Bromley had joined the physics department at Yale and stayed for the rest of his career, and he became a very well-known science advisor for George Bush. That's where I first met him, and he liked what I was doing for research so much, he ran off and offered me an instructorship in physics from Yale. I would have taken the assistant professorship if I was going to Yale, but I also decided that Berkeley knew me better, and we had the new cyclotron coming in and all the other accelerators, so I declined Yale. It was really nice to go. My wife's parents could not understand it because they'd heard of Yale and Harvard, and nobody could turn down Yale, for example. But anyway, I did.

It's interesting, so this period of '59 to '61, it was also all the Nobel laureates were coming into Berkeley. So, for three years, there was Nobel Prize fever because in 1959, Emilio Segrè and Chamberlain got the physics Nobel Prize for the antiprotons. Then, in 1960, Glaser had moved from Michigan to Berkeley in 1959, and then he got the Nobel Prize in '60. So, I'm sure both places claimed him for the bubble chamber. Then, in 1961, also that fact that I remember seeing Calvin in the hall, Melvin Calvin got the Nobel Prize in biochemistry. So, it was quite a time for that.

03-00:06:07
Burnett: Heady days.

03-00:06:11
Cerny: So, I taught in fall semester, 1961, and added some new kinds of experiments to try to do all I could. I knew I had to go into the Army for six months because the ROTC program required that, and I was prepared to go in, all the spring semester of '62. But actually, the Berlin Wall crisis developed, and all the Army tours got changed. My Army tour got changed to being for sixteen months. Some red tape actually got in sixteen rather than twenty-four, which was red tape in my favor, which was because I'd signed a piece of paper when I graduated that they could only call me up for two years within a six-year span of graduating, and I'd been postponing that with my Fulbright and

graduate school, so I didn't have but sixteen months left. So, that was really good, from my point of view.

03-00:07:09
Burnett: So, you prepared to go there?

03-00:07:15
Cerny: The first thing, we went east, and I went back to do ten weeks of Army boot camp again, at Aberdeen Proving Ground, in Maryland. So, that was quite an experience.

03-00:07:28
Burnett: Loud, I imagine.

03-00:07:30
Cerny: Well, you had to stay on base for the first six weeks all the time, and I guess in the four weeks, you could go off. So, it was okay, but you had to get over it. Then, most junior officers went to some kind of a professional school, but since I had a Ph.D., they decided to not do that. I would have gone to ammunition school somewhere. So, I didn't do that, and I was sent directly to Picatinny Arsenal in Dover, New Jersey, to be in the explosives and propellants laboratory. I went there by May, and then started looking around for what to do. I thought of a really good experiment. After thinking about it, I then worked with the chief scientist of Picatinny Arsenal, J.V. Richard Kaufman, who was an MIT Ph.D. in inorganic chemistry.

So, my idea was to take another way to look at the so-called hotspot theory of explosive initiation. It's still around—I sort of think it's what it is. The whole explosive normally doesn't go off, and particularly in accidents, you end up creating a small hotspot somehow. That, then, when the heat developed is more than you need, it'll accelerate to give you really the beginning of the explosion. They talk about microscopic hotspots, so they wouldn't be all that big. I knew another way to make a very high-tech hotspot, and so, that was to use pions. Pions are elementary particles that are created in the upper atmosphere by incoming cosmic rays. You create the showers, so these pions, they have a mass of, like, 273 times the mass of an electron, but they're very strongly interacting. They decay on the way down in muons, and muons come down to the Earth. But some pions get down here, too. So, muons are really just like heavy electrons, and weigh about 206 electron masses. Pions weigh more, and they interact differently.

So, for my oral exam for the Ph.D. at Berkeley, I was going to do a pion experiment of a different type, trying to decide how the pions were to capture, if we were capturing around nuclei with different Zs [numbers of protons]. How many would capture around carbon, compared to nitrogen or oxygen? So, there was a Fermi-Teller Z Law, but no one had actually proved it. So, from my oral presentation of an experiment not in my regular field, I'd learned about that. Okay, so here, I said, "So, we have to look a little bit at this mechanism." Even though the pions only live 2 x 10^{-8} seconds, that's long enough for them to be made an accelerator, and I'll come back to that. If you have a pion coming towards an atom, it's so much heavier than the electron. It's going to slip underneath all the electrons, but then it's going to start dropping down where its own electron kind of shells would be. As it does it, it's going to be kicking out real electrons as it's dropping down. You're beginning now to set up a spherical hot zone because these electrons are being ejected and they're going to be heating up the electrons in the material right then. So, you get a lot of that as it comes down. Then, when it finally gets down, it interacts with a nitrogen nucleus and it blows the nitrogen nucleus up into three helium nuclei and a couple of neutrons. So, now you've made, way inside this piece of material, a source of alpha particles, which are very hot, very short-range. So, this really can generate a beautiful, spherical hotspot.

I did enough rudimentary thermodynamic calculations to convince myself that this might work. Certainly, nobody had ever tried this before, and probably never again. So, then, how would we do it? Well, it turned out there was a synchrocyclotron in Upstate New York, called the Nevis Cyclotron. The Navy was sponsoring some research there and we were in the Army, so there was a way to get on it. So, then, I went over and talked to the deputy chief scientist there and said what I wanted to do. I wanted to see if you could put these pions in these different kinds of explosives, and would they go off?

So, we ultimately had six different explosives: three primary explosives—and these are horrible things like lead azide and mercury fulminate—and then secondary explosives are like TNT and RDX. So, I had six of those, and they'd agreed that we could do the experiment. There was this African-American gentleman that worked for Picatinny Arsenal that would load all these things. I wouldn't go near them. So, he loaded the explosives into disks and stuck them inside Styrofoam, and then stuck them so they'd fit in a box, so you could put them in this box. If you could then get the pion into this and hit it, if it did explode, it was pretty big, but it wasn't that much explosive. These cyclotrons were called, like, pion factories, and so this synchrocyclotron was similar to the 184-inch at LBL, which also was an early pion factory.

So, they knew how to make pions, either positively or negatively, and you can make them by the beam striking some kind of target, and then they're going very fast, so they get pretty far even though the half-lives are very short. So, they set it up for us so that we could then put one or two of these boxes in this beam and knew exactly how many pions would be there, and we could run on them. First, we had to go over a bridge to get from New Jersey to there. We had to get somebody's permission to carry these explosives over the bridge. Although, really, the amount of explosive would, like, blow your hand off to some extent, but it wasn't going to do anything other than that. So, we did that. The chief scientist came over. We ran, for a very long afternoon, at least 100,000 pions stopped—at least—in each of the six different explosives, and nothing happened. Dejected, we went back to Picatinny Arsenal, but then I ultimately wrote it up with him, and showed according to our calculations, it should really—going on my pretty elementary thermodynamic calculations—it should have gone off. We got that published in the *Journal of Chemical Physics*, which is a good journal. So, that was good.

Another way you could do this, people had tried to look at fission fragments setting off explosives, if they could embed a fission emitter in an explosive. Now in this case, the two fission fragments, really they'd be cylindrical patterns of charge distribution rather than spherical, but they would be very intense. That had been done before. I thought, "Well, maybe I'll try to do that in liquid nitroglycerin." So, the Army was having me come back out here to go down to help a group at Santa Barbara, which was doing some kind of electron irradiations at a commercial place. I came out several times to go down and do that, which was nice. On one of the trips back, I went to my colleagues at LBL and got a minute amount of californium-252, which is a spontaneous fission emitter. So, in those days, you just put it in a little paper box and took it on the airplane and didn't think about it much.

At Picatinny Arsenal, then, I told them what I wanted to do, and they were beautifully equipped for explosive detonation experiments. Great shielding, and this, that, and the other. So, we dissolved the californium-252—I knew what the activity was; you couldn't see it or anything like that—in the nitroglycerin, and then all these explosives have an autocatalytic point. So, when you raise the temperature to that temperature, it'll go autocatalytic and you can't stop it, and it blows up. So, then I preceded to run it five degrees below the autocatalytic point, four, three, two, and one, behind all this shielding. It's nitroglycerin sitting on something. Then, nothing happened, even though I was one degree below it. So, then I remember saying, "Nuts to this," and turned the thermostat up on the heater, and heated it up to that, and it really blew, like, very impressively. So, clearly nothing was wrong with the nitroglycerin.

We actually wrote that up, too, for another paper, without any of this other part of it. There was a *Star Wars* component to the first one, which I had mentioned in seminars, which was for the pion issue, if it had worked, you would then have to think about how it affected missiles. When the missiles would go up in the upper atmosphere, you were going up to an increasing pion density. So, they're up there, so they're not down here so much, but as you go up, they're there. Particularly in a plutonium bomb, you have a conventional explosive sphere around it because it goes off by implosion. So, you actually go up, and you could imagine these pions up there, wrecking the implosion part of the bomb in the missile. So, had that worked, I would have had a different career, and I'd have been a general in the Pentagon, and not a professor at Berkeley. But it didn't work.

03-00:17:54
Burnett: Were you thinking about the use of pions as detonators, or was this to model, you were using pions to model the hotspot explosion?

03-00:18:06
Cerny: I was using it as another way to get a spherical hotspot, way deep inside an explosive, and just see if on a microscopic level, it would go off or not. So, that's what we reported. It didn't do it. So, now they think maybe that the size of the spot has to be defined somehow, and maybe it needs to be bigger than a pion could generate. Then, we were generating 100,000 of them in thirty minutes or something, so there were a lot of these going through. All these experiments were done in air, so the beam would come out in air and then go into our boxes and everything, were in air, and so that was straightforward.

03-00:18:53
Burnett: So, you had a sixteen-month period when you were doing some training, but you also got to do fairly interesting work. It was a diversion from your research career, but it was an interesting set of problems.

03-00:19:06
Cerny: It was very interesting, yeah, and we wrote it all up.

03-00:19:11
Burnett: And while you were working with other people. Did you keep in touch with these people?

03-00:19:16
Cerny: No, actually I didn't, really. I had a couple of other Army lieutenants work on some of these things. First thing I did when I got there is I saw one of their publications was wrong, and the person who'd done the ranges of protons and alpha particles in explosives, actually, was completely using the wrong equation. He was using the equation that would apply to fission fragments, rather than very light things like protons and alpha particles. Which, even if that person had read a regular textbook would have seen was wrong. So, the first thing that had to be done is that had to be completely redone.

03-00:19:52
Burnett: Well, the benefits of the ROTC program, then, have people circulating through to do these kinds of things.

03-00:20:05
Cerny: We did two things that were interesting, again, since, from time to time I'm going to mention travel things—

03-00:20:11
Burnett: Extra-curricular, yes.

03-00:20:12
Cerny: So, the people at Picatinny Arsenal liked me so much, and I told them, "Well, gee, there's a meeting in Padua on the kind of stuff related to my Berkeley

research," and I wondered if the Army would send me there. Well, the Arsenal thought that that was a great idea. The Army, at the last minute, did—it was literally about the last minute. Barbara went, too, because she really liked to travel. The thing that was so amazing was you could now fly your 707 from New York to Paris, but TWA advertised that if you did that, they would fly you *for free* to three more places in Europe. For free, and I just wish I had those advertisements. I remember what they looked like.

Anyway, so we went and had a great time in Padua, went to Venice, and then we decided we'd go to Egypt, so we flew to Cairo. We got into Cairo and back then, the airline made sure we had a place to stay, even. We'd have been lost anywhere, but we got a place to stay. For a while, I thought we were staying in a house of ill repute with the other people who were staying with us, but then later on, I discovered they were really cabaret dancers who slept there. We got to see the sphinx and the pyramids, and we got to see, in September of '62, the whole Tutankhamen collection because there hadn't been any of the wars yet, really. They would leave it all out. Now, a lot of that stuff is just not out.

03-00:21:49
Burnett: I was going to ask, when you went to visit the pyramids, you used to be able to go on them, I think, but did they have some kind of barrier up?

03-00:21:58
Cerny: No, you could climb up inside of it. The other thing, we didn't have too much time to take the free trip, so we also took an overnight train down to Luxor. We went to Luxor and Karnak and got to see the Valley of the Kings, and queens, and also Karnak, and then take another overnight train back. Then, we flew to Istanbul, and then we flew to Athens, and then we flew back. So, that was great.

03-00:22:28
Burnett: Did that then inspire you to continue to travel? Whenever you could, you would?

03-00:22:33
Cerny: Yeah, we were both inspired that way, and we met in England, in Manchester, right? So, we did that. The other thing on the different tack was that I was also in the Army for the Cuban Missile Crisis, and so, that really electrified Picatinny Arsenal and all the regular Army types were getting excited, "Gosh, we'll get to go to war and there'll be promotions," and things like that. Of course, I was just generally angst-ridden for the whole situation, but in addition, since, as I mentioned, I hadn't been trained as an ammunition officer, I thought it would be really interesting if all of a sudden, I had to be one, reading the manuals for the first time on my way to Cuba, or something. So, luckily, it went away.

03-00:23:20
Burnett: Yes, yes—for everyone.

03-00:23:22
Cerny: For everyone, exactly. Then, we're about to come back to Berkeley, and our first son, Keith—it's '63—was born in January '63. I think just to get the next son on the record, Mark was born in August '64, after we were back at Berkeley for a while. That sort of gets me through getting back here.

03-00:23:50
Burnett: You're returning and your Ph.D. was completed, and you went on this—did you keep in touch with people at Berkeley? You knew you were coming back and you've taken the job, and then you had said, "I have this deferred ROTC thing that needs to be completed." So, they're accommodating of you, of course, but at the same time, you must have been anxious to get started. How did you manage that, and what were you planning on doing when you hit the ground?

03-00:24:35
Cerny: In the first place, I wrote to the dean in the College of Chemistry regularly, so he would remember I existed. But because I had to go to Santa Barbara to help on these experiments, I would each time visit Bernard Harvey at the new cyclotron being constructed, when I would be here. I also had a Colombian physics graduate student who was sort of trying to do stuff while I was gone, and I would see him, too. But he ended up giving up. So, I think I got back in June and the lab paid my salary in June, and then I went on a joint appointment between the Lawrence Berkeley Lab, or the "Rad Lab," then, and campus.

So, when I started, the younger nuclear chemists had two-thirds/one-third appointments. I had a two-thirds appointment on campus and a one-third appointment at LBL. But everything was paid over a twelve-month basis, and it was very complicated to work it out. After a number of years, we got regularized to where we all had full FTEs. This two-thirds/one-third I've never seen again, but there are now half-time appointments which some physics faculty have.

03-00:26:03
Burnett: It's assumed they're doing other work?

03-00:26:05
Cerny: Yeah, then they're doing research at LBL. They have a half-time teaching load and doing advising and everything, yeah.

03-00:26:13
Burnett: I think Seaborg gets approval for the 88-inch in '58, maybe, the money's promised. They start planning and building. Did you know what you were going to do with this machine, or did everyone have a laundry list of experiments that they wanted to work on?

03-00:26:38
Cerny: Well, that's a good question. I started thinking, by spring of '63, about what I wanted to do. I knew that the cyclotron would have a really good proton beam at higher energies, and so, I started doing that. Let me say something about the cyclotron: the sixty-inch cyclotron on campus was a fixed-energy cyclotron. It could produce 48 MeV alpha particles, or 24 MeV deuterons, or 12 MeV protons. The fixed frequency, that was about the maximum energy you could do without having to worry about the relativistic mass increase in the particles as they got going faster.

03-00:27:22
Burnett: Right, closer to the speed of light.

03-00:27:23
Cerny: This new cyclotron was going to be one that would get around that problem. I think the design had been out there, and somebody was just waiting to do it, and so Seaborg saw his opportunity and got it for Berkeley. It was accomplished pretty fast, but it was the very first of its type. It's eighty-eight inches, is the magnet size, but as you get going faster and faster, you're getting heavier. So, what you have to do is have a way to increase the magnetic field with radius. With the old ones, the magnetic field was just always the same, but now, inside the main magnet, you place some special coils. These coils then are adjustable and they go the whole, to the outer side of the magnet. They're adjustable so you can increase the magnetic field, depending on the energy with the radius, so you can keep everything in sync to get out.

The other thing you need, if you're going to have variable energy, is you need to have a radiofrequency system that will cover quite a range. So, the sixty-inch would have had just one radio frequency, whereas now, you have to have something, it goes from five to fifteen megahertz, or whatever. You had to have that. Then, the other thing you had to know is that you tend to lose the stability of staying in the center of the focal plane. So, you have to have a way, so as you get heavier and are going faster, you have to have something that keeps kicking you back a little bit into the center. Our cyclotron is constructed with spiral ridges, so there are three sets of spiral ridges. As you went over those ridges, it's all adjusted to give you a kick so you'd stay in. Ours are spirals. You can do it with triangles. So, the cyclotron worked.

It took a fair amount of time between when you barely had a beam in the cyclotron to having a beam out of the cyclotron. Then you had to build up all the experimental areas. There were at least three different caves—now, there are many more than that—so everybody had an experimental area called a cave, loaded with concrete blocks for shielding. You had to do all that. About the time I got back, they'd mainly been doing experiments with external beams for just a few months, to any extent, so I hadn't really missed very much. Bernard Harvey had arranged that I have a postdoc, then, and it was Richard Pehl, one of these two graduate students that I had worked with, the older of the two. He and I had worked very closely together, and so, that was really great. He'd been around there, so I had him to work with. I said, "So, what would I really do? Well, I'll use the proton beam." The protons could go up to 45, 55 MeV (million electron volts), whereas the sixty-inch could only have 12. So, you had a lot of energy, so I thought, "Well, I'll just do some reactions like I'll bring in a proton and take out two neutrons, make it as a triton, and get the triton going out."

I'll stay in the light elements because not that much was known. When you really looked at it, there was a ton of things that could have been found out in the light elements. The nuclear chemists also taught physical chemistry, so I taught thermodynamics, and the other rest of physical chemistry was kind of two separate semesters. So, I taught that sequence and at that time, everybody in nuclear and physical would go down and help in freshmen chemistry labs. You lead the discussion section and watch the lab for a while. So, I did a lot of that for a number of years.

03-00:31:24
Burnett: That's your yeoman service, right?

03-00:31:26
Cerny: Right. So, then I started acquiring graduate students, and it was still a heyday for nuclear chemistry. There were tons of really good students. I just said, "I'm only going to take two a year because I don't want my group to grow any faster than that." I want to do it first come, first serve. They've all been admitted, I'm not going to go shop around and upset people to try to sneak off the very best ones and make people be discouraged. So, I just did that, and so, more or less my group just grew in that period, then. Till, ultimately, I'd say in four years, I would have eight or nine Ph.D. students and by then, everybody was also doing postdocs. You'd have one or two domestic postdocs that we paid, and then one or two international postdocs because the cyclotron was very popular because it was clearly successful from the start. Oak Ridge had a different version of it that wasn't so successful, and ours was so successful, it was immediately copied at Texas A&M, and then it was copied in Japan, and then ultimately, many years later, we had a very similar one in Finland.

We had a good accelerator; then what we needed was good detectors and good electronics. Just as I was leaving for the Army and coming back, people were really trying to do things. We had a guy who was pretty good doing things with vacuum tubes, but in fact, he couldn't do it. The beam intensity would upset the system enough that you just couldn't do it. So, luckily for me, a person named Fred Goulding came to Berkeley by way of Chalk River, born in England. That's how Bernard Harvey came over, too. Goulding was a technological superstar for both semiconductor electronics and detectors, and he began working with a guy named Donald Landis, who was extremely good. So, I sort of had these experiments I wanted to do. The techniques I needed, the physics of the techniques I needed—I needed delta-E detectors and E detectors, I needed to be able to separate out-coming particles. I knew all that from my thesis work. So, they were able to provide state of the art amplifiers to do this work, and then we could develop more complicated boxes that actually let me set the gates so that I could figure out, from the outgoing particles, which ones they were and set gates on them. Say, "Okay, the deuterons are going here and the alpha particles are going there."

Then, already, they'd been working on semiconductor detectors, and so we had silicon detectors in our counters, which were very thin ones, which were now the ΔE detectors, which had started with my thesis work, also. But by the Ph.D. work, they were making them thick enough to be the E detectors, so you had really good resolution. For a while, Bernard Harvey was letting me work in his scattering chamber, but in six or eight months on this, he said, "Okay, we're throwing you out. We've got this other scattering chamber made at Livermore. We're giving it to you. You can put it in this cave—you're going to have to figure out how to get the beam there and all that." So, we did that, and that all worked fine, once we got it figured out. So, then I had my own set-up in Cave Two that was strictly mine, and we were kind of off doing things.

03-00:35:27
Burnett: Generally, you're concerned with the behavior and structure of the lighter elements when they're pushed to the limits of their stability?

03-00:35:39
Cerny: What I'm really concerned about is, well, I wrote it up ages ago that makes a little sense. So, what we really looked at, we wanted to find light nuclei with unusual neutron-to-proton ratios, that could just barely hold together. So, you would ask the question of what's the lightest carbon isotope that can hold together? So, carbon-12 is six protons and six neutrons, is stable. Carbon-13 is stable with six protons and seven neutrons. Carbon-14 is unstable, but lives a very long time, six protons and eight neutrons. So, the question particularly that I looked at most of the time was, I want to go to the neutron-deficient side, and I want to find out the lightest carbon isotope one can make that will hold together and decay, and it's going to have to decay down here by beta-decay. There are these three decay modes—there's alpha-decay, which is alpha particles leaving the nucleus, that's mainly in the heavy elements, beta-decay is either electrons or positive electrons, called positrons, coming off, and the third decay mode is gamma rays, which are going off all the time. So, it was already known that carbon-11 and carbon-10 would hold together and then beta-decay, and the question was, is carbon-10 the lightest one, or in fact, is carbon-9 going to be the lightest one?

Because we had this fancy accelerator with high-energy beams and sophisticated detectors, we could combine it, we could bombard a carbon-12 nucleus with a helium-3 beam, grab three neutrons and make it helium-6, and have the product that we're measuring be carbon-9. By measuring the helium-6 energies, we can know what the carbon-9 mass had to be, and we could figure out carbon-9 would hold together and then beta-decay. Later, to prove it, we did other techniques.

So, we could do that, and then we were pretty sure carbon-8 cannot hold together, so carbon-9 lives like 100 milliseconds, and just the way beta-decay is, its fastest is 1 millisecond, and then after that, it can be very, very, very long. It can be 10^{23} years. So, all these things in beta-decay are at least a millisecond, whereas if you're unbound, it's like 10^{-20} seconds. Then, you have to do a different technique to look at that.

03-00:39:03
Burnett: So, there's this huge ridge, there's this huge difference or gap or frontier between these two isotopes, right, in terms of their half-lives?

03-00:39:12
Cerny: Right.

03-00:39:13
Burnett: Is it a kind of descendent of Glenn Seaborg's research program? He was interested in these islands of stability.

03-00:39:24
Cerny: That's way, way out. It's very, very different. It's more like the main competition was Cal Tech physics department, which for years had looked at the light elements, and try to see what their properties were. So, what I really wanted to do was to map out, on a chart, try to figure out the lightest isotope of each of these elements that would hold together, and would it have interesting decay properties, in addition like the beta-delayed proton emission. So, we did a host of experiments like that. I was really helped by the fact the summer I got back, I was sent a copy of a pre-print of three Russian theorists named Baz, Goldanksii, and Zeldovitch, who had really looked at the light elements, and told you all this wonderful stuff you can discover, somebody could do. They had their own mass tables—you needed the mass tables to figure out what was likely to be stable or not, so they developed good mass tables, they developed good theories. So, I had their paper as kind of a blueprint, and then all I had to do was figure out how to match the techniques I had to their predictions.

03-00:40:51
Burnett: Was McGill also doing some of this work?

03-00:40:55
Cerny: Right. McGill was, and Goldanskii had predicted this beta-delayed proton emission, where you made a nucleus so far off stability, instead of just beta-decaying, the energy in the daughter state was so high it could actually emit a proton, so you could measure the protons. So, yeah, McGill pioneered the beta-delayed proton work, and then Brookhaven followed up on that, and then

we also got into it in our own way. We also had the advantage, though, of doing these other sets of experiments with the helium-three and the helium-six coming out, and measuring all these nuclides. A number of things that had fit into the theoretical schemata in this paper. We were following up on the beta-delayed protons, but really pioneering a whole bunch of this other stuff. It tied exactly into what I wanted to do, too.

03-00:42:13
Burnett: These experiments built out a research program in the very light elements, really, and you're working with helium.

03-00:42:24
Cerny: Originally, there was a lot of interest in nuclei like carbon-9, and the other big question was whether helium-eight exists. So, you had this helium nucleus, so one of the interesting things, when you look at all the masses in the periodic table and you're having to do the astrophysics, you have to worry about a major problem. The major problem is that there are no stable nuclei of mass-5 or 8. So, they're not there. Everything there lives 10^{-20} seconds or less. So, helium-5 or lithium-5 falls apart in 10^{-20} seconds. You can't even build up on them. Beryllium-8 is unbound and falls apart, a little slower, 10^{-16} seconds. Turned out there was helium-6, then the real question early on was, does helium-8 exist, does it have a half-life, and what's its mass?

As we developed more sophisticated techniques, we decided we could measure the mass of helium-8. A colleague named Arthur Poskanzer, who then came to Berkeley and I worked with some, by doing experiments at Brookhaven, showed that he could capture the helium-8 and get its decay. But what we want to do is measure what the mass was, and so we had to do a very complicated reaction where an alpha particle hit a magnesium-26 nucleus and came out as a helium-8, that we would measure the helium-8, and leave the magnesium-22. So, by measuring the helium-8 energy and knowing the other masses, I could tell you the real mass excess of the helium-8. We did that. A whole bunch of physicists tackled the issue of the helium-8, which was interesting. There were emulsion stack experiments in Russia, they saw one event like that. There was a physicist in Oregon who didn't know whether to claim it or not, so he didn't claim it. So, different people had different aspects of the program, but we measured the mass, the first time, with large error bars. I was delighted to see, as time passed, the error bars went way down but our number was really pretty good.

03-00:44:50
Burnett: Error bars? You had great precision in your experiments.

03-00:44:57
Cerny: No, our error bars were large, but as people got more sophisticated after you found it—

03-00:45:02
Burnett: They reduced the error, okay. So, that's, we're talking, in the mid-1960s, at this point?

03-00:45:19
Cerny: Yeah. This is a lot of what we were doing in the sixties.

03-00:45:25
Burnett: This is, of course, in the heyday of big science. Around this time, they were proposing the next big thing on the physics end of things—Ghiorso was trying to get an omnitron built, and was turned down. He begins to think creatively about how to do more with less, so this is a kind of pioneering thing at Berkeley that they're doing, that they're thinking about how to get more creative with funding, and then thinking about that. Was that something that you were aware of, at the time, in the mid-1960s, that this was a question? Were you hoping that there would be an omnitron? It's not something that you would use, necessarily, but were you thinking that this would continue, the funding of new projects?

03-00:46:31
Cerny: Well, this was a time when the next big accelerator was going to be in the Central Valley, right, and then it was finally decided to build Fermilab. So, it required building Fermilab, and then the Berkeley physicists were dejected and didn't want to help.

03-00:46:48
Burnett: They felt betrayed, didn't they?

03-00:46:49
Cerny: Yeah. Lawrence had died in '58, so we lost a huge political operator, scientific, everything. Ghiorso did think of the omnitron, but first then, he'd kind of gone from the HILAC to having the SuperHILAC. I actually had tried experiments running with Harvey when I was a graduate student, even, at the HILAC. It was a beast to work with for certain experiments. Everybody had a really tight program in what they did, and we every so often got a Sunday we could go in and try to do an experiment with helium-3—because I wanted to do something with helium-3 beams. When I was a graduate student, we didn't have helium-3 beams. Once we had the eighty-eight-inch cyclotron, then we did. It was really tough to work up there. These were bunched beams, so it hit, and so when they'd hit, the electronics would jump, virtually, with these vacuum tube amplifiers.

Ghiorso was so focused on very heavy elements and this group of Diamond and Stevens were up there doing things in very heavy elements. I knew the stuff was going on, and I had a marvelous new accelerator that I was one of the major group leaders on. I was far more interested in doing my own experiments, when there were so many to do.

03-00:48:21
Burnett: So in a sense, nuclear chemistry was kind of the humbler cousin, in terms of funding and finance, the particle physics stuff demanded so much greater outlays of money, right? These are talking a quarter of a billion dollars for a new accelerator versus $20 million for a new machine that might be used in nuclear chemistry. So, it was so much cheaper to work in nuclear chemistry that you were quite happy with the state of the art machine that you had at the time.

03-00:48:53
Cerny: Well, I was, because that's what I wanted to do and I wasn't interested in high-energy physics, either. The Berkeley physics department had moved out of doing nuclear physics. It had certainly done it, but even at the time that I was an assistant professor, they'd completely moved out of it. I remember that Geoff Chew was a distinguished theorist. I went to his lectures on nuclear physics, and his lectures on nuclear physics went like nuclear physics all the way up to the deuteron, and then we were essentially doing high-energy physics. They just weren't doing it at all, so the nuclear chemistry course was doing nuclear physics. I taught some of the nuclear chemistry sequences starting in the seventies, and we were teaching the nuclear physics in the nuclear chemistry course—probably with a little less math or something, but basically.

03-00:49:46
Burnett: So, in the mid-sixties, you have done a lot of great work, you've published a number of papers with respect to the work on the light elements. That is what gives you, you become an associate professor in '67. Is that right?

03-00:50:05
Cerny: Right.

03-00:50:08
Burnett: That is for those contributions?

03-00:50:10
Cerny: We were publishing a lot of *Physical Review Letters.* Dick Pell was a great postdoc, and we had these international postdocs come in. They were very good. So, I was a group leader, I was independent. If you're hired in the Berkeley faculty, you're independent. I had access to as much cyclotron time as I needed. The foreign postdocs very good, but they couldn't quite believe that an assistant professor could act independently because of the structure of the European labs. I think it was an inspiration to this one, Claude Detraz who came to work with me, probably '64 or '65, he came from Orsay, highly recommended. Really smart. We did all kinds of great experiments together, and he went on to be the head of a big French lab, GANIL, and then he was in the French NSF, and then he was the second in command at CERN toward the end of his career.

He wasn't the only one like that—this incredible Japanese showed up, and some Italians. It was just remarkable, the people that were coming through as general free postdocs in addition to which we had the funds to support a postdoc or two. The dynamic was great, and we just published a lot of stuff that has sort of been highlighted. I understood isospin really well, and that let me really deal with that. There was one initial conference on isospin in probably 1964, at Argonne. Then, the second one, I was one of the people that organized everything. It was in about '69, at Asilomar. It's more than we want to get into here, but I sort of had all the background I needed. It was just like he discovered a new text.

03-00:52:21
Burnett: Right, it's almost like you've got this Rosetta Stone here, and everyone descends and you begin to work it out. It was a really exciting time, and so there's a legacy there. You've got a network that begins to circulate. These postdocs come and they've done this research and they go off back to their respective countries. As you said from the engineering side as well, I imagine—and this is what I've been told—Berkeley engineers were going to these other countries to advise them on this is how you build this kind of cyclotron. Notwithstanding the resistance of the physicists, I suspect there was no small amount of engineering help for Fermilab, as well.

03-00:53:07
Cerny: Yeah, no doubt. We had the cyclotron, which was terrific, and initially there's this guy who ran the machine shop who was a superstar. He could just get it all done. It really got things going, and then he actually went and helped on the higher, the other, heavier accelerators later on, which needed the help and we didn't.

03-00:54:34
Burnett: It's a fascinating interaction amongst their theoretical people working, there's the experimental scientists, and then this army of engineers and technicians who respond to the question, "I need this and that," with, "Sure, I'll just do this and that." It's obviously much more complicated than that.

03-00:53:52
Cerny: Well, it was incredible because Goulding and Landis could produce prototype instruments for you, literally within two months when you said you needed something, really new. Not eight months or two years and what they developed was given free to Oak Ridge to a commercial group that built copies, so I don't think they made a lot of money on this. They really could do that, and then I needed highly specialized things because when you really had to get the really low background, you had to identify the particle twice because then I'd learned that the detectors weren't perfectly uniform. If they're not perfectly uniform, they could simulate something when you're measuring extremely small yields. So, I needed a way to clean that out, and so they could provide that. I worked very productively with them for ages. They were doing lots of other stuff, too, and we jointly published. I wrote our paper

on carbon-9, there were only four authors, and they were the other two. I was the lead author, and then they put my name on the equipment, when they wrote that up, with them first, obviously.

03-00:55:05
Burnett: An incredible collaboration, and that's a story, probably, that's reflected in all of these great research programs. They need that intense collaboration. We're getting through the 1960s, and perhaps we want to talk about Harwell, I imagine. First, you started talking about the climate at Berkeley, and what was going on at Berkeley, and how that changed in the early sixties. I imagine that became more dramatic towards the end of the 1960s, as you were working here.

03-00:55:45
Cerny: Yeah. We always had a really strong group, and by then, by the early seventies, more of the Germans had the money to come. We also hired people who were foreign, obviously. Just to mention two interesting travel things. I don't know where to throw stuff in. So, in April 1967, the Russians decided to invite some nuclear physicists to come to Russia. The high-energy physicists had already been invited a year or two earlier, so we were like the second wave. So, I got invited to go talk about our kind of research we've been talking about in Tbilisi, in Georgia. So, I flew to Moscow, and then I was met, and then we flew down to Tbilisi. I'll dwell on that in a second. There, it was wonderful. Couldn't believe it. It's a totally different climate and environment. You're below the Ural mountains, and the sponsors said the heavy hand of Soviets wasn't so heavy down here, so you could actually go to kind of a modern art museum. There weren't very many foreigners at this meeting, so it was incredibly hospitable. That was really nice. Then, we went back to one of the main nuclear physics labs, Dubna, outside of Moscow.

03-00:57:31
Burnett: So, you did get to see that?

03-00:57:31
Cerny: Went to Dubna and gave a talk.

03-00:57:35
Burnett: What was that like?

03-00:57:37
Cerny: Well, Flerov was running the place. I knew a lot about heavy elements, too. His big push was heavy elements. It was very interesting, and totally different looking equipment. We got to meet a lot of the greats of Soviet physics, particularly heavy element, stuff. The person that invited me was really more from the Kurchatov Institute in Moscow, and so he was doing more light elements.

03-00:58:06
Burnett: Goldanskii, did you get a chance to meet him?

03-00:58:58
Cerny: I probably met him that trip, actually, yes. I met him several times, and he was a gentleman, and he was a really great guy.

03-00:58:20
Burnett: He must have been following your work.

03-00:58:21
Cerny: Oh, he was, and we'll get back later to another thing that he suggested that we found. Whenever I would do something, I would send it to him, so we had some correspondence. That was nice. It was really Russia and it was really uptight, and all your visas. We went there, saw Moscow, looked around. Then, actually, I went to Leningrad, and my first wife, loving to travel, I can't imagine how we did this, she'd gone skiing somewhere and so she flew into Leningrad. For May Day, we watched it snow in Leningrad, and watched out of our hotel room as they marched in front of us.

03-00:59:12
Burnett: Wow, the big parade.

03-00:59:15
Cerny: So, that was fun. Then, we came out through Prague, and came out through there because I'm half-Czech and she's completely Czech. Her father actually came over when he was two, and her mother was born here. My father's side of the family was Czech and his mother had always kept up with one of her sister's daughters, nicknamed Sarka. We were used to the short and dumpy mothers and grandmothers of Czech, you see all over, and Sarka was beautiful and tall and spoke English and was married to a very well-known Czech architect.

03-01:00:10
Cerny: She had a job, she was fluent in English, taking people around biology institutes in Prague. So, we met her, loved her. She was more like an aunt. I've been to Prague twenty times. We'd see her, stay with her, after the first trip, and then when we were in Europe, we'd have her come out and stay with us. That just turned out to be wonderful, as an aside. I've seen Prague from '67 to 2003, about every other year.

03-01:00:42
Burnett: You saw it before the Spring, and probably immediately after? Did you go in '69?

03-01:00:47
Cerny: We were back there in '69. When we flew out from Russia to Prague in 1967, I always thought it was the Prague Spring, compared to the way Russia had been. The waiters were happy to work, in Prague. They weren't happy to do that in Russia. So, I guess it loosened it up, some, but to be honest, it seemed so refreshing, then. The real Prague Spring was '68, and then actually, we were back in '69.

03-01:01:13
Burnett: Was it a different atmosphere then?

03-01:01:14
Cerny: Not too much, no. You still had to have your visas and things.

03-01:01:22
Burnett: In terms of in the streets, was there a lot more, were people a bit more close-mouthed or reserved as a result of the crackdown?

03-01:01:32
Cerny: Well, I don't speak Czech, so I wouldn't know. I don't know—it's interesting. They didn't look terribly depressed. After the first trip, either we would stay with Sarka, or she would put us up in somebody's place, just rented. So, then you just had to register with the police, and so she would take us over and we'd register with the police somewhere. A lot of these people knew her.

03-01:02:03
Burnett: Right, so she had some pull.

03-01:02:06
Cerny: She'd been around, she had pull. The early days, the art community there, there were sculptures of her in the Czech museum. She's a beautiful woman. Didn't have any children.

03-01:02:19
Burnett: That must have been an amazing experience for you. Well, that's fantastic. I think that might be a good point for us to break, and then we'll come back and talk about your second trip that you wanted to mention, and then we'll talk about Harwell.

[End Audio File 3]

[Begin Audio File 4]

04-00:00:04
Cerny: If we got to beta-delayed two-proton emission, that'd be fine. If we didn't, we could fit it in somewhere.

04-00:00:15
Burnett: This is Paul Burnett interviewing Dr. Joseph Cerny for the University History series at Berkeley, California. This is session two, tape four, on March 14, 2014. So, we were talking about travel experiences during the 1960s that were interesting and had some impact on you. You went to Prague and you developed this friendship with this second cousin, your family developed this friendship with this second cousin and her family, and that lasted for twenty years.

04-00:00:55
Cerny: She was a second cousin, but she was like an aunt. The only other thing that came along that also was interesting is that at the last minute, in spring of ’68, I was asked to give a series of lectures in Tokyo for the radioisotope school, and the Japan Atomic Energy Research Institute had a series of radioisotope schools that were in Japanese. Once here in the spring, they would do it in English, and so they had to invite somebody. So, at the last minute, I got invited, and talked the department chair into letting me go for three weeks. Again, Barbara wanted to go. We weren’t paid—obviously they paid the expenses—but they gave me a round-trip first-class air ticket, which I cashed in for two tourists, and so we went that way, and we went on Japan Airlines. They knew that I wanted to see Japan, too, so I didn’t give a lecture every day for three weeks. I bunched it all up, and so we went to Kyoto and Nara first before we even started, and then I did these lectures, and then they took us around other parts of Japan. Toyota and other car companies were coming up; it still wasn’t on the horizon, and it was still the old Japan, a lot, and so it’s interesting to see that.

04-00:02:24
Burnett: Did you go back, many years later, to see it?

04-00:02:26
Cerny: Yeah, I’ve been back a number of times. In fact, my younger son married a Japanese girl.

04-00:02:34
Burnett: So, you’ve seen the difference, the rising industrial power of Japan?

04-00:02:35
Cerny: I’ve seen the difference, right.

04-00:02:46
Burnett: Another key influential period for you is getting a sabbatical year that you take in ’69 and ’70, and this is in Harwell, this is in Oxford, in England. Can you talk a little bit about the importance of that for your research?

04-00:03:07
Cerny: Well, it turned out to be very important, and so I did have a Guggenheim, which helped pay for the year. We went to Oxford. When I got there, I had to plan a research program. I didn’t go to work with anyone in particular, and so I said, “Well, what research program do I think I can do?” They had some Van de Graaff accelerators at Oxford, and there was this heavy ion cyclotron at Harwell, which was about forty or fifty miles away. I ended up setting up, and then it seemed like I could just be an independent agent. I set out two research groups, largely with visiting Canadians who were there. One of the groups was with the visiting Canadians and with a graduate student; the other group was with two really good physicists who’d gotten their degrees at Oxford, but who weren’t postdocs, were beyond postdocs, but were still not free agents. These latter two were David Scott and Alan Shotter.

So, one of the experiments I wanted to follow up on, it's a kind of nuclei like carbon-9, which you want to go way on up. They're in a chain, and you can go way up. I'd done a lot of work, up to the last nucleus you could get to, which was titanium-41. After that, you had to have heavy ions. So, I said, "Okay, we can get carbon ions out of this Harwell cyclotron, carbon ions, so in calcium-40, I can get up to iron by just having them go together, emit three neutrons, and I've got it." I want to look at the beta-delayed protons. I knew that enough in advance, I actually had gone out to Harwell, and a really nice physicist there had shown me around his equipment. He was leaving for his own sabbatical, he showed me around the equipment, said that we could use his equipment, do this, that, and the other, and that was nice.

So, then, I wanted to do this experiment (and the Canadians and the student wanted to help), so then I went to Ken Allen, who was the professor of nuclear physics running the Oxford nuclear physics laboratory. The other famous physicist there was Denys Wilkinson, who was "the professor of physics." We also talked to him, but Ken Allen was kind of running the place. To run at Harwell, you had to pay for it, so Ken Allen actually got me the money to run out there and pay for it. So, we went out to do this experiment, and it worked. I knew what we wanted to do, and we had these other four really good people. We were able to collide carbon-12 on calcium-40, make iron-49. It was a beta-delayed proton emitter, it was easy to see, published in *Physical Review Letters*. Ken Allen was really happy. I then needed some more money because I wanted to do the next one, the next one in the series. That would have been nickel-53.

Well, so we set out and we got an oxygen beam, and to get away from the backgrounds of everything, you've got to run two detectors, like always, you've got to prove they are protons and get rid of the electrons. In the first run, we saw a lot of stuff hitting just at our low limit of measuring things, that we were sure were protons, but the energy was way wrong. So, we then had one more run and went back, and got a little bit better detector. We could actually measure the energy of the proton and get its rough half-life. That was about when I was actually done with all this and coming back, and I gave a talk at Oxford. I remember first proving our data were right, and second, proving it couldn't happen. So, it was a nice seminar. So, we couldn't figure out what it was. Luckily, I had planned two experiments on the accelerators at the nuclear physics lab, and one of them was measuring gamma rays coming out of a related kind of reaction. In that experiment, which got finished while I was coming back, we got a clue what was happening. Peter Jackson was the dominant Canadian in both experiments.

The clue was there was this long-lived isomeric state in an iron-53 nucleus that was emitting gamma rays, and we could see those easily. But its counterpart, in the cobalt-53 adjacent nucleus, could actually be emitting. So, what it looked like is that instead of making the nickel-53, we'd made an excited state in cobalt-53 at 3 MeV excitation, that lived 200+ milliseconds. It was emitting gamma rays, but it was also emitting protons! If that was true, then this would be a new radioactive decay mode because this would be the single-step emission of a proton and like single-step emission of an alpha particle, and not a beta-delayed proton. So, when I got back to Berkeley, I had to find a completely different way to do it because we didn't have heavy ions yet. We did a complementary experiment, made it another way, proved everything, and published two papers back to back showing this.

04-00:08:37
Burnett: This is spontaneous proton emission? Direct proton emission?

04-00:08:44
Cerny: It's direct proton emission, yeah. The proton is being emitted directly. It's not beta-delayed. So, the group is sharp, and we were able to do several things to really prove it absolutely had to be coming from that. I even did a coincidence experiment to prove that it couldn't be a beta-delayed proton really fooling us. It didn't show up, and I'd already proved the equipment was working correctly. So, that was really nice, and so essentially, it was the fourth radioactive decay mode. That really was very good.

04-00:09:17
Burnett: So, there's alpha-decay, beta-decay, fission, and this.

04-00:09:25
Cerny: A colleague and critic of mine says, "Well, it's not much of a discovery. There's really no difference from alpha-decay." "Well, in the physics, they're no different from alpha-decay," he would say, but you could almost say the physics are not that different for spontaneous fission. [laughter] It's still a barrier penetration problem. So, that did work, and for a while, people thought you might find more examples of these isomers directly emitting protons. In reality, it took a very long time and they're really only low energy, very low energy things if it's happening, whereas this was a 1.6 MeV proton, it was of very substantial energy.

So, that year really paid off well with that, and the other experiment being able to help confirm what it was, and the second experiment, I also ran it at the nuclear physics lab, also ended up being published as a *Physical Review Letters*. So, it had become very productive, and all these really great people were there, and they were underutilized. They were all happy to work on this, and Ken Allen loved it because I would complain positively about various things—they're not happening here like they might in Berkeley!—which I'm sure they didn't like to hear.

04-00:10:50
Burnett: Can you tell us a little bit about that? This is not news about Oxford, right? I think it has a bit of a reputation across disciplines. Maybe it's not that way anymore. What was it about their research programs that kind of, I guess in your view, hindered creativity or hindered experimentation?

04-00:11:13
Cerny: Yeah, they just weren't imaginative. I think a lot of senior people really weren't doing very much anymore. There were a lot of talented people who weren't moving on to other jobs in industry or somewhere else in England. I just came along with these other ideas of things we could try to do, and they were willing to join in and do it. This Canadian group plus this British graduate student, Nicholas Jelley, that then actually came to be a postdoc with me and is now a full professor at Oxford—he did very well—I just happened to talk them into it. I was just struck that a lot of the things weren't being geared for being in a hurry, or, well, it'll be all right if we figure it out a year from now, or whatever. That certainly wasn't how I felt about it. If I'm not mistaken—and I'm not going to mention any names—two nuclear physicists I knew from the East Coast had more or less gone to Oxford for a year and hadn't done anything. They just, like, sat on their hands.

04-00:12:28
Burnett: It was kind of a sinecure or something? Like, you've been dubbed?

04-00:12:30
Cerny: Yeah, but they were on a sabbatical, and they just, instead of doing something, I couldn't figure out what they'd done.

04-00:12:40
Burnett: So, it wasn't just the people at Oxford, it was people coming into that Oxford orbit that were not either pushed or asked to do anything?

04-00:12:48
Cerny: Yeah, I guess. I just went in and got along well with Ken Allen. But I even complained—

04-00:12:56
Burnett: Did he talk about it? Was he very diplomatic? Did he ever talk about it?

04-00:13:00
Cerny: He was very diplomatic to them, but he made it clear to me that he appreciated my being there and stirring the pot a little bit. He was diplomatic.

04-00:13:11
Burnett: Sure, sure, but he made it clear that your observations had some validity, that there was something different in the culture there?

04-00:13:19
Cerny: Right.

04-00:13:20
Burnett: Nonetheless, you got something out of the people that you were in contact with. Did you get help with your ideas, or it was collaborative in that you can bounce ideas off of a group of people?

04-00:13:31
Cerny: Oh, yeah, these guys were smart. David Scott came to Berkeley, worked with Bernard Harvey, as kind of a senior postdoc. Was put on our staff, immediately—then put on the senior staff. Left LBL to go to Michigan State, as a named professor and ended up being the president of UMass Amherst.

Alan Shotter ended up, the professor in Edinburgh that came over here to run the big Canadian accelerator in Vancouver for five years, and has gone back.

04-00:14:36
Burnett: The moment you did, you attracted a bunch of people, or they came out of the woodwork?

04-00:14:39
Cerny: I just talked them into it, yeah. I knew I wanted to try to do the experiment at Harwell. Beyond that, then I started to think what you could do with the tandem, and then germanium detectors to measure gamma rays, and find some cute things that way. Nobody was doing anything like that, or I would have joined a group. So, it was very strange. That was really good talent. I remember the budgets were being cut, and I remember another physicist was really good. I remember from back here, writing him a letter, saying, "If we can help you out or you want to come here for a few years and then do something, let me know." He found a job. These were really good people.

04-00:15:21
Burnett: Well, you did mention money, and I think that would have been a kind of perennial crisis for Oxford and British universities in general. That might have affected the culture in that way. It was an amazing experience for you, and you made this pretty fundamental discovery. As I recall from what I read, the work wasn't finished by the time your sabbatical was over. You got a kind of start with it, and then you had to come back to Berkeley to complete a lot of the proofs with different experiments.

04-00:15:57
Cerny: This heavy ion experiment was complete there. That part of it was complete. So, what I wanted to do was find it another way, to make sure. So, in my case, it was doing a lower-yield experiment using protons on iron, but I was able to do it. So, well, once I'd read the letter from Oxford about the gamma rays, it gave us the clue. I remember I had just gotten the letter, my family was going up on a vacation in the mountains, and I just couldn't stop thinking about it. My wife said, "Look, let's go back to Berkeley," and we went back, I think we worked ten days straight, to put together the equipment to do the alternative experiment. Maybe we had a day off. I remember we had a brand-new postdoc, free from Canada, a really excellent guy, had a great career at LBL. At about the eighth day he said, "Joe, my wife isn't going to be able to take it if we're going to be working like this all the time." [laughter]

04-00:17:00
Burnett: Well, that maybe is a work culture of another kind.

04-00:17:05
Cerny: This is something I really wanted to prove now, right, to get it published. The experiments were over in England.

04-00:17:07
Burnett: Of course, there's an urgency. Apart from this one critic colleague of yours who said, "Well, it's another version of alpha-decay," right, is that what he said? Physically, apart from that, I imagine it was well received?

04-00:17:34
Cerny: Well, the discovery got me the Lawrence Award. So, by 1974, I had the Lawrence Award, which was for studies of nuclei on the edge of stability, instrumentation development, and discovery of proton radioactivity.

04-00:19:50
Burnett: At that point, you have this tremendous recognition from this, and the kudos, and it also results in your becoming, it's one of the things that contributes to an accelerated promotion to full professor. That's usually the moment in the academic story where someone decides to become a bit more Oxfordian, as you described, in this approach to research pace, perhaps? But this is not when you slow down. This is where we're starting to bleed into the beginnings of an administrative career, right? You assume directorship of the chemistry department at that point. Can you tell us a bit about your ongoing research in nuclear chemistry in the mid-1970s?

04-00:21:01
Cerny: So, I came back in '70, and I was going to mention it later, but I can mention it now. One of the things I got sucked off into doing was a four-volume series for Academic Press on nuclear spectroscopy and reaction so, that took four years, from '71 to '75. I don't know why I did it. There'd been a very important two-volume series published by them in the late fifties edited by Fay Ajzenberg-Selove, and she asked somebody at first, who wouldn't do it, and then she asked me. So, mine quickly expanded to four volumes, fifty-five authors—native English speakers, French, German, Swedish and Swiss.

04-00:21:42
Burnett: Herding.

04-00:21:43
Cerny: I had to correct many contributors' English.

04-00:21:46
Burnett: Herding academic cats.

04-00:21:47
Cerny: Right. Getting the manuscript in and doing galley proof and page proof, having the first type-setter go bankrupt. So, anyway, it took four years. It did come out and I was really glad I did it. We found other ways to study interesting nuclear reactions and look for different kinds of excited states.

04-00:24:22
Burnett: When you were talking about the explosive potential of the research that you were doing—the literally explosive potential of the research you were doing—at the Aberdeen, was it at Aberdeen?

04-00:24:36
Cerny: No, Picatinny Arsenal.

04-00:24:41
Burnett: It made me think about the nature of the work that you were doing over this time. In 1965, at MIT, there was a major fire when, I guess, one of the bubble chambers exploded. A number of people were burned, a graduate student had major burns on his body. So, I had a question about risk and safety. As it turns out, Berkeley's protocols were very important and had been followed for a very long time. They hadn't been implemented fully at MIT at this time. So, some of the stuff, some of the risk you experienced at Picatinny must have been a little bit familiar to you. These are dangerous pieces of equipment and dangerous materials to work with. Can you talk a little bit about the risk of some of the stuff that you were doing and how you managed that and how you worked through the procedures?

04-00:25:56
Cerny: You're doing beams on accelerators, they're very intense, and you don't want to be in the room with it. You're behind all these concrete blocks, and there are all kinds of safety devices. There's even a way, if something went wrong, you somehow are in there when the beams come on, there's various things you can punch to kill the beam. There's an escape way out the front and side of most caves. So, people knew from lots of years of this kind of stuff, you had to be safe. So, it wasn't new, like the bubble chambers.

04-00:26:56
Burnett: I was impressed with that when I visited the Advanced Light Source. In each of the stations, there's this set of cards, it has the range of emergencies, like radioactive problem and chemical spill, and it just goes through this long list. It occurred to me it's a very safe place because of those procedures that had been developed, but there is a lot of dangerous equipment and gases and things that are surrounding you as you work.

04-00:27:31
Cerny: There are people who want to work on radioactive targets, and you really have to have a lot of detail with that, and interlocks and things to prevent contamination. You're bombarding a target and you're going to do so in a vacuum, open it up, and then you don't want radioactivity all over the machine. Ages ago, Ghiorso had, I think, a curium fire. They had a radioactive target break at the SuperHILAC, and it got over part of the building. Then, you have to go paint the building down and really take care of it.

Coming back from my Guggenheim, I'll tell you about that trip. So, my wife's parents came over to London and took the two boys home, and she and I went around the world to the east. So, in late spring of ’70, we then went briefly to Iran, but for three weeks to India, and then Thailand and Hong Kong and Japan and back, which was nice. In India, it was the first of three trips we have taken there. They decided to build a copy of our cyclotron, as a sort of industrial development project. So, they built it outside of Calcutta, towards the east, in a swamp—nearly a swamp. It was sort of like a national demonstration project, so we had a lot of Indian physicists come and work at our eighty-eight, which was nice. That trip and other trips, I would go to Calcutta and give a talk and see how their cyclotron was coming along. So, that was really interesting. India was really fascinating, too. We went three times, two other times, with NSF help, so you’re giving lectures and things. It’s nice.

04-00:29:41
Burnett: You got to see and spend time with your family as well, with your wife, in this case, just going around the world. This is, of course, during a big development push. There’s a lot of economic development projects going on in a lot of these places. I think there was a proposal briefly for a nuclear research institute in Ceylon, which became Sri Lanka, in the late fifties. So, there was a lot of interest in nuclear energy as a development issue.

04-00:30:20
Cerny: They had a big power reactor complex down near Madras that one would always go to and see what that was doing. That was interesting. In Bombay, there’s the Tata Institute, so you fly to Bombay and you can see that. They have accelerators. I’ve done that a couple of times.

04-00:30:41
Burnett: You’ve seen most of the infrastructure that they had for nuclear research.

04-00:30:44
Cerny: Back then, right.

04-00:30:58
Burnett: With these experiments that you did, in general, what did they allow us to conclude about nuclear structure? When you discovered the direct proton emission, is it possible to generalize from this experiment?

04-00:31:21
Cerny: No, this one’s actually so interesting, it’s a highly angular momentum forbidden decay, but that you really have a theorist trying to calculate the improbability of it because it’s extremely inhibited. So, it’s kind of a miracle we saw it all, but basically, the proton escapes from the nucleus. Normally, the proton comes out with zero or one or two units of angular momentum, but it required nine. So, there’s a huge angular momentum barrier in addition to Coulomb barrier. In principle, you could see another state in the daughter, there could have been another proton group with less energy, but only needing seven units of angular momentum change. We did a couple of difficult experiments to see if we could see any hint of that, and we couldn’t. This

one's sort of like, just a fluke, but it's really there. When you're doing other experiments now, it's an unwanted background if you're doing a particular kind of experiment.

04-00:32:25
Burnett: You've discovered something that gets in the way. That's important, right? In a sense, it becomes a kind of noise that has to be isolated because that's what you do when you're doing all this detection work, is you're kind of isolating the background noise so that you can detect what you want to detect.

04-00:32:46
Cerny: Right. Well, these are real events coming in that you just wish weren't there because the experiment you're trying to do now is so much more difficult in its way than that one was, then.

04-00:33:04
Burnett: There's quantum tunneling involved? Could you talk a little bit about that? That's fascinating. What's interesting, perhaps, about it in the general sense is that you've really confirmed how strong these barriers are, or was that already well known, the kind of barrier to get out of that?

04-00:33:27
Cerny: I think that's well known now. One of the triumphs of quantum mechanics was that one could predict alpha-decay once you understood the quantum mechanics and how the alpha particle and heavy elements are actually bound. If you really do the quantum mechanics and the alpha particle has a wave function, and it hits this barrier and it decays away and away and away and then, finally, there's a little bit of wave function outside the barrier, and that's what permits the alpha particle to get out.

04-00:34:02
Burnett: It's much smaller. That was the same for direct proton emission?

04-00:34:05
Cerny: Right, that is the same.

Later on, then, in the early eighties, Goldanskii wrote another paper, and this paper said that it's possible to find beta-delayed two-proton emission. So, I was aware of that paper and had some really good graduate students, particularly Mike Cable to whom I assigned lead on this project. I said, "Okay, we can surely do this." So, here, it was really tricky because let's say the first nucleus Goldanskii said that might show this is aluminum-22. Well, aluminum-22 has five fewer neutrons than stable aluminum-27, so the first question is, what's the half-life of aluminum-22? Is it going to be more than a millisecond and beta-decay, or is it going to be 10^{-20} seconds and you're wasting your time?

So, the first thing we had to do, and the mass series predictions were ambivalent about whether it was bound or not, was say, "Okay, we've got to prove that aluminum-22 is bound." So, by then we had a set-up where we would bring out the beam from the cyclotron, bombard targets in helium and catch the recoils by putting an aerosol in the helium, so that the radioactivity you wanted would attach to that aerosol, and you could transport it very rapidly from this box it was in to a detector station. All of this stuff is in a concrete cave. Get it to the detector station, spray it on a rotating wheel, and let that wheel rotate around in front of your detectors. You watch it decay. You bombard it, run it out, and then you do that. So, we saw a lot of background from regular beta-delayed protonemitters, huge backgrounds. Nonetheless, luckily, the protons from aluminum-22 were more energetic, so we could see actually that aluminum-22 did emit very high-energy beta-delayed single protons, so that it had to be beta-decay.

So, then we set up special detectors for this, where you now had to have one detector divided in half, which is a delta-E detector, followed by an E-detector divided in half, so you can get signals from each side, so what you want to measure are two protons in coincidence coming from this decay. You're slowly rotating this wheel, have a couple of sets of these detectors. That's where we actually saw beta-delayed two-proton emission because we saw protons in coincidence and the energy matched out if you plotted it appropriately, it'd be perfect. So, we saw two cases like this. Aluminum-22 did it, and the next nucleus up, things kind of go in alpha particle chains. It's phosphorous-26. It did it, too, and both of these had half-lives of like, forty to ninety milliseconds, so it was short.

So, that was really nice, and I don't think anybody else in the world was trying it. So, we saw it, and in rapid time. That's why the 1984 American Chemical Society award for nuclear chemistry said we found proton radioactivity and beta-delayed two-proton radioactivity. We're still in light nuclei with aluminum and phosphorous, right, we just keep finding these new phenomena as you get to these really extreme conditions.

04-00:38:12
Burnett: Goldanksii has been a kind of touchstone for you, for much of your career, that you had this kind of, he would work out this theory and you would develop it and then do experimental work that confirmed or disproved it.

04-00:38:30
Cerny: Right. Also, earlier, when he predicted beta-delayed proton emission, that's when McGill physicists picked up on it and ran with it. I think that the guy running the McGill group actually had been in Moscow and had talked to him about it.

So, during this period, now you were also chair of the department, and you had those responsibilities.

04-00:39:20
Burnett: Did that change the amount of time you could devote to this kind of moving these research programs forward?

04-00:39:30
Cerny: Well, actually, at that time, I was an associate director at LBL and head of the Nuclear Science Division.

04-00:39:54
Burnett: Well, there's some interesting innovations that I read about. I'm backtracking a little bit, but the development of the helium jet to transport. With some of the older experiments, when one thought there was a much longer half-life for some of these particles, they had what were called runners? You'd physically transport these particles—like, a person would transport. Do I have that right?

04-00:40:29
Cerny: An example I know is an older colleague of mine who allegedly, when he was at Brookhaven, thought there would be beta-delayed proton emitters, and that he would run with them. But the half-lives are too short. They're 100-200 milliseconds. So, you really can't do it, and if you understand the basic nuclear theory at all, you'll know you won't try because they're too short.

04-00:40:53
Burnett: So, in terms of detection, what were some of the solutions that you had to transport these particles?

04-00:41:04
Cerny: That's when ages ago, we really got interested in these helium jet transport techniques. We did a number of things. Initially, if you looked for beta-delayed protons, you could bombard a target and you could be in your evacuated vacuum chamber. You could pulse the beam on the target, have detectors there, protected by a slotted rotating wheel, so the detector doesn't fry. Pulse it in sync, so that when the beam's off, this wheel's not in front of the detector pack, and you can measure protons. So, that's what we did. That's what we did to measure beta-delayed proton emitters without a helium jet. There were a whole bunch of experiments we wanted to do with the helium jets, so then we developed our own helium jet techniques. So, we had both experimental technologies.

04-00:44:01
Burnett: The circulation of experts, postdocs and scientists from around the world, did that continue through the 1970s in your work with these kinds of research projects? Was there a lot of circulation through, or was this with a group that you had built up gradually, over time, of students and junior faculty?

04-00:44:29
Cerny: No, I never had any junior faculty. I kept having really good US and European postdocs coming through, and then we had some really good Finnish postdocs who have gone on to very successful careers. One of them, Juho Äystö came in the late seventies and then came again. He was a postdoc and he was a

visiting professor, and he's very successful in Finland and in the European physics community. So, that really worked that way. My group just about died in about 1983, when two postdocs quit cold, and my really very good student who'd done the beta-delayed two-proton emission work decided he really wanted to go to Livermore and work on fusion. My group just about went through zero, and then I hired a Ph.D. student back who had worked with me, and then gone to Oak Ridge. This was Dennis Moltz and he essentially became a co-group leader, particularly when I was vice chancellor and dean and things like that.

04-00:45:41
Burnett: I was going to ask about that, about how—and we're getting ahead of ourselves a bit—you kept the research program going. That's a tremendous draw on your time and energies.

04-00:45:58
Cerny: Dennis is an excellent experimentalist. He knew what was going on and he knew how to run things. Then, I got some more Ph.D. students in. He came back on a gamble that he'd get a permanent position. I hired him and I made it clear to the senior staff, he's coming on a five-year term position, and one of the reasons is my group just up-out vanished. I didn't want to build up a big, permanent group at all, and actually, I was against it in principle. It was something a lot of people liked to try to do. Well, it'll be really nice, and then you suck off all your money on these career people. I didn't have a choice if I wanted to continue, virtually. He was very familiar still with everything because he'd just been at Oak Ridge for four years, or so.

04-00:47:06
Burnett: That's almost a philosophical approach to work that you have, is that when you did your sabbatical year, it was kind of an artifact of having one year to do something. This notion that you want a kind of agile work process, that you can steer the ship in different directions, and if you set up—what would have happened if you had done that, if you had tried to set up a kind of a Cerny institute within LBL?

04-00:47:44
Cerny: Fellow colleagues wanted to do it, and I remember as an associate director, saying, "We just can't do this. We can't afford it." I knew another nuclear chemistry professor had one when he was working there, when I was younger and more critical, and the guy he'd hired to work permanently with him wasn't very good. I had a colleague who completely agreed with me that he wasn't very good, but there he was. He was virtually a permanent member. Nobody hires assistant professors to work with—maybe you're stuck in high-energy physics somehow, but in what we do, you don't do that.

04-00:48:25
Burnett: During this time, of course, at Lawrence Berkeley Laboratory, there is this reorientation in the early seventies, that the laboratory as a whole tries to

expand out from its original mandate of working on, well, there's a medical division, there's a nuclear chemistry division, and then there's the physics division. The director tries to update, I guess, the mandate of LBL to environmental analysis, environmental research, a lot of different avenues of research. Part of that, I imagine, is to attract funding, to handle the reorientation of government funding to the labs. Nuclear chemistry – I'll pose this as a question – how did nuclear chemistry adapt to that climate in the 1970s, going on into the eighties? Or did it?

04-00:49:45
Cerny: Well, this will also show up when we talk about that. We changed from being the Nuclear Chemistry Division to being the Nuclear Science Division, so that we brought in two high-energy physicists into the division, and the people that were working on the BEVALAC. The atmosphere in the lab was different, and we can talk about it then, too. Basically, the lab still had three operating accelerators. They had an eighty-eight-inch cyclotron, you had the SuperHILAC, you had the BEVALAC, and you were busy. The government was still funding all this stuff. So, these other things were going on and could give you a different dynamics or not in the lab, but you had enough to do.

04-00:50:56
Burnett: This is expensive infrastructure, as well. The Advanced Light Source, today, this is one of them. It's $6,000 of electricity per hour, so these are impressive outlays, just in that one sense. So, you were recognized for these contributions. For direct proton emission, you get the Lawrence Award. Could you talk a little bit about that, and how did that feel?

04-00:51:36
Cerny: Well, it felt really good.

04-00:51:39
Burnett: That's an answer I would expect.

04-00:51:42
Cerny: It was really nice to be recognized. It's quite a formal ceremony. You go to Washington and your parents come up, and you give a little speech.

04-00:51:58
Burnett: How did your parents feel? They must have been very—

04-00:52:01
Cerny: They thought it was great.

04-00:52:04
Burnett: Yeah, I bet. Did that change things for you, in that you became a full professor? We'll talk about that in the next session, about the administrative changes, but did that change your outlook at all on the research that you were doing?

04-00:52:27
Cerny: No, it didn't really do anything. It was nice, right, but there's work to do. Well, Dave Shirley had gotten it two or three years earlier, too.

04-00:52:48
Burnett: He had been hired prior to you, and so I guess you were just a couple of years—

04-00:52:58
Cerny: He was two years older and four years ahead, somehow. [laughter]

04-00:53:09
Burnett: He was one of your colleagues, of course. Did you have a kind of cohort of people who were hired when you were hired, that you worked closely with? You said you were independent, and you had your own cave with your own graduate students and postdocs, and you were on your own track. As you said, some of this research, Goldanskii inspired you theoretically, but very few other nuclear chemists were doing the kind of work that you were doing. It's a kind of small group. Can you talk about your interactions with the other faculty, intellectually?

04-00:53:52
Cerny: Well, there were other international scientists working with helium jets. Dave Shirley was doing a whole bunch of experiments in physical and nuclear chemistry, and was out there, doing different things. I was really focused on just the nuclear part of stuff. So, we were very good friends. Basically, the early theoretical joint paper was important. Russian Goldanskii was important. A lot of these other ideas just came along. Some by luck or some by design.

04-00:54:45
Burnett: There was an international group of chemists.

04-00:54:49
Cerny: People were interested in this kind of work, yeah.

04-00:55:00
Burnett: You were in contact with them and you worked with them to talk about the design of your experiments. So, I guess apart from Goldanskii and the immediate group of engineers and technicians that you worked with, were there others who inspired you in your research work, or who were giving you, helping you with ideas and doing your experiments? Or were you, in a sense, doing them on your own?

04-00:55:42
Cerny: We were just doing them on our own and for the sixties, we just did a lot of stuff we haven't talked about. There were things we could do to help out another experiment by using our tricks or things like that.

04-00:55:58
Burnett: That's fascinating. So, like, in the ways that those physicists helped you, when you started, others might come to you and say, "How do you do this?" You can bring your experience to bear on their research.

04-00:56:18
Cerny: People were very interested in looking at certain kinds of resonance states, and the resonance states that people at the Van de Graaffs could make, they wanted to make a particular type of very highly excited state, which we'd found first, typically, by doing our experiments. We knew the state was there and we knew its properties, but we didn't know its property of how sharp it really was. It was more sharp than our resolution, so we didn't really know. A lot of people in the tandem Van de Graaffs wanted to try to find these states and see them as really sharp little resonances, as you varied the energy in a different scale. They couldn't find these, particularly a professor at Stanford couldn't find them. We said, "Okay, I know we can do a really difficult experiment where we can make this state, and then we can watch it decay with our detectors." So, we can figure out whether an excited state in magnesium-24 is decaying by emitting an alpha particle and going to neon-20, or is emitting a proton and going to sodium-23, or is it going to some excited state of something, so it'll make it hopeless? We did that. It was a very difficult experiment. We found the way you can get it into the state, and then we told him, and he successfully looked at this resonance. Of course, we published our results on this separately.

04-00:57:46
Burnett: When you're talking about excited states, you're talking about the nucleons that are kind of—

04-00:57:55
Cerny: Well, these are isospin-2 states. It's all very technical. They're very highly excited but they're very sharp resonances. Can we see them that way and how sharp are they? We can compare that to theory.

04-00:58:11
Burnett: This is enormously complicated, as I've been reading. So, there's a collaborative atmosphere, and this is what Seaborg said about nuclear chemistry, it's comparatively, as far as he could tell, a more collaborative kind of environment than, say, high-energy physics. Because you're working on something highly specialized, it was a relatively small group that dealt on the work that you were dealing with. There were these moments when you would be in communication with others who needed your assistant and needed your assistance, and that was just part of the environment.

04-00:58:58
Cerny: Yeah, just part of it. I think we kind of ran on our own. We had this Russian "bible" that came, that had a lot of stuff in it, and that gave you all the ideas to extend it. In the 2000s I met a guy who worked at the Kurchatov Institute in Moscow, who was Russian, and came over here, and he's at Texas A&M. I

met him, and he was saying, "We just waited for your papers to come out in *Phys Rev Letters*. We just wanted to know what you were doing next." Go back in the seventies—they couldn't compete and do it, but they were very interested in the light elements. Just because they didn't have the right accelerator. Not that they weren't smart, but they couldn't.

04-00:59:41
Burnett: In a sense, they were aware that this is an experimental program that follows out of a theoretical tradition, with Goldanskii and others, right? So, it's kind of part of a research community.

04-01:00:00
Cerny: There were a lot of heavy element people in Dubna, and there still are. With this group of light elements, the Kurchatov physicists that invited me to come to Tbilisi, but they just couldn't do these. So, they read these papaers, but they couldn't do the experiments. They just sort of saw me as working my way through the different ideas in this paper. You couldn't do them all at once.

[End of Interview]

Interview #3: April 3, 2014
[Begin Audio File 5]

05-00:00:09
Burnett: This is Paul Burnett of the Regional Oral History Office, interviewing Dr. Joseph Cerny for the University History Series. This is session three, tape five. Dr. Cerny, last time we were talking about your research career. We're going to backtrack a little bit and talk about the beginnings of your administrative career. In the early 1970s, you're an associate professor coming on to being a full professor. Can you tell us a little bit about your experiences in the early 1970s in the Chemistry Department?

05-00:00:51
Cerny: Yes, I'd be glad to do that. Actually, I became a full professor in 1971, and that was the year that David Shirley, my colleague, became department chair. We shared a lot of interest in improving the department. One of the great things that Dave did was he created a planning committee, which we had not had at all. So, this planning committee had a chair from our faculty, and really the heavy hitters from the different subfields of chemistry that represented the department. I was on this committee. Dave went as department chair all the time, kind of as an observer, as did the dean. This was really important because we needed to do some faculty hiring, and we didn't really have a planning process. So, he set that up and that worked really well, so after this committee got its recommendations, then you would go to the whole faculty that could vote appropriately, and you would vote on it. So, we weren't circumventing anything, but we were really doing planning, and since you had to have five-year plans, it also should tie into that.

Now, every so often I complain about our sister department, physics, and physics to this day may not have a good planning process, but perhaps it got better in the late part of the last century. There, they would really hire "the best possible person," regardless of field. So, what you end up with is, first place, a lot of political jockeying, and second place, you can end up with a very misaligned department where there are way too many people in one area and not enough in others, and they maintained that model a long time, I think to their detriment. So, we got away from that, and so that's something I'm going to continue when I become chair.

05-00:02:46
Burnett: That was a decision that was made in the early seventies, or this is part of the character difference between the Chemistry Department and the Physics Department of long standing?

05-00:02:57
Cerny: Dave Shirley created this planning committee, and then I as chair maintained it, and it has become our tradition. If anything, more people attend, perhaps *ex officio*, but it's very important and has been very important to us. It was really interesting that one really critical hire we wanted to do is we wanted to get an

outstanding person in molecular beam scattering. Yuan Lee had had graduated from Berkeley with a Ph.D. under Bruce Mahan, one of our important professors, had gone to a postdoc with Dudley Herschbach at Harvard, and gone to the University of Chicago, and had become a full professor in short order. So, we knew Yuan Lee was really, really good. We had, however, hired one of Herschbach's really smart graduate students, and he had come into our department as an assistant professor. He was a really nice guy, but he had attracted weak graduate students, and he just couldn't get his act together. We were really worried about our own internal conflict of interest. We thought it'd be great to hire Yuan Lee, but we can't really do that unless we release this assistant professor.

So, we actually gave this person a whole additional year beyond all the norms on the campus, by the approval of the budget committee, to do that. We did that and he still wouldn't have passed muster, so we let him go. Then, we hired Yuan Lee. Yuan came in 1974. I remember, he was an outstanding, brilliant scientist with somewhat limited resources, and at Berkeley and LBL—he was an outstanding, brilliant scientist, with semi-infinite resources. Which didn't hurt, so he came to our department, did outstanding work, and by 1986, had gotten the Nobel Prize, which we were all sure he would, which he shared with Dudley Herschbach at Harvard and a theorist in Canada. So, that really worked out. Yuan Lee got his Ph.D. at Berkeley. We have four Nobel laureates directly from our faculty, and he's the only one who didn't begin as an assistant professor in our department but that we raided. The other three—Seaborg, Calvin, and Giauque—had all started as assistant professors.

So, Dave had wanted to do that, we did it, and I think we did it in the fairest way possible. Another problem that was haunting the department was the preliminary exam must have dated from when G. N. Lewis was the dean. It was very difficult because in addition to talking about your research to this committee of four or five, you had to advance a major topic in an area not at all in your field, and then a minor topic in another field. The trouble was most of the graduate students could not do that. So, initially, David asked me to talk to a lot of graduate students, get ideas from them about what maybe we could do to improve things, and I did that and we didn't get anywhere. So, then Dave decided to appoint me as chair of a faculty-student committee to do it. We met and we finally restructured that oral exam to the way it is now.

So, again, you talk about your research, but then you're supposed to give a critical analysis ideally of a controversial paper published in the most recent literature that's really interesting—still not directly in your field—and then you attack or defend that. That's a lot easier to do because you don't have to do as much independent thinking through of something, but kind of analyze something. That's the model we use still today. As a faculty member, I'm not so happy with that, either, because a lot of these graduate students are just repeating the paper back, and they don't require enough independent thought for my taste. In any event, that got rid of the graduate student and the faculty dissatisfaction. We went on. By January 1975, I thought I was due for a two-quarter sabbatical. We decided to go to Canberra, Australia, which had an electrostatic Van de Graaff, but my wife, Barbara, and my sons, Keith and Mark, and I all decided to leave the US in January of 1975, and take three months to get to Australia. After spending some time in Europe, we then flew to Cairo, and as before, we saw the sights around Cairo—the pyramids, the sphinx, the Egyptian museum.

05-00:08:03
Burnett: You went back, you returned.

05-00:08:05
Cerny: Actually, I skipped a time we'd been there in between when the four of us went over Christmas while in Oxford, but this is the last time we went. We took a boat on an Egyptian cruise line called Eastmar. It was really nice because it was a funky boat, it was ship-worthy enough for the Nile, but it went about half the speed of all the other, more fancy boats bringing the people in from Europe, which I thought was more the Nile's pace anyway. So, that was really nice, and they would fill the ship, more or less. It didn't hold that many people. They would put on an interpreter for every nationality. We were the only English speakers, so the four of us had our own interpreter. We tended to use local guides, and so we started, then, in the boat at Karnak and Luxor, so you again spend time, can go and see the Valley of Kings and Queens, and then you know by then to say, "Come on, show us the Temples of the Nobles," because that's where you see all the things that give you the real life of the people, and not just the formal art of the leadership.

So, we did that, and it went kind of slow along the Nile, like everything was going on there. You went both directions, you went back north and then went south, because we're going to end at Aswan. But there are a number of Ptolemaic temples along the Nile, and that's the only way you can get to those, pretty much. So, you get to those on the boat, get out, have a nice tour. One of the ones—and I didn't look it up—is the one that has the famous painted view of the night sky that was done in 400 AD or less. Of course, the French took it and put it in Paris, and there's a copy. That was really great, and we'd done it before, but it worked well. My younger son Mark was eleven and was a pretty good chess player, and the guide liked to play chess, so they played a lot of chess while we went around the Nile.

05-00:09:57
Burnett: So, was this an opportunity to show your children, who were now old enough, to enjoy what you and your wife had enjoyed when you first visited? Is that kind of the motivation for this?

05-00:10:06
Cerny: Yes. Well, and we loved it. I'd go back to Egypt probably now, if it were safe. I wanted to, one more time, but not likely to do it right now. So, then to branch out, we decided that we would do our own trip to East Africa. So, we

flew from Cairo by way of Entebbe, Uganda, which at that time had Idi Aman in charge, killing everybody. The plane just landed. We were on the Egyptian Airlines, so I thought we'd get through all right, but I remember three worried-looking British gentlemen, young men, walking off the plane, who had to do business there. So, they did, so then we flew to Nairobi. A couple of days in Nairobi and we pre-rented a VW bus camper, so we did, like, eleven or twelve nights going around the game parks on our own, and just got provisioned up, and then we went around. You're near Kilimanjaro.

So, we're going in Kenya and Tanzania, so southern Kenya and northern Tanzania, and one night we camped on the bottom of Kilimanjaro. Then, we saw all the tourist sights on our own pace. Frequently, we were the only people in the campgrounds. I have many side anecdotes, but we probably don't want to take up the time.

05-00:11:31
Burnett: Just to clarify, you were able to enter the game parks with your own vehicle? I don't know if that's still possible. I think they take you, now, and you pay.

05-00:11:38
Cerny: That may not be possible. I don't know, but then, you could take yourself. We went to one famous site, Ngorongoro Crater, which is now a UNESCO site. So, this is a perfect caldera that is 2,000 feet deep and covers 100 square miles, and so the animals got trapped in it, millions of years ago. For that, you had to go down in a Jeep, you couldn't go down yourself. Other than that, we drove ourselves, and one night, they told us to stay in a gas station parking lot because the Masai were on the warpath. Another night, we were out in this park all by ourselves, you're not supposed to travel at night, so we hadn't. We'd gotten there in time. This other car came in and we're there, and the person gets out of the car with a rifle, and the second person gets out of the car with a machete, and I was thinking to myself, "Oh, God." The third guy got out in a park uniform, and so they went to look over at this other car that had come in and left us alone.

05-00:12:40
Burnett: So, this is in contrast to today's fashion of childrearing, which is to protect children to the nth degree. You had an adventure with your children. You took them out into potentially dangerous territory. It's a bold kind of travel plan.

05-00:13:03
Cerny: Well, the wild animals don't like to smell gasoline, so they'll leave you alone in your VW bus. We didn't do anything too dangerous, but we certainly were around by ourselves a lot, and every so often, we did wonder if we'd run out of gas, what we'd do, and things like that.

05-00:13:21
Burnett: Did you get advice from people or did you just gradually develop experience traveling in different countries where you didn't know the language? English is a common language.

05-00:13:30
Cerny: Well, they speak English—some people do. No, I'm sure we had some advice, but we got all our shots that we needed. We weren't that old, and we went. So, that really worked out well, and you're supposed to see the big five—lions, leopards, elephants, buffalos, and rhinos—which we did. The one that's hardest to see is a leopard, but we finally saw a leopard, so we could say that we'd done that.

05-00:14:00
Burnett: Do you have some photographs perhaps we could put in the oral history, at the back? We could put some slides or something.

05-00:14:07
Cerny: The twelve-year-old son Keith thought about where his mother took photos, and he actually wrote it up and it got published in the Sunday edition of the *Chronicle*, at some point. So, he did that, and I have a copy of it I just gave to him. He's supposed to show to his children, what he was at twelve. We'd taken the kids around a lot of places, so I think the younger one got a little upset because I was teasing him a little bit. He had to sleep, when you put the top of the camper up, he was sleeping up there and I was saying, "Well, I don't think a lion will jump up there." So, I probably shouldn't have done that.

Then, we went to India, and I was on a three-week exchange for the National Science Foundation. There's the United States-India exchange of scientists and engineers. We went to Bombay again, and I generally visited labs and gave seminars, talked about the cyclotron they were constructing in Calcutta, that I'll get back to.

05-00:15:21
Burnett: There was some question in the history of science about national cultures of science. Some sociologists of science go to different countries and they visit physics labs and they spend time, they notice that there are differences in social practices around the doing of physics, for example. It's common for American physicists not to sort of register this—there's just physics, you do physics, and there's nothing social about it. Just to ask the question, did you notice significant or interesting differences in how the science was worked out socially among the scientists? Was it more hierarchical in different countries, less hierarchical, more collaborative, less collaborative?

05-00:16:16
Cerny: Since I didn't really do any experiments in India, I can't answer. I've said earlier that Europe was more hierarchical than the United States, by far, but in India, I was just really kind of giving a seminar and meeting people. You could meet them all. As an aside, I noted earlier that I ran into a Russian

colleague when I went, who ultimately went to Texas A&M years, years later, and that's how I met him again. When I was in Russia, visiting the Kurchatov Institute, he said he was there and he had met me, but he wasn't allowed to speak to me. Only the boss could speak to me. So, he'd seen me in 1971 or '67, but actually, he couldn't. That was different.

05-00:17:00
Burnett: A visiting senior physicist to LBL would be taken around and there would be open opportunities for even graduate students to interact with this person?

05-00:17:09
Cerny: Sure. Yeah, it'd depend on what they want to do, or say he would be visiting or she would be visiting research groups, and then everybody'd be there, right. So, I don't really know. I'm sure India is hierarchical, but I didn't see it that much. So, an aside on the travel, once you're in Bombay, you really want to fly to Aurangabad and see Ajanta and Ellora. They really are wonderful. You have to fly into this town, it's 300 miles or something, and it's a day trip to the caves. These are the famous caves that were discovered in the early nineteenth century with cave paintings dating from around 200 BCE, with Buddhist art for several centuries.

That's very interesting. That's Ajanta, so that's a collection of caves, and it was discovered when British were out hunting and discovered a tiger coming out of a cave, and went into that cave and found them. The other one is called Ellora, and it is incredible. It is a whole bunch of temples carved out of the rock, and temple sixteen in Ellora is a giant temple carved inside-out. So, you start with this cliff and you go work your way down, and you build this incredible, huge, perfect temple. It's just absolutely worth seeing, so we did that because we'd done it once before. That was really nice.

So, then my Indian hosts made sure I was well taken care of, then we flew to Bangalore and gave talks. The people in Bangalore were really nice and they said, "We've got you booked, there's nothing to do here, we've got you a car and a driver for you and your two kids. You'll see these temples, come back in two days, and give your seminar." So, we did that. Then, we went to Madras and gave more seminars, and I knew somebody at Madras. Then, we went to Benares, which is the famous place on the Ganges.

05-00:19:07
Burnett: Yeah, with the festivals.

05-00:19:09
Cerny: It's just unbelievable—then, anyway.

05-00:19:12
Burnett: Were you there at a time when they had some of the festivals?

05-00:19:15
Cerny: No, but just anyway, it's like a medieval village, and near the Ganges. You're there and you don't know the protocols at all, and you look at the Ganges, and first there's a dead dog floating by, and then there's a dead cow floating by, and then there's a dead human floating by. Then, this woman, I remember, asked me for some money. I couldn't put it together fast enough because she wanted money to incinerate her husband on the funeral pyre, and had I had my senses collected, I would have given her some money, but I didn't. Then, we finally went up to Calcutta, which was building this copy of our cyclotron as this federal demonstration project. It was still being put together. So, we had lots of conversations about what they do, what the beam lines should be like, what electronics they needed. So, I was kind of a useful resource, and I saw a lot of people there. They were all building it together, and were just keen to do things.

It was in unbelievable condition. They were out virtually in a swamp, and the previous time, in 1970, I was there, there had just been the Bangladesh War of Independence, and all the refugees had come in, and so that slowed everything down. I can't remember which trip it was, but one of the two trips, people were stealing copper by going and cutting the power lines. But of course, they'd cut them hot and they would wear gloves, so they'd go up on two power lines and attempt to simultaneously cut it, or they'd kill themselves. So, they had problems we don't normally associate with our high-tech projects here.

05-00:20:55
Burnett: Was it a collaborative development project? Were there engineers from different countries helping, or was it Indian or Bangladeshi engineers who were helping?

05-00:21:06
Cerny: Calcutta was Indian, and so, well, they wanted them to do it largely on their own, but I'm sure they had people going over and giving advice, but the idea was that they were going to do the indigenous manufacturing. It was a conventional cyclotron like ours, but still to do the manufacturing to get it to work, and they did do that and it worked and it produced good physics. So, that was a nice visit. Then, we flew to Singapore for three nights, and then we were getting tired, so we skipped going to Jakarta and went to Bali for five nights. Then, just took in Bali, before it became so easy for everybody to get there, and then we flew to Sydney. Then, we got to Canberra, where I spent three months of the sabbatical.

05-00:21:57
Burnett: So, your second around the world trip, basically?

05-00:22:00
Cerny: Yes. So, Canberra was a relief. You fly into Sydney, and Canberra's the capital, and it's a nice, wonderful—back then; I'm sure it is now—modern

city. You went back thirty years in time, you could leave your front door unlocked, 100,000 civil servants.

05-00:22:23
Burnett: A quiet, sleepy, but big city.

05-00:22:27
Cerny: Had a nice group of physicists, and they were far more Americanized than European. The director of the lab actually was someone I knew fairly well, who worked with other groups at Berkeley. He was English, but he'd worked with other groups there, and so it was a nice, open lab. I had an experiment I wanted to do with their equipment on the neutron excess side of stability. People got very interested in oxygen-22. There are three stable oxygen isotopes: oxygen-16 is the most common, 17 and 18 with extra neutrons, and a big theory was, what was the real mass of oxygen-22? It was almost sure that it was going to beta-decay. There had been one measurement by a Russian group that really disagreed with the theory, and there were a couple of other theories. So, we had to do a pretty complicated experiment, we set up from scratch because they didn't have the necessary detectors.

Actually, I had a colleague that I knew well from Michigan State University, which has a cyclotron. He was Australian and he was there, too, visiting home for a quarter or a semester. So, we set it up. It worked after I left—it nearly worked, but it didn't get finished and published until later—and our number agreed with theory and didn't agree with the Russian work. We were happy, so that all worked really well. My family was so tired. I went to Melbourne to give a talk, and other than that, the lab director had a nice place on the coast, so we would go down and visit him weekends, and we didn't go anywhere except to Sydney, to see what Sydney looked like.

05-00:23:58
Burnett: This makes me think to ask this question about workflow and the burden of work when you're setting up these experiments. You mentioned elsewhere that in an earlier experiment, you had a student or a postdoc whose wife was worried about him because he was spending so much time – So, can you give us a picture of what the pace is like? When you finish this, you're exhausted. What is it like, setting up an experiment like this and running an experiment? What's your workday like?

05-00:24:35
Cerny: Well, the earlier example, I really had to set up a new system at Berkeley ASAP to see if I could prove something, and so we were working day and night. Normally, you just work the day. The other physicist that came from Michigan State and I both did sort of similar kinds of experiments, so we knew what we wanted. We put together a group at Canberra, and a whole bunch of physicists there helped us. We had to set up something they didn't have, but we knew what we wanted. I figured we probably would need detectors they didn't have, so I actually brought some detectors with us so we

could do the experiment. I'd calculated, I knew what the beam energy they had was and I knew how thick or thin the detectors we'd need to do the experiment. So, I did that.

05-00:25:19
Burnett: These are borrowed from Berkeley?

05-00:25:22
Cerny: Yeah, I'd borrow them from Berkeley.

05-00:25:25
Burnett: How big is a detector?

05-00:25:27
Cerny: Well, these are pretty small. They're like a dollar. The sensitivity part would be the inner third, but the whole detector would be like a silver dollar, or something.

05-00:25:37
Burnett: How expensive is this to manufacture?

05-00:25:39
Cerny: It'd be hard to know—a couple thousand dollars.

05-00:25:44
Burnett: That's part of a much larger apparatus?

05-00:25:46
Cerny: Right, much, and we had a lot of accelerator time, and that's what's really expensive. I just didn't think they'd have them, and they didn't, or I'd been in correspondence with them and we knew they didn't. Yeah, so that worked out and I was pleased with that.

05-00:26:02
Burnett: Great. Did you maintain contacts with these folks over the years?

05-00:26:12
Cerny: Yes, the lab director comes through here and we've seen him, and another physicist works with colleagues at LBL so I've seen him regularly. Most of the others we worked with weren't permanent, they were kind of postdocs or whatever, so they'd floated off. So, we didn't maintain that. It was a very nice experience. To return, since we now had rested up, we thought we'd go to New Zealand for eleven days. So, we went to New Zealand and started on the South Island, and went up to the North Island, and it's just beautiful. Just beautiful. Then, we flew to Fiji, and then came home.

05-00:26:53
Burnett: So, you have seen a lot in these trips.

05-00:26:56
Cerny: That's the only trip I've done like that. It was sort of helped out for finances by the National Science Foundation giving me a travel grant, and I'd won the Lawrence Award, which gave me some cash, and I spent that on the trip. [laughter]

05-00:27:15
Burnett: Well spent.

05-00:27:15
Cerny: Right, well spent. So, while I was there, I got a letter asking me if I'd be department chair. So, upon my return, I was doing that.

05-00:27:29
Burnett: You were to assume those duties.

05-00:27:31
Cerny: I think I arrived June 30 and started July 1. Now, we'll transition to my being the department chair for four years, starting July 1, '75. In thinking about it in abstract before coming back, I knew there were two things that I wanted to do. One, I certainly wanted to maintain the concept of the Department Planning Committee, and I set my own committee up because most of the people stayed on for the four years, but then I thought we should rotate it around. So, I did that. Secondly, and it's a point I'm going to return to but I'm going to do things in a different order, of crisis order, I knew we badly needed to hire a woman on our faculty because Berkeley had never had a woman in a tenure track position, even, on its chemistry faculty. We'd had a few lecturers.

05-00:28:32
Burnett: In the report, they had said that they had tried three times. I don't know what that means—did they make offers to candidates who were women who then turned them down?

05-00:28:45
Cerny: I don't know whose report that was.

05-00:28:47
Burnett: That was a claim in the '75 – I think.

05-00:28:54
Cerny: I could be wrong, but I don't remember any department meeting. Conceivably, we made offers to assistant professors, maybe that's what we did, and they got turned down. We may have done that, but no, they didn't accept.

05-00:29:31
Burnett: There were no women on the chemistry faculty, although offers had been made to three candidates between 1972 and 1976.

05-00:29:45
Cerny: Okay. I know we weren't successful, and I remember in my first year of being chairman, at some point, *Chemical Engineering News* had a heyday listing us

as the largest department without a woman in the United States, with fifty faculty.

05-00:30:03
Burnett: It was something that was being discussed, and this was something in the air at the time, of course. That was a priority that you wanted to manage right off the bat.

05-00:30:17
Cerny: Right off the bat, I started working on that with the planning committee, and saying we just had to do something about this. When I arrive at Berkeley and go to the chairman's desk, I find a notice that we're going to have the first ever review of the Chemistry Department, which is fine, by the graduate dean. Which was supposed to start in the fall, that fall of '75, but the person who was going to be chair got recruited somewhere else and the review became delayed for a year.

More disturbing was I had a memorandum signed by 155 of the 300 chemistry graduate students, saying they'd lost faith and confidence in the department. I'm sure they could have had more votes than that because they didn't pick up many of the graduate students on the Hill who were with faculty on the Hill. They're in the 300, but I don't think they circulated the petition up there. I thought that was pretty bad, and, well, I was gone, I'd been gone for six months. I believed it was triggered over an organic chemistry junior faculty member who'd been let go, and his students had been more or less abandoned by everyone, but I don't really know. I think it was also a concern over jobs, concern over condescending faculty, and total lack of communication within the department. I have a memento from that time that I have to show you.

05-00:32:19
Burnett: Okay, can we get this on camera?

05-00:32:21
Cerny: We can get it on camera because we're going to return to it, perhaps at the end of this talk today or not.

05-00:32:27
Burnett: Okay, let's see if I can zoom in tight, here.

05-00:32:30
Cerny: You can see this again, but this, I got from that period. So, this is Ph.D. production in science and engineering.

05-00:32:40
Burnett: It's not focusing; I'm going to pull back a little bit. There we go. Ph.D., and there's a curve.

05-00:32:46
Cerny: Right, and it's going up then, and you see what it's really doing is falling off. So, this is the dramatic drop in Ph.D. production, about 1971, in science and

engineering. These students had come into graduate school in that period, and now that the job prospects weren't very good from their point of view—

05-00:33:08
Burnett: Across the board there was this kind of great recession in hiring in the early 1970s, in the American university. There'd been this massive expansion and these massive cohorts of graduate students coming in. Also, in the larger context, there's a question of labor grievances, right? Perhaps the very beginnings of graduate students, I don't know, maybe you can confirm or deny this, but graduate students beginning to see themselves as kind of workers as opposed to pre-professional scholars.

05-00:33:52
Cerny: Well, I think in part it was really concern about the job market. If we get to it today, but if we don't, I have data that I actually show. There's this huge drop, and the sad thing is that the white males left, never to return. So, they went away and it went down to some new equilibrium level, and luckily, women came in to want to do science and engineering, and a lot of international students came in. But the white males left, and a lot of them just clearly dropped out of graduate school. Part of this was all the intricacies of the Vietnam War and the draft, and then around '68 or so, one switched to a lottery system, sort of, and then it all faded out. So, I think a number of people may have gotten to the programs and decided, "Okay, I'm not going to be drafted, I don't care about this."

There's a dramatic drop in physical science and in engineering, and I've got data from this report from '79, looking backwards. Biology went up and plateaued, and biology has really grown after that, enormously. Social science actually went up. So, what I did to try to deal with the graduate student unrest was I wrote a letter to each of the 300 graduate students and asked them to tell me what their problems were. Then, I got an enormous list of problems, and then I thought about all the problems and tried to figure out what were most important.

05-00:35:43
Burnett: Could you give a sample of some of the types of problems, the themes?

05-00:35:46
Cerny: It was really mistreatment by the professors, plus lack of communication in the department on what's really going on here. Why don't we really know anything about anything? I'm sorry I didn't save my notes for that because ultimately, lack of space, poor treatment by faculty, uncertain funding, whatever, so I ultimately had a two hour seminar in our largest seminar room—I must have had 200 graduate students come. I had a good friend named Herb Strauss, who was my assistant chair for physical chemistry, come sit there as the only other faculty member in case it became a mob. So, I really went through everything—from the fire extinguishers don't work, the roof's

leaking, finances, this that and the other, there's no maintenance—I can't remember them all, but it was just almost everything under the sun.

05-00:36:53
Burnett: The report that you prepared for the provost in November of '75—

05-00:37:04
Cerny: I did it in early fall.

05-00:37:07
Burnett: This is prior to this coming down?

05-00:37:11
Cerny: No, it came. I had in July 1, '75, and I had my meeting early in the fall quarter.

05-00:37:22
Burnett: One of your suggestions—and this is an early report about problems—you recommend decreasing the limitations on graduate student enrollment and suggest a 10 percent increase in enrollments to cover—that's purely in terms of the teaching requirements, basically let more graduate students in. There was difficulty retaining technical research support staff, that was an issue, for the set up and maintenance of the sophisticated equipment. And the postdocs needed to be treated better by the university. They were being treated like staff. As far as the graduate students, there was a recognition of a slump in funding for some areas, but an increase in funding in others. So, the NIH is coming in with funding for health sciences and chemistry is benefiting from that, is kind of riding that, as well, and in areas of energy conservation and environmental research. But in these other traditional areas, there may have been discontent. So, that was kind of a report for the university as a whole, but you hadn't had time, really, at this point, to even deal with the total nature of the responses. At what point had you gotten back the survey from the students?

05-00:38:55
Cerny: I got the surveys back right away. Some of those things were probably mentioned, particularly not enough technical support for the equipment, so when they wanted to analyze their experiments, they couldn't. Things like that. We'll return to that 10 percent, that's interesting, because that's something I did do. I think they just wanted to be heard, and there wasn't any avenue to be heard. So, after this meeting, which would have been no later than October because we were on the quarter system, then the next summer, I collected a survey again and again held an open seminar. By then, some of the comments had become foolish, and so I decided that we could go on and not worry about it.

05-00:39:49
Burnett: Just for some context, this is a massive department. From the 1977 report, 51 regular faculty, 280 graduate students, 90 postdocs. This is right at sort of the preamble to this report, I'm maybe getting ahead of myself here, but this is compiled by the chairman of the Graduate Council, Nadine Lambert: "Heavy

emphasis on research indoctrination and a concomitant lack of focus upon the graduate, himself or herself, as a student, as a prospective possible teacher, and as an individual."

05-00:40:38
Cerny: This is from the review committee?

05-00:40:42
Burnett: This is the review by the Graduate Council of the Department of Chemistry, June 13, 1977.

05-00:40:50
Cerny: Yes, so that's what happened '76-'77, and she was the head of the Graduate Council. The Graduate Council runs the reviews, and so you ultimately go back to the Graduate Council and discuss the review with them, which is what I would be getting to next. I've got to admit, I don't have all this resource base. [laughter]

05-00:41:14
Burnett: I can make them available to you. [laughter]

05-00:41:17
Cerny: That's all right, I have enough nightmares. Yeah, so one thing leads directly into the other. We did the review in '76-'77, and its chair Ian Carmichael came over early to meet me. He's a truly distinguished geologist, and he's great.

05-00:41:42
Burnett: This is the first time you've worked with him?

05-00:41:44
Cerny: Yeah, even met him. He came to my office, and he's only six years older than I am—unfortunately, he's died—I was sitting at my office, and I had actually looked up all these people up who were going to be on this committee. Just to see who the hell was coming to review my department. So, in the middle of this, as Ian was sitting there, I said, "You know, I've looked you all up." [laughter] He loved it, he just loved it.

05-00:42:17
Burnett: So, he has a sense of humor, right? Everyone else, did they laugh as well?

05-00:42:22
Cerny: This is just him and me, just the two of us. He was the chair. He was an associate dean of the Graduate Division, and the dean, Sandy Elberg, wanted him to chair this review because there had never been one, and chemistry's a famous department. He thought that was great, and so we agreed, rather than have the review process drag out for years, we would do it all, in a year. We would both work very hard. He did, and I made sure people cooperated. So, you had all this written material, you had the faculty, this, that, and the other. Ian had this committee, a lot of Berkeley faculty and a couple of outside

professors would come for the big things. He interviewed the senior faculty, the associate professors, the assistant professors, the graduate students, and the undergraduates, all separately.

05-00:43L15
Burnett: For the graduate students, it must have been in focus groups or something. Invited to come to a town hall kind of thing.

05-00:43:25
Cerny: Right, they did that, and anyone who wants to complain tends to go. So, they did all that, and the review committee put together a big report that I agreed with. Now that you mention it, I actually got to know Nadine Lambert very well later on. The report was really good because it highlighted a lot of the problems we had.

05-00:43:51
Burnett: Just to clarify, the Graduate Council is the graduate student union, basically?

05-00:43:57
Cerny: No; the Graduate Council's a key standing committee of the Academic Senate, and it's responsible for all graduate degrees, everything about graduate students. You'll no doubt hear more when we talk about my being graduate dean, so the dean works with the Graduate Council and is actually an *ex officio* member. So, I would go to every one of their meetings. Actually, the dean is an *ex officio* voting member. Not that I ever disagreed with them, I think. My associate dean was also there. Ultimately, they're also responsible for the reviews, and then they report to the Academic Senate, and if the department did really terribly, then the administration would hear about it and something would have to happen. So, all these reports do go to the administration.

So, it's definitely a very important Academic Senate committee, and everything about graduate students, all of them except the law school, is under their control and the dean's control. There's some argument from the law school that they're independent, and I got tired of fighting with the law school deans. But there are graduate groups—graduate groups are Ph.D. programs that are put together from different departments and they can give degrees. Well, the law school has some graduate groups, we all agreed they belong to me, when I was dean, not the dean of the law school. Returning to the Graduate Council report on Chemistry, since it turned out well, the first thing I did is I actually asked Dean Elberg for 10 percent more graduate students, and got them.

Now, unlike many, many places, Berkeley has had a controlled level of graduates, total graduate students. I think a lot of schools take as many as they want, whatever, but in fact, we don't. We used to have a deal with the City of Berkeley about how many graduate students there would be and how many undergraduates. So, there used to be 9,000 graduate students, (not including the law school) master's and Ph.D.s. So, the graduate dean controls that number, and you can only make admissions up to a total of, in this case, a working population of 330 in Chemistry. So, you get a quota based on the response rate.

Another big issue from the review was poor communication. So, I decided that we needed some kind of internal communication, and I asked Dean Elberg for a half-time editor/writer, and I got the money. So, I then had this position report to me, and we hired an editor/writer, and this person was going to produce two publications. One publication was called *Dimensions*, and it came out about four times a year and it covered internal things that are going on in the college and the Department of Chemistry, and that way, you could learn what the faculty were doing, and graduate students were doing. Another one called *Potentials*, because I thought, well, we should send something to our alumni, too, in case they felt like donating some money to us. So, that maybe came out two or three times a year. I have some copies at home, so I was looking at those. That was really fun, interviewing for this position I even had two ex-*Ramparts* editors, which had just collapsed in San Francisco, come interview for this job.

05-00:48:29
Burnett: Interesting, that would have been interesting.

05-00:48:33
Cerny: It was, but we ended up hiring someone else. They were a little high-powered, it was a half-time job, but we interviewed them honestly. We hired someone else. So, I set this up and at this point, I learned that the campus had no central fundraising at all. We had an alumni association that raised scholarship funds for undergraduates, and that was more or less it. Little parts of the university did a little bit of fundraising, there was no central anything. It was a disaster.

05-00:49:11
Burnett: You sound surprised.

05-00:49:12
Cerny: I was surprised because I wanted to write all our Ph.D. alums and say, "Would you like to give us a little bit of money to help out?" I did write a number of campus offices, but first they said, "Well, you have to go check with this group." Well, I checked with this group, and this group says, "As long as you don't ask the undergraduates, I don't care what you do." I said, "Okay, I wasn't planning to write the undergraduates, who'd got their undergraduate degrees." That all worked out. I got a little bit of money. Chancellor Heyman became chancellor in 1980, and he knew we were in real trouble, and so he changed all that. I'm sure he talks about it in his oral history. He set up the first capital campaign, from 1984 to 1990, which was successful. But he really saved us—we were going downhill, it was unbelievable, a major university of Berkeley's caliber didn't have a central [fundraising] thing at all? He had a great amount of difficulty dealing with the alumni association, who wanted to

keep it for themselves, he had to set up a Berkeley foundation, and anyway, I'm sure that's in his oral history. I truly appreciated that, and later on, since he ended up appointing me to my position, I thanked him for doing that. He saved us.

05-00:50:23
Burnett: Did you think that, in the 1970s, that was a result of expanding costs of operations or was it declining funding from the state?

05-00:50:37
Cerny: I think it's just not having your act together and imagining the state's going to take care of you. I'm sure UCLA must have raised money from 1925, right? You go down to UCLA in the late nineties and you walk through their buildings, almost the little sprinklers have got somebody's name on them. So, they got ahead of it. I can't imagine why Berkeley didn't do it.

05-00:51:01
Burnett: Well, Berkeley had special kinds of favored sponsorship, so for example, when Seaborg asked for your eighty-eight-inch cyclotron—he never asked for it, in fact. He was invited to speak for a Senate subcommittee and they said, "Dr. Seaborg, is there anything the University of California needs?" He said, "Well, as a matter of fact, yes," and that was kind of the climate in the 1950s. Fast-forward twenty years, and it's inflation and stagflation, and a whole different socioeconomic climate.

05-00:51:35
Cerny: Yeah, so we got that. There was LBL, but we weren't raising any money for everything we do now, right? We needed to do that to build new buildings. When you look at this place, we were stagnating, and we were stagnating in the seventies and trying to survive all the trouble of the sixties and the early seventies, and then by the time Heyman got us geared up and some lucky things happened, we've had a building boom since the early 1980s. It's been unbelievable; it has been nonstop. Major projects. Anyway, it was just a vacuum. So, I tried that, and that worked.

05-00:52:25
Burnett: The criticism or the claim that was made by the students was lack of communication in the department, and the solution was to have a newsletter published four times a year. Were there other things that were done? It was perhaps something to say to the whole community, there's a problem with communication, and at a micro-level, did that change things? Did faculty respond?

05-00:52:52
Cerny: Well, I think the fact that I'd showed I was available as chair, by writing everybody twice, had to be a big help. Presumably, when they told me about things like the instruments aren't being fixed fast enough or whatever, we solved those problems. The job market must have improved. There was just hardly any communication. We have some now because of these things that I

actually did, the *Potentials* and *Dimensions*, which actually lasted to the late eighties. But then when Heyman really started that we had to raise money, then it got the deans energized, and then these publications disappeared, so the College of Chemistry started putting out its publications. It lasted a decade to get the word out. I don't know—the chemistry faculty gets together for lunch on Mondays, and it historically has done that at the Faculty Club. The trouble is it's gotten poorly attended.

The four years I attended the Monday lunch as chair, I actually tried to make it interesting, so almost every time, I would have something to talk about. So, I think it also helps. I remember writing all the faculty and saying, "Okay, tell me how many of your graduate students have got jobs and where are the jobs?" So, then I would give a report to all the faculty about that, or then I got the information, well, what is the real information on time-to-degree and completion rate, and got that out. We don't have a mandatory amount of time, but that's when I learned that the time to the Ph.D. in chemistry is 5.3 years, and it's been 5.3 years since 1930 or '40, more or less. When I was graduate dean, I didn't have these data, but UCLA had some interesting data. So, I think just kind of showing that somebody cared.

Not just me—I had some very proactive, vice-chairs for physical and organic who realized we had problems, too. So, they were working on it, and we just had an ethos of, all right, let's try to communicate, let's try to fix all this stuff. There's this list of things that the students were unhappy about, many of which are reasonable, and so you go with it.

05-00:56:12
Burnett: I think there's also perhaps a certain amount of anxiety that was provoked by events. As you said, there were perhaps some precipitating crises that were localized.

05-00:56:23
Cerny: I think there was, but I think also the review helped because it came at just the right time, too, to then interview everybody. It was clear the committee was there reviewing the department, and you had your opportunity to complain, as a graduate student or undergraduate, whatever, junior faculty. So, I think it was probably very fortuitous it was that year, but it was probably perfect to also help.

05-00:56:48
Burnett: Departmental reviews are regular at Berkeley, every five years or every ten years?

05-00:56:51
Cerny: They're supposed to be. It's too many departments to do it that often, but maybe every seven or eight. You're supposed to do it every five, but there are eighty Ph.D.-granting departments, and more than that in small graduate groups.

05-00:57:09
Burnett: When was the next review of the Chemistry Department after '77?

05-00:57:14
Cerny: Well, there have been some. I just can't tell you.

05-00:57:19
Burnett: It would be interesting to know if they had addressed if their graduate students felt the climate had improved.

05-00:57:28
Cerny: Well, hopefully they're not still there. [laughter]

05-00:57:31
Burnett: Yes, definitely not the same graduate students.

05-00:57:32
Cerny: Although, I can tell you when I'm the graduate dean, when I did visits to departments, I went to the Sociology Department and came back ten years later, and one of the graduate students was still there.

05-00:57:41
Burnett: That does happen sometimes in some disciplines, yeah, absolutely.

05-00:57:48
Cerny: Yes, I'm not saying I totally changed the department, but all these things working together changed it.

05-00:57:54
Burnett: Changed the culture a little bit, helped?

05-00:57:56
Cerny: I think getting jobs, seeing the situation wasn't as dire as people were imagining, I'm sure if the equipment was broken, there weren't enough technicians to get it solved, then it probably hadn't been forcefully brought to the attention of the dean. The dean is kind of, in the College of Chemistry, responsible more for those kinds of things.

05-00:58:16
Burnett: Perhaps we should stop at this moment and change the tape, and come back.

[End Audio File 5]

[Begin Audio File 6]

06-00:00:07
Burnett: This is Paul Burnett of the Regional Oral History Office, and this is an interview with Dr. Joseph Cerny. This is interview session three, tape six. So, Dr. Cerny, we just finished talking about the review of the Chemistry

Department and the efforts you made to get information from the graduate students about what they were unhappy about, and the measures you took to make improvements in the department. Given the department size and degree of specialization, how do you improve? Is there an inevitable problem of communication with a program that size? The newsletter is obviously a solution to that, but was that a problem prior to their complaints? Is that something that you experienced when you were there? Was there a degree of specialization when you were a student in the program?

06-00:01:30
Cerny: There is a specialization, obviously, from what you're doing for research. You get specialized that way. I think the department, I think it used to be more cohesive, and it used to be—and it faded out in the sixties—that on campus, there was this main seminar on Thursday afternoons, and actually, all the chemistry faculty were expected to go. You would just hear about what, generally speaking, other faculty were doing in your department. Physics has maintained that, but chemistry, it just really went away in the sixties. So, there is this chemistry lunch that all the faculty are supposed to go to, and maybe thirty go out of fifty, and it's a mixture. So, that's fine, and separately, the organic chemists have another lunch some other day of the week, and the inorganics, and the physical. We try to do that.

06-00:02:31
Burnett: Right, so there are specialized groups that have their own meetings.

06-00:02:40
Cerny: Because I do my research at LBL and because ultimately, I spent fifteen years in the central administration, I sort of got more remote from the faculty than if you were talking to someone who'd been there all the time. It's just impossible to do that. Particularly this professor from England who was on our faculty thought we really should have something like you would have at Oxford, where you can hardly get any work done at Oxford because you have morning tea, and then you have to go to lunch, and then you have afternoon tea.

06-00:03:16
Burnett: So, you were not impressed by that?

06-00:03:17
Cerny: It's not possible to get any work done!

06-00:03:19
Burnett: So, you were not impressed by the sort over-socializing aspect?

06-00:03:24
Cerny: I went to morning tea or coffee, but I skipped the afternoon, and we had a quick lunch. [laughter] So, there's a balance here. One of my main concerns was successful faculty hiring of a woman, and the tactic I took was to work with this planning committee and said, "Okay, we've really got to solve this

problem, and I want you to talk to people and suggest to me role models, and let's start having some role models come in and give our key seminars in whatever field they happen to be." So, we started a practice of doing that, and we had a number of people come through. Also, of course, the number of female grad students was slowly increasing in the department as time passed. Chemistry's done pretty well—okay, unlike physics, which seems to be really stuck.

By my third year, we had a truly outstanding candidate, Judy Klinman, who was a biophysical chemist, and we had her come out and give a talk. The planning committee finally agreed that she met our standards. So, we then sent it to the department, and the department voted on it. We actually made her an offer at tenure. Even then, the rear guard, various places on the Berkeley campus, tried for a while to make it be an adjunct associate professor, which is not tenured but can become tenured, which isn't what I or we wanted. So, we ultimately hired Judy Klinman in '77-'78, as a full-fledged, tenured associate professor. That was really great, and it opened the paths—and they were going to open anyway—and we have a respectable number of women on our faculty.

As part of this hiring, then, I actually hired three more people in these four years at tenure, so the other three were as full professors. We hired Robert Bergman and Earl Muetterties, both came to Berkeley as senior inorganic chemists, and Sung-Hou Kim came as a professor of biophysical chemistry. Muetterties actually came as a member of the National Academy, but the other three all got into it, so I had a perfect record for hiring people who ultimately get elected to the National Academy of Sciences.

06-00:06:21
Burnett: There's something also that's extraordinary about these hires because the pattern of hiring at UCB Chemistry is through the ranks—you hire an assistant professor, you get someone green and who's shown their potential, and then you promote them through the ranks. It's too expensive and you're not Harvard or Princeton, you don't have those resources. They tend to cherry-pick senior people. These are senior hires, are they not? That's pretty unusual, isn't it, for Berkeley?

06-00:06:56
Cerny: Well, it was. We really like to hire assistant professors and work their way up, but particularly in biophysical chemistry, we really didn't even have any junior faculty. So, you really need some full professors to help you develop, and that's kind of why we did it then. We were also a little weak in the inorganic side. Although now, we hire a reasonable number of people as full professors, and we still have quite a number of assistant professors. I agree, most do it through the ranks. In the four years that I was chair, I also hired seven assistant professors, but the ratio was a little high with the four, I agree.

06-00:07:50
Burnett: One of the extraordinary achievements of your time is that you did so much hiring. That's not easy to do. This is a very long process, a lot of meetings, a lot of sifting through applications and herding the cats of the committees. So, that's seven assistants—

06-00:08:21
Cerny: One associate at tenure, and three at full professor.

06-00:08:26
Burnett: So, eleven hires in four years. That's a lot.

06-00:08:30
Cerny: Prop Thirteen kicked in, in July 1, 1978, and we still hired. Well, I lucked out—we had a really good case, and at one time I was told—I can't believe that it's true—that we hired four of the six people at tenure campus-wide during those four years, but that cannot possibly be true. These were really good people, and the chair doesn't have to find the money. The way the College of Chemistry is set up, the dean has to find the money and the space. The chairs of chemistry and chemical engineering deal with the faculty, they deal with the undergraduate program and the graduate program, but the dean is responsible for space and resources.

06-00:09:17
Burnett: But you are the person who pushes? You have to put in the requests?

06-00:09:27
Cerny: Oh, you have to put in the requests and you have to get the faculty to vote and you have to write the case, and you have to convince these people to come.

06-00:09:35
Burnett: Can you tell me how you did that? You have to justify and get buy-in from people at a time when there are fiscal concerns. How did you sell it? Is it case by case?

06-00:09:55
Cerny: Well, it turned out that Muetterties and Bergman wanted to come together, and so, that sort of really worked so that we could do that. There was a substantial number of physical chemists who'd become biophysical chemists who really felt this was a new field and we needed to be in it. Some of the older chemistry faculty, physical chemistry faculty, didn't believe that at all. But we were very strong in physical chemistry and that's where chemistry made its name. So, nobody was really worried about that, and we thought we ought to go into these other areas. Yuan Lee was a physical chemist, but we knew what we wanted and we made the bid when we had to do it, when we felt we could ethically do it, and he wanted to come back. It went actually pretty smoothly, and I also think that I looked like somebody you can trust.

06-00:11:04
Burnett: We'll get a close-up here.

06-00:11:10
Cerny: The dean I was working with, Norman Phillips, was very supportive of all of this, and he, of course, had to look like somebody that you could trust because he had to really provide the resources.

06-00:11:28
Burnett: I guess there was talk at the time about how, your original report before all the stuff went down with the graduate students, when you reported on the state of the Chemistry Department, one of the things you wrote was that in the foreseeable future, chemistry is going to be in incredible demand because one of the areas they're expanding – there's expanded NIH funding, and student demand is going to be very high because of the health sciences. This is something that's really beginning to explode at Berkeley, and they need chemistry. So, that can be part of the pitch, how chemistry fits into the larger ecology of what's happening in the Berkeley campus. I think that's probably part of it, as well.

06-00:12:19
Cerny: That's part of it, and so much so that now, we have, in addition to a bachelor's of science in chemistry, we have a bachelor's of science in chemical biology, which has more students in it than chemistry, more undergraduates in it. It's really good because it's a tougher biology sequence and more likely to get you into medical school rather than just a straight biology exam because you have a lot more math, and things you really will wish you had, in my opinion. So, that's been very successful. I didn't have anything to do with that, but that's where we are now, and so that's kept our total enrollments up. I'm still told that 50 percent of undergraduates take a chemistry course at Berkeley. I don't know how it's possible, but yeah. Some of the farsighted people, some of the hardcore physical chemists said, "Look, we've got to do this. Let's just do it, there are really good people," so we're very strong, there. That worked.

One of the main jobs the department chair has to do is you have to prepare personnel cases for all these faculty with our system, and so you have to send these personnel cases to the dean, and then to the budget committee, and to wherever, with the cycles of two or three years, and then prepare outside offers you're making. So, I was writing twenty-one to twenty-three personnel cases a year, and it would take me five hours a case, after I had all the materials I wanted. Now, it's worse than that because the chairs are having to fend off all these universities who want to raid our chemistry faculty, so they're having to write retention cases. So, an enormous amount of the chair's time goes into that.

06-00:14:21
Burnett: Who is raiding faculty?

06-00:14:24
Cerny: Many US universities. An example is that MIT just tried to steal a husband and wife team from us and got turned down. Raiding Berkeley faculty who also do research at LBL is very difficult, given the LBL resource base.

06-00:14:32
Burnett: So, retention cases, when they're going to say, "This is what they've offered, can you match it?" You have to justify an increase in salary.

06-00:14:40
Cerny: Right. Well, and you have to justify the person's really that good.

06-00:14:59
Burnett: But you were trying to be thorough.

06-00:15:07
Cerny: That's come at the cost of getting communication from the department chair to the faculty in other ways, because you really don't get much of that. I had a couple of other minor things that just seem to me to be interesting, that people don't do anymore. So, we worry about teaching evaluations for each course, and so I said, "Gee, well, you want to talk about teaching when you get promoted, so why don't we get a questionnaire for all the graduating bachelors of science in chemistry, and bachelor of arts in chemistry, and get them to fill out this questionnaire when they graduate? Then, they can tell us the best professor, and the not-so-good professor or whatever, and then I'd have material to use in these personnel cases, rather than just teaching evaluations." So, we tried that. I think that went away. I tried my best to interview a number of the departing Ph.D. graduates, just to interview them and what their experiences have really been like, and what can I learn from things like that? That's something that will be carried over in the graduate dean's office much more successfully.

06-00:16:18
Burnett: Perhaps this is a good time to ask you, where – it's this concern for quality across the board that wasn't there before? People were not asking those questions—why were you interested in that, at that time? Is it because of the graduate student crisis?

06-00:16:55
Cerny: There has been increasing concern about undergraduate teaching, and just having teaching evaluations isn't really great. So, trying to capture the undergraduate comments made sense to me, and it's easy to do.

06-00:17:11
Burnett: The qualitative comments, yeah.

06-00:17:14
Cerny: You can say, "Who were your best teachers?" and whatever questions like that. Very easy and they should fill it out.

06-00:17:22
Burnett: Were other departments doing that?

06-00:17:24
Cerny: I don't know. I just decided to do it. Ultimately, I'm going to create an exit questionnaire for Ph.D. students as graduate dean, which we'll return to, which is a goldmine. But this in particular, I think it faded away. Chemistry, in this time, I want to mention her name for the record, had an exceptional management services officer. We were a relatively small-staffed office. Her name was Dorothea Crane, and she was with us many years. She was outstanding, and she could just do all this work and train the chairs that come through, and had a great personality, and it's the kind of staff that universities need.

06-00:18:12
Burnett: That's often the case with large, prominent departments, is they have an incredible administrative backbone.

06-00:18:18
Cerny: Right, that's true. In the college office, when I was an assistant professor, there was a woman who just terrorized the assistant professors.

06-00:18:29
Burnett: For the greater good?

06-00:18:30
Cerny: I hope. Before I get to ancillary things, Prop Thirteen actually got passed in June '78 and it kicked in July 1 of '78. I had a year to go, although I didn't know that, because I wasn't tapped to do something else till February 1979. Promptly, two of our three administrative assistants quit, and we had a hiring freeze. So, there was this other person, me, and Dorothea, called Dot. So, I put out this cute sign on the door but I didn't mean it, and it says, "Due to Proposition Thirteen and local bad karma, the Chemistry Department office can only be open on alternate Tuesdays, 1:00 to 3:00 p.m., please take a number." Which I thought perfectly summarized our situation.

06-00:19:32
Burnett: The fiscal crisis, yeah. Prop Thirteen is really significant, of course, in the history of California. Then, you also have a second oil crisis that hits, and a kind of recession that lasts until the early eighties. There's yet another period of malaise in the economic sphere that impacts that. We can talk about this along the way at several points because it affects everything that you're in touch with. At the time, did you have a sense of how significant that was going to be? Or were you confident this is just a kind of ripple in the economy?

06-00:20:30
Cerny: No, we've had these budget shocks so continuously, it's been a shame that the state isn't willing to have a tax structure that can maintain the university. It is the premier public university system in the world and it really needs the financing to support it. That's a real shame. That's another reason we're lucky

that Heyman started fundraising. Now, we have money to supplement tuition for undergraduate students at low income levels. So, it's been surprising that the university's been able to beat off the competition because the salaries right now are probably 10 or 20 percent less for typical faculty, compared to outside offers.

06-00:21:34
Burnett: But it maintains that level of excellence.

06-00:21:38
Cerny: Different things keep people here. I think that in science and engineering, and now in parts of biology, I mean LBL is an asset, right? So, you say, "Okay, well, I could go to Harvard or MIT, but what do I really care about? A title, or all these resources that are sitting around to do major high-level research, that are maybe shared because you've got LBL sitting there with a humongous budget, and a lot of great colleagues and outstanding graduate students on campus?" Many faculty will stay because they know they can do more research and they don't care about the next $50,000.

06-00:22:19
Burnett: They care about the weather, too? That has always been an attraction for faculty and retaining faculty. They often think about blizzards in Boston, as opposed to lovely weather.

06-00:22:32
Cerny: This is a good year to think about it, too.

06-00:22:33
Burnett: It is a good year to think about it, that's very true.

06-00:22:38
Cerny: Luckily, somehow the humanities and social sciences have been able to maintain really high-quality faculty also. I continued in the department chair, then two or three more little things came up. So, one of them was that Calvin had been the head of the laboratory of chemical biodynamics since it was formed, around when he got his Nobel Prize. He had to retire because we still had mandatory retirement that caught him and Seaborg, so they had to retire. They asked me if they had to retire and I said, "Well, you do." They both retired when I was the department chair. So, then I got a phone call from Chancellor Bowker, and Chancellor Bowker had been gotten to by the biologists.

So, we're talking about the late seventies. Biology was becoming a disaster because it had fragmented in all these little micro-departments, which were, "sequestering resources and sliding into oblivion." A big national study by the National Academy of Sciences came out in '82-'83, and biology had slid all over the place. That's why Heyman and Vice Chancellor Park, who was a botanist, ended up doing this biology revolution at Berkeley, with help tremendously from Dan Koshland. That was a little later, so we're now talking about ’78-’79. So, the biologists had gotten to Bowker, and he thought it’d be really great that we should hire some kind of a physical biologist to replaces Calvin as head of Chemical Biodynamics.

So, Chancellor Bowker appointed me as the chair of a three-person committee, with Dan Koshland, and a Nobel laureate at Stanford whose name I’ve forgotten. That’s the committee. We never met; we talked on the phone. I got some suggestions from them, and the first suggestion was a person whose name I don’t remember, but I wouldn’t quote it if I did, from the East Coast, who had a Nobel Prize. So this person looked good to me, so I went to Bowker and said, “Well, this person looks pretty good.” So, we don’t go through many committees or anything, right? Bowker phones him up. Well, he wasn’t interested. We went back and did all that again, talked about some more people. There was an English person, and the English person, actually a little later, got a Nobel Prize, but didn’t quite have it yet. Still looked good to me, I went to Bowker and said, “Okay, try this guy.” He tried him but he didn’t want to come.

So, *then*, having worn down the biologists by two good attempts, and because probably Koshland had a high opinion of Pimentel, George Pimentel from our chemistry department got suggested. So, Bowker called him up and he took it, so that’s how Pimentel became the director of Chemical Biodynamics. But at that time, he was still the deputy director of NSF and he couldn’t come for a couple of years, so Calvin then went on and ran it for a couple more years, and then Pimentel came back. It turned out to be a really good solution to the problem, from the point of view of the chemistry department, and hopefully the biologists. Anyway, by then they were heading into their reorganization.

06-00:26:40
Burnett: Right, which is a dramatic one, and it’s one that other biologists have talked about in oral histories, too. It saved the life sciences at Berkeley. It completely revolutionized—

06-00:26:57
Cerny: Oh, it did, yeah. I think they had to do this real peer review, and Park knew all the key reviewers, and they brought in these really good people from outside who said, “Yup, there’s a problem here, and you guys got to fix it.” They would come back and beat the heads of the recalcitrant ones, or the shoulders, or something.

06-00:27:16
Burnett: It’s very, very difficult to hire senior people, of course. They have their commitments, they’re settled, they have families, it’s very difficult to make people move.

06-00:27:29
Cerny: This is more, not so much hiring, you had to have the new facilities—the renovated life science building and two brand-new, huge biology complexes.

So, a lot of new facilities for people, so you can attract younger people in those areas. This reorganization was written up in *Chronicles of Higher Education* as a real success story.

I think it was now the continuing budget crisis at the federal level that affected LBL. The lab needed a committee to kind of do staff classification, that just hadn't been adequately done. Although there were senior staff and regular staff, I don't think the prerequisites had been worked out at all. So, if you were heading into RIFs [reductions-in-force; staffing cuts] or things, you didn't have any rules. I was put on that committee, and I know a colleague of mine who'd come to Berkeley from Brookhaven, even, who was just employed at LBL. So, the faculty weren't worried about RIFs at LBL, but other people were. So, I remember we were on that, and saying, "Okay, well, what would make sense? How many days should a regular staff member have for notice if, all of a sudden, the lab budget's been cut so much? What do you do?" Then, if you're a senior scientist and you pass this hurdle to be a senior scientist, then I think now, as well, you'd be protected for a year with your salary, if at all possible.

06-00:29:31
Burnett: There weren't those rules in place?

06-00:29:32
Cerny: There must not have been enough because I remember talking about it. I kind of find that hard to believe, too, but I stand corrected if you can find these things.

06-00:30:14
Burnett: We will talk about your associate directorship of Nuclear Science at LBL, but in the seventies, there is a big reorganization by Sessler in '73.

06-00:30:32
Cerny: About when he immediately came in. McMillan was director until he resigned, when an external committee came in and told him he had to resign. Then, Sessler was there when all this energy and environment money came. The Nuclear Chemistry Division switched to be the Nuclear Science Division somewhere in there.

06-00:31:02
Burnett: Later, yeah.

06-00:31:04
Cerny: I remember the high-energy physicists, but not the super high-energy physicists, came into our division.

06-00:31:14
Burnett: I don't know if you can clarify this for me, but there were two physics divisions and before Sessler came, they were called Physics I and Physics II, and then he renamed them—one just became Physics, and the other one

became the Accelerator Division. Which is a little bit confusing, so that means that anyone who's working with a particular one of these, like with the Bevatron, or I don't know if it was divided that way, that if you're working with that accelerator, and if you're working with this other accelerator, to get part of these. In other words, were the divisions divided by machine?

06-00:31:59
Cerny: If I had to guess—because I don't know—I would guess it was divided by purpose, and that one of the divisions was the real physicists doing the experiments, and the other division was the Accelerator Division, which became an accelerator and fusion division, or whatever. They were the ones that would build the accelerators and run them.

06-00:32:29
Burnett: Then there is this separating out that takes place.

06-00:32:31
Cerny: Then maybe that's when these not so high-energy people—there weren't that many of them—came over into our division.

06-00:32:37
Burnett: Right. Well, one of the important things that changes is that LBL is liberated from, I think it's until the early seventies, they were required to be funded through the Atomic Energy Commission. Those rules are eliminated, and so, Andrew Sessler is allowed, for the first time, to solicit funds from outside. You could still get funding from other sources, but it had to come through the AEC. The funds would be transferred to the AEC, the AEC would disburse. So, it's this kind of siloed funding stream for LBL, and that's liberated or opened up in the early 1970s. Sessler immediately begins to go after these new domains. That's a whole can of worms, and what I want to talk to you about when we talk about your time in the early eighties is what kind of problems and what kind of opportunities that opens up for LBL.

06-00:33:46
Cerny: It's a big change also with going from the demise of the AEC to ERDA, and then to the Department of Energy, very rapidly. Of course, the lab gets renamed twice in this period. It goes from the Radiation Lab, and it goes to Lawrence Radiation Laboratory, and then it goes to Lawrence Berkeley Laboratory, and then ultimately becomes Lawrence Berkeley National Laboratory, but I think that's much later.

06-00:34:23
Burnett: So, LBL is right at the center of a lot of the shake-up in terms of government funding streams, and in terms of social and environment, social concerns about the environment, and trying to offer relevant—"relevant" is the wrong word—turning the LBL into a multipurpose laboratory. Some of it is applied science, and that really begins to really take root under Shirley in the eighties as well. That's right when you're in at that time. So, in the late seventies, is there something you wanted to talk about while you were chair of the

Chemistry Department? Things that were going on at LBL that you were involved in?

06-00:35:14
Cerny: No. The only thing was that committee. What I would talk about is this National Science Foundation committee.

06-00:35:25
Burnett: Well, let's go to that, then.

06-00:35:29
Cerny: It was a very interesting committee, and this committee meets in '78 and '79. Actually, it meets in the spring of '79. It's dealing with university-based accelerators. There had been a huge proliferation of – you, know, nuclear physics was hot, everybody needs their own accelerator. You're looking at Department of Energy funding and you're looking at—

06-00:36:24
Burnett: Let's see if I can get it here. Oh, yeah, it comes out.

06-00:36:28
Cerny: If you are, it's kind of interesting.

06-00:36:31
Burnett: They can pause and see that, but we can also submit these. We'll put them in the back, as an appendix.

06-00:36:39
Cerny: It's very interesting data. The first one, I didn't give you, I maybe put it at the bottom, but it isn't as important as these others, which are really interesting. Yeah, that one's there. This is now '78, '79, but you see the decline in funding, and a dramatic decline in the number of university facilities. Then, the next picture in here will show what we've been talking about, which is the one of Ph.D. production. You probably want to do that. So, this is the Ph.D. production dropping off, but this one, the one you're looking at, then, is the decline in physics Ph.Ds. and nuclear science Ph.Ds.—really going away.

06-00:37:45
Burnett: I'm going to insert this as a still, I think.

06-00:37:51
Cerny: So, this led the NSF to say, "We've got a crisis here, and we need the committee to check it out." So, this committee was supposed to review eleven of the nuclear physics laboratories at universities, paid for by the National Science Foundation. You've kind of gotten to the best ones, so it's: Cal Tech, Florida State, Maryland, Notre Dame, Pennsylvania, Pittsburgh, Princeton, Rutgers, Stanford, Stony Brook, and Rochester. NSF had realized it couldn't support eleven of them, and they wanted this committee to cut it at least by two and redistribute some money. So, they had two different funding scenarios. One, enough money where you could drop two of these places and

keep going, and the other scenario was, "Well, we're going to cut your budget," and that one would go kind of at cost of living. Their second option was you would take a cut and then you'd decline at 4 percent a year. So, this was an emergency-like set of meetings.

There were twelve of us put on this committee, and we met in March 9-11. They started in February. We heard reports from all eleven places. April 17-18, we had a draft report, and April 27-28, you had to have a final report. So, I had three trips to Washington, an innumerable amount of reading, and the committee was really good. So, it had Allan Bromley as chair, who was a distinguished experimental physicist at Yale, it had Feshbach from MIT, it had well known theorists, it had well known experimentalists. They had one guy from industry. I was definitely on the junior side of all this. My guess is nobody [on the committee] was funded by the National Science Foundation, did not have any conflict of interest. So, the meeting, you had to make decisions and you had a really tough guy from Brookhaven who was on this committee, who was a high-energy physicist at Brookhaven, Nick Samios, who, in a few years, becomes the director of Brookhaven.

So, various people were whining about, "Oh, can we cut this?" One of the distinguished theorists says, "Well, maybe we should just not agree with what our agenda is, and say it's not possible to cut all these really nice places." High energy physics having been cut a lot already, right, at the good places. Samios really had a tough line, saying, "We've got to be realistic. This is what we were asked to do, do it." A really interesting table then shows up, which is in the bottom of your packet and must be in the bottom of my packet. It looks like this.

06-00:41:18
Burnett: Tandem accelerator?

06-00:41:19
Cerny: This shows the situation at Stanford, and what it shows is Stanford's functional budget being eaten away by inflation and overhead charges, so that the combination of overhead increases at Stanford, which were dramatic, and inflation, cut their effective budget to be one-third of what it was.

06-00:41:57
Burnett: From when to when?

06-00:41:59
Cerny: From '69 to '79, I think, the effective budget. They had $471,000, but correcting for inflation—this is quoting Bromley, and Bromley's very good at all this kind of stuff.

06-00:42:10
Burnett: Constant dollars.

06-00:42:14
Cerny: He had actually told the committee we should have a much more upbeat thing, a third scenario, which we did have just for the spirit of things because he liked to do that. The committee just looked at that and said, "Well, at Stanford, they do good work, but the budget at Stanford, the overhead's going to eat everything up." Then you can't make it. In these eleven institutions, three of them actually had had the accelerators bought by the university. So, Florida State had paid for a lab in 1960 and an accelerator, and Princeton, it's hard to believe, even had paid for a copy of the Michigan State University's first cyclotron, which was a pretty nice, smaller cyclotron. Stony Brook, the State of New York had bought that one. Our committee agreed to actually terminate the Maryland cyclotron and to terminate Stanford, terminate the funding for both the Maryland cyclotron and the Stanford program. We had to find two places to close.

06-00:43:36
Burnett: So, you decided that because Stanford had been starved for so long, it was effectively dead anyway?

06-00:43:43
Cerny: Yeah, it was effectively dead anyway and Maryland had had a bunch of equipment and machine problems that they just couldn't get around. We ended up saying, "Okay, you should fund these guys to go work somewhere, some other accelerator."

06-00:43:57
Burnett: But this is not the linear accelerator at Stanford—that's the national?

06-00:44:01
Cerny: No, this isn't SLAC. It was some kind of a tandem Van de Graaff accelerator. It was small nuclear physics, the budget was only $483,000. SLAC's budget must be enormous. But still, it was Stanford and it was a good program. It was interesting because the committee had people on it that knew, in addition to the presentations, they knew the real status at all these places, and how many good young people there were, and how many old duffers there were, and whatever. So, there's a really detailed aspect that we had to include in the report. To me, it was very interesting to see what was happening, and seeing what the older members had felt. "We really can't do this kind of thing"—"Yes, you have to," and, "How are we going to do it?" I think that the individual reports are really interesting, and so it was really a very knowledgeable committee. I know that's what happened at Maryland and Stanford. It was kind of really interesting, was the first national review I'd been on like that.

06-00:45:20
Burnett: Can I ask you about process and criteria? You said that they really knew the status of each of these programs and how many students they served and the quality of the students. Was there a calculus of geographic distribution, for example? Like, how many people have regional access to accelerators or

things like that? Or was it just about kind of the health of each accelerator program and its robustness and its capacity to do good research?

06-00:46:09
Cerny: It's really that. I'm sure they had to provide a summary of what they can do experimentally, where they are, what they hope to do, how many students they have, how many Ph.Ds., what they're doing, what the CVs looked like for all the faculty—would all have to be there, so you could flip through this and say, "Yeah, this guy really isn't doing anything anymore, but he's getting some money from NSF." So, you would have an oral presentation and then you would have had all this back file to have read before then. When you're really active in this, you know what was really going on in these places. I think you worry about you should have access as an outside group, and so with airplanes, you can do that. The point with Maryland was, okay, there are good physicists there, but we can't afford spending money on this cyclotron—it's broke and we can't seem to fix it.

06-00:47:15
Burnett: These were weak, straggling outside of the pack, basically, and so they could be killed effectively.

06-00:47:25
Cerny: But it was the dynamics of it, and seeing a particularly distinguished theorist just saying, "Well, should we do this at all?" "But you have to."

06-00:47:37
Burnett: It sounds like a kind of political moment for physics and nuclear chemistry because there is this clamor of fiscal responsibility, this is the run-up to the Reagan Revolution, right?

06-00:47:53
Cerny: Yeah, right.

06-00:47:56
Burnett: In September 1984, I was going to bring this up next time, but we might as well just talk about it here, Leon Lederman from Fermilab wrote an article in September of 1984 to *The Christian Science Monitor*. It was an op-ed piece decrying the contraction of funding of basic science research overall since 1967. He argued that high-energy physics had lost almost a third of its funding overall when expressed as a percentage of gross national product in this time period, from '67 to 1980. Accelerators have been retired at Berkeley—I guess he's talking about the Bevatron, that becomes BEVALAC—at LBL and elsewhere, in the three remaining large facilities, Fermilab, Brookhaven, and Stanford, are only operating at half capacity. Europe is now spending double on physics—Europe as a whole—than what the United States does, whereas the United States facilities were just either treading water or worse. This is kind of an interesting moment, when the scientific community is really becoming vocal, clearly, in the press. I don't know if Lederman was involved in this siting committee?

06-00:49:19
Cerny: No, he wasn't on our committee. He's a high-energy physicist, and these were all either theorists or low/medium-energy.

06-00:49:25
Burnett: Van de Graaff, okay. But there's something in the air, clearly. There are these funding cuts at the state level and at the federal level, and a sense of gradual erosion. It seems to be a moment when physicists are becoming organized and people in science, generally, begin to organize to protest what has happened over that time period. That's very interesting. So, you learned a lot, being part of that.

06-00:50:00
Cerny: Right, and I don't know whether the Bromley—the next key thing he chairs is the electron accelerator siting and design committee, and I now appear on that. Like, my colleague might say, "Well, what kind of people would you like to see on your committee?" Or, "Here's a list."

06-00:50:18
Burnett: That was for the siting committee [for the 1 GeV electron cyclotron]?

06-00:50:22
Cerny: Yeah, the technology and the siting.

06-00:50:27
Burnett: When does that take place?

06-00:50:28
Cerny: Eighty-three.

06-00:50:31
Burnett: I don't know whether we should cover that in the next session? Talk about that in some detail.

06-00:50:33
Cerny: Oh, yeah. We should cover that in the session on being associate director of Lawrence Berkeley Laboratory. In a sense, I may have less to say about things there, and it's a different kind of environment than being the department chair. I'm on a lot of committees—some more interesting than others—but that one was a very, very interesting committee. I was put on as the chair of the nuclear physics part in the National Academy of Physics study in that period, too. So, those take time, and I'll have to think about it.

06-00:51:33
Burnett: That's all right. I imagine as an associate director of a national lab, you can get pulled into national questions more easily, or was that a factor for you?

06-00:51:51
Cerny: No. Well, yes, I think getting on this Bromley Committee for the electron accelerator was because I was, yeah, associate director, and that's why I was picked to do the nuclear physics part of the physics survey. I'm a nuclear

chemist, right, but they decided they didn't care. So, I had physicists on my committee, but they wanted me to do that. So, that came along with the title, but luckily, when I was doing that, you didn't have to go to Washington all the time, like you do now. I certainly went some, and of course, going to these meetings, you had to go. I tried to minimize unnecessary trips to Washington.

06-00:52:38
Burnett: Save your travel for leisure, yeah.

06-00:52:42
Cerny: Although I'm sure I went to all three of those NSF meetings in that period of time, when I was kind of finishing as department chair and it was late in the year, so it probably would work.

06-00:52:55
Burnett: Were you slated to continue as Chemistry Department chair, or was that interrupted by the request to serve as associate director of LBL?

06-00:53:02
Cerny: Well, chairs don't serve more than five years. I would have done another year, but typically, they've been doing three or four. I would have done a fifth because I enjoyed doing it, but it so happened that Bernard Harvey wanted to stop being head of the Nuclear Sciences Division, and then Sessler asked me to do it.

06-00:53:27
Burnett: Bernard Harvey had been deputy associate director when Sessler did the re-org, and then he was director.

06-00:53:35
Cerny: Then he was director, and he's the person I did my thesis with. Well, then Seaborg got to being an associate director at large, so then got moved into a position where he had influence but didn't have to do day-to-day running things.

06-00:53:53
Burnett: I do wonder, you said did he have to retire, and he did.

06-00:53:59
Cerny: Only on campus. Well, I don't know if he was paid. I have no idea whether they paid him something.

06-00:54:09
Burnett: But he continued to have a role there?

06-00:54:11
Cerny: Oh, he had a major office and role there till he died, yeah.

06-00:54:17
Burnett: That was his life's work.

06-00:54:18
Cerny: That was his life. In his office, he had his photographs, so when the Russians came, he put up all the pictures of all the Russians he'd seen, and when the Chinese came, he'd take them all down and put up all the Chinese. Sure, hey, Glenn created the field I'm in. What can I say? I was happy in a sense because he was chancellor '58 to '61, and then '61 to '71, he was at the AEC, and so by the time he came back, I was a full professor.

06-00:54:55
Burnett: It brings to mind something I was going to ask you earlier: I was reading an article that we can talk about, which is about the Ninov Affair—we can talk about that later or not—by a science popularizer called Simon LeVay. In his description of the lab and Berkeley chemistry, he was talking about this heyday of nuclear science of these element hunters who were discovering/creating these new elements of the periodic table, obviously implying that that's gone. The Ninov Affair was explosive because it was the promise of Berkeley coming out on top once again.

Do you think that element discovery was really important in the public perception of the value of what was going on there? That because that's not happening so much anymore, because the problems have become more sophisticated and harder to explain to a lay person—if you tell someone we've discovered a new element, it sounds pretty fundamental, but if you talk about excited isospin states, it might not be as easy to explain. Do you think there is a kind of public-relations difficulty in general with some of the science that's being done there? How do you manage the perception of the value of what's happening at LBL?

06-00:56:46
Cerny: That's probably a subject for a longer discussion, but the element hunting, I think, is of interest. Berkeley dominated it so long, and the *California Monthly* or whatever has an article, even with it in it, as branding. Have you seen that?

06-00:56:59
Burnett: No, no.

06-00:57:00
Cerny: It's talking about branding, and so it's talking about Seaborg and the elements up through Californium.

06-00:57:06
Burnett: Berkelium.

06-00:57:07
Cerny: Yeah. Glenn announced one of the elements on this children's show, ages ago. It's really interesting. We still have a group at the cyclotron which is under-funded, but it is able to do it, that can do competitive work. So, LBL's still competitive. {audio break} But I liked working in the light elements and

could do my own thing because I couldn't have caught up and worked with Ghiorso or any of that stuff. Nobody could work with Albert Ghiorso.

06-00:57:52
Burnett: Why was that? Was he notoriously difficult to work with?

06-00:57:56
Cerny: Well, he did things his own way. Once it was all laid out, it was fine. The chemists could run up and do something. So, it's still true, and so the elements up to 118 have been really discovered and I believe it, and the question is, there probably are higher ones. The problem there is we're going to need a really new way, somewhat different way to make them, because up to 112, you could do it with non-radioactive targets. The Germans couldn't use radioactive targets, so they did all their work, but then the Russians could use radioactive targets, which they got from Livermore, and they could use calcium-48 beams by dedicating one big, heavy-duty cyclotron to producing calcium-48. So, these have all been made largely by calcium-48 on radioactive targets.

Unfortunately—or fortunately, whatever—the elements that they're making don't link into the ones that are known well, so they're out on a separate branch. You had to work your way down that branch till you finally came across. That has all happened, and so, now people do believe them. Element 116 was named for Livermore, and so it was just a big celebration last summer at Livermore, on the discovery of Livermorium, right? I went out to the talks, scientists got the key to the city, celebrations downtown. I think people are interested in that. In light elements, you maybe could have new astrophysics, or people are interested in the big laser at Livermore.

06-00:59:46
Burnett: Right, exactly. There has to be a lot of work around sustaining public interest in science, and fortunately, there are other domains that are accessible to people, and scientists work to present those to the public in ways that are appealing, so you can sustain and hopefully increase public support. So, perhaps we should stop there, and we'll continue on to talk about the early 1980s and your time at Lawrence Berkeley Laboratory as associate director of Nuclear Science.

[End of Interview]

Interview #4: April 18, 2014
[Begin Audio File 7]

07-00:00:15
Burnett: This is Paul Burnett interviewing Dr. Joseph Cerny for the University History series. This is session four, tape seven. So, Dr. Cerny, last time, we were talking about your very successful run as chair of the chemistry department, and also the beginnings of your involvement in committee work as a kind of national level. The late 1970s, for you, are a period of tremendous change. There's a lot going on. Can you tell us a bit about how your life is changing in the late seventies at this time?

07-00:01:05
Cerny: Well, actually, my personal life changed because I actually separated from my first wife in the summer of 1981, and we got divorced by October, 1982. I then remarried someone who was divorced, named Susan Dinkelspiel Stern. We got married in November of '83. She had three children—Elizabeth, the oldest, David, and Catherine, the youngest. So, we've been together for just over thirty years, and we didn't have any children together. My two from earlier and her three are the children that are around, and I have four grandsons and she has six grandsons and one granddaughter in that group, yeah.

07-00:02:03
Burnett: How old were they, at the time? The three children.

07-00:02:07
Cerny: They were in their teens.

07-00:02:13
Burnett: Your two sons—

07-00:02:14
Cerny: Were also in that, they're all in that group, yeah. So, like, my older son was fifty last year, and the younger one will be fifty in August, and her oldest child is fifty in May, so they're pretty similar.

07-00:02:30
Burnett: Your two sons are rather accomplished, aren't they? They've turned out fairly well.

07-00:02:35
Cerny: Right. We can talk about it later. I thought I'd also say that David Shirley, who is the lab director, was a chemistry colleague, and his wife, Virginia, and my first wife and I were very good friends. In the long run, Dave's wife, Virginia, died of cancer in the mid-nineties, and he actually remarried and married Barbara, my first wife. So, just to bring it all to some tight little circle, there. They've lived different places. I think they left Hawaii and are now living in Palm Springs, but anyway, so that's sort of also what was going on.

07-00:03:23
Burnett: In the late seventies, you were chair of the chemistry department, but there were other career changes waiting in the wings. Can you talk about that transition and how that came about?

07-00:03:38
Cerny: Right. Well, yeah, it became clear at LBL that it was time to bring in new leadership in the Nuclear Science Division, which was by then—and I'll get back to it—it was its name. Before I do that, let me just say something else. So, I became division head and associate director July 1, '79. The tradition in chemistry was not for us to go 100 percent to LBL. I had a 50 percent appointment at LBL, when Dave Shirley was the head of the Materials and Molecular Division, he did that, also. I ended up with a 50 percent appointment, and we were on the quarter system, so I ended up teaching physical chemistry the spring quarter of 1980 through '83.

I taught a course I really like, which is the introduction to quantum mechanics and its application to atoms and molecules. I think it's a fun course. I did that for four years, and then by January 1, '83, Chancellor Heyman had lobbied heavily that we go back to the semester system, which we did. I remember, originally, when we went from the semester to a quarter system I had voted against it and lost, and then when we were going back, I voted against going back to the semester system and lost. So, I have a perfect record.

07-00:05:26
Burnett: We haven't really talked much about teaching. Can you talk a little bit about teaching and where it sits in your work? You mentioned enjoying quantum mechanics, teaching quantum mechanics. What did you enjoy about that course? What made it special for you?

07-00:05:49
Cerny: Well, it's a junior-level course, typically, and you really are introducing quantum mechanics, which I had learned a lot about. I really had some simple way to introduce the Schrödinger Equation that someone had proposed that I liked a lot. Then, you had a lot of really good applications. So, I really liked teaching that. I had taught the whole sequence—the next part of it goes to more complicated aspects of physical chemistry, but this was really pretty straightforward, and I liked that. In this period, I wasn't teaching the nuclear chemistry sequence. I had done that a little earlier, and I had taught the basic nuclear chemistry class, and then there's an advanced class that I'd taught some of. The basic one was basic, and the other one sort of covered some research areas I was interested in, some general things.

07-00:06:48
Burnett: How large are these classes? What do they range?

07-00:06:54
Cerny: They were about thirty people.

07-00:06:55
Burnett: That's a nice size.

07-00:06:56
Cerny: Yeah, and maybe thirty-five in the nuclear courses, at the time. When I came back to teach, after I'd been vice chancellor and dean, they had me teach this class again. At that time, they consolidated everything, and they gave it to me at eight o'clock in the morning to punish me. So, I was teaching seventy-seven people this introduction to physical chemistry. But I liked it; it's just I had to really make sure nothing had happened in the intervening twenty years in physical chemistry that I didn't know about.

07-00:08:03
Burnett: In my teaching experience, I've found that there's something about being forced to explain something complicated to people who don't have a lot of exposure to that. You have to wrestle with that communication. I'll ask that as an open-ended question: in your teaching career, did that give you any kind of sympathy or understanding for the larger mission of the university? When you became graduate dean, when you became in charge of research, did teaching give you any special insight into the larger functions of the university that you were involved in?

07-00:09:03
Cerny: Well, not much beyond what you'd expect, I would say. I did believe in really teaching what the material in the course should be, as opposed to the material in the course that I was professionally interested in. So, some of my colleagues distort their classes—particularly at the graduate level—way too much, I think, and do stuff they're interested in. I thought one ought to be teaching appropriately. Actually, a colleague named Bruce Mahan, who was our department chair for three years and who had written a great book on freshman chemistry, complimented me on the material I put in this physical chemistry course. So, I was quite pleased by that because he was a physical chemist and I'd sent him my lecture notes. So, I said, "Okay, I must be teaching at the right level." He'd written this very successful freshman chemistry book, so he knew.

One thing kind of related to this, in the winter and spring of 1980, a colleague of mine was at Berkeley who I knew from when he was a graduate student in English at Berkeley while I was a graduate student. He never finished his thesis, but he got on and ultimately was teaching at Menlo School and College. He'd taken some time off to try to finish his thesis and was living in Berkeley. He didn't have enough money, and I said, "Well, how would we like to team-teach a course on written and oral communication for our students who don't know anything about any of that?" So, we did that for two quarters, and the department paid him like a TA. He was really an excellent teacher, and so we had juniors, seniors, and graduate students in the classes for the two quarters. We discussed reading and speaking and they had to write papers and give talks, and I came around, some. So, I did that kind of innovation of what I thought was important. It wasn't anywhere in the curriculum.

07-00:11:06
Burnett: I suppose what's beginning to happen more and more, as the student population becomes more internationalized, is there an English Language Learner issue for the chemistry class, in terms of presenting ideas in English and writing? Is that something that the chemistry department felt a need to adapt to, or address?

07-00:11:36
Cerny: No, not really. I think most of the people that are admitted actually have pretty good English. I did have one graduate student from China who we all believed had a speech defect in English, but had been admitted. I've had a number of international students, so that's not a problem. This was just my feeling that chemistry students didn't have any courses to do writing, particularly, or practice speaking, and it was just a thing that ought to be more regularly done. But I don't believe it was taken up by anybody. There was this opportunity and my colleague was really good at it, and it got really good reviews. I was pleased to have done it.

So, since I was 50 percent time, the department chair then thought it would be nice if I was the chair of the chemistry department planning committee that I mentioned last time. So, for two years, I was the chair of that. However, the faculty hiring at the tenure level had fallen way down, and so we hired one professor in 1981, a full professor named William Lester, who was a theorist. He was actually our first full professor, African American. In those two years, I'm sure I helped hire Darleane Hoffman, who was a distinguished nuclear chemist, who was in Los Alamos for years. Seaborg really wanted her to come to Berkeley toward the end of her career, and so we did that. Darleane was probably the second full professor, female, in the department.

Oh, yeah, and I was also a member of the physics department review committee, which was interesting, for the Graduate Division. That was with my 50 percent time appointment.

07-00:13:39
Burnett: At the same time, as far as the LBL side, can you talk a little bit about what happens then?

07-00:13:52
Cerny: I think I'd break that up so it's really two jobs—you're an associate director at the lab, so that means you're involved in lab management, and then you're the head of a division and running your division. So, Dave Shirley became lab director in early 1980, and I have great difficulty getting the exact month, but it was early. It must have been a high point of the early couple of years, when there weren't a lot of high points since Reagan's budget-busting started off early, was to celebrate LBL's fiftieth, which was done on October 3, 1981. A

wonderful set of symposiums and dinners, and I was on that planning committee. That really worked well. In looking at my notes at what did we talk about in the associate directors' meetings, well, you could imagine.

It's overhead issues, it's budget crises, it's contract renewals. Finally, Livermore and Los Alamos were split away from Berkeley, so Berkeley gets its own contract. We have constant discussions about high performance computing struggles, that we need better computers. There are major space fights as everyone's trying to accommodate to energy and environments still being absorbed at the lab, and all the other changes are going on, space issues. How to handle postdocs, collective bargaining, staff classifications. I'm not intending to dwell on all these by any means, but they're part of the management scene.

07-00:15:33
Burnett: But this is not regular workaday issues. Something tremendously different is happening at this time in the history of LBL. It has a fairly long history—one is this opening up of the mandate of LBL, which has been going on since the sixties, really, this new emphasis on the environment, this new emphasis on getting outside funding, which isn't permitted, I understand, until the early 1970s. So, there's a reason why Andrew Sessler is going after this stuff, it's because he's allowed to, for the first time. But there's also the organization that funneled money towards LBL, the Atomic Energy Commission is split in '74 into ERDA, the Energy Research and Development Administration, and the Nuclear Regulatory Commission. Then, there's another reorganization in '77, and there's a new Department of Energy that's created. That's now a combination of ERDA and this Federal Energy Administration. The federal government itself is going through tremendous shakeups, almost every couple of years during this lead-up to this.

07-00:17:13
Cerny: Well, it plays into a lot of budget uncertainty, that's for sure.

07-00:17:25
Burnett: One of the things that David Shirley said, when he took over in '80, within the first two years, he had let go 19 percent of the staff.

07-00:17:43
Cerny: Yes, well, we'll come back to that. I'll come back to that, just an anecdote, but that's correct. We were really having to deal with a lot of things relative to staff support. In this process, high-energy physics and nuclear physics were put into the Department of Energy as a national trust, whatever that meant. So, they were put in with a little different wording within DOE, having come from the AEC. I remember that being quoted years ago. Whether it does any good for anything now, I don't know, but it was there.

Dave, by 1983, was extremely interested in having the Advanced Light Source. So, he really started, in '83, trying to do that. Originally, I think he was just hoping to do it without any peer review, and just because that's how Berkeley used to get things. George Keyworth was the science advisor, and he and Keyworth talked about things like that, and Keyworth actually was a keynote speaker at the banquet for the fiftieth anniversary. Dave realized fairly quickly that he had to get some Washington contacts that we didn't have, and so he created an associate directorship for planning and developing. He hired Martha Krebs, who was a physicist, away from the energy part of the House Science and Technology committee. So, she came in, July 1, '83. I'm sure she was helpful.

07-00:19:40
Burnett: She had insider knowledge of Washington, which is the story that I've read about.

07-00:19:46
Cerny: Some of the straitlaced associate directors—and we had some—still didn't believe we really needed anybody doing that kind of stuff in Washington. I'm afraid they were not correct. So, Dave ultimately succeeds, so that by 1988, in fact, there is the groundbreaking for the Advanced Light Source, which has been, after a rocky start, a great, great success. Dave was in the job until '89. My own worst experience as an administrator, as long as we're talking about budgets, by November '81, Reagan's idea was to put a 12 percent cut in the science budgets. This is already in '82, but we're on a continuing resolution. So, you're already into '82. If he proceeds to do that 12 percent cut in January, you cannot meet payroll. You can't do anything because you've spent your money for four months, and you have layoff rules for staff scientists of three months' severance pay.

So, I remember, literally, the only time I've had sleepless nights, wondering what in the heck I was ever going to do? You know, laying off which of the senior scientists to get through this. It was just awful. That was kind of the end of a whole series of real budget shocks to the lab. Anyway, that got cut back from 12 percent to about 5 percent, and we staggered through. I remember it was the worst kind of management experience that I had to do because it was almost impossible to imagine what you'd do.

07-00:21:33
Burnett: Even with 5 percent, did you have to do some kind of rolling furlough for staff?

07-00:21:40
Cerny: Some people were retiring. Over the years, I had to find ways to get people to retire early, and do this, that, and the other, and I'm sure I did a number of those things. I think Dave got rid of people by organizational changes. There were a lot of people left over from the fifties and sixties, who were near retirement age, who probably saw the light and retired. Part of this relates to what I'm going to get back to, about how our division changed from the Nuclear Chemistry to the Nuclear Science Division.

The other thing kind of related to budgets and salaries is on the professional salary committee—so, I was on it, too—the people from energy and environment who were running their divisions were largely out of the concept. So, they thought that their salary should be somewhat proportional to the amount of money that they were allegedly bringing in. We had to disabuse them of that idea, that just because their division all of a sudden had a lot of money, that meant they were going to have astronomical salaries, because they were spending it. So, I remember we had to, the hardcore science and engineering people, disabuse them of that, and we went forward.

07-00:23:10
Burnett: So, these were functionaries?

07-00:23:12
Cerny: No, these were the associate directors for those divisions, but they somehow thought, you know, the division had a lot of money—and of course, they didn't really raise it themselves, directly—they ought to get some kind of a salary in proportion to how much money that was. Of course, all the standard divisions had a lot of money, too, and we knew better than that, right? Take the accelerator budget—you're not going to get a salary in proportion to—

07-00:23:33
Burnett: Proportionate to the cost of the accelerator? (laughter) So, they were out of touch?

07-00:23:38
Cerny: They were out of touch, and we went through a lot of fads and fashions on energy, and then went to seminars, where you heard all this stuff. It was definitely a confusing time. We got through it, and by today, I would say the lab's all over the place and doing really good work.

07-00:24:05
Burnett: What's terrible about that time is that there's this effort to adapt, there are clearly these oil shocks, and there was an earlier response by the national labs to do environmental stuff because of Barry Commoner's research on strontium-90 radiation getting into baby's teeth, and there's all that stuff about that the national labs had a responsibility to do environmental research. That's starting in the '60s, and they start, I think – is it Brookhaven and Argonne – had environmental programs from 1970 on. There was this idea that there are going to be new areas of research. They're going to promote efficiencies in new product development, it's going to have industrial applications, this is going to be really great, *and* we're going to save the planet from pollution.

At that very moment, when you're becoming associate director, the Reagan administration *hates* that stuff, moves to cancel a lot of it, and as far as the labs are concerned, they should be building weapons—that's all they should be doing. All of this "namby-pamby" stuff about energy efficiency; what you really should do is build more power plants. That's the kind of Reagan line.

Talk about being caught between a rock and a hard place. You've got these multiple directives that are in direct conflict with one another, and that must have been a bit maddening, I imagine.

07-00:25:48
Cerny: Right, and I'm sure that's related in the personnel decline that Dave Shirley mentioned. Ultimately, though, we've come back to that world and we're doing, obviously, a terrific job, and most of the labs are doing a terrific job in all these areas. So, that's really good, but that time wasn't so great. I had other things that I was supposed to do, too, like just to mention two, then I was on the search committee when Dave decided he wanted to have two deputy directors, so we had to go through that whole thing, we'd have two. I was also the chair of the search committee for the head of the Materials and Molecular Research Division, and an associate director. So, I did those because I was close to that division, which is where a lot of the Nuclear Chemistry Division people had actually gone.

07-00:26:44
Burnett: Why did David Shirley want to have two deputy directors? Is there a rationale for that? Deputy director of what and what?

07-00:26:55
Cerny: I can't remember, but my guess is it's more science and engineering on one side and management on the other side. It's sort of like what I did in my own Nuclear Science Division. I took two of our people we already had and I knew very well, and asked them to be 50 percent time assistant directors. So, I had Janis Dairiki, who worked with me the whole time, and she kind of did the administrative, all that kind of stuff for me, and Mike Zisman, who was a postdoc with me and got a Berkeley Ph.D. with Bernard Harvey in nuclear chemistry. He was doing the science and technology stuff. So, I did that. I would imagine that was Dave's model.

07-00:27:46
Burnett: So, you had that hire for those two deputy directors.

07-00:27:49
Cerny: Well, they were already there, so I just convinced them to work 50 percent time on this and do research 50 percent time, so we didn't have to hire any new people. Okay, now moving to the Nuclear Science Division, where I spent most of my time and effort.

07-00:28:16
Burnett: So, you were associate director of the laboratory as a whole, and now, you're still division head, which is an associate director position in the staff chart? You're the head, but it's called the associate director of nuclear science.

07-00:28:32
Cerny: Yeah, we've got two titles. I don't know why, but you're in this box—maybe it says both—but you're running a division. Seaborg was associate director at

large, just for advice. So, what about the Nuclear Science Division? Well, the Nuclear Science Division, at that time, I started July 1, 1979 as did Hermann Grunder, a major figure who was in the Accelerator and Fusion Division, he became its head, too. So, we both start at the same time. The Nuclear Science Division had three accelerators: we had the eighty-eight-inch cyclotron that we operated, we had the SuperHILAC, and the Bevalac, that were operated by the Accelerator and Fusion Division. So, Hermann and his gang operated them. We operated the cyclotron. I had scientific responsibility for all three accelerators and the theory group.

So, I had to have scientific directors in place for each of those. Ultimately, there was friction because the budgets in Washington were being set in different ways, and pumping money into Hermann's division for running these other two accelerators, which might not exactly be what we would have wanted if we could control it. So, Hermann and I tried to get along using the scientific directors because every so often, though, you could see where Hermann thought it'd be really neat to make this improvement on the SuperHILAC that cost a lot of money, to keep his people busy. But in fact, that the science might not have needed it so much. But anyway, we got along fine.

07-00:30:22
Burnett: So, this is a problem that Shirley talks about as well, in that the funding is directed from Washington, it's earmarked there, and I'd like to know a little bit more about what that means. He says that 90 percent of the LBL funding came through Washington, and was earmarked for specific work. He says the Department of Energy thought it ran everything, but it was a pastiche organization—it had a little bit of NASA, it had a little bit of energy concerns—but his accusation is they didn't seem to really care about the programs, specifically. So, the administrators had no ability to expand, no ability to build new equipment, to think forward, to think ahead.

07-00:31:16
Cerny: Oh, no, that's not true. I mean, once you got the money, you could do—within the bounds of the money—what you said you were going to do. If you wanted to improve the SuperHILAC by adding something, if they agreed you could do, they weren't going to tell you how to do it in any great detail. It was just how much money did you have, and are you striking the right balance between getting research done on the SuperHILAC, which is my concern, and running it, which is their concern.

07-00:31:44
Burnett: So, it's the flow that's earmarked from Washington.

07-00:31:48
Cerny: It is true, in those days, we were working with two administrators named George Rogosa and Enloe Ritter, and you did not have to make the number of trips to Washington that people do now. We did a lot more on the telephone. I

mean, later on in my talk, you see, I'm going to Washington and all these national meetings, but I was not having to go to Washington very often to talk to DOE managers. Now, it's much worse.

07-00:32:07
Burnett: Is that because they want more direct oversight?

07-00:32:10
Cerny: Yes, they want more direct oversight, for whatever reasons. Whether they're more anal-retentive or that's whatever's happened, but yeah. So, it's much more you're going there. I could do a lot by phone, and you know, they would come out for reviews. One of the things I immediately had was the issue with the Bevalac, and so, I want to back up in time a little bit to get to the Bevalac.

In 1974, much earlier, Albert Ghiorso had conceived the idea of using the SuperHILAC, which was a heavy-ion accelerator of moderate energies, on the hill a little bit above where the Bevatron was as an injector for the Bevatron. If you built a transfer line, you could get heavy-ion beams to go down into the Bevatron, accelerate them in the Bevatron, get heavy ions at quite high energies, which nobody had. This was called the Bevalac and it was cheap. On the scale of any of these things, it was cheap. There were two phases—this was the initial phase, this started in 1974. High-energy physics, having lost the 200 GeV proton cyclotron, and anyway, always going to higher energies, high-energy physics at LBL was not going to go work at the Bevalac because they didn't care about heavy ions. They care about protons hitting protons. The beams for the initial phase were iron nuclei up to 2 GeV per nucleon.

07-00:33:45
Burnett: When you say "having lost," you mean in the 1960s, there was that original bid to build a giant proton accelerator, and that got delayed and delayed and delayed, and eventually, that became Fermilab?

07-00:34:03
Cerny: Didn't get too long delayed, but yes, then it became Fermilab. It's certainly true that Berkeley high-energy physicists—which you can read in Catherine Westfall's article in the Chicago series on the history of science, it's a very excellent article—were used to controlling the accelerators, right? We weren't going to let anybody else run on ours, so that's one of the other things we had to break down after all these years, this inherited attitude that all the LBL accelerators were ours, but ultimately, had to become national laboratory accelerators. By then, the physics community had had it, and I'm sure that's one of the reasons they went to Fermilab. Of course, the other reasons were the malaise that had set in at the higher echelons at LBL.

The initial problem with spending more to develop the Bevalac is high-energy physicists weren't interested. The people at the SuperHILAC had more than they could do, and they didn't really want to go do higher energies. Luckily, and I'm not going to go in a lot of detail here, Europeans saw the light, and the Japanese, and Shoji Nagamiya, who was a very successful Japanese experimentalist, came over, did work. A number of Germans who were very good came over and worked with people at Berkeley because Germany was building this UNILAC accelerator at Darmstadt, which has been very, very successful. It's a big, major lab, and it's where I went on a sabbatical I'll come to later.

In 1974, then, Sessler decided that he wanted to try to sort this out. So, he created this Accelerator and Fusion Division, and initially put Ed Lofgren in charge of it. By the way, Ed Lofgren is still alive, and he's 100 this year, I read. He was in charge, and they were in charge of the Bevalac and SuperHILAC operations. Sessler eliminated the Nuclear Chemistry Division and the chemists and the people who weren't doing nuclear chemistry and physics, but were doing other kinds of more radio-tracer stuff. They all went into the Material and Molecular Division. So, he created the Nuclear Science Division, and the physicists who were starting to work at the Bevalac, like Les Schroeder and Harry Heckman and his people, came over to us.

So, about then, Bernard Harvey is the head of this Nuclear Science Division and we picked up the people from the Physics Division, including one theorist from there, named Świątecki, who was very successful.

07-00:36:59
Burnett: I'm not sure if I understood all of that correctly, but nuclear science ends up being nuclear chemistry plus low-energy physics, would you say?

07-00:37:10
Cerny: Now, it's also relativistic nuclear physics at the Bevalac. So, what we've created is an accelerator and a whole new research area, and by 1982, when more improvements have been made, in phase two you can accelerate uranium up to 1 GeV per nucleon. So, it's the beginning of accelerator-based, relativistic, heavy-ion physics, and that was in our division. Even though the energy certainly sounds like it's high-energy physics, that's in our division, and still is. So, the people in our division are running at RHIC right now, and are running at CERN, but it's still called the Nuclear Science Division.

So, we started to get some new people in. I do want to mention, of course, for the record, that the Bevalac did a lot of early medical work. Perhaps a third of their time, certainly a quarter, was spent on pioneering work using heavy ions for cancer destruction. They used carbon beams and neon beams and argon beams. That was really good research and so that worked out well.

07-00:38:30
Burnett: It's a cylindrical structure, right, the Bevalac? Oh, no, it's two accelerators that are joined?

07-00:38:40
Cerny: So, it's a linear accelerator and then it's a transfer line, and then it's a big, circular accelerator.

07-00:38:46
Burnett: Right, and then the beam would come out at a station and there would be a patient there?

07-00:38:50
Cerny: Yeah. There's a special beam line set-up there that's kind of like a hospital room. I used to be part of showing the dignitaries around because it was under our division. It was exploratory research. The work was followed up by some Japanese nuclear medicine types, very, very well in Japan. So, a certain time was scheduled for that, and it worked very well.

07-00:39:31
Burnett: The Bevalac, the first phase is completed mid-seventies?

07-00:39:36
Cerny: Mid-seventies, you're doing experiments with iron nuclei to 2 GeV per nucleon. You're realizing that you have to build particle detectors that are going to have 1,000 elements in them, ultimately. Because when you get to uranium on uranium, you are going to make that many particles. You want to measure all of them, you're kind of using sodium iodide or plastic detectors to get all these signals. It's small compared to what they're doing at Geneva right now and you have to have state of the art computers to record the data.

07-00:40:08
Burnett: Right, the data, the number-crunching for that, must be—

07-00:40:12
Cerny: Right, and it's unbelievable by now. It was tough, then. But these German physicists were very good.

07-00:40:20
Burnett: Just be clear, for our audience, here, that it's 2 giga-electron volts. When you say "2 GeV."

07-00:40:27
Cerny: That's 2 GeV, yeah. So, it's MeV, and then it's GeV, so it's 1,000 times the MeV. So, Poskanzer has always played a major role in this research; Art Poskanzer started as a real nuclear chemist that we brought from Brookhaven. I'd written a *Scientific American* article with him, and we did a experiment together on the Bevatron, which was very interesting. He actually got the nuclear chemistry award for that kind of work, then he did so much Bevalac-type work, and moving over to do the stuff at RHIC, that he actually, in 2008, got the Bonner Award in nuclear physics, which is given to a nuclear physics experimentalist. It's very neat. So, Art was always there. He was the scientific director, though, and by 1979, he was probably tired of doing that.

It was clear in 1979 that we needed new scientific leadership. We needed two new leaders. The most important one is we needed to bring in an outside scientific director for the Bevalac even more high-profile, and I had actually, that February, even before I became division head, had permission from Bernard Harvey to write a whole bunch of people and ask for their comments on the future of the Bevalac. I had twenty-five to thirty responses, none saying that new science blowing us away has occurred yet, but there's a lot of potential here, and it's not expensive, and you should continue to do it. So, I felt happy that I really touched base with the United States and the community in Europe and Japan. So, that was fine, and I was happy with that.

Then, the letters said, "We need a new director." So, we'd arranged to post an ad in *Physics Today* to come out in July '79. I knew who we wanted to hire—we wanted to hire Howell Pugh, who was educated in England but a distinguished physicist who'd been five years, I think, the assistant director for math and physics at the NSF. He was very aware of this area, and so we wanted to hire him. He'd actually been at Berkeley when I was an assistant professor, so I even knew him. He was there for three years and we overlapped at the eighty-eight-inch cyclotron. So, he was actually keen to come back to Berkeley, keen to do the job, he was a perfect appointment. By October 15, 1979, we'd hired him. So, that's getting started.

07-00:43:23
Burnett: When had you been appointed?

07-00:43:26
Cerny: July 1. I hired him by July 15. I had a majority vote, a unanimous vote from the people in our committee that voted on appointments.

07-00:43:42
Burnett: When had you begun soliciting feedback about the status of the Bevalac search?

07-00:43:48
Cerny: I wrote that in a letter in February 15, and asked for responses by April 1, or something, so I had them. Then, I felt comfortable going forward. I probably couldn't have stopped the momentum anyway, but I was very comfortable with what was going forward, and the improvements that led us to the uranium beam were already in mind, and they weren't that expensive. So, we knew we could get there. Then, Howell Pugh had a really great career, and was scientific director for quite a while. Unfortunately, he actually died in '89. He had a very strange kind of leukemia that finally caught up with him in '89, but he was a really good addition.

The other person we needed was a scientific director and operations manager for the eighty-eight-inch cyclotron. Now, when I was at Oxford on my sabbatical, '69-'70, there were a bunch of crack people at Oxford, I mentioned before. Just their careers weren't going anywhere in the United Kingdom.

David Scott was one of them, and he was really good—really good. So, I went back and talked to Bernard Harvey and said, “You know, why don’t we invite him over?” It made sense to work with Bernard Harvey and see how it might go. So, we brought him over and it worked out like a charm. So, he was probably a senior scientist in three years. He had clear leadership potential.

We made him the scientific director of the eighty-eight in 1978, and then, Michigan State University, which has an extremely strong nuclear physics program, extremely strong, Center of Excellence for the National Science Foundation for years, from the sixties, now has the next big accelerator that is going to be built there. A great place in nuclear physics, top of the line. So, they hired him as a chair, named professor, so unfortunately he left to go there—because, of course, our physics department wouldn’t hire him. So, he went there, and actually, to finish that story, after a number of very successful years as named chair and then he was provost at MSU and then he became the chancellor of the University of Massachusetts at Amherst for six or seven years. He had quite a career, and he grew up on the Orkney Islands, and was sent to boarding school to get his education on the mainland. Anyway, so he was great, but he was gone, and we didn’t have anybody else.

We knew who we wanted—we wanted a guy named Robert Stokstad, who was at Oak Ridge, and we knew he might want to come to California because his parents lived out here. So, by November, I’d hired him, and he came in January. He didn’t become scientific director for a year, but we had in place what we wanted. Within no time, I had what I wanted, and the leadership team that I thought was going to work. The SuperHILAC, Dick Diamond, who’d been a career person here, continued to do that job and he did it fine. Then, I really had an advisory committee that I wanted. So, I had Howell Pugh, Dick Diamond, Bob Stokstad, the head of the theory group, Norman Glendenning, Luciano Moretto who was a fellow nuclear chemistry colleague, and me. So, this was kind of the main advisory committee to me, and I felt very, very good that we’d moved that fast.

07-00:47:44
Burnett: It’s often said that a big part of management is hiring well, so it seems like you did that.

07-00:47:51
Cerny: Or sometimes hiring poorly, and it goes the other way.

07-00:47:54
Burnett: Right. Well, you want to avoid that, if you can.

07-00:47:57
Cerny: This was really breaking down our LBL insularity, also, against hiring outside people. So, we got through these, and Howell Pugh and Bob Stokstad were well known in the community, so it wasn’t exactly like they were unknowns. That worked. Our other big problem was here we had three big accelerators.

How many theorists did we have? Well, we have two. We have Norman Glendenning, who'd been there with us a long time, and Świątecki, who'd come over from physics, and that was it. So, Washington was saying, "Where is your theory department?"

So, luckily, LBL had decided to create a new category called the "divisional fellow," and our division uses them a lot. A divisional fellow is somebody who's got a Ph.D. and maybe six or seven years of experience, and they're hired in. It's like an assistant professor, at the end of which, you're tenured. So, if you're a divisional fellow, it's a five-year appointment, you have a mid term review at year two-and-a-half, and a final review. If you make it, you're a senior scientist. So, we really took advantage of that, and still have, and so, we immediately hired—before I did it, the division did it—Miklos Gyulassy, who actually had gotten his bachelor's and Ph.D. in theory at Berkeley physics department, and another theoretical physicist named Jørgen Randrup, who was from Scandinavia.

07-00:49:26
Burnett: We may need to get those names spelled out.

07-00:49:30
Cerny: So, that was great, so we had some really good young people, and we've continued that kind of appointment to this day. Gyulassy was particularly interested in Bevalac work, and he's actually, ultimately, got raided—because Berkeley physics would never hire anybody—to Columbia. He even got the Lawrence Award not too long ago. So, he was terrific, and I'm sorry we lost him. He'd lived in New York, but he got all his education here. So, that got us in pretty good shape.

07-00:50:10
Burnett: In pretty good shape, I mean, this demand from the feds to get a theorist, in contrast to earlier times, when Berkeley defined what it needed. So, I'm getting a sense that there are now federal or national best practices that a laboratory needs X number of theorists, or a certain cohort of theorists, to be part of it.

07-00:50:42
Cerny: Absolutely.

07-00:50:44
Burnett: So, they've worked out some kind of formula for the running of a laboratory, and this is coming from on high, a little bit?

07-00:50:50
Cerny: Well, I mean, it's just the review of the visiting committees. Anyway, by this year, we also had developed regular visiting committees. The visiting committees would come through and say, "Where is your theory group?" The theorists like to be employed, too, right, so they would ask those questions. We're having all these theory Ph.D.s, why aren't you hiring any? What had

happened at Berkeley was high-energy physics, the physicists always went to higher and higher energy, and then the theorists went to higher and higher energy, and they didn't rehire at the lower energies. So that some nuclear chemists, like myself, just moved into low-energy nuclear physics because I wasn't competing with anybody and they didn't care.

There were no nuclear theorists in the physics department, and only one at LBL, Wladek Świątecki, who had been around for a long time and was very good. He didn't go to high energies, but he was an LBL staff member, not a campus physics professor. He came to our division. Yes, there were physics theorists, distinguished, but they were doing protons on protons. So, we just had to get our part of our act together.

07-00:52:05
Burnett: This is how you like to work as well. You like to get feedback from outside, you like to see how things are done, you get lots of input to run things. This was very normal for you, to just have this kind of outside feedback.

07-00:52:24
Cerny: Right. We've had really good people, and we've used this divisional fellow route now a whole lot in our current senior staff. Many, not all, but a large number of them came in as experimental or theoretical divisional fellows and got promoted. We didn't actually have to deny anybody a promotion, but some people came and got assigned right away to work on these experiments in Geneva, at CERN, and they were never around here and there wasn't much interaction with Berkeley, and they finally decided they should just work in Geneva and forget our part of it.

07-00:53:08
Burnett: This is the time when the reports are coming out, at the end of the seventies and the early eighties, that Europe is outspending the United States about 2:1 in physics, and Germany and France each have physics budgets that are approaching that of the United States, even though they're a fraction of the population. Over time, is there a bit of a brain-drain to what's going on in Europe? You just mentioned there are these single cases because that's where the action is. CERN is doing a lot of it.

07-00:53:57
Cerny: It's the subject of my next comment or two. [laughter] I don't know how close we are to the end of this tape.

07-00:54:14
Burnett: I'm fine, actually, cutting now, and then we can take up, if we want to go really in depth with something, let's maybe do that. You've got a short comment to do? We've got a few minutes.

07-00:54:28
Cerny: Okay, we can continue in this, the things related to it. What we really faced in doing all this in the five-and-a-half years, I then had to manage separate

reviews of the eighty-eight-inch cyclotron, the SuperHILAC, the Bevalac, and the theory program. Separate, completely separate reviews, to prove we were up to snuff. I have a t-shirt to prove it, that one of the staff made.

07-00:54:57
Burnett: Oh, you're not kidding.

07-00:55:00
Cerny: "Survivor," it says. [laughter] We got through all that. In the big shift that Sessler did, I think a lot of the people that were kind of burned out at research and would have gone to the Molecular Division, just decided to retire. He would count that. So, we didn't really have to deal with that. The hard-core people were relatively younger, or had decided to retire early. To one of the older people I had to say, "You know, either you retire a little early or I have to fire this guy that works for you, but you're way going to be paid enough. I will pay you the difference for the remaining two years to get to your maximum salary, but I want you to retire." So, he did. That all worked. Didn't cost this person any money, kept the younger person in a job. He retired, but I paid him the difference. We had to do a few things like that.

Your question is a good one. The United States has been very competitive in a lot of these fields. Clearly, at the moment, we're not competitive in high-energy physics, but we're still very competitive in running at RHIC or other things that are going on. I did look at our senior staff ratio, and I looked at saying, "Okay, in 1981, we're relatively mature, we've had our fiftieth anniversary, what's the ratio of people who got their Ph.D.s in the United States to people who got them abroad?" Fifteen out of twenty-one had gotten them in the United States, so that's about 70 percent. Today, we have fifteen senior staff members, only four of whom got their Ph.D. in the United States, and that's 27 percent. So, the trend you see of the decline of nuclear chemistry and nuclear physics is certainly real in this country, and there are all really good people in Europe and Asia.

We've hired a lot of those as our divisional fellows, who've now moved up to be senior scientists, because there are completely open, international searches. We'd be more than happy to hire distinguished United States Ph.D.s if they were there. This is mirrored many places, that it's somehow lost its charm, but it hasn't lost it in Japan and in Europe. Europe, particularly Germany, is so successful, and France is pretty successful. England kind of gave up nuclear science.

07-00:57:58
Burnett: Seems to be the case. That's maybe what you encountered at Harwell, when you went there in '69-'70: people who are so talented, but kind of unmoored. They didn't have a place for them anymore in the United Kingdom.

The United States has done fairly well in physics and nuclear chemistry is still competitive, but in terms of American science, a lot of it is being done by people who become Americans, who come from elsewhere.

07-00:59:00
Cerny: I'd say our senior staff has shrunk quite a bit, from twenty-one to fifteen, and that's probably understandable, too. They've been theorists, but one of the new senior staff, actually, is a very crack experimentalist, working on RHIC, these big experiments, who got a Ph.D. in Amsterdam. He was just really great. Others will come in that way, too, yeah. This trend, declining interest in nuclear, is just the reality, here.

07-00:59:40
Burnett: It had its tremendous centrality because of the Atom Age and all of the promise that went with that, but the pool is smart people who can do math, right? For those people, there's computer science, there's the life sciences, there are these other fields that have so much growth and so much potential, that it draws them away, presumably, from this field. That is this ongoing challenge for people in your fields around the accelerator programs, is how do you attract people to this kind of research?

07-01:00:26
Cerny: The white males left about 1973 in the United States, and they're gone. I don't know where they went.

07-01:00:33
Burnett: Silicon Valley, probably.

07-01:00:35
Cerny: But this is looking at science and engineering—it would probably even include computer science. Women have certainly helped bring that up some, but it's interesting. We're just importing clearly a lot of foreign talent in computer science and those areas like that.

07-01:01:01
Burnett: As well, yeah, absolutely. Perhaps we should continue this discussion on the next tape, so we can be fully confident that we can continue.

[End Audio File 7]

[Begin Audio File 8]

08-00:00:11
Burnett: This is Paul Burnett, interviewing Dr. Joseph Cerny, for the University History series. This is session four, tape eight. So, Dr. Cerny, we were talking about your time as associate director of Lawrence Berkeley Laboratory. As part of this job, you get tapped to do committee work, issues of national

importance. So, can you tell us a little bit about some of the committee work that you did.

08-00:00:44
Cerny: Okay. So, the first thing I need to talk about is that nuclear science has something called the Nuclear Science Advisory Committee. This committee was established in October, 1977, by the nuclear physics community. It reports to the main managers in the Department of Energy and the National Science Foundation. It really follows a model set up in high-energy physics, where high-energy physics has a group called HEPAP, which is the High-Energy Physics Advisory Panel. This was created to take the politicking out of scientific decisions, and to try to get a community consensus on what you wanted to do, so you wouldn't have end runs to Congress or whatever, trying to push a particular accelerator or program, but to try to have the field agree on what they wanted, and rank order the priority, typically done after a lot of planning and a week-long workshop, where you hammer out a long-range plan, and you hammer them out for a number of years, depending. I think that's been critically important to high-energy physics, and it's certainly been critically important to nuclear physics.

08-00:02:11
Burnett: That's been going since '77?

08-00:02:13
Cerny: I don't know when HEPAP started—earlier than that [ed. note: 1967]—but that's when we started. If you look at the charge, the current charge, I'm going to read it because I found it. Just part of it—you're appointed to one-year terms, for a maximum of three, and you're to "provide advice to the director of the Office of Science in DOE and the assistant director of the Mathematical and Physical Sciences Directorate of the NSF," so, that's really high up, "within the field of basic nuclear science research." You talk about what are the research frontiers, what is the consensus on new facilities, on operations, on instrument development, on training of nuclear scientists, on the relationship of nuclear science to other fields.

I was asked to be on this for three years, so I was on it in 1980. It seems to be calendar year appointments: '80, '81, and '82. As part of this, I was asked to be the chair of its manpower subcommittee in 1980 and '81. So, when I got going, then, actually, they got started in '77. They had the first long-range plan in 1979, and the second one was to be completed in December, 1983. That's going to tie into some of my later comments because though I was off NSAC by 1983, I was still involved in all this. This is going to tie into accelerator planning, which one of the main things has really been if nuclear science wants its next big accelerator, what order are we going to put them in?

What's the most needy one? Is it electron physics? Is it high-energy, heavy-ion rings? Whatever. Otherwise, they'll just fight with one another, and so, that's why it's really worked out. In 1979, the long-range plan found a critical need for a high-duty factor electron accelerator, which means running the beam as much of the time as you can, rather than little pulses separated by long periods of time. So, electron beams with variable energies up to several billion volts, or several GeV. We will see that this will lead to a recommendation for a machine, an accelerator capable of going to 4 GeV. Before getting to that, I can't remember a lot of details of what we covered in the years I was on NSAC.

NSAC had done a manpower survey in 1978, and so they thought they needed another manpower survey. The chair of NSAC was a distinguished MIT nuclear theorist who thought, really, that only nuclear chemists and women nuclear physicists were suited to do surveys. So, he'd asked a woman to do it in '78, and so he asked me to do it in '80. Fay Selove was her name, and she's a nuclear physicist at Penn. She did a great job, and we've been great friends. She actually helped a lot because she agreed to be on my committee to help because she'd done the one in '78. Well, this turns out to be a huge amount of work. I'll actually return to a survey I did which was much more interesting, when I talk about what I did after being a central campus administrator because I was asked to do something for NSAC on education in 2003 and do a lengthy survey, which could be done electronically.

08-00:06:46
Burnett: The purpose of this is to report to the government at different levels—the NSF and also the Department of Energy?

08-00:06:54
Cerny: Yeah, because almost all of the nuclear physics is either done in DOE or NSF. Typically, the high-energy physics, is much more done in DOE. The survey took a lot of time, so for example, we wrote everybody that we could find in September, 1980, with a questionnaire to the principal investigators—you know, who are the people? You're trying to find all the people who are nuclear chemists or nuclear physicists, what universities are they at, are they a postdoc, a graduate student, or a professor? What's going on? You try to get them to respond.

So, we tried to locate them and struggled, and we did a final report in 1981. It was just a harbinger of what's going to happen, so if you looked at the 1980 report compared to the 1978, you see there's another 10 percent drop in the number of nuclear chemists and physicists, and probably nuclear chemists are going away even faster. The assistant professors were only 13 percent of the nuclear science faculty, which was the same for physics generally, and physics had the lowest percentage of young faculty members in science and engineering fields.

08-00:08:34
Burnett: Period?

08-00:08:35
Cerny: Period. So, these things are useful.

08-00:08:42
Burnett: I guess I'm a bit surprised that the NSF and DOE weren't already tracking this information, that they needed to get a committee together to go get this data. Is that still the case, or do people need to report manpower statistics to the NSF and the DOE as part of the course of getting funding, for example? I imagine it's a lot more informatized or computerized now.

08-00:09:12
Cerny: Yeah, you'd think so. [laughter] I remember then that when I was division head, and later, DOE would write us and say, "Okay, how many nuclear people do you have on your payroll?" I don't know what they did about campus, actually, but certainly at the lab, they wrote that. You know, how many Ph.D. students you're educating. You would send all these forms back in to them because they really wanted to try to track it. The National Science Foundation was probably doing something similar. This sort of thing is really slow to do, particularly when you're doing it by paper and the mail.

Later on, in 2003-2004, as chair of the NSAC subcommittee on education in nuclear science, I was given the names of all the principal investigators who had grants, and their email addresses. Then, you could email them this letter and ask them to fill out the forms; you could ask them for the names of all their recent Ph.D. graduates and what their email addresses were, and things like that. You could ultimately do a pretty good job.

So, this 1980-81 report was weak. Now, you can actually, if you work at it, do it. So, we did a very good job on the nuclear science survey in 2003-04, and high-energy physics at DOE really wishes that someone would do it for them, but of course, the high-energy physicists are not going to take the time to do all this work.

So, anyway, we turned this 1980-81 report in, and then actually, we said things were such a mess, we don't recommend doing one in 1982 because with all the budget cuts at that time it would seem premature. Right when I got off this, the agencies were really interested in this idea of an electron accelerator. A high-intensity, high-energy electron accelerator. The head of the DOE component, a guy named James Leiss, really, clearly it wanted to happen.

08-00:13:28
Burnett: Is there a scientific rationale for this, why it would be important?

08-00:13:32
Cerny: It's another of probe you have, and you know, it's an electromagnetic probe rather than a nuclear probe, and so you get all kinds of different interactions. You can hope to see the effects of the gluons and the quarks bouncing around inside the nuclei. You get a whole other set of information. It's an area I know

the least about, in what I do, because I understand heavy ions and light ions, but I've never done this kind of work. Like SLAC, you know, it has a huge electron accelerator, and it's different but similar, where it really gives you this information. SLAC, for example, in their experiments, could prove to you that actually, the protons weren't just a proton that looked all the same. It had little things in it that they originally called partons, and now, one imagines, they're the quarks or the gluons. You get different results, but SLAC was a very high-energy, very low-duty cycle accelerator. Very, very low. A huge pulse every two microseconds.

So, this electron community wanted a continuous beam, or so close that it won't bother you. So, those people are saying, "Okay, chair, we think we maybe should do something now. Let's have a long-range plan." Anyway, the community in addition had another report I'll mention on wanting an electron accelerator. So, by spring of 1983, the Nuclear Science Advisory Committee appointed a panel on electron accelerator facilities. Now, that's different from a subcommittee, so it didn't pick people off NSAC itself. They set up a panel, and the panel, I'm sure the scientists and engineers on it were selected by high level administrators in Washington and the chair of NSAC. It's not some random thing the chair would want to do.

So, they put me on that panel, versed as I am in electron studies. [laughter] I think they put me on for perspective. They also put Hermann Grunder from LBL. He's the associate director of the Accelerator and Fusion Division and he was put as the chair of the technical sub-panel. So, Hermann Grunder is a key figure in these discussions. D. Allan Bromley was made the panel chair, even though he wasn't on NSAC any longer.

08-00:16:48
Burnett: He was also on the other committee that you had for as chair of that committee that dealt with closing the two programs.

08-00:16:56
Cerny: Yeah, and he wrote this report also, and he knows how to write. He went on to be science advisor for Bush I. So, they put him on. As time was passing here—'80, '81, '82—there had been a Nuclear Science Advisory Committee subcommittee on electromagnetic interactions, with a guy named Peter Barnes as chair. They had recommended explicitly a high-intensity, high-duty factor electron accelerator able to achieve 4 GeV. Now, perhaps unfortunately, this subcommittee had noted that lower energies could be interesting also. Maybe they felt they had to say that. So, as a result, five proposals came in for a meeting in Washington in February of '83 for our panel. The five proposals: one was from University of Illinois for 750 MeV, and one was from the National Bureau of Standards to go to 1 GeV, and they were complete proposals, but it rapidly became clear that the momentum was for a 4 GeV accelerator, and there wasn't going to be enough money for another one.

Anyway, there were five, in all. Argonne National Lab came in with one that would go to 4 GeV. MIT appeared to come in with one that would go to 4 GeV, from the Bates Lab, and the Southeastern University Research Association, which was a consortium, also called SURA, came in with one that would go to 4 GeV.

08-00:18:50
Burnett: That would have been based in Raleigh or something like that?

08-00:18:53
Cerny: That was to be based in Newport News, Virginia, where NASA had just abandoned a space effects radiation laboratory. So, they were going to use two huge buildings in Newport News, really big, one of them, so they had some structure. Well, it was a very interesting meeting, where I learned a lot. I don't know how many hours they had to present—we were there probably two days, they probably had two presentations, and then we would have conversations and stuff. Everyone gave a good presentation except MIT.

MIT had written down some numbers of things that they thought they could do, but they had essentially done no work. It's in the panel report, it says that, the current scientists and engineers were so stressed out just running what MIT had already and some improvements that would get them to 1 GeV, that they hadn't done anything. It was even stated, for example, that with the present manpower, no design work toward construction approval would be started for half a year. So, you're now making a proposal to this distinguished panel for a huge accelerator, and you really haven't done any work.

08-00:20:39
Burnett: Did they just think that they were so overwhelmed with their own operations that they didn't have a chance, so they just threw their towel in before they even got out the gate?

08-00:20:53
Cerny: Well, you know, my notes, it's not in there. A couple of hapless MIT administrators had come down as part of this, and one of them just kind of talked about MIT in general, and the other guy just said he wasn't sure why he'd come, or why they'd brought him. It was really bizarre.

08-00:21:23
Burnett: That sounds like they just assumed they didn't have a chance and they just went through the motions.

08-00:21:28
Cerny: But then they shouldn't have gone. That wasn't a requirement, to go. It was like ignoring the fact that there's this panel because somebody wants to build a 4 GeV accelerator. It must surely have been known by then that there was going to be a 1983 long-range plan, and that planning meeting was going to be in July of '83. The community wanted to get this approved by NSAC in July

of ’83, and not to be told, “Well, we’ll start thinking about it in six months because we’re the only people who can build this.”

08-00:22:09
Burnett: Oh, you think there was more of an arrogance, that, “We’re MIT, and if you want something built, we’ll build it. We’re busy right now and we’ll get to it?”

08-00:22:19
Cerny: Yeah, it could be, and there were certainly other comments saying, other places, they wanted to build something like this but they would hire in senior people as faculty to help build it and run on it, and MIT wasn’t doing that kind of thing either. So, a lot of things in this report are very clear. My comment about the two administrators is not in the report, but it was memorable. What you really had then was an interesting dynamic because you’re left with two proposals. They’re very different proposals, and they’re complicated.

The technical sub-panel went out for site visits and reviewed all these proposals prior to this meeting, had gone and talked to all these people and had arranged for questions and things to be answered before then. The Argonne design was certainly state of the art, but the sub-panel wasn’t convinced that you could actually get to 4 GeV. What they knew for sure was you could not get above it. There was a real consensus in the community that you’d like something, that if we discovered things were really interesting at 4 GeV, there’d be a way to go above that energy.

08-00:23:57
Burnett: Right, to tweak the machine, yeah.

08-00:24:00
Cerny: Yeah, or get up to 6, or whatever. That became a real issue, and beyond me, in terms of the technical stuff. Again, this Peter Barnes committee had said, “You know, we’ve got to have high intensity, high duty factor, all that, but it’s got to be at least 4 GeV.” Now, the Southeastern University Research Association had a more traditional design of a stretcher ring, and running through a racetrack, you would have an injector and you’d go around the ring and come back and go through again, and ultimately get up to 4 GeV. There’d be no reason that you couldn’t extend that by available techniques. They were all comfortable with that.

SURA was a bunch of young people. They were eager, and they had a lot of things lined up. They had things that appealed to the panel and our declining number of nuclear physicists. So, in addition to a design, and this had been looked at really critically by this group, basically it was a solid design and the monies were probably similar, but this consortium offered five new tenured Commonwealth professorships, twenty-five new tenured or tenure track experimental nuclear physics faculty positions, and at least five new tenured or tenure track theory positions. They clearly had their ducks lined up. It was sort of a green site. It certainly didn’t have the airport connections that

Argonne does. But basically, the panel just went with this Southeastern University Research Association.

08-00:26:14
Burnett: What are the major universities that are part of the consortium? Roughly?

08-00:26:20
Cerny: Today it is a very large collaboration of southeastern universities. The panel agreed that the Newport News approach was probably the best. So, this panel report was sent to NSAC, and NSAC's report recommends exactly going with it. It's very straightforward and it says, "This should be the next accelerator built in nuclear science."

08-00:27:27
Burnett: Was it built?

08-00:27:29
Cerny: Yeah.

08-00:27:32
Burnett: Do you know its name?

08-00:27:33
Cerny: Yeah, now it's called Thomas Jefferson Lab. At the time, they had a name called NEAL for it, National Electron Accelerator Lab, and nobody liked that. So, by the time for funding someone had wisely changed its name to the Continuous Electron Beam Accelerator Facility, otherwise known as CEBAF. Their final recommendation was that: "The proposal of the Southeastern University Research Association for the construction of a LINAC stretcher ring accelerator system capable of providing high-duty factor beams throughout the energy range, from .5 to 4.0 GeV, be accepted."

08-00:28:33
Burnett: It must have been an incredible proposal because without much on the ground except a couple of buildings, they were able to show that they had really thought about how they would do it.

08-00:28:49
Cerny: SURA had done the technical stuff. They had satisfied this technical subcommittee of the panel. They'd done all their homework, they had lined up a whole bunch of people who were willing to go there and work. They'd done all the calculations and everything they needed to do it, and you had to have a time plan and a budget plan. It was a great presentation. It was like, you know, I've observed things before where some young guys and gals come in and they want to do this, right, they really want to do it and they've worked hard and put it together, and they're going to carry the day because they've just done it that well.

It's interesting that in my notes, here, there's also a letter from Argonne, writing to the chair of NSAC, perhaps, or Washington. Anyway, they went to protest in Washington. So, in Washington, ANL management had gone down and talked on June 13. By June 19, I have a letter from Argonne top management discontinuing their appeal, explicitly stating that they were worried about a stalemate and nothing getting built. Now, if we reflect upon the fact that the NSAC long-range planning major and only meeting is July 11-15, I think they knew they better throw in the towel before that committee stalemates and the people who want a relativistic heavy-ion collider say, "You know, if you guys can't make up your mind, we've got something else over here we'll be glad to go with," or whatever.

So, I think it was also because this DOE high-level administrator, Leiss, really wanted it to happen. The last thing he would want would be a stalemate. It got built. At some point, they needed higher-level management than DOE had, and they told Hermann Grunder he had to go do it. So, Hermann Grunder went there and did it. He knew how to do it and used superconducting magnets rather than conventional magnets. Later on he went to be director of Argonne.

08-00:31:38
Burnett: We'll fact-check it. [Dr. Grunder became Director of the Thomas Jefferson National Accelerator in 1985 and Director of Argonne National Laboratory in 2000].

08-00:31:40
Cerny: Anyway, so DOE also tapped Hermann to build it. It was really one of those things that you were happy about. All the work had been done, and Argonne had done the work too, it's just that their design was pushing it to get to 4. Two was easy; 4 was pushing it, and beyond, it was impossible.

08-00:32:03
Burnett: You said that you didn't really know why you were there because it's not really your expertise, or you suggested that. Perhaps one of the things you brought to the table was this kind of equanimity that you have. You have experience with a lot of other kinds of projects, and it's almost a fresh pair of eyes to judge feasibility, from almost an outsider's—

08-00:32:27
Cerny: Well, particularly if you've got a technical sub-panel that can do all that for you, and then you just have to kind of evaluate how important are these many professorships? Argonne was going to get two. University of Chicago was going to add two faculty positions.

Changing topics. Then, I leave, strange enough, into my third assignment for that year.

08-00:33:13
Burnett: This is also 1983?

08-00:33:14
Cerny: Yeah. Well, it ran on. So, in 1983, the National Research Council decided to establish a Physics Survey Committee. It was going to do physics through the nineties. So, a big survey, and they appointed me as a member of the National Academy of Science's Physics Survey Steering Committee, and the chairman of the Nuclear Physics Panel. So, a guy named William Brinkman, who was really great, he was then at Sandia National Lab, was the chair, he's the guy that called me up and said, "We'd like you to do this job." What are you going to say? "No, I'm busy?" So, it was going to be an eight-volume survey of physics. I was chairing the Panel on Nuclear Physics with eight other members.

So, our panel met in May '83, and then we knew, right, because we're all nuclear physicists, that we all had to go to the NSAC long-range plan meeting, right? Our group—and it had better say the same thing, or it's not going to work. So, we all went to this week-long workshop in July, while they were doing the long-range plan, and we went as participants and not observers. We were at Wells College in Aurora, New York, on one of the Finger Lakes. It was actually pretty nice. We naturally endorsed the recommendation of CEBAF, so that's our key recommendation, too, in this volume. At the meeting, the relativistic nuclear collider raised its head, and so we also endorsed that, ultimately, to be RHIC, after a series of things, as the next thing for the field. So, the field is doing this planning, this has gotten in place, and it's the only way you're going to proceed, right, if you're doing expensive facilities and you want to go with the community.

08-00:35:56
Burnett: Yeah, consensus.

08-00:35:59
Cerny: Now, the review process, though, for the Academy is really interesting. So, we met in May '83, we met in the summer, and we met in January '84, and we wrote our report. Then, it had to be reviewed by eleven selected community reviewers. They're actually picked and named in here. I don't know whether that stuff was sent to me anonymously or not. So, then, after we had incorporated their comments in the final manuscript, we submitted it in May '84. Then, it went to another swath of reviewers, every comment of whom had to be replied to. You don't necessarily have to change your report, but you've got to reply to the comments. So, we were finally done in August '84. The reports weren't all released until April 9, '86, but there was a press conference in '85.

In any event, I could never have done this, except I hired a writer. I hired a guy that was really good—Fred Raab. He could do the technical rewriting and the editing and reply to the last group of reviewers. I could go through and mark up the text. Anyway, so, we did that, but what was interesting to me is that every so often, Fred would disappear, and he'd just be gone for three weeks, which was a little stressful for me. It turned out he was a private detective on the side, in addition to being a technical writer, and he was off doing drug busts of some type.

08-00:38:06
Burnett: I smell a *Masterpiece Theatre* miniseries. A private detective?

08-00:38:16
Cerny: Well, I couldn't believe it either, but we made our deadlines. But he wouldn't tell me—well, he didn't know, I guess.

08-00:38:29
Burnett: Was it some kind of DEA thing, that he couldn't tell you? [laughter]

08-00:38:32
Cerny: Yeah, I mean, he couldn't tell me he was going to be gone.

08-00:38:38
Burnett: That's an added headache that you didn't really need. Just to recap, the total responsibilities you had at this time, you have a 50 percent faculty appointment, so you are teaching, doing some experimental research?

08-00:39:00
Cerny: Right, actually discovering beta-delayed two-proton emission. [laughter] Off giving talks about that because it's really interesting.

08-00:39:11
Burnett: On the lab side, you're associate director of the laboratory, and you were associate director of the research of the Nuclear Science Division, and you are on three committees in rapid succession, or concurrently. Sounds like you needed several private detectives to help you.

08-00:39:36
Cerny: It may have been the timing worked out that most of this stuff was done before spring quarter because if I was teaching three classes a week, too, it would have just been hell. I know I didn't miss any of the meetings.

08-00:39:52
Burnett: Well, if these were all academics, they must have really worked the schedule a bit around the calendar.

08-00:39:56
Cerny: Oh, no, they'd just call them. They just tell you when they're going to be, unless you're the key figure. I think there is just enough time to go through the other thing I was going to do. So, interestingly to me, I had gotten somehow on some hot list of people who might want to be a higher administrator.

08-00:40:30
Cerny: Okay, so it just turned out I must have been on some list because I was phoned by the search committee at Rensselaer Polytechnic Institute to ask me if I'd like to be provost. So, I knew a little bit about them, and I said, "Well, you know, I'm going East, anyway. I'm going to a meeting in Finland in early

1983, and so I'll swing by." I was interviewed by the search committee, which is a broad swath, a large number of people. They'd been having trouble for some reason filling the appointment. I really liked the people. It was very nice. There was a lot of rapport.

Now, the president was George Low, and George Low was a technological superstar. He joined NASA early and he was in Houston, and after the Apollo 1 fire, he took over the Apollo project. He made it click and work and get back on the schedule. He then became NASA deputy administrator for about seven years, and was pushing the Space Shuttle, but I remember we had this link-up between one of our satellites or whatever, and the Russian one. He did the Apollo-Soyuz link-up. Anyway, he was president, so I was there, I was taken to his house, and he had these mock-ups that people that worked for him had made of this stuff, you just wouldn't believe. It was like a stage set.

08-00:42:16
Burnett: For a sci-fi film.

08-00:42:18
Cerny: Yeah, I mean, God. He was really smart, so that was a plus. I went back again and talked to all the deans. First, I guess you did the search committee and talked to a few people, then I went back and talked to all the deans, and then, actually, Susan and I went back and looked the place over.

08-00:42:43
Burnett: So, you got fairly far along?

08-00:42:45
Cerny: Oh, yeah, I got way along, and I got a huge formal letter, offer letter. A good salary and a free house because it turned out he didn't like the house the president had, so he'd gotten another one, and so I could have that one. It was clear you could learn a lot from working with him. I don't know why they had trouble filling the position, but I think I could read people pretty well, and he was fine. I just finally decided I couldn't do it, so I said no. The offer letter was just amazing, and I had a copy of it. The Berkeley chancellor even wanted to see a copy of it.

08-00:43:42
Burnett: When you decide to stay, it's a very nice thing to have in play, I suppose. So, you wanted to stay at Berkeley?

08-00:43:52
Cerny: Yeah. The more I thought about things like that, which I'd done when people have had unfortunate changes in their career, is you go to somewhere like that and the person in charge wants you, but then something happens, and the next person doesn't want you at all. In my case, you'd left Berkeley, which you really adore, and you wouldn't like it. I know somebody from Michigan State who went somewhere else and I'm sure isn't really happy, and somebody from Brown who was the dean of the Graduate Division went somewhere in

the South. The first job was fine, but the administrator changed, and then it's not too good.

Now, I knew at the time, it was really interesting, because George Low told me that he'd had a melanoma. This is during '83. He actually died in July '84 at age 58 of the melanoma. So, I would have had that kind of a problem in spades, and with a new president coming in who might have a totally different idea. Anyway, I had really pursued it, I liked the people a lot. I don't know, I saw the things that you could do there, and it's like no one had been representing them very well. It was strange because then, Hopkins called me, probably in '84, and the faculty search committee at Hopkins called me. I said, "Sure, I guess I'll go look." So, I went to Hopkins, and again, I got along really well with the faculty committee. The president was something … different.

08-00:45:39
Burnett: This was for just a full professor?

08-00:45:41
Cerny: No, this was to be provost at Hopkins. When I got there, you looked at the organizational structure, and most of the deans came in through somebody to the president. There was a provost, but one of the interviews was just bizarre. I went out to see the dean of the medical school, and it was one of the worst two experiences in my life. The guy was insulting. He didn't know if he needed ever to talk to the provost, but if he did, he might make time for me. I'm only exaggerating a little. The other guy that I'd had the worst experience with was the dean of the medical school—not the current one—at UC San Francisco. He was horrible, too. Just their egos were beyond belief. I just had to fight briefly with the guy at UC San Francisco. When you really looked at the job at Hopkins, it was kind of a public relations for the community, you know? It wasn't the job that I wanted at all, so I didn't pursue getting any formal offer. As a compliment to me, anyway, whoever took it left in two years because they couldn't stand it, and the chair of the faculty search committee called me again, about 1986, and said, "Have you by any chance changed your mind?" I didn't tell anybody about my experiences with the dean.

08-00:47:24
Burnett: But you had refused, so that told them something, I suppose.

08-00:47:28
Cerny: Yeah, I just had dropped out. I didn't say why.

08-00:47:34
Burnett: It seems that you've had these experiences—early in your graduate career, Harvard, you visited, and you just got this wrong kind of gut feeling about the climate, let's say.

08-00:47:46
Cerny: I never got to Harvard. I just knew it. I didn't ever visit Harvard. I would have gone there, but switching to nuclear chemistry, don't go to Harvard—you want to go to Berkeley. So, no, I mean, I've been to Harvard, but I didn't visit then as a graduate student, prospective graduate student.

08-00:48:14
Burnett: You had your experience with Oxford, and you can get a sense of the human culture. The human climate is a huge factor, of course. You were very happy here, it seems.

08-00:48:33
Cerny: Yeah. So, in concluding, so along the way I'd gotten an Alexander von Humboldt Senior U.S. Scientist Award, and so I decided to take it for seven months, starting in January '85. Five-and-a-half years would be up by then, and I had a five-year appointment. I didn't want to continue because Dave and I, we were getting along and I really liked him and all that, but anyway, I was tired of being an associate director. Just tired of it. So, I said, "It's a good time just not to get reappointed," and so, then Susan and I and her youngest daughter age 16 went to Darmstadt, to this big heavy-ion accelerator outside Darmstadt. It's in the woods. It's really nice. You stayed in an old mansion that had been broken up into a lot of these little apartments. During our stay, Susan's parents and her other two children came to visit.

So, I was told several things. One, there were these two research groups that I might want to work with, and I'm sure the head of one of them nominated me for the Humboldt. I was told by both of them that they'd be happy to let me work on their equipment, and they would help me, but I couldn't join any of their ongoing research. So, as long as I proposed my own experiment to do on their equipment and got it through their program advisory committee, that they'd help me. I did that—I did a major proposal for one of them and got it approved, and we did the experiment. Though it didn't get finished then, it ultimately got finished and I'm on the paper. The other was an even more high-powered group. I wanted to use their equipment. We didn't have to do a proposal because it was an exploratory thing—could you do this?—because they worked in very heavy elements. I said, "Well, I think vanadium-42 might be a direct proton emitter, and maybe we could see it." It turned out their equipment just couldn't go down from mass 200 to 42.

The leader of the smaller group also told me, "You know, it's really unlikely that you'll be invited out by anybody once you're here, but at the place you're staying, we often have parties."

08-00:51:36
Burnett: Interesting. So, there's parties for the visiting people because they're aware that other people in other countries have things like parties? [laughter]

08-00:51:49
Cerny: It was unbelievable, but by then, I was—

08-00:51:52
Burnett: That's fascinating, yeah. So, there are these very, very strong differences from not just country to country, but from college to college, very, very strong.

08-00:52:09
Cerny: Yeah. I mean, I have a senior visitor come, they can propose something, and then can join my experiment. I've never had anybody say, "You can't join my experiment," and we did a really great experiment with a visiting professor from the Midwest who was on it. That's fine. I mean, I don't care, and if they proposed it, then they could be first author.

08-00:52:43
Burnett: There's an etiquette here that may not be clear to outsiders, that in general, the way it's done is if there's an ongoing experiment and there's a visiting person, they're usually welcome to participate in some way?

08-00:53:01
Cerny: Yeah. The one was their super-heavy element group, and I wasn't going to horn in on they're looking for an element – and add my name to their hunting for element 109, anyway. The other group did miscellaneous and interesting stuff, but miscellaneous. It's beyond me why they thought that, but why couldn't I be the sixth name on a six-person paper? It was interesting, and I survived it.

08-00:53:34
Burnett: Well, were you invited to do your own experiment? They welcomed you to use their equipment, I suppose.

08-00:53:41
Cerny: Yeah, they did the experiment [that I proposed]. It worked and it just needed to be repeated. It worked fine, they put effort into it, and I can't complain about that. But it really was surprising to get there, you're there for seven months, and you're told you have to do that. Then, you've got to really get through things in front of some program advisory committee in time to have an experiment for you while you are there. You've got to move right along, figure out what you're going to do. This Humboldt Senior Scientist Award, it's a monthly stipend, so you get a year's worth of it. You can use it later, so I used the seven months, and then you can go back and use three, and ultimately use it up. It's a very nice deal, but it's not a cash award. You have to be in Germany to get it.

08-00:54:30
Burnett: You have to be affiliated?

08-00:54:31
Cerny: Yeah, and you should be working at that accelerator, or whatever.

08-00:54:38
Burnett: It was an honor and you also got this kind of interesting cultural exchange, as it were.

08-00:54:47
Cerny: When I was leaving, there was a really nice lunch that one of the head people there certainly had for me, you know, a few of us over.

08-00:55:11
Burnett: So, that takes us through into the next phase, and so, on the next session, we'll talk about the transition to the Graduate Division.

08-00:55:21
Cerny: Right. I'd applied for very few jobs, but the few I applied for, I'm not sure I was truly interested in them, anyway. I never got approached. On the other hand, with the jobs I've gotten, I've never asked for. So, when I'm there, Berkeley asked me, "Would you like to be graduate dean?" So I am in Darmstadt, being interviewed in a room with this six-person committee, of whom I know one member in Berkeley, and I'm being asked questions about what I would do as graduate dean, and particularly, how would I solve graduate affirmative action? God, well, you know? I figured it out once I had the job. I think that the committee reviewed my accomplishments as Chemistry Department chair and contacted a lot of the Berkeley Chemistry faculty as references.

08-00:56:34
Burnett: Right, right. Well done. The art of the job interview that is unsolicited, you should write a book on that.

[End of Interview]

Interview #5: June 4, 2014
[Begin Audio File 9]

09-00:00:07
Burnett: This is Paul Burnett, interviewing Dr. Joseph Cerny, for the University History series. This is interview session five, tape nine. Dr. Cerny, we last left off talking about your Humboldt Fellowship period in Darmstadt, Germany, in the early 1980s. You also mentioned last time that you must have gotten on a hot list of people who might want to be a higher administrator in the university, so RPI tried to poach you as provost, as did Johns Hopkins. You intimated that perhaps that made you think about your own plans vis-à-vis Berkeley. So, you're in Darmstadt and these things have transpired. How does it work out that you end up back at Berkeley in a new capacity?

09-00:01:05
Cerny: Right. Well, it certainly was a surprise. There's some background to this, so I thought I would really talk about the Graduate Division at Berkeley and then what was going on at Berkeley while I was at Darmstadt, and then come into it. In the modern era at Berkeley, I'd say there were two graduate deans before me. There was Sandy Elberg, who had been dean from 1961 to 1978, and everybody viewed him as the dean's dean. He also had been around before the Free Speech Movement and through everything, so probably from about the end of Seaborg being chancellor all the way until toward the end of Bowker being chancellor. He was very successful and a well-known campus figure, and probably the only administrator who was around that period. Then, William Shack was appointed from '79 to '85, it turned out, and he was a distinguished African American anthropologist who had joined the Berkeley faculty in 1970, and they had actually had him be department chair from '72 to '73, of anthropology, which is known worldwide as the most difficult department at Berkeley.

09-00:02:32
Burnett: Difficult for whom?

09-00:02:34
Cerny: For the chair. They supposedly throw chairs when they are having an unpleasant faculty meeting.

09-00:02:41
Burnett: That's fascinating.

09-00:02:47
Cerny: I thought first I'd mention what the dean's historical role is at Berkeley. Of course, it's to provide leadership in all the areas of graduate education, admissions, degrees, fellowships, and all that. There's an enormous number of groups a dean belongs to, and so the dean actually represents Berkeley at the University of California-wide council of graduate deans, and that's all the campuses, including UCSF. Then, they meet three times a year at different campuses, so it's really quite interesting, as you move around the system. I

actually ended up chairing that for two of my years that I was the dean. I was also on the Joint Graduate Board for joint programs between UC overall and the CSU system. That didn't meet very often, but that was interesting. Nationwide, there are two big units. So, there's the Association of Graduate Schools, which is under the Association of American Universities. The Association of American Universities are the sixty-two or so top research universities in the United States, with two from Canada, Toronto and McGill. Years ago, actually, the AAU started focusing on graduate education, but after the Second World War, it all moved around and the Association of Graduate Schools was developed. It meets as a group once a year in the fall. Then, there's the Council of Graduate Schools, and that's all the degree-granting schools—non-profit—above the bachelor's, and so it's quite a large meeting and there's one meeting held somewhere. Again, you sort of feel obliged to go. In addition, there's a very important group called the Dwarves. The Dwarves, when I joined it, there were ten, and so that's nine private universities and Berkeley. It's the usual suspects of Harvard, MIT, Yale, Columbia, Princeton, Brown, Cornell, Chicago, Stanford, and then us.

09-00:05:08
Burnett: Where do the Elves fit in? It's called the Dwarves and the Elves, is that right?

09-00:05:11
Cerny: Yes, the Elves tend to be the associate deans and the staff. So, the Elves meet while the deans are off in one room, meeting. The Elves are where the associate deans and the key staff are talking about the nitty and gritty, while we're talking about high policy.

09-00:05:28
Burnett: Right, okay. I see.

09-00:05:30
Cerny: Of course, the difference is, they have a whole private university outlook and Berkeley has a public university outlook. Many of their problems, we don't have, and vice-versa. It was still interesting to be on.

09-00:05:42
Burnett: How often did they meet?

09-00:05:44
Cerny: Well, actually, they met twice: because the Association of Graduate Schools meets once in the fall, this group would get together for a dinner meeting during that. In addition, we would meet at somebody's campus in the spring, so it's really quite nice. So, then you got to go around the different campuses and see them in the spring. So, we really enjoyed that. It's a very collegial group. Okay, so that just keeps you busy if you don't want to do anything.

09-00:06:14
Burnett: Right, your basic membership obligations and your community obligations.

09-00:06:19
Cerny: So, now, the dean of the Graduate Division is a unique position because in addition to being a central campus officer and dealing with the administration, the dean is, in fact, an ex officio member of the Academic Senate committee, called the Graduate Council. The Graduate Council is virtually totally responsible for graduate education. It's a key senate committee. The Senate actually views the dean as the chief of staff for the Graduate Council, but certainly, I attended every meeting as a voting member. Associate Dean Joe Duggan would come as support, and Carol Soc, who was our director of academic affairs and an outstanding staff member, would come also. So, on the one hand, you're part of the Academic Senate, and there is one other position—and I can't remember what it is—that actually, there's an administrator who can vote on a senate committee, but it is not a central campus one. So, that kind of makes you unique.

09-00:07:27
Burnett: Yeah, and there's three graduate students on that committee, too, right? There's twelve faculty, three grad students, nominated by the Graduate Assembly?

09-00:07:35
Cerny: Right. Yeah, and actually, they turned out to be just fine. When I talk about the Association of Graduate Student Employees, that was more of a political operation. This was fine, and we needed the graduate students, so that was really good. Now, the Graduate Council is actually final on many graduate matters. I mean, the law school's not part of this—they manage to insist that they were a separate entity and they are not part of this. They admit their own students, they have a quota, but we don't talk about them, I guess. Other than that, the Graduate Council would propose new Ph.D. programs or master's programs and handle everything, and advise the dean and the dean's office what to do. I think a new degree program, obviously the chancellor or the executive vice chancellor would have to say, "Yeah, we think that's a good idea," too, but there's a system-wide Coordinating Council for Graduate Affairs. It would really have to be the one that would say, "Yes, Berkeley can have a new Ph.D. program," and whatever. So, the Graduate Council, I really enjoyed working with them all. It was all very collegial, and Joe Duggan was outstanding, and Ms. Carol Soc was always great. Now, as we talk about reviews and things in addition to all the departments that there are, there are actually twenty-five graduate groups, and graduate groups were put together to be interdisciplinary, cutting across departments, and yet, not enough people to form a department or whatever. So, there are twenty-five of them or so, and three examples would be: ancient history and Mediterranean archaeology, where you obviously bring in different people from different departments, energy and resources, or logic and the methodology of science. So, you can imagine where they come from, but they're actually under the Graduate Council and us to monitor. There aren't lots of people in them. In addition, just to keep us busy, the graduate dean is also ex officio on the I-House board of directors, and the chancellor's the chair. I remember some grumbling from

chancellors about that. So, that was done, it was set up long ago when life was easier.

09-00:09:56
Burnett: I guess so.

09-00:09:57
Cerny: I guess so.

09-00:10:00
Burnett: There's a huge purview for the Graduate Council. They're supervising any changes that happen.

09-00:10:05
Cerny: Yeah, everything.

09-00:10:07
Burnett: And admissions of all the graduate students?

09-00:10:10
Cerny: That's what we're taught. As we'll talk about, of course, the graduate dean's office does the admissions, but the advice on standards, or this, that, or the other, or whatever, would go to the Graduate Council. Any time you wanted to make a change, or I wanted to end up appointing all kinds of people in departments, and so you had to go to the Graduate Council. For example, when we needed affirmative action advisors on the faculty, which I'll get to, you had to go to the Graduate Council, convince them we had to do something, this was it, they had to approve it. Once they'd done it, I and the chair of the Graduate Council would write a letter to all the departments saying, "Starting now, we're going to have to have a graduate affirmative action advisor in each department; please select one." We wouldn't check with anybody. I mean, you kept the higher administration informed of what you were doing, obviously, but basically, we were final. So, while I was off in Darmstadt doing research, in '85, what was happening was Berkeley's Office of the President was really pushing a lot of the campuses to be more affirmative, and Mike Heyman was completely in agreement with that. So, he was very big on pushing that. He had a lot of help because he appointed a vice chancellor for undergraduate affairs, who had a large entourage, and they could do outreach, this, that, and the other. The chancellor admits the undergraduates, and generally speaking, the colleges do. The colleges get a quota or something, but basically, the chancellor does it in a way that's very different than the graduate students, who are admitted by departments.

09-00:11:52
Burnett: Was that always the case? Was it decentralized to the departments at a certain point?

09-00:11:57
Cerny: Well, I think admitting the graduate students, I bet, was always part of that. It just had to be.

09-00:12:01
Burnett: It had to be a part of the departmental…?

09-00:12:02
Cerny: Yeah, it's a departmental thing, the graduate students. You don't know what the undergraduates want to do, but the graduate students – So, I think that must have been from day one. Allegedly, one year Heyman was sufficiently irritated at something, he just said, "I'm going to admit all the undergraduate African American students that applied," which perhaps were 100, and that was a year when the African American percentage went from 5 percent to 8 percent, and that was fall '84 to fall '85, so that may be when that happened.

09-00:12:36
Burnett: Okay, so you're not sure exactly what year that was, but you suspect?

09-00:12:40
Cerny: I suspect that, and so if that's true, it makes it a hot issue on the campus to some people.

09-00:12:48
Burnett: Yeah. What was the reaction to that? I mean, that's a story that you heard at the time?

09-00:12:53
Cerny: Yeah, I heard it at the time. Well, the trouble is, I think, you would then be admitting more people with no support structure and all the things needed to go with it. I don't really know all of what the vice chancellor for undergraduate affairs was doing. I know they were trying to provide some support structures, but if you admit a lot of people, you really need to do it in a way where you put extra resources in and if I had to guess, I would guess it perhaps didn't happen, but I don't know. Okay, what the chancellor wanted from the new graduate dean was more affirmative action, and Dean Shack was being criticized for not being very aggressive. Now, since the graduate students are admitted, we just said, by departments, and they're acting with the Graduate Division, or Graduate Division does quality control on it, Heyman had given a talk, he always met with the senate chairs at least once a month, and he'd given a talk in a standing meeting in fall '84 that the new graduate dean needed to work on affirmative action because by then, Shack had said he wanted to retire in the coming end of June '85. He expected that the new graduate dean would take a major role in this. Professional schools were doing better than the letters and science type schools.

09-00:14:26
Burnett: Can we go back to that time for a moment? Everything that happened subsequently, in terms of increasing—because the language changes and the initiatives change after SP-1 and SP-2. We're getting ahead of ourselves a little bit, but for the mid-1980s, tell us what affirmative action meant, or the range of what affirmative action could mean, in terms of policy. Is it merely a blanket quota at the end of the line, or is it some recognition that, as you said,

there needed to be supports or work to help underrepresented populations get to the stage where they could access the university system?

09-00:15:10
Cerny: Well, I think it was different for undergraduates and graduate students, since for undergraduates, I think they probably were working on support programs, and to some extent, I think at the graduate level, probably nothing was being done, and probably the complaint was the departments were spending all their money on white male students, fellowship money, and not particularly worrying about women or underrepresented minorities. In general, when you look—certainly like in the physical sciences—finally progress was being made of having more female graduate students and whatever, except in some intractable areas like physics, where it's been very slow. That was something we started to work on a lot. So, undergraduate affairs had recruiters, they were out doing it and getting good students, and they were just being admitted by the admissions director, looking where these people had qualified to get in and the basic rules, and they were in. Graduate admissions is different. You have a graduate admissions committee; they're looking at what they want to see, people want researchers in a certain area, or someone to come in with them, or whatever. The Graduate Division at that point was certainly just looking at, you know, do they have the basic requirements to be a Berkeley graduate student?

The Senate, however, what the Senate wanted in a new graduate dean was somebody who cared primarily about graduate education and research, and was not a flack-catcher for affirmative action. Dean Shack, in his meetings—because he was meeting with some of the faculty—said that in his meetings with Heyman, Heyman behaved like a monomaniac on the subject of improved graduate admissions. The key senate chairs feared that the search for quality ethnic and minority groups would be the central role of the Graduate Division. So, four senate chairs, very important ones, the Graduate Council, the Senate Policy Committee, the Committee on Educational Policy, and the Committee on Research, all wrote Vice Chancellor Park about their concerns. They were really concerned, so all these people stayed in their role as chair for the fall '85 semester to wait and see who had arrived as graduate dean, and to talk to that person, so they all stayed. I don't think the chair of the Budget Committee, who wrote its own letter, stayed. So, the faculty really, well, they wanted some assurance that the new graduate dean would not admit unqualified minority students, but other than that, there were a lot of other concerns. The Graduate Division style had not adapted itself to modern circumstances. It was a mammoth and slow bureaucracy. There weren't enough fellowships. They were handed out slowly. Nothing had been decentralized. It was all very frustrating for the departments because they had these great students that are all panicked, you know, "if we don't get this chemistry student right now and make him an offer by the 20th of December, he'll go to Harvard." So, they're in a panic, but you know, if you have a monolithic, slow bureaucracy, they don't notice that. So, there was a lot of concern about that. There were things going about the transition between having teaching assistants, change to graduate student instructors; there were a lot of graduate student titles which, over the years, had been everything in sight, had to be regularized for various bureaucratic and other reasons. Clearly, all of a sudden, people really thought TAs [teaching assistants] or GSIs [Graduate Student Instructors, the title for teaching assistants at UC Berkeley] perhaps ought to be trained, and perhaps should speak English. So, these themes would go through my encounters, which I won't dwell on, but there was a lot of complaints about people in math who were the upcoming GSIs from foreign countries, who were very smart but didn't really speak English.

09-00:19:11
Burnett: Right, right, so they needed English language supplemental, remedial training or certification, and that kind of thing.

09-00:19:17
Cerny: Exactly, which they weren't getting, right.

09-00:19:20
Burnett: Could you just reflect for a moment on the nature of Berkeley bureaucracy? Is it a mystery to you, or having spent time looking at other institutions—this is not the first time I've read about or heard of the notorious Berkeley bureaucracy. What accounts for that?

09-00:19:45
Cerny: I don't really have any experience dealing with other campuses in this. I think it was probably pretty easy while, you know, the troubles were on and everything, and when Elberg was in place, and it turned around some. I mean, I certainly remember grumbling in Chemistry when I was chair about the Graduate Division being so slow, and we needed a way to get these hotshots an offer. So, I think they just went down and beat on the appropriate people and got it, right? I mean, it's Chemistry. Physics probably did the same.

09-00:20:18
Burnett: For those departments that had the clout, there's an ability to move things in their direction, but I wonder—

09-00:20:28
Cerny: It was asleep. Shack was a really nice person, but he was not an administrator. As we'll see, Elberg was somewhat proactive, and I don't think Dean Shack was very proactive at all. So, I think that's why all these kind of things had come to a head, together.

09-00:20:51
Burnett: Right, he was coping and he was reacting to problems as they came up.

09-00:20:54
Cerny: Yeah, and I think he viewed an administrator as somebody who sat in their office and adjudicated on position papers and tried to represent the best interests at the Chancellor's Council. You know, "Yes, we need more

fellowship money," and all that, but didn't really worry about the fellowship competitions or the best way to be fair to everybody or whatever.

09-00:21:14
Burnett: Right, or ways to improve the system as a whole.

09-00:21:22
Cerny: One of the things I really had to do is when you bring in these graduate students here, okay, if they're in the state of California, you're okay, but if they're out of state, we had to have what are called tuition waivers. Well, they weren't waivers—they were money—but they were called waivers. The first thing I'd do was convince the faculty that they weren't waivers. So, that was a nightmare because you wanted to get all these good out of state graduate students, and how do you have the money? So, I remember spending a total rainy Sunday going through how the tuition waivers had been handed out to the departments. It didn't make any sense.

09-00:22:00
Burnett: So, there's a question. I had always assumed that tuition waivers are a kind of, an accounting maneuver on the part of the university. The university's paying itself. There's no lost income, in a sense.

09-00:22:18
Cerny: There's lost income. You had to pay it. You had to have a fund, or you had to pay that. So, you had, over the years, accumulated a certain amount of money that was so many tuition waivers that you could go fight with the Office of the President for more, but that was money that they, perhaps, expected.

09-00:22:39
Burnett: This is money that's coming from the total core budget?

09-00:22:43
Cerny: Fellowship support.

09-00:22:46
Burnett: Right, and it has to be allocated.

09-00:22:47
Cerny: I think some of it came probably from a big grant from Earle C. Anthony, who also was responsible for the Pelican Building. I think money just had to go into those things, and as tuition kept going up and up, it was more of a problem. So, when I looked at how they were distributed, there were obvious anomalies in the system, and one of them was because people had gone into Dean Shack and said, "You know, we really need these." Statistics was a department that would go in, and they were very effective at it, and they said, "There aren't any U.S. statistics students and they're all from abroad, and they won't possibly come unless we give them the tuition waiver." So, then I had to figure out how to get a policy out of all this mess. So, then I said, "Okay, so here's the maximum percentage of the new students these departments get, and how many tuition waivers would help that, and then what's 20 percent

beyond the max?" That kind of might be the place I'd go. Then, you'd see Statistics and some other department just way above. So, I said, "This is unfair." Then, I talked to Carol Christ, who was the provost of Letters and Science, when I did that and said, "Here's all my data, here's my analysis. I'm just going to tell them it's not fair." So, she agreed, and I told them, and I acquired a certain number of enemies and went forward.

09-00:24:10
Burnett: That's also your style, a kind of "get the data and then present an argument based on the data."

09-00:24:21
Cerny: Yeah. So, you know, the tuition waivers and this whole thing, I didn't even go to these notes when some of these things were so complicated. There were these in-candidacy fee offset grants, which were the same thing as tuition waivers, that you could hand out to needy people. Well, we spent Council of Deans in the UC System's meetings on *this*, how we were ever going to get that to work? So, I don't know with the outrageous tuition now and everything what's going on because I'm not dealing with that. That was another source of concern.

09-00:25:02
Burnett: So, equity is an issue, but also just a kind of economic rationalization, so that there's fairness, but there's also a distribution of resources that's relatively equitable.

09-00:25:15
Cerny: Of course, after one year, a domestic student could become a California resident, but an international student can't, so then we'd try to get a deal set up where if they got advanced to candidacy, then they had vastly reduced tuition for three years.

09-00:25:30
Burnett: That happened eventually, right? Later.

09-00:25:34
Cerny: That happened. We fought over it and fought the Office of the President, fought Larry Hershman, fought, fought, fought. He didn't care.

09-00:25:40
Burnett: Right, and a good incentive for the students to finish up quickly.

09-00:25:44
Cerny: Exactly, yeah, because then they've got three years, and it ought to be a two-year thesis. Yeah, I didn't go into all the time we devoted to things like that.

09-00:25:55
Burnett: Well, we can just have an assumption, really, that each one of these things, that we can assume there are weeks and weeks of meetings involved in each of these points.

09-00:26:06
Cerny: The other big thing that the faculty was concerned about is that the reviews of graduate programs had just kind of collapsed, that they slowed down and no one was pushing them, and the faculty was unhappy that nobody ever heard any results from them. So, that needed to be revived. Those things were going on. So, you can imagine, however, when I was sitting there, I must have had an office I was using at the big accelerator lab outside Darmstadt, when I was on a speakerphone with this group of people, of whom I knew one, named Ignacio Tinoco, who had been just chair. He was chair of Chemistry right after me, but I guess this was after being chair. He was on that committee, and mainly, they wanted me to talk about affirmative action. Also, the search committee had been characterized by the Senate as being very soft on affirmative action. Well-known people like that. Well, knowing now my personality, how can I answer a question I haven't had a chance to think about, right? So, I don't have many data on this subject, and so, I probably didn't do very well. I couldn't imagine that I did. Now, the other hand, Tinoco was on this committee, and though I don't know for a fact, he knew that I was the person that had introduced gender into our chemistry department, so he knew very well that I could accomplish what I wanted to accomplish. So, my guess is that—and perhaps some rumors that I'd had these other job offers—they said, "Well, maybe we should think about him."

09-00:27:46
Burnett: Maybe. I mean, I'm assuming that that might have been part of it? You mentioned once that you like to think you have a face that people can trust, but behind the face, I think there is a reputation for dealing with people fairly, and that seems to come through in the conversations I've had with others with whom you've worked over the years. So, that's part of the story as well, I imagine.

So, when you said they were soft on affirmative action, it wasn't a priority in their search? Or they felt that affirmative action, they were strongly in favor of affirmative action? What did it mean for them to be soft on affirmative action?

09-00:28:30
Cerny: Oh, I mean most of the departments were just ignoring it.

09-00:28:36
Burnett: Whether they supported it or not, they were not doing anything about it.

09-00:28:39
Cerny: Yeah, I mean, they're already looking for the best graduate students, and you'll explain that you're sending all the money to the best white male students, right? So, the Office of the President, having understood that, would send that money to the different campuses, targeted for women and minorities. There certainly were programs that we enhanced and embellished that were particularly focused this way. So, that gets me back. So, I think I didn't get appointed till around the end of August of '85, and the first thing I did was I

went around and talked to all these senate committee chairs, all the ones on that list, plus the budget committee, plus the chair of the search committee, and just showed them who I was, and told them I was going to work on the problem, and I didn't know how, yet. The obvious things—we'd need somebody to do some recruiting, we need to get graduate affirmative action on our radar screens so I know what's happening—but just to prove kind of who I was. I also talked to all the key academic higher administration chancellor's cabinet types, where I also learned that they felt the Graduate Division had gone to sleep, and a lot of the policies just needed to be shaken up and things needed to happen, and do some innovating thinking, please, and things like that.

09-00:30:18
Burnett: Is that when you created this committee? It's the Graduate Affirmative Action Advisory Ccommittee?

09-00:30:26
Cerny: Right, it certainly is. Just before, since you want to put this in, in the process of all the Senate being very concerned with this, there had also been a whitepaper by Trow, around 1980, which perhaps had come out for the previous dean's search, but was still useful. His committee had suggested that research also ought to be a component of the graduate dean's office. The Senate bought into that idea, so I think they'd tacitly agreed that that was a good idea, and that even Vice Chancellor Park and Heyman had agreed that should Chang-Lin Tien step down from being vice chancellor for research, we probably should combine that with the graduate dean, which is what, we'll return to, was happening, but I didn't actually know that.

09-00:31:21
Burnett: I had heard that those roles at other campuses, were they already combined at that time, or was Berkeley the first?

09-00:31:33
Cerny: We probably weren't the first. Dick Attiyeh was a very long-standing dean at San Diego and very good, and he was doing both. UCLA had two people. I think we were the only two, actually, once I did it. Dick Attiyeh was somehow doing it—and it's a big university, too.

09-00:31:56
Burnett: Yeah, a big research university.

09-00:31:57
Cerny: It's a big job, unless you've got really good people working for you, and luckily I had really good people working for me. I wanted to talk about things I wanted to do, and then I want to get to that committee. I was really lucky, so I started in early September, and Chancellor Heyman had his senior staff retreat in mid-September, at Lake Tahoe. So, I got to go, and so I was up for three days, or whatever it was. It was really great because I got an introduction to everybody, knew who they were, and could really land knowing what the

cabinet looked like and all that. So, that worked out perfectly, and Heyman recognized, from my point of view, that that was a chance thing that was really good. The only standing meeting that I haven't mentioned was, of course, there were all of these standing meetings, is the Graduate Assembly, of course, had to meet with the chancellor once a month, and I always had to go to those meetings, too, you know? It was always a very irritating meeting, one could say.

09-00:33:07
Burnett: Was it kind of militant?

09-00:33:10
Cerny: Yeah. In a lot of the things—and in the Association of Graduate Student Employees and in this—the third-year law students didn't have anything to do. They just coast after two years. They get two years of being in that for their degree. So, they'd stick around making trouble for the third year, so they were happy to do that. So, they were around.

09-00:33:29
Burnett: Getting practice.

09-00:33:30
Cerny: But there weren't a lot of physical scientists and engineers, right, in the Graduate Assembly, right? They had things to do. So, that was just irritating. I just sat there and kept my mouth shut and learned whatever Heyman said. "Joe, why don't you try to solve that?" Which probably were insoluble problems. Then, in the course of this, what did I learn? I learned two wonderful things. First, I learned that I inherited this unbelievable grad student database, which had been set up by the associate dean from Anthropology named Eugene Hammel, in the sixties.

09-00:34:03
Burnett: Yeah, '62, really early.

09-00:34:05
Cerny: Which really was unbelievable, and very few universities had anything like that. Secondly, Judi Sui was hired. She's gotten probably honors in math here and had directly gone to work for the Graduate Division, and she knew how to manipulate the database and do everything. So, there I was, another goldmine just sitting there, virtually untapped. Graduate Division had looked at it for time to degree because that was easier, but attrition isn't that hard if you know how to run a database, right? It wasn't like there were a bunch of papers I could read, "Look what we've already done." There wasn't anything like that.

09-00:34:49
Burnett: Just raw data that had to be processed.

09-00:34:52
Cerny: Yeah. Well, I'll just mention this at the moment—so, the third key part of this was that I also knew I wanted to do some kind of institutional graduate

research, and so I asked Martin Trow, who was the director of the Center for the Study of Higher Education, who to recommend. He recommended Maresi [Nerad]. Well, I met Maresi, and she was just terrific. So, she was a graduate student and getting her Ph.D. in education, but she quickly finished that up, and then she was kind of a postdoc for a year, and then I made her a staff member, and then she was the director of graduate research. So, we just agreed what we wanted to do, and she would do all this stuff. We had some good ideas together and she had good ideas separately, and she knew about communication, and all these many things I won't have time to talk about. She knew, of course, it's very important to talk to the graduates at some point later on, you know, that I'll go out and explain more to the graduate assistants rather than once a year, and all kinds of things.

09-00:35:54
Burnett: So, a real resource in many different ways.

09-00:35:56
Cerny: A real resource. So, Maresi was great, and Judi Sui didn't mind if I called her up at night and said, "You know, Judy, it would be so great if you'd calculate this stuff for me tomorrow." In 15 years, I probably did that once a week because we had all this information. She didn't mind. So, I had some really good people.

09-00:36:15
Burnett: Maresi Nerad said that just by the same token, I think it goes both ways, she said that you read everything, everything you gave her to read. If there was something she had, something she needed your feedback on, and no matter how big it was, how voluminous, the next day or whenever it was, you would have it ready. I think that was something that you set an example when you were working with your team about the kind of work you did. So, although maybe you called this person up in the middle of the night—

09-00:36:48
Cerny: Well, it wasn't in the middle of the night because she was happily married. [laughter]

09-00:36:53
Burnett: —in the evening or after-work hours, to get some information, your door was always open and you were always available as a resource. So that's part of the story, too.

09-00:37:04
Cerny: Yeah, so as long as we're on people, I'm going to go quickly back to the issue. I worked with some really good people, so in all this, I had three associate deans. These were 50 percent associate deans, 50 percent time. One of them was Ian Carmichael, who I met when he reviewed Chemistry, and we'd become very good friends. He was very interested in reviews. He was from Geology. I appointed him to revive all the reviews, and then when I also became provost for research, then I had him also be my associate dean over

there. So, he just carried a lot of stuff in the research office, and he worked with me the whole fifteen years. I'll tell you other things he's done, that he did later, but definitely we saw eye to eye on things. We only had one thing we didn't agree on in fifteen years, it's hard to believe. He has opinions too, right? So, he was great. Then, Joe Duggan and I started working together after two years, somebody had stepped down, and I had met him. I'd gone to see him to help me out on something that needed to be done, and I met him and I liked him, and so I appointed him. He was [from] French and Comparative Literature, and so he did the appointments and the degrees and admissions and all that kind of stuff, and ultimately did all the stuff dealing with the Association of Graduate Student Employees. Then, I had three fellowships and Graduate Minority Program associate deans. The first was Clara Sue Kidwell, who was a Native American professor, and she started with Shack and did three years with me, so she did '81 to '88, and then Margarita Melville, in Chicano Studies, did '88 to '95, and then Renee Sung, in Plant Biology, did '95 to 2000. So, they were really good, and all these people were just excellent at their jobs. Finally, Ollie Wilson came in. He was a music professor, very good, and Heyman really wanted him to get some more experience kind of in a central administration role. We all agreed that he'd come into the Graduate Division as an associate dean, and help out on special appointments and international affairs. So, he did that, too. So, that was fun. That's what we did.

09-00:39:36
Burnett: Can I ask about the graduate research office? Is that something that was built out during your time there, or was it always there, and was it just not being utilized?

09-00:39:51
Cerny: There was no graduate research office.

09-00:39:56
Burnett: In the office of the graduate dean.

09-00:39:58
Cerny: There wasn't one.

09-00:40:03
Burnett: So, Maresi Nerad becomes graduate research director.

09-00:40:06
Cerny: Yeah, but just it developed. First, she was a graduate student assistant.

09-00:40:11
Burnett: So, it's essentially her?

09-00:40:12
Cerny: Yeah, it was her, and then I had her hired as a postdoc, and then it expanded what we wanted to do, and so she got a title and we needed people to help her. By this time we're doing stuff all over the campus, we just created an office.

09-00:40:25
Burnett: It evolved and you developed, okay. Okay, great.

09-00:40:30
Cerny: If I'd inherited one, that'd have been great, perhaps. Okay, so, still in the big picture of getting into this, so what are we talking about? Well, we're talking about, like, 7,800 graduate students, and 900 are in law and they're separate. More than 100 doctoral programs. Lots of central control. One thing, of course, I really didn't know until I was in that job, so all the graduate divisions in the UC system actually have a quota, you can have only so many graduate students. So, the graduate dean's office sets these quotas, and so, ultimately, Joe Duggan would set their quotas working with Judy Sui, who would run all the numbers, and you could set the quotas. So, you could adjust quotas, somebody could make an appeal for more graduate students, so everybody was very serious about these numbers. So, when our review had gone so well in Chemistry, then, that's when I asked for 10 percent more graduate students in the quota, as a prize or whatever. So, you did all that, so that's very important. Too many fellowships were centrally funded. Some of the stuff should have been delegated out. We were approving all the teaching and research appointments. There were lots of fellowships and some were for anybody, Berkeley fellowships. We sort of talked about some of these other issues, but we didn't have enough packages, you know, there clearly weren't enough three-or-four-year packages if you're going to compete for the best students, Harvard, Yale, Stanford, whatever. So, we needed to do that. And the other mess was all the research titles, which I'll come back to, briefly.

These are things I'm not going to repeat because they went on for fifteen years. We had to modernize the Graduate Division. Computers were coming in. When I went there, there was one IBM display writer, you've probably never seen one, where you could write, and up on top, you could read it. There was one in the Graduate Division, that supported us. So, I went to Vice Chancellor Park and said, "We need some computers." We delegated tons of stuff to the departments, we would trust them on the domestic students. On the international students, you had to follow rules, and so, that's where it could be really slow, in the Graduate Division, because there had to be rules, what were the graduate deans really like from all over the world. So, ultimately, the people there, I replaced a lot of people and moved them to other jobs, and then a lot of them retired early. So, we put together a handbook, so you could put together a routine handbook on international graduate degrees that are acceptable, so we did that, so you can delegate that out, and then you can just double-check it. So, that sped everything up, but all this stuff occurs over years. We started reviews, reviewing departments.

09-00:43:24
Burnett: Systematizing, standardizing.

09-00:43:28
Cerny: Yeah, just get it where it could be faster.

09-00:43:32
Burnett: It's not a boutique operation.

09-00:43:34
Cerny: Now, I think the faculty are more paranoid than need to be, you know, but they are like that. "We got to get this offer out to that student." If somebody really wants to come to Berkeley, they can probably wait two more weeks, but you can't convince the faculty of that.

09-00:43:45
Burnett: Right, right. Well, I guess there's always this concern that if they get a full ride from Harvard or Princeton, that there's always that concern.

09-00:43:55
Cerny: Okay, so then, how are we going to do my main responsibility? So, the first thing I did was I created this Graduate Division affirmative action advisory committee, with me as chair. How's it going to work, unless you're the chair? So, I'm the chair, Clara Sue Kidwell, and her successors are the vice chair. We met once a month. We met the whole time, probably, till it was outlawed. You're going to have to do it—you can't delegate this to anybody else. Then, I had to get the Graduate Council to say, "We need an affirmative action advisor in every department." That worked, then we had an affirmative action advisor so they could come meet with us, when we met with them.

09-00:44:37
Burnett: You created a plan, didn't you, they were almost five-year plans?

09-00:44:42
Cerny: Right, something like that.

09-00:44:43
Burnett: Yeah, like 1990 to '95. Well, that's, I guess, later, that's a little bit later.

09-00:44:46
Cerny: Oh, yeah, I did those, yeah.

09-00:44:49
Burnett: Once you get the system rolling.

09-00:44:57
Cerny: So, we would meet with this group, and what were our goals for affirmative action, or our process? Well, to increase the percentage of minority students at Berkeley, you need active recruiting, you need better financial assistance, you need mentorship, and you need other support programs. We hired a full-time coordinator for graduate minority students who went out and did that and went to all these graduate-type school meetings all around the country, and we had a person constantly who liked to travel doing that, so we did that, and that was really good. Jasmine Williams was the first one to do that. You need better recruitment packages for everyone, you know, you need two- and four-year packages for regular or minority or women or whatever—I don't mean

"regular." And then the first year, I thought really to get it going, Associate Dean Clara Sue went with me. We visited thirteen departments, so we went out in the field and visited thirteen departments. We met with the chair, the chair of the admissions committee, and the department—now that there was one—affirmative action advisor, and said, "This is really serious, folks. We've got to do something." I didn't tell them what we had to do, but we had to do something. It's important to get out and you see them. You don't have a meeting in California Hall. So, we did that. I don't know how I picked the thirteen departments.

09-00:46:30
Burnett: Probably the most numerous?

09-00:46:32
Cerny: Probably.

09-00:46:34
Burnett: Was it consultative? Did you ask them what they thought they could do?

09-00:46:39
Cerny: Sure, I am sure we did that. We weren't going with any formula, that's for sure. Then, just to say what happened, so over the time till 1996, when the Board of Regents voted in '95 to dismantle affirmative action in admissions hiring, and then Prop 209 in November '96 passed, extended the concept, then we had increased the graduate minority enrollment by 36 percent for blacks, Hispanics, Native Americans, and Filipinos. The Asians, there were always a reasonable number of Asians, and then with the Tiananmen Square business, and so many students who were in this country getting Green Cards or some special thing, you couldn't look at their admissions numbers for several years because they were put into a privileged status and you couldn't sort it out.

09-00:47:40
Burnett: Right, right, almost a kind of asylum situation.

09-00:47:41
Cerny: Right, and so we went to diversity. So, instead of talking about affirmative action, we were at least allowed to look at diversity, so you moved to diversity. Okay.

09-00:47:55
Burnett: Operationally, what's different? Affirmative action is quotas, in effect, but not just that. The diversity is you're not allowed to have racial preferences because SP-1 and later Prop 209, you're not allowed to have racial preferences in your admissions decision-making. But diversity is different—how does that work, operationally, for the Graduate Division?

09-00:48:29
Cerny: Well, I think now you look at personal backgrounds and things like that, and not have everybody cut from the same mold, so you really look at life experiences. I think that's what they do at the undergraduate level, too.

09-00:48:40
Burnett: So, it could be a larger socioeconomic patterns, someone is [attending university] later in life, just trying to guarantee greater accessibility and trying to implement policies that facilitate underrepresented groups. Well, but not underrepresented categories of student, but student populations, let's say, that are not as well represented.

09-00:49:11
Cerny: Right, and so we did that. Okay, and the other thing I started doing, which I did every year, in the first year, I did six, but then I cut down to four, is we would go visit selected departments. So, I'd go with Joe Duggan. We'd go chat with the chair about how the department was going, what was happening, faculty concerns, just learn what they're doing, and then we would go take the chair and the graduate advisors to lunch. Then, we would go meet without them, with fifteen or twenty graduate students, and I would send in advance or go armed with all my data on their department, and I would have by then the results. So, the other gold mine: the Ph.D. exit questionnaire. So, the very first year, Maresi and I set up an exit questionnaire. So, I think no university in the world had had an exit questionnaire for Ph.D. students. It was very long, and they filled it out because we kind of pretended to them we weren't going to give them the degree if they didn't fill it out because they were sitting in the graduate offices, you know, and said, "You got to fill this out." They're honest, then, because they're done. As an example of its usefulness Chemistry has very poor teaching, as does Chemical Engineering. Well, you will not see that in the departmental teaching evaluations. You will not see it because I'd look. But there, after I had several years' worth of data, you can see that out of eighty departments with enough data, Chemistry and Chemical Engineering were sixty-first, sixty-third, not first or second or twentieth. So, everybody's really honest on leaving, and so, there were all kinds of questions. There are all kinds of open-ended questions, where students could tell you anything that you wanted. You learned a lot, and we looked at all that. So, in all the surveys we did—and Maresi was smart enough that we used to have all these things with open-ended questions, and the follow-up surveys I've done, I've always done that. They too are goldmines because you really learn what people think. So, that was terrific because then, we really knew what the students were saying, and then you could feedback anonymously to the department and tell them, "You know, the last group of students said this, that, and the other, what do you think about that? Is it true? How could we improve it?" Or you then, when you're talking to fifteen or twenty graduate students, you can just say, "What do you think about the department? We're here just to take notes, and we're not going to identify anybody." I could also take it back—with their permission, I would anonymously say the kind of things that the students were concerned about but I certainly wasn't identifying anyone. That worked out very well. I think they discontinued it after I stopped being dean.

09-00:52:24
Burnett: That's what Dr. Nerad said as well.

09-00:52:26
Cerny: Yeah, and I think that's too bad because it really gets you out of your office, you know, in the administration, gets you out in the departments. You get to see the campus. It's just good.

09-00:52:37
Burnett: They're your constituents in a way, right?

09-00:52:39
Cerny: Right, yeah.

09-00:52:39
Burnett: Effectively, if you see it in that way. This is qualitative data that you're gathering. Did Maresi then code it?

09-00:52:49
Cerny: Well, we had quantitative degrees of data. We had time-to-degree. Judi Sui and her staff coded it.

09-00:52:52
Burnett: In the exit questionnaire itself, when you're asking them to fill out things, sometimes you can take qualitative information and recode it?

09-00:53:00
Cerny: Yeah, it was all coded. Sure, but we had all our time-to-degree and completion-rate information, and how many fellowships they had. Otherwise, you know, but you wanted to know how was life with your dissertation supervisor. What were the oral exams really like? All kinds of things where people can answer.

09-00:53:19
Burnett: What were some surprising results that you got? Later, you start to prepare a report, but over the years, it dispelled some myths. There's some conventional wisdom about graduate school and it reinforced some of these stories, and it dispelled some others, I think.

09-00:53:47
Cerny: I don't know—by then, we were flooding them with enough information about themselves, so it wasn't a surprise that the times to degree were so long in the humanities or social sciences, or that, you know—because we had told them. The attrition was also high. So, it was really more, what can we do to help the department out? We had focused money on dissertation-year fellowships. We had focused money on advancement to candidacy. The strangest thing is I went to Sociology once when I started this, and maybe Sociology was in the second year, or the first year. I went back to Sociology toward the end, and there was a poor graduate student who was still there.

09-00:54:43
Burnett: After how many years?

09-00:54:45
Cerny: Eleven.

09-00:54:46
Burnett: Wow. Wow.

09-00:54:49
Cerny: Ten or eleven. At one point, I couldn't believe that sociology was that strange. So, I actually, on my own, just looked up how long that the people had been in graduate school who we'd been hiring on our sociology faculty, it was ten or eleven. Now, these are very small samples that I was doing.

09-00:55:11
Burnett: I think there are these cases. I knew, in graduate school, there were folks who had been there for a long time, and the university doesn't quite know what to do with them, and has kind of given up on trying to mentor them or usher them through. But they are good teachers, usually—or often—being a kind of resource, so they kind of get stuck in some of these cases.

09-00:55:35
Cerny: I'm not sure they'd be allowed to teach that long here, actually, particularly with a limited number of such appointments. It probably wouldn't be the case. Well, some departments are impervious to feedback because I wrote Chemistry and Chemical Engineering about their placement twice about, you know, you all think you're doing really well, but here's what they really say, and they totally ignored it.

09-00:56:06
Burnett: Yeah. What matters is the ranking, I suppose. Well, there was something about how the conventional wisdom, I think—this comes from your report—was that it's these stragglers, what do you do about those people in year seven or year eight? You found that a quarter of the students drop out in the first two years. There's an early burst of attrition that needs to be addressed. I think that was quite a surprise, somehow, quite a surprise to people.

09-00:56:42
Cerny: It was a surprise, but these are Ph.D. seekers. It's not an undergraduate degree. The undergraduate degree can expect high completion rates. Here, I mean, you can notice that in Chemistry, we have people who come as chemistry graduate students who had not even thought about what they want to do. They're on a track, they're going to go to graduate school as a chemist, and after you've paid the out of state tuition for them and they've been around a semester, that's the last thing they want. They're not failing out; they leave. So, even in the sciences, you see where you lose 10 percent the first year and 10 percent the second year. They're not failing out here, they're just—

09-00:57:10
Burnett: Making different choices.

09-00:57:11
Cerny: They're making completely different choices, and overall, it's about, like, 25 percent of people have dropped out. You can then move to departments where advancing to candidacy takes longer. Some students dropped out before that, and then the others drop out after that. I certainly had graduate students come and join me and then decide that they didn't like me.

09-00:57:40
Burnett: You yourself said when you went to graduate school, you were intrigued by it, but you weren't sure what you were going to do. You didn't exactly know what being a graduate student was at all.

09-00:57:55
Cerny: Doing research and all, right.

09-00:57:57
Burnett: Yeah, doing research, and it was a complete baptism, and that's your story. So, this shines a light on a lot of the habits of the departments. You also got to see the contrast between how different departments operate. Did you start to see patterns of best practices? Did you notice that there are some good things happening in some departments that could be applied to all?

09-00:58:27
Cerny: Yes, at various times, we certainly did that. We published stuff talking about the job search because it was important, no one was getting help on the job search. We put together compendia at least of what the campus was doing, and all these different departments, just so we could share the ideas. We did things like that. Maresi and I, in the first publication we did—which I'll mention later—there, there's this data on these two columns, the differences between the hard sciences, let's say, and the soft sciences and the humanities. They're totally different worlds. So, that came out. That was in our thinking, and that came out very early as a publication.

09-00:59:12
Burnett: That's a really great contrast that's preserved in the graduate report, I think, a version of it is.

09-00:59:21
Cerny: The Council of Graduate Schools has a magazine, and so this was the special edition in May '91. So, we'd gotten enough data by then that we had put this out, and you could see this contrast, and then you could see where you needed to put your effort. A lot of the time, later on, you visit these departments, you have a lot of information—how long it's taking all the students, what all they had to say. They had criticisms, they were there, you could get the faculty to talk about the criticisms. You could get the graduate students to talk about it. It was bound to help because it was all anonymous.

09-01:00:03
Burnett: This is the reaction with all the departments, but above, how did the chancellor react to this kind of initiative?

09-01:00:13
Cerny: Oh, I think fine. One of the nice things about the graduate dean is most of the time, nobody told me what to do.

09-01:00:24
Burnett: Except for the initial push of, "Deal with affirmative action, get something going."

09-01:00:28
Cerny: Fair enough—I was in favor of that. Yeah, but other than that, as long as you were making progress—we did give talks, and some years, we had to give major talks for the senior staff, and I had a lot of these data, and particularly showing, as when I have a few numbers later, how we were making progress on reducing attrition.

09-01:00:56
Burnett: What we should maybe do is switch the tape, and we can pick up where we left off.

[End Audio File 9]

[Begin Audio File 10]

10-00:00:09
Burnett: This is Paul Burnett, interviewing Dr. Joseph Cerny, for the University History series. It's June 4, 2014, and this is session five, tape ten. So, Dr. Cerny, we were talking about the early years in the Graduate Division, when you created a council for affirmative action and you did surveys of graduating Ph.D.s of all the departments to see where they were going. You gathered data, longitudinal data, on the progress of the departments, as well as qualitative data that you would give as feedback to those departments, to help them to improve their outcomes. So, there were other things you did at that time as well, as part of the work in affirmative action, you created a Graduate Opportunity Program? That was in the early nineties.

10-00:01:11
Cerny: Yeah, that was a fellowship program. There were different fellowship programs, yeah.

10-00:01:14
Burnett: Okay, so this was to improve access to and outcomes for students from underrepresented backgrounds.

10-00:01:24
Cerny: Yeah, just following up on that, so things like that, we had to completely reorganize admissions with computers and things, and so I reassigned someone and brought in a crack person, was actually already there, named Betsy Livak, who could do that. So, that got us in a new world that we had to

be in. Even within three years, almost all departments were admitting minority students at close to the white male rates, so that moved along really fast. As I mentioned already, Judi and Maresi were looking at time to degree and completion rates, but I'd also met John D'Arms, who was a classics professor at Michigan, who was their graduate dean, and he had a great database. So, we agreed to share information, because then we could see which of his departments were doing better than ours, and vice-versa, and then we could learn what was going on to try to improve it. So, that was another great step because we had another big public university, some of our departments had better time-to-degree and completion rates than some of his and vice-versa.

10-00:02:35
Burnett: Would that have happened with a private university?

10-00:02:41
Cerny: It probably would have. It's just I happened to—Michigan, of course, is in the AAU, so I met him at the Association of Graduate Schools meetings, and we got to be friends. He actually is also going to be on the National Research Council study with me, too, so we were friends about that. I don't know what kind of databases the privates had. I know some, for example, once we were really out and preaching all this stuff, like, Rice didn't have one. There were a whole bunch of places that just didn't have a database at all, and had enough money, they could have. There was always institutional research in the bottom of some building at every university—who knows what they were doing? But they weren't providing anything for me. So, that exchange worked really well, and so, anyway, even by '89, we had made a lot of progress at decentralizing, and I didn't have to worry about that, so we just had to worry about getting more money and surviving all these crises. We were slowly trying to improve things, and the first thing that we wanted to improve was that there was not any health plan for graduate students. So, we then tackled, in '87-'88, the Student Health Service really did the work, but we had to work with them. We put in a graduate student health insurance program. Of course, then we had the Association of Graduate Student Employees, who didn't know about that, and it had to be mandatory, of course, because if we don't make it mandatory, it won't work. So, we all worked really hard. I remember a really irritating piece in the *Daily Californian*, where they found some student who must have been an early Tea Party member, who wanted to be an individualist.

10-00:04:48
Burnett: "I get to opt out of my insurance."

10-00:04:50
Cerny: "Who needs it? We should be tough and strong and be on our own." It gave it a big coverage, and I said to myself, "Good grief."

10-00:04:59
Burnett: "What's happened to Berkeley?"

10-00:05:01
Cerny: But when we finally had the vote on that, it was 80 percent in favor, so that took care of that. So, we then had a health insurance plan for graduate students.

10-00:05:14
Burnett: It had to be a package that was relatively reasonable because there was some work to do on scaling it?

10-00:05:21
Cerny: They had to do a lot. They were really good. They set the plan up; we just saw that it made sense and interacted with the graduate students. The Health Service wanted to do it, and they did. Within a year and a half, one of the people had a $2 million health problem, just like all these things happen when you have a group of people. So, anyway, they got through that, somehow, and it's been a real success. It's just these clean-up things. When I talk about postdocs, which I'm going to do next time, total clean-up was necessary on postdocs.

10-00:05:58
Burnett: On the research side?

10-00:06:02
Cerny: Yeah. Well, either the graduate dean's in charge of research, or the vice chancellor of research, as I quoted in one thing, since I was both, it didn't matter.

10-00:06:11
Burnett: So, you passed the buck to yourself?

10-00:06:14
Cerny: I put it over there, and right now, that's where it is. That needed a lot of work. But I guess what was interesting, I mean, all this stuff was interesting to me because it wasn't like you walked in and all the data were there. I had data I found somehow—I knew the time to degree that chemistry told us was three years, was a lie, and it was 5.3 years, and it hadn't changed since 1960. We had some data ultimately from UCLA that was more or less 5.3, 5.5 years, since then. So, I knew those kind of things, but I didn't have any idea what the attrition was. I could guess what was happening in the English department because I had some friends who didn't get their degree, or got it really late, or whatever.

10-00:07:03
Burnett: As a result of the study, wasn't there an overall increase, an average increase, of one and a half years over a given time period in time to completion, or is that not right?

10-00:07:14
Cerny: No. If anything, completion times are probably coming down. There's more money fed in, so they certainly should be, because it was really rate-limited.

10-00:07:30
Burnett: Oh, I think I was referring to the original Graduate Division report that used data from earlier, to the point before you started making those kinds of interventions. No?

10-00:07:44
Cerny: There was one number in one of the reports which jumped around, yeah. In fact, one of our big studies, since you have to wait eleven years, is what I've even put in this nice report there, is actually stuff that the students, the last group had come in to '84 to '86. So, all our improvements, they'd have affected some of them. I'd be interested to see more data on that. So, we did lots of special things. I haven't really listed them all, but one can see them, and I will come back to one or two of them. [BREAK IN AUDIO] Particularly in the humanities and social sciences, we tried to get a lot of money, there. One thing that Maresi and I got going was what we called "invitational seminars on special topics," and we did a bunch of those. Maresi organized them, and there, you would have faculty and department chairs and graduate students and some administrators, just to talk about some topic. Not too many people, maybe twenty, just to see what kind of ideas you got. That worked very well at co-opting groups into thinking in new ways, if you wanted them to. So, in '88 and '89, we actually had a year-long seminar with Neil Smelser helping us, and it was under the auspices of the Center for Study of Higher Education, on graduate education. That's where we really started now looking campus-wide at time-to-degree and completion rates, and we were comparing Michigan, and we got some data from UCLA, and we got some data from the Dwarves. So, just the chance to look around and see how Berkeley was doing and get ideas we could then feed out.

10-00:09:45
Burnett: Well, Maresi Nerad said that you believed in truth in advertising—you felt that if you could make public, the university should make public their time-to-completion and completion rates, and that you approached the Dwarves, but they didn't want this information to be public. Is that correct, or is that part of the story?

10-00:10:18
Cerny: You know, I honestly can't remember. She may have discussed this with the Elves.

10-00:10:22
Burnett: That was her recollection, but she said the NSF had data that they had collected, and that the Mellon Foundation gets involved.

10-00:10:31
Cerny: It does, and I'm going to tell about that. So, the Mellon Foundation definitely gets involved in all this, and actually, it's something coming up. They did have some Dwarf data. They were private and so they had more private outlooks.

10-00:10:51
Burnett: Need to know.

10-00:10:55
Cerny: So, something we did, happened three times, it was very unusual. So, by then, by late '88, summer '88, I'd visited enough departments that the graduate students had complained about isolation. They weren't meeting any other graduate students, and I'd heard complaints, complaints, complaints. I said, "Okay, Berkeley's a big place," and I guess we had money, so we sponsored a party. So, we had a party in October '88 at the Faculty Club for all the graduate students that wanted to come in architecture, comp lit, French, art history, math, and mechanical engineering, so kind of a mix. We had music, dancing, and good food. They liked it, and I actually did three of those, one each October till the real budget crisis came in and I had to abandon it. So, again, we did something like that with a mix of students, and it didn't cost all that much. It was a real attempt to see if you can break down some kind of barriers to communication.

10-00:12:02
Burnett: Well, if that hadn't been done before, I know that that's become standard practice at a lot of universities, that the university supports the social life of graduate students to a degree. Sometimes, it's in contrast or in opposition to a graduate student union. So, at the University of Pennsylvania, there's a graduate student council that has these kind of social events, and their budget is almost entirely devoted to social stuff. The graduate student union has its own social. So, there was a rivalry, almost, between the "cool" party and the "square" party, at Penn, anyway. [laughter]

10-00:12:43
Cerny: Well, we did this, and it was staffed by the people from the Graduate Division. They had fun anyways, so we could do it. So, the other issue had been graduate student research titles, and they'd become a huge mess, and they were all over the place. There, I don't know how much time was spent on it. We got the title through. You had to fight the Office of the President, and AGSI didn't like it because they were hoping, I think, to unionize the graduate student researchers. But as it turns out, they can't, because without going into any details I didn't review, PERB [Public Employment Relations Board, State of California] decided that you couldn't unionize the research side. So, now we have graduate student researchers, and I guess because we weren't regularized, we couldn't really put tuition into the grants until '90-'92, which was costing us a lot of money. Once you've done that, then you could get a certain amount of your tuition in the grants, and that's a big help. Engineering had attitudes about that here, in one way, and UC San Diego had a plan that they funded all their science and engineering graduate students out of pooled resources, and they got it to work, but Engineering kept proving to me that we couldn't get it to work. So, after a while, I just gave up. Then, we had another of these invitational seminars, just for the spirit of things, so now we were on

to evaluate the campus learning climate for women and graduate students of color, so we had one of those. They were all successful.

10-00:14:19
Burnett: Well, can I say what Maresi said about that? She said that there was a real cleverness to these invitational meetings. You'd discuss educational reform, you could use these invitational seminars to float balloons to see when it was the right time to implement policy? You turned it into an honor to be invited. So, you created a kind of cachet to this, and you discussed key issues of the day—gender equity in the classroom, conflicts of interest for engineering students who worked for professors' companies. These were hot issues and they're still hot issues. So, there is some cleverness in how this was cultivated and on campus; it really became something.

10-00:15:08
Cerny: Yeah, it seemed to really work. We had very few people say no. Yeah, and we tried to do it with controversial subjects, and that diffused them, some of them.

10-00:15:21
Burnett: Right, starting a conversation preemptively.

10-00:15:27
Cerny: Yeah, I really enjoyed them. Maresi organized almost all of them, needless to say. She didn't pick all the topics. [laughter] Yeah, because even in this period, well, we had a panel discussion on the view from the other side of the desk, which was nice, that I went to with five professors from the humanities and social sciences, talking to graduate students. So, that was just kind of a singular event that they got together to do, but that was fun. The Graduate Division, by then, was sponsoring workshops on dissertation writing. All the things to get help out to the people where they needed it, and then we had to create an office of graduate student training. By now, there was a lot of pressure from the Office of the President. Undergraduates were crabbing about graduate student instructors. We hired Jacqueline Mintz to do the job as an academic coordinator. She was very active. So, then the Graduate Council ended up agreeing that each department that was large should have a faculty advisor for graduate instructor issues. We had Outstanding GSI awards, and state legislature sent in special money to train foreigners to speak English.

10-00:16:50
Burnett: They did?

10-00:16:51
Cerny: Yeah, or pressure, or something, or said, "Use some of your current funds to do it." Those kind of complaints pretty much went away. Then, in a monumental thing, the Graduate Division was housed in the basement of California Hall. You've probably never been in the basement of California Hall, but that's where it largely was. In '90-'91, as other changes occurred, they moved us out of the basement to the third and fourth floors of Sproul.

10-00:17:24
Burnett: Is that a Loma Prieta [earthquake] reaction?

10-00:17:26
Cerny: No.

10-00:17:27
Burnett: No? It was before that happened, I guess.

10-00:17:29
Cerny: No, it was after.

10-00:17:30
Burnett: Right, that's right, it was two years after.

10-00:17:32
Cerny: No, it was just pretty crummy conditions. Even by '90, I had enough data on time-to-degree and success rates and certain answers to the exit questionnaires to just send them out to the departments. In 1990, the Mellon Foundation really liked us, so Berkeley was one of ten campuses to get a five-year block grant of $600,000 a year from Mellon for Ph.D. programs in the humanities because we had so many successful Mellon Scholars. So, that was really good. It was a substantial amount of money, and so, that went to classics, comparative literature, English, history, and art history, and that helped—we hope.

10-00:18:30
Burnett: It has been and it's continued to be. Did you mention, in 1989, didn't you start a resource center for the graduate students? Is that what you were talking about? The graduate student instructor teaching?

10-00:18:51
Cerny: Yeah, that's its title.

10-00:18:55
Burnett: So, that's become an important feature.

10-00:18:57
Cerny: It's by now called the GS Training and Resource Center, is probably what it's called now, yeah.

10-00:19:02
Burnett: At other schools, it's the Center for Teaching and Learning, is typically what they do.

10-00:19:06
Cerny: Ours has changed its name. I wrote down its original name on my notes, right.

10-00:19:11
Burnett: These impossibly long acronyms. GSITARC.

10-00:19:26
Cerny: In ’91, we did start publishing a little bit on what we’d been doing, which was really nice. Then, in October ’92, we had our first AGSE strike. So, in the late ’90s, the United Auto Workers had run out of things to do and had to decided—

10-00:19:48
Burnett: Late nineties or late eighties?

10-00:19:50
Cerny: Late eighties. They started to unionize graduate student employees, if they could. So, ours joined up. What they got from it was a fully paid steward. So, I’m not going to go on at length about it. In November, we had a strike. The physical sciences didn’t strike, but a lot of the humanities and social sciences did. I remember going to one faculty meeting with Joe Duggan, and it was a fairly hostile group of faculty staring at us, that wanted us to just cave in, please. It was basically a strike over unionization. The Office of the President was against unionization.

10-00:20:19
Burnett: Well, there was this whole question [BREAK IN AUDIO –edited PB] I suppose over the legal status of—and this is a classic across a number of campuses where there’s been this issue—are they students or are they employees, and what category do they fall into? California had been declaring them students rather than employees.

10-00:21:12
Cerny: Right. So, the one thing I learned that I didn’t know in this process was all the department chairs vanished in the humanities. They weren’t there anymore. So, if you wanted to talk to a department chair in the humanities about why their students weren’t teaching, you wouldn’t be able to find the person. Anyway, so we worked around that. But they just disappeared.

10-00:21:48
Burnett: How long did this go on for?

10-00:21:50
Cerny: Well, this strike, actually it ran through the end of the semester.

10-00:21:57
Burnett: What date did it start? It ran until the end of the semester?

10-00:22:04
Cerny: I don’t have my dates here, but it ran through the end of the semester, and we had a very complicated arrangement that somehow, if they wanted to get paid, they had to send in some form to Carol Soc, and state how many days they worked, if they wanted to get paid. A lot of people didn’t get grades, and the whole thing became a mystery to me. The physical sciences and the biological sciences were not affected. So, that happened, everybody went home for Christmas, everybody came back, it was like it never happened. Nobody

talked about it. We just started back up again. The GSIs showed up for their new classes and started teaching.

10-00:22:49
Burnett: Well, what happened to the students who didn't get their grades?

10-00:22:54
Cerny: Same thing that happened when we had that incursion into Cambodia, I suppose, which I wasn't here for either, but I was on sabbatical.

10-00:23:05
Burnett: What was the solution in that case?

10-00:23:07
Cerny: I think in Cambodia, they just all went home. I don't know—I was gone. I was here for this, but I was in science and I wasn't teaching.

10-00:23:17
Burnett: Yeah. The students may have been more in support of that than they would have been for—I think there's a mixed student support for graduate student unionization. So, they probably wanted their grades at some point for the work they did.

10-00:23:31
Cerny: So, I don't know what happened.

10-00:23:37
Burnett: Well, it's interesting that's that early, and they're sporadically, twenty-five days' worth of strikes at five different campuses in 1996 and '97. This is later, but this just keeps going.

10-00:23:51
Cerny: Yeah, we're going to come back to that. It keeps going. Tien was out talking to the strikers, I went out and talked to a few of the strikers, but it wasn't very productive. It seemed to me that just, we had to collect this information. I didn't even remember talking to Carol Soc about how many people. If they said they taught, then we were going to pay them, but then I suppose you'd come back and ask, "Well then, you should turn the grades in." So, I don't know what any of the results were, and I don't remember the *Daily Cal* full of tons of complaints from students about how horrible this was. Maybe I was blanking it out in my mind.

10-00:24:35
Burnett: It made your job a little bit more difficult, I would imagine.

10-00:24:39
Cerny: It did. So, then, so the interesting thing that I was then doing is I was put on this National Research Council study of the research doctorates, and that lasted from '91 to '95. This, you know, was these huge studies that occur about every twelve years, and so this was to look at all the Ph.D., well, look at

forty-one disciplines and look at all the Ph.D. programs that graduate a certain minimum number of people per year. It's like 3,634 programs, 274 institutions, 41 disciplines, and 8,000 faculty questionnaires to be filled out on the peer review of their teaching and things. On the committee was Dick Atkinson, who was chancellor at UC San Diego, and he was there to watch the rise of San Diego and trying to catch up with UCLA, and I was on it, and John D'Arms from Michigan was on it. Debra Stewart, from North Carolina State, was on it, and she's become since then the president of the Council of Graduate Schools. The co-chairs were Brendan Maher from Harvard, in psychology, who'd actually been the graduate dean at Harvard, so I met him when he was a Dwarf, and Marvin Goldberger, who was a well-known physicist, who was the dean of natural sciences at UC San Diego. He seemed not very interested. He did come to the meetings. I remember one of the committee said, "Well, classics has lost so many, has disappeared from so many schools, maybe we should drop it." I screamed at that, said, "This is ridiculous. It's been around here for a long time, it should stay in." Of course, since John D'Arms was a classics professor, he was glad I said that and he didn't have to, and so it stayed in. So, I don't know—the book's out and it was published.

10-00:26:55
Burnett: Right. Didn't you say something at one point about nuclear chemistry falling below a certain threshold at some point?

10-00:27:02
Cerny: Yes. That's in the National Science Foundation data gathering for Ph.D. production. So, as a sub-discipline of chemistry, then how many are in chemistry, how many are in physical, organic, whatever.

10-00:27:19
Burnett: Right, all the branches.

10-00:27:20
Cerny: But we fell below five a year or whatever small number it was, which was too bad, so we lost out of that.

10-00:27:28
Burnett: Right, so you have some experience with—it's not that nuclear chemistry is being closed, but it's just it's graduating fewer Ph.D.s, and if it's no longer recorded in those rankings, that's difficult.

10-00:27:47
Cerny: It doesn't help, and didn't help in this. This was just there were fewer classics departments, but they weren't tiny numbers. So, anyway, I thought it was ridiculous, so they stayed in. So, we did very well, so all I'm going to do for this is say, as the *New York Times* said on September 13, 1995, "UC Berkeley, whose budget was cut 11 percent from 1990 to '93, had 35 of its 36 departments rated in the top 10 in their fields, in terms of faculty competence and achievement, far more than any other university in the nation." So, we

were happy about that. Actually, I remember giving a talk to the faculty that wanted to hear it because I could give a talk the minute that the results were released, and I had early results. So, the day after it came out, then I gave a talk at the Faculty Club to the interested faculty.

10-00:28:50
Burnett: It's a double-edged sword, though, in the sense that for the budget-cutters and the streamliners, it's a bit of a coup and they can say, "Well, you see, there was all this fat there that could be trimmed and it didn't affect their rankings one bit, so we can cut more, or this is something that can be repeated elsewhere. In fact, it may be that the harm, if there's harm to come from the cuts that occurred at that time, the harm may be more long-term? Or do you feel that those cuts were, in fact, rationalizations that improved the delivery of services in the university and did not really harm the outcome for the university?

10-00:29:49
Cerny: Well, you know, we hung in there, right? I'd say UCLA has hung in there, and UC San Diego did get very close to UCLA, or pass it, in the records. Atkinson came to the meetings. Well, I think Berkeley's just had to be very imaginative. I don't know how. We luckily had finally these capital campaigns, which are sources of other money, and that's how we've been able to do construction, and faculty enhancement, and I would hate to see it because I think once you start to spiral out, it could be too bad. The fact that thirty-five out of thirty-six were in the top ten is sort of amazing, and it also showed that biology had recovered from its problems in the eighties to now. So, I thought that was terrific.

10-00:30:42
Burnett: An endorsement, yeah. Great.

10-00:30:52
Cerny: Okay, so, then, I really wanted to look at whether all our work was doing anything, and so you have to wait a long time to see how completion rates change. We looked at some data from early on and looked at it eleven years later, and tried to look from people who first came in '75-'77, to '84-'86, but what were, like, the overall campus completion rates, and how does that compare to what you'd like to see? So, what you'd have to like to see, we know that there's a lot of attrition in the first two or three years, and my choice for the asymptotic university is Princeton because Princeton has a very boutique graduate program, and they readmit students every year. Their asymptotic completion rate was 65 percent, so that's the best you can hope for. So, we moved the overall campus from 49 percent to 60 percent. So, I think that's really good. When you looked at the numbers, physical science and engineering fluctuate 63 to 66, but the social sciences had gone from 43 to 57, humanities had gone from 29 to 43, and I would hope, with newer data, were I still dean, I would have a new set of data and see what had really gone on. I thought in terms of preaching the importance of all the things going on, that

this shows you can affect these, and you need better finances and better mentoring and annual reports on student progress to try to help do things. So, that was really good. I was struck, in collecting all these data, how few universities had done that yet, although a lot of the people started doing it in the course of the nineties, even somewhat smaller privates just decided they had to do it.

10-00:33:22
Burnett: You did talk about the efficiencies gained from the computerization of administration, that this changed things a lot, allowed the university to save a lot of money. Did it also facilitate this kind of data-driven research, so that smaller campuses could do that?

10-00:33:48
Cerny: It had to, yeah, because what I'm going to talk about right now will tie into that, actually. Inadvertently, it's a perfectly good lead question. So, what else do I do in '92? So, in '92, Bill Bowen and Neil Rudenstine came out with a book called *In Pursuit of the Ph.D.* Bowen is a long-time president of Princeton, and by then was the head of the Mellon Foundation, and he's a labor economist. Neil Rudenstine also was at Princeton a long time. He has an English Ph.D., and he was the current president of Harvard in 1992. So, this book came out, it must have been very early in '92. It was like 300 pages of dense text and 100 pages of appendices, and it was a lot of data. They did just like what we had been doing. They picked ten universities; they got time to degree and completion rate. They looked at six fields—English, history, political science, math, physics, and economics—and they used largely Dwarves and us, although a couple of differences. We were producing comparable data to what I needed for them, so for us, I think we were really cheap because we could do whatever we're doing for me, we can do for them. I think the other places, tying into your question, I think they actually had to pay for it, and by then, with the computing being better, it probably made it a lot easier to do. Getting retroactive stuff would be hard. So, for the October 1992 Association of American Universities meeting, which was in Seattle, of all the presidents, they decided they'd have an hour and a half devoted to this book. They were going to have three discussants, so they were going to have Rudenstine discuss it because it was his book, and Frank Perkins was the graduate dean at MIT, and he was the President of the Association of Graduate Schools that goes with this. I was the vice president and the president elect. So, what they wanted was for Rudenstine to comment on the book from the point of view of a university president, Perkins was going to comment on recommendations from the viewpoint of a graduate dean at MIT, and from the fact that all the graduate deans had actually discussed this book at the Association of Graduate Schools meeting in September, and this was October. I was supposed to talk about recent actions at Berkeley that responded to some of the book's recommendations. Well, that wasn't too hard, since I'd been working on exactly that.

10-00:37:12
Burnett: Right, you've been working on this for some time, yeah, before.

10-00:37:17
Cerny: So, except for the fact that it was a very scary audience of sixty chancellors.

10-00:37:28
Burnett: They were aware that before this was published, you had been working on—

10-00:37:32
Cerny: Bowen was, yeah, but I don't know that anybody else was. By then, he'd been at Mellon and he was funding this. So, he knew it, but I think I was speaking because I was going to be there. At this point, up till then, they'd allowed two graduate deans to come to their meetings, and shortly after this, they stopped that. Up until then, from after the Second World War up till the mid-nineties, two graduate deans, the president and the president elect of the AGS, got to go to the meetings and listen, which, of course, was entertaining. The book had nine recommendations and I tied in their nine recommendations. The book had a lot of data, but ultimately, it was more focused on the social sciences and the humanities. I was just looking at something under Bill Bowen and they didn't have it listed as one of his most important books. Anyway, it was something I did, and three of the presidents actually asked for copies of my overheads. The president at Stanford, Gerhard Casper, did, he was a nice guy; and Gordon Gee at Ohio State; and the president of Clark. Clark, of course, was this outlier that used to be doing graduate education back in the twenties. Somewhere, I have never looked it up, some president must have lost all the money or something, and the graduate program disappeared, essentially. If I'd have been hit with what's Berkeley about to do when the book came out and I hadn't bothered to read it and I was sitting around, wondering what I was going to spend my summer on. It would have been a problem, but it wasn't.

Okay, so, then we went on. I may finish on time, here. We had the crises, I don't want to go into it, but we had these three very early retirement incentive plans. They were in 1990-91, 1992-93, and 1993-94. We lost 448 faculty, we lost 25 percent of the faculty, and particularly bad hit were physics, math, and engineering. I think we survived in part because most of the faculty didn't want to go anywhere else, and so, maybe twenty-five, this is my guess, of the 450 went somewhere. Then, everybody else stuck around and continued to do research, perhaps taught a little, or sat at home, or enjoyed themselves.

10-00:40:12
Burnett: Yeah, you almost had this wealth of human capital that was hanging around, teaching the odd course—?

10-00:40:23
Cerny: Well, you let them still have graduate students, where they could still do that anyway. Maybe fifty left, but it wasn't an exodus.

10-00:40:31
Burnett: Well, you described in some departments, I think in nuclear chemistry, that to some extent, they'd become a bit sclerotic in that the proportion of senior faculty to junior faculty was very high. So, with some of these early retirements, you create a new momentum.

10-00:40:51
Cerny: Right, and that was important, although our really sclerotic ones had retired much earlier. Tien did something because the third VERIP[1] could have reached down to people aged fifty-seven, and he looked at all the data, it could have been fifty-seven to sixty. It was at the other campuses, but he looked at how many we'd lose, and he changed it to fifty-eight, and nobody sued him. So, that probably helped some because we might have been really hit. Then, of course, in all this, Heyman had started—as I've said before—this capital campaign from '85-'90, which helped us with construction, and Tien started the next one in '96. I didn't realize it was so late. He stepped down in '97, and it didn't conclude until 2001. Then, in '98-'99, we had our next, as far as we were concerned, GSI strike. So, this one began in November '98, and they were planning to have it last until the end of the fall term, but on December 3, the legislators talked with the Office of the President and twisted their arm, I guess, and I guess they said something like, "Just have elections on all the campuses." So, they called the strike off. So, the GSIs at Berkeley had to certify in writing that they'd met their obligations during the four-day period, but it was only four days. We actually had a vote, and I'll say one thing, I was happy about it because it was decisive. So, we had a vote in spring '99 and the graduate student instructors, including a lot of them in science and engineering, Berkeley voted 833 in favor of unionization, to 293, so it wasn't even close. That was great. As far as I'm concerned, you've got the science and engineering, you've got some sympathy with the others, whatever, but it was done, we're through with that. So, that was great, they're unionized.

10-00:43:10
Burnett: It remained for them to get a contract until May 2000.

10-00:43:15
Cerny: Little things like that, right. Little things like that.

10-00:43:20
Burnett: It was a persistent desire on the part of the students. It was seventeen years from the time they started until the time they got a contract, that they had been trying to form a union of graduate students.

10-00:43:34
Cerny: Yeah, but it wasn't seventeen years since they'd been supported by the UAW.

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[1] Voluntary Early Retirement Incentive Programs, rolled out in three waves from 1991 to 1994, resulted in the retirement of 30% of UC Berkeley's faculty.

10-00:43:38
Burnett: Right, right, but there had been this kind of activism around it, and perhaps it was a small group that then grew, but once the people in the graduate student, I guess, would-be bargaining unit were polled, they were in favor of it, in the end. That was trying for you, and it was trying, I guess, for Joe Duggan as well because he met with them regularly to talk about their demands. I suppose that took a bit of the burden off of you in terms of the daily burden.

10-00:44:15
Cerny: Oh, yeah, I never went to any of those. I just couldn't have done it, so he did it, and Carol Soc did it, and the woman from labor. Yeah, no, I just couldn't do it. It was a huge strain on him. Okay, so by the time I left—which I haven't left yet—we had actually reviewed all the Ph.D. programs, and we had reviewed, with the direct help of the Graduate Council, all the twenty-five graduate groups. Some of the Ph.D. programs had been reviewed more than once.

10-00:45:02
Burnett: It's eighty-two graduate programs, is that right? Something like eighty-two, and then twenty-five graduate groups.

10-00:45:09
Cerny: They were all reviewed in that period. The graduate groups are easier to review because they were small.

10-00:45:14
Burnett: This is the largest graduate program in the country.

10-00:45:18
Cerny: I forgot to mention it. Earlier on, I should have said, right, in addition to all these students we graduate, anyway, we used to graduate the most Ph.D.s of any university.

10-00:45:26
Burnett: Yeah, 800 Ph.D.s a year.

10-00:45:28
Cerny: Or more, yeah, 830 or something. Some other big publics are close, but it's a lot.

10-00:45:38
Burnett: Yeah, Madison, I guess, would be the next.

10-00:45:39
Cerny: Texas, I think, yeah. I forgot to mention that.

10-00:45:47
Burnett: So, that was a massive achievement with a fairly small staff. I mean, with the assistance, there's institutional support, institutional assistance, but the actual Graduate Division staff were quite remarkable. Maresi speaks with great admiration of both you and Joe Duggan going and spending the day with each

of these departments. You would spend the day with graduate students and talk with them.

10-00:46:19
Cerny: No, we spent like three hours. I mean, it was like noon to three. Eleven-thirty to three. It's the bulk of the day, right?

10-00:46:32
Burnett: Yeah, and there's eighty-two of them.

10-00:46:36
Cerny: We didn't make all of them. Now, with the reviews, were different. Ian Carmichael was handling all the reviews. Of course, I was at the end of the review process. I didn't go into all that because at the end of the review process, then we'd have a meeting in my office, and there's the recommendations, and then the executive vice chancellor would be there, and the head of the Graduate Council, and the head of the budget committee, and me and Ian, and then we would comment on the recommendations and what we wanted to do. So, we had to do that.

We accomplished almost all the things that I wanted to, unless something—hopefully it won't matter. So, by then, it's '95-'96, and now, I read the Mellon book, and they didn't do any outcome analysis. I've been on the NRC committee, and it didn't do any outcome analysis. You want to say, "You need outcome analysis—why isn't anybody doing it?" So, I suggested to Maresi that we should do it, and we thought about it for a year, and then we decided that we should do it. So, that's when we start our last big program. So, we decided to pick six departments—Biochemistry, Computer Science, Electrical Engineering, English, Math, and Political Science—to look at a Ph.D.s ten-to-thirteen-years-later study, started with people that graduated July 1, '82, to June 30, '85, and see what their careers had been like. We added computer science because the Mellon was funding us. They said, "Well, you know, I think electrical engineering's fine, but there are more women in computer science, so why don't you add that?" So, we did. We got money from the Mellon and we got some money from the National Science Foundation because it's doing other kinds of tracking, but it's not doing this kind of tracking. This was going to be a mail survey, like in the olden days, and we worked with the Berkeley Survey Research Center to mail everything out in '96-'97. So, we picked sixty-one universities. We couldn't do anything less than twenty pages in anything we did. It was a 22-page survey form, 6,000 surveys, and it was essentially 57 percent of the total U.S. Ph.D.s awarded in those six fields, and it was matched to NRC things, this, that, and the other. We got a great response rate, it's hard to believe, and you had to find all these people with no Internet. It was a real production. We got 66 percent of the domestic Ph.D.s and 52 percent of the international ones, and we looked at some here. Some had gone home. We had specialists who could find people, pre-Internet.

10-00:49:37
Burnett: Almost like private investigators.

10-00:49:43
Cerny: That's what you really want to know—the employment history, the job search process, factors important in the decision to accept the first and current positions, as well as a retrospective evaluation of the doctoral program and the usefulness of the doctoral degree. We looked at gender, ethnicity, international status. We looked at positions in academia, as well as what we called BGN, for business, government, and non-profit, and we looked at families and careers. We had data on everything. We had a lot of open-ended questions, and again, they're just priceless if you're doing something like these things because if you're writing up something of interest, the quotes are just—I'll try to remember one I talked to you about the last thing, a great quote from one I did for nuclear science. We can ask, "How satisfied were you with professional relations with your Ph.D. supervisor?" or "advice to beginning Ph.D. students in your discipline?" Just a sample thing, so you looked at the biochemists, and you'd have a question, "Would you definitely, now you're ten to thirteen years out, maybe you've got a job or not—" I'm so glad we did biochemistry because I hadn't even realized the enormity of the multiple postdoc issue in biochemistry. I hadn't really realized that. So, "—would you definitely do the Ph.D. again?" In the same field, the biochemists said 69 percent, the electrical engineers said 79 percent. At the same university, the biochemists said 53 percent, the EEs said 68 percent. If you multiply those together and say "definitely satisfied with your program," it's 37 percent for the biochemists and 52 percent for the electrical engineers. A lot of the advice from the biochemists was, "I should have gotten an MD for all the time and trouble I put into this." So, then we got recommendations for doctoral students for trying to get a job: be interdisciplinary, show breadth – these are what they said were important. Maybe we got a list, and they could pick and add. "Focus, define goals, choose the field that you love," and that showed up a lot. So, we got this out as a special issue of Council of Graduate School's *Communicator*, and a bunch of the stuff there. When I talk about postdocs, I'm going to talk about what we learned, so I'm going to talk about that with the vice chancellor of research stuff. Then, we did a big study on English Ph.D.s, and so, that came out as another CGS *Communicator*. That was really interesting, and it was reprinted, the English departments liked it so much it was reprinted in the Association of Departments of English *Communicator* in winter 2000. I think right now—because I look at *Inside Higher Education*, about every four months, there's complaints about the humanities and the social sciences—I'm tempted to mail it to this guy and say, "Just read this, it's probably all still here."

10-00:53:05
Burnett: It's still relevant, right, right, all of the ills and solutions.

10-00:53:10
Cerny: When you really look at it, well, in those years, 53 percent of the English Ph.D.s were tenured, and another 5 percent were tenure track, still, but less than a fifth worked at Carnegie Research I institutions, at which most were trained, because we're talking about the research universities that we picked up. Fifteen percent were in non-tenure track, and that's a total of 73. Sixteen percent were in this business, government, and non-profit, and when one looks at this write-up on the English departments, none of the people there are really happy, too. Five percent weren't in the workforce, and 6 percent, there wasn't much information. I think there's a quote like this in this nuclear science one, too, that's just worth reading: "Do not enter this field unless you feel that you would never be happy doing anything else. You should have as strong a sense of vocation as one entering the ministry because the sacrifices required for teaching in this field are as great as those required of a pastor. You'll have no time to call your own and you never will be paid what you are worth."

10-00:54:25
Burnett: That's pretty good advice. Maybe that's true of the Academy in general, I mean, at least compared to the private. There are some fields of some life sciences and economics, where you're definitely paid what you would be paid acting in the private sector, but still. Calling it a vocation isn't such a bad thing, perhaps.

10-00:54:50
Cerny: Maresi, she set up and we had a career management series for the Berkeley English Ph.D.s to really talk about their careers, and she got a two-day workshop for them. She got thirty-seven doctoral students to go. It was held on the weekend, their choice, because they didn't want the faculty to think they were insincere by doing it during the week. It was wonderful. They had a standing ovation at the end of it.

10-00:55:19
Burnett: It sounds like, from the report, they created some permanent supports for professional development of graduate students, how to get on the job market. Not just regular seminars, but there are actual institutional supports that are permanent and regular, including not just handbooks, but support networks and so on. So, that's become part of the legacy of what you and Maresi and others—

10-00:55:49
Cerny: Yeah. I don't know what's continued, and I really would like to see what the numbers were now, just to see how much this has continued. Many times, of course, when an administrator comes along, they want to do their own thing and not what the predecessor did. So much of this seems to be so important that I hope it has continued.

10-00:56:08
Burnett: Well, I don't know exactly what has continued specifically at Berkeley, but I know having gone through graduate school later on the East Coast, that a lot

of those became best practices. So, this is what's done, and then comparing this with other graduate students at other institutions, there was a lot of talk about and demand for professional development. Some of the things that you and Maresi and others innovated in the nineties did not come online at the University of Pennsylvania until the late 2000s. So, there was definitely, I know that it's being imitated and emulated elsewhere, or they're spontaneously coming across these, because of the demands and the changes in the job market, it's just in the air. This is what needs to be done for graduate professional development.

10-00:57:08
Cerny: Well, I'll tell you, I really loved this job, actually.

10-00:57:10
Burnett: It's clear.

10-00:57:15
Cerny: I didn't need the other job, and I would have just pulled the postdocs into this job and solved that problem, [laughter]. I was concerned about postdocs, but I'm just going to talk about it over there.

10-00:57:30
Burnett: Yeah, sure, but I think that you have a strong service bent and what the other folks that I interviewed said was that you are very principled and you like to provide service to the institution and to the students. You really care about improving outcomes for the graduate students, and I think that really showed. Well, perhaps we should pause, and we'll continue in our next session to talk about the vice chancellor for research position.

[End of Interview]

Interview #6: June 6, 2014
[Begin Audio File 11]

11-00:00:07
Burnett: This is Paul Burnett, interviewing Dr. Joseph Cerny, for the University History series. It's June 6, 2014, and this is session six, tape eleven. So, Dr. Cerny, this is the period of 1985 to 2000, and we're talking about the twin positions you held during that period, dean of the Graduate Division and your roles as the vice chancellor for research. Before that, there's some context you need to set because the positions themselves have been transformed a bit during the time prior to and leading up to your taking over. Can you tell us a little bit about the research positions and how they evolved?

11-00:01:01
Cerny: Well, actually, the vice chancellor for research position at Berkeley was a relatively new one. Engineering professor George Maslach had been the provost for the professional schools and colleges for a number of years. When Heyman came in as chancellor, I think he wanted to set up something central for research, and he decided—and Maslach had been in the job for a long time—he asked Maslach to come be the first "Vice Chancellor for Research and Academic Affairs," it was called. He did that only between 1981-83. Then, he stepped down and the chancellor asked Berkeley mechanical engineering Professor Chang-Lin Tien to come be the vice chancellor for research. Chang-Lin did that from 1983 to end of 1985, when he returned to the faculty.

Now, following my earlier discussion of the graduate dean situation, a background paper had been prepared by the Academic Senate, which, in thinking about the graduate dean, thought that should the research leadership position become vacant, it actually ought to be combined with the graduate dean position. So, the administration knew that, and most of the senate committees had signed off on it, and so it was very easy. They didn't have to run a search. So, Chancellor Heyman and Vice Chancellor Park just said, "Joe, why don't you do that?" That was that. The only thing I wanted was a different title because I thought it made sense to me here for it to sound more like an academic position than a chancellorial position. The campus then had the provost and dean for the whole College of Arts and Science, and it had a provost for the professional schools and colleges. I thought a provost for research sounded more academic, and after a while, Heyman agreed with me and we decided to do it that way. The reporting lines didn't really matter—I saw him when I needed to. We did that, and that title stayed until we make a big change in '94.

11-00:03:28
Burnett: Can I ask you about the rationale? Was there an explicit rationale for combining the two positions? They had goals in mind?

11-00:03:38
Cerny: Yeah, the senate committees thought that research was so important that it ought to be really—at that point, there wasn't any central administrative research in the title—related so much to graduate education, and therefore, it ought to go with the graduate dean. Now, they saw a different structure, where the graduate dean was up here and there were three junior deans of some type.

11-00:04:05
Burnett: Right, right, the associates.

11-00:04:09
Cerny: I think that was the feeling. Again, the campus wasn't used to having had one. I think now, the campus will certainly always have a vice chancellor for research.

11-00:04:21
Burnett: Well, yeah, it's surprising. These days, you can hardly imagine a university without a senior position that manages that, but that's part of the changes occurring. We'll talk about that organically, as that comes up. That sets up the two positions that you now have.

11-00:04:49
Cerny: I actually asked Chang-Lin, would he please stay on another semester so that I could maybe get used to the first job before I had to do two, but he wasn't interested in doing that. So, after four months as being dean, then I also was a provost. It was actually a really small operation, so when I started, it was very small. It had the sponsored projects office, which is Berkeley's name for the contracts and grants office, and it had the committees that were responsible for human safety. So, it had the protection of human subjects, bio safety, radiation safety, diving safety, and not a whole lot more than that. So, with that on campus, then, in terms of the Office of the President, the provost for research represents the Berkeley campus to the systemwide Council of Vice Chancellors for Research, which is just the administrators, to the Technology Transfer Advisory Council, which was trying to figure out what to do with the new Bayh-Dole stuff and whatever, and there's also a Council for Research, which had vice chancellors and research types on it. Then, this position interacts a fair amount with the Academic Senate Committee on Research, and the Committee on Educational Policy. So, you had all that.

Now, when I started, the key staff member that I inherited was Linda Fabbri, and she remained as my key staff member the whole time, and as we required more responsibility, she kept going through the administrative staff ranks. So, by the mid-nineties, she was actually my assistant vice chancellor for research, and so she was extremely competent, indispensable. She was a great organizer, very hard working, very smart, all these things. She was very connected to campus gossip, which my wife teased me about.

11-00:07:07
Burnett: That's always valuable.

11-00:07:10
Cerny: She really was an excellent hire, bringing the high quality people in, we knew, to work for us as our role expanded. So, almost all these people came from within the university, and staff in our office liked that. She was very good at bringing people in like that. An example would be Susan Hirano, who came in from I don't know where else at Berkeley, but was an outstanding budget analyst and could look at all the books and everything. We ultimately acquired more units than you can count.

11-00:07:42
Burnett: Right, well, let's talk about that a little bit. Presumably, some of the push for expansion is coming, especially when you're talking about technology transfer, that's coming from on high. The whole UC system is trying to figure this out at this time because they had a very, very small Technology Transfer Office, I hear, at the central office. It was a couple of people. So, they're trying to figure that out, and they're trying to build it out. How much of the expansion came from other sources or from you yourself, when you decided you wanted to do things differently?

11-00:08:25
Cerny: Well, I can talk about that, I can answer that now, and not when we really do it. In my staffing up, I also had Ian Carmichael as an associate dean in the Graduate Division, so he also becomes an associate dean of research. Ian and I got very concerned about technology transfer, but it was all locked up in the patenting and licensing operation in the Office of the President, and they really wanted it locked up there. So, we went down and talked to them, didn't get much of anywhere. Ian and I went to Stanford, which had a great Technology Transfer Office. We were blown away by seeing what you could do. We came back, yelled some more, and it took till 1990. But January 1, 1990, we then had permission to open our own Office of Technology Licensing. Up till then, the Office of the President had campus liaisons, and so, Berkeley had a liaison person who would come chat with the people that had come out to Berkeley to talk to you. I mean, that's just totally hopeless.

11-00:09:35
Burnett: Time-consuming and bureaucratic.

11-00:09:37
Cerny: Doesn't go anywhere, so we did that. Just jumping ahead, so we did set that up. We had a really great committee to review people for the director position. Ian thought we should also have really formal presentations of how you would run a technology transfer office under the Bayh-Dole. A person had just left one of the big biology companies, named William Hoskins. He was just terrific. He was the number-one interviewee and he came from bio science, and it was clearly going to be perfect. So, we hired him, and by some time in 1990, he was on board and started creating the office. We had to find the money for it ourselves, too, and the Office of the President wasn't sending us

any money. Even by then, there wasn't any money to send. So, then I had to root around and get the money to get it started.

11-00:10:33
Burnett: Were people petitioning you? In other words, were the departments saying, "We've got this crazy set-up with central campus, or UC Central Office?"

11-00:10:44
Cerny: To some extent, there were some faculty—not so much even departments—saying, "You know, we really want to do something and this is hopeless." I'd say it was on the faculty level, not the department level. Yeah, so they were saying that.

11-00:11:01
Burnett: You had these examples—you would go around and you could see Stanford was going great guns.

11-00:11:04
Cerny: We used that and said, "Look, this is what they're doing." So, UCLA and we, we're the biggest and screamed the loudest, and so we both got permission to start. I teamed up with the vice chancellor there. The President's office had a staff vice president, who really didn't want to give anything up, but he let us do that. Hoskins went on to be very, very successful, and I'm not going to be dwelling on that a lot, here. We had some really nice hits, we were able to fund our operation, but it was a constant fight, also, with the main budget person here, who would swear that he couldn't find the money that we said we'd made. The Office of the President controlled when we could sell the stock—they controlled it. So, we could have lost money in the collapse, but luckily, one of the big stocks we did have was sold in time. There, we made enough money to pay my expenses for five years, but I could never get this guy, even though he's a friend of mine, to admit it. [laughter] So, I think we did a good deal. Hoskins was terrific—actually, he'll return in my discussion of Novartis, later on.

11-00:12:21
Burnett: Did you get an explicit rationale from the Office of the President as to why they wanted to hold on?

11-00:12:26
Cerny: No.

11-00:12:30
Burnett: Was it redistributive?

11-00:12:31
Cerny: They didn't trust the campuses—how would we know what to do? They had patent and they had their counsel, they had all this stuff. They just wanted to control the campus as much as they could, and they hadn't related to Bayh-Dole's sweeping changes, and the fact you needed to move in a hurry.

Actually, I'll have a few, assuming we get to it today, side comments on this toward the end.

11-00:12:58
Burnett: About getting moving?

11-00:13:00
Cerny: About getting moving? No, this was about Novartis. Okay, so, we got really good staff, we had Linda, and if you then took a look at what was going on in the campus, the administrative elephant looming on the campus was all these Organized Research Units. So, you have all these Organized Research Units. They are collections of faculty around a common research interest that isn't tied directly into a department, necessarily. Back when you had state funding, it was a really good deal. A lot of faculty were interested in it. Actually, the first ORU was the seismographic stations, so they started in 1887, and then the ones up until the forties were mainly kinds of museums. In the fifties and sixties, when there was money, the state was funding ORUs and we acquired a very large number of them, which also promptly stopped in 1971.

11-00:14:08
Burnett: Was that a response to federal funding opportunities? If the departments were relatively inflexible in terms of faculty lines and the kinds of directions of research that they would undertake, the ORUs represented an opportunity to develop a research cluster around new or pressing concerns or advancements in the fields?

11-00:14:37
Cerny: Faculty getting together, but this was state funding of it, so then going in the state budget and being spread around and making proposals. You would try to tie it into things, and some, of course, would be good for leveraging state money or federal money. Some were social sciences, and there were a lot of social policy ORUs, there are a huge number of international area studies ORUs, none of which are bringing in lots of money. Faculty like it because if you are the director, then you have release time, and maybe you get 50 percent time to do this, and do half your teaching. You don't get a lot of extra pay, but it was very popular.

11-00:15:16
Burnett: But it has its advantages.

11-00:15:17
Cerny: But it had grown, and the review process for these ORUs, however, was really peculiar. I actually, early on in the job, had to give a talk in '87, so I did a little research, and I could find that talk. Nominally, they were reviewed every five years, and the Academic Senate did it. However, from 1961-75, these ad-hoc Academic Senate review committees had a confidential membership and a confidential report.

11-00:15:56
Burnett: Seen only by the Chancellor?

11-00:15:58
Cerny: Yeah, I guess—if that. [laughter]

11-00:16:02
Burnett: Buried for posterity—we'll have to go into the archives and see.

11-00:16:05
Cerny: We would have to see, I know. So, as my notes say, "which limited its usefulness." So, the ORUs then, for historic reasons, they reported three places. They reported to this provost for and dean for the College of Arts and Science, they reported to the provost of the professional schools and colleges, and they reported to the graduate dean, three different places. It depends how you count them—the numbers here, there will be no consistency in my number of ORUs for various reasons. That will become clear, some of them. By now, since there were the three provosts, I just said, "Look, they report all three places. I'm willing to take them. What do you think?" They thought, "Good idea."

So, in 1987, if you look at that list, we now acquire all the ORUs, and they transfer over to us. The graduate dean, I already had some over there, but I got mine moved over, too, so there were none under the graduate dean. Then, we needed a review, and a senate committee did this. Here, I'm a little hazy—it was done, it was looked at, I can't believe it was a major operation. Anyway, it was enough done that everybody was happy in July '88, for the moment. But other things needed to be done, and clearly, they needed standardized annual reports. Somebody needed to appoint the ORU directors and try to enforce the idea that it was a five-year appointment, which could be renewed once. So, the other provosts agreed, well, that'll be me, and so I would then have to go through the process of looking at all these ORUs. How long had the directors been in, what should we do about that, what did that review really look like, that the Senate did? I'm sure I cleared it with the executive vice chancellor, at least. If it were controversial—then you'd have to clear it with the executive vice chancellor and the chancellor, and I knew that.

11-00:18:22
Burnett: Were there some sinecures, where people had been there since—

11-00:18:26
Cerny: Yes. At this point, there were three ORU directors with tenures of thirteen to twenty years, so it was a real sinecure. You know, it's academic privilege, you don't want to dislodge anybody from these, and you know you make enemies for life faster in universities than almost anywhere else. So, then I learned that even straightforward faculty reviews of ORUs unanimously recommending removal of a director who had served six or seven years appeared to require two years' notice. So, I would go to the Faculty Club and have one of these ORU directors over and buy him a drink and say, "It's time to step down,

don't you think?" Then, I foolishly would say things like, "Well, you know, it should be in one or two years." They at least took the bait and took two years, and then we didn't have any acrimony. I did that a number of times. It was a joke with Linda, when I would go off to one of these fatal glasses of wine.

11-00:19:39
Burnett: I guess that gives them time to wrap up whatever research they were directing at the time?

11-00:19:46
Cerny: Well, there were all these other faculty, right? They had their own research, but you know, they have staff in these—they're all over the map, so it's all over the place. The faculty involved, there's no formula that really works, they're just totally different. They probably had some of their own research. Some people had been in way too long and the faculty didn't know how to get them out, and these provosts earlier on just weren't going to spend their time on it, I guess. We did that. In the course of this, a good thing happened, which there were so many international and area studies that the Senate recommended that a dean of International and Area Studies be appointed, who'd be in Letters and Science. That was really good because we had Middle Eastern studies and Southeast studies and South Asia, I mean, and Southeast Asia and East Asia, and Slavic and East European, and it all made sense to have a dean worry about that. My numbers would have gone up on this list, and they would have gone down by eight, and these sort of movements occur. So, they went over there and I stopped worrying about them. I think that worked out pretty well because this dean just worried about that, didn't have other responsibilities, so it was probably an improvement.

11-00:21:05
Burnett: Sure, and it was a class of research. Area Studies had emerged, and there had been federal money attached to some. [Federal revenues from] PL-480 went into Asian Studies. So, there had been these specific arrangements, but all clustered around the same theme, with the same goals.

11-00:21:26
Cerny: And there'd been new money in those areas, and in fact they were very good. It wasn't horrible examples or anything. Later on, maybe talking about rogue outfits. I don't apply that to those.

11-00:21:43
Burnett: Well, I guess ORUs are unique to Berkeley in terms of their nomenclature, but did you look to other universities to see what they had done with area studies? Did you have conversations with deans of research at other institutions?

11-00:22:04
Cerny: No, I didn't. I just decided that it actually was a good move, we should get them under some humanist and dean in area studies, and that person could work on that problem. So, in terms of the ORUs, it's been kind of trying to,

ultimately, improve the ones we had and to get new ones, but I never spent a lot of time comparing ours to others.

Just to mention some miscellaneous things, so, on the national scene, I actually was appointed to be a member of the National Science Foundation's advisory committee for physics, from 1987-88, and '88-'89. So, that was kind of interesting. This is the whole directorate for physics, so it has elementary particles to solid state. So, I did that, and then I was put as the chair of a sub-committee on program balance within the Physics Division. When I went to the first meeting of that, if I'm remembering correctly, it looked like a horrible waste of time. We were all supposed to come in and spend time looking at proposals to the NSF, and looking at the referee's comments, and then decide whether the people had been fair in accepting or rejecting them. It was really kind of hard to imagine how you're going to match one discipline of physics with another.

So, I suggested that we just don't do that. We have all the subdivision heads come in and tell us what they thought, when they'd analyze their own area, and we'd have a meeting where they all told us that. Then, we'd actually open up other meetings, when we could talk about policy in the NSF. So, that was accepted, so for that year, we did that. I don't know what happened after that.

11-00:23:56
Burnett: What did you talk about in terms of the policy of NSF with respect to physics?

11-00:24:01
Cerny: Well, there, just the money distribution in different areas and how it's going, or what does the director's office say? It's sort of then, in that case, informing us of the pressures that they would have on them and how did we feel about it—you know, it's a bunch of physicists, and you'll have an honest, candid discussion there about whether that made sense, rather than just some sort of advanced bookkeeping, to make sure that nobody was screwing over elementary particle physics or something.

11-00:24:27
Burnett: Right, it seemed to be a superfluous review process.

11-00:24:31
Cerny: It did, as far as I could determine. I don't have notes on that, but I sort of remember that as the gist.

11-00:24:46
Burnett: Well, I am still trying to understand, in general, being given two or being asked to do two jobs, and your first reaction is to enlarge one role in one area, and to make it more work for yourself, in a sense. So, in terms of your approach to your work, if you had to analyze these kinds of choices as an administrator, what does this represent?

11-00:25:21
Cerny: Well, it just looked like a disaster. I mean, if you sit there, I had the units reporting to me as the graduate dean, and you're saying, "What's this?" Right?

11-00:25:32
Burnett: And then when you get the answer, "We don't know."

11-00:25:35
Cerny: I mean, I know what the Hearst Museum of Anthropology is, but you know, why is it reporting to the graduate dean, and why are these others? There's no rhyme nor reason, necessarily. It just looked to me like a problem that we needed to start working on. I think, yeah, the other two provosts probably agreed. Also, we were just getting into, of course, the budget crises, and the major budget crises. Then, they're all in one place, and then this one place, you just assign one number, and that poor person gets to take care of the problem.

11-00:26:10
Burnett: So, some initial work upfront, and then it pays off, presumably, as things get smoother.

11-00:26:15
Cerny: I continued to worry about communication, and so, we did do, I'll mention two variants, we created several—not a lot of them—campus-wide brochures on research, which I thought were very attractive, just to show what was going on. I think that's died out. They were professionally done. I don't think they were terribly expensive. The first one was hard science to soft science to humanities to social science, it was all kind of research, it wasn't just science and engineering. That worked really well and we then produced a professional technology transfer brochure called *Knowledge at Work*, that was kind of highlighting the things that had been going on at Berkeley.

11-00:27:15
Burnett: Those were circulated on campus?

11-00:27:17
Cerny: Yeah, and I'm sure we sent them out to other campuses and things.

11-00:27:22
Burnett: To the press? Was this also a communications director issue as well?

11-00:27:27
Cerny: Right, right.

11-00:27:32
Burnett: I suppose that fits into one of the many mandates. I read this description—and this is, of course, presumably on the tail end of your tenure there—the nature of your job is to maintain an effective environment for research, to be responsible for all research training, and all compliance with laws at the municipal, state, and federal levels. Responsible for the dissemination and

outreach of research at Berkeley—so, that's presumably the brochure work that you were doing—and then the responsibility for these now twenty-eight organized research units, eight research museums, five field stations, the Office of Technology Licensing, the Sponsored Projects Office, which was originally there, always, the Office of Laboratory Animal Care, and the Office of Radiation Safety, and the Committee for the Protection of Human Subjects.

11-00:28:44
Cerny: Right. I hadn't acquired all those quite yet in this time sequence, but they're coming, right.

11-00:28:50
Burnett: You're just beginning to lay out what you're doing. To remind the audience of what this becomes, it balloons into quite a massive operation.

11-00:29:01
Cerny: It comes in, yeah. Of course, we'd been in a lot of trouble with animals before I started, so that was all gone and I didn't have to go back into that because we had mistreated animals, from the point of view of some people. We were setting up a new laboratory animal care building, and we had protests. We had a guy up on a crane for thirty days—however many, twenty or thirty—in the early days of Heyman.

11-00:29:31
Burnett: That was because of the reorganization of the life sciences, and they had to create a whole new facility. This critic came in and saying, "This is the end of the world and this is just such a terrible thing," and I think the guy just ended up proving himself to be a bit of a nutter. There had been some problems, but they were kind of localized, and the new facilities had been described as a kind of hotel for animals. They were pretty fancy facilities.

11-00:30:07
Cerny: They were pretty good when I went to see them, yes. The main veterinarian or the main person in charge had bulletproof glass on all his windows put in the building.

11-00:30:19
Burnett: Because of the—

11-00:30:21
Cerny: Protests, or someone just shooting at work. So, it was pretty tense. I remember seeing the guy that was up on the crane. I think that was actually before I took this job. Yeah, so we ultimately acquire all these things. We hired Hoskins, which was good, and then, this was late '90, I somehow talked Chancellor Heyman into a desperately needed upgrade of our seismographic stations. This would have been in '90, so it would have been after the Loma Prieta Earthquake, so maybe that would have helped. So, we got $2.86 million over four years, which I'm sure was being matched by some kind of federal and state money, and so I think this made a huge difference because our

seismographic stations had been really going downhill. So, that was a lot of money, when you look at it.

11-00:31:20
Burnett: Yeah, it's hard to believe that that would not be a priority.

11-00:31:31
Cerny: Heyman decided that ten years was enough.

11-00:31:34
Burnett: This is 1990.

11-00:31:35
Cerny: Yeah, and so, Chang-Lin Tien, after stopping being the vice chancellor of research, went back to engineering, and then he went off for two years to be the executive vice chancellor at Irvine. Then, he reappeared as our chancellor in July 1, 1990, which was the first Asian chancellor of a major university. Susan actually, when I went to the few AAU meetings, she went to the women's thing. She thought it was neat, you know, there was Berkeley, and there was one Asian American chancellor, not bad, and a whole bunch of old, white males.

11-00:32:23
Burnett: And a much beloved chancellor.

11-00:32:27
Cerny: Well, he'd been here, right, and he'd been in the administration, and so, he knew who he wanted to get rid of. So, he removed the vice chancellor for undergraduate affairs and replaced him with somebody else. He removed the main budget person who had done the job a very long time, and brought in someone else. Vice Chancellor Park left with Heyman, and so, John Heilbron became the executive vice chancellor. I don't think it was called that then, the vice chancellor. John Heilbron, I knew personally. He's a distinguished historian of science and a smart guy. He'd been chair of the Academic Senate for two years, right in there, and so a lot of the senate people who didn't know Tien knew John. So, he moved right into that job really well. So, he did it for four years, and I really liked working with John. In the huge VERIP crisis, he's the one that thought up that we should have this "Professor in the Graduate School" [appointment]. So, he thought that up and I implemented it with the Graduate Council. So, one of the reasons that a lot of the faculty didn't leave—I mean, leave after they'd retired—was they could become a professor in the graduate school, and they could still go to faculty meetings as appropriate. Anybody can have a graduate student who's an emeritus professor, but the Professor of the Graduate School is much better position for applying for grants and it appealed to a lot of people. So, he's really responsible for that.

11-00:34:03
Burnett: It's a fantastic idea, it really is, and that does happen at other universities. But to make some kind of institutional commitment to emeritus professors is really, really crucial.

11-00:34:17
Cerny: Well, we had both. I decided not to be a professor in the graduate school because unfortunately, my research by then had waned to a point I didn't think I could sustain the requirements, even though I basically was a co-developer of the project. But as an emeritus professor, you're still on thesis committees, you're still a member of the department. A lot of people I know on the chemistry faculty are just an emeritus professor, so they don't throw you out. John had a really good idea and it got implemented really fast, so that was great. John was just somebody that I could really talk to. We had some kind of a scandal that was going on, trying to get in the newspapers, about some African Americans who'd been injected with plutonium sometime in the mid-forties. It was carried over, coming out big in the *Chronicle*. That was kind of when UCSF was literally here as part of us, so it wasn't us, really, so it began to get murky. The chancellor was worried about it, but I remember talking to John. I said, "John, this has got no traction, this is not going to go anywhere." He said, "Well, you're probably right, but you should give me some money so I can hire a graduate student to look into it." So, I gave him some money and we did that, and it went away.

11-00:35:47
Burnett: To look at the base, the historical foundation for the claims?

11-00:35:49
Cerny: Yeah, what was really going on and who did it.

11-00:35:54
Burnett: Those are listed as some famous cases of bioethical malfeasance, right? So, they're on the books. They happened at many places; I think the ones that I know of were in Chicago, and a couple of universities in the South. So, this was something that was happening, but you and Dr. Heilbron both felt you needed to protect UCB from allegations that seemed to be unfounded, right?

11-00:36:31
Cerny: Right. It was really hard to imagine what we could do about it, and it wasn't probably even us.

11-00:36:48
Burnett: And the graduate student found that that indeed was the case?

11-00:36:52
Cerny: Yeah, it went away. Okay, so then what other people stayed. So, Jud King was the head of the professional schools and colleges. He stayed. Carol Christ was provost and dean of letters and science, and I stayed. So, really Tien only changed the vice chancellor and he had a bunch of people that he more or less knew, and so that all worked out. Just about the same time, of course, we have

the collapse of the Soviet Union, and we have the collapse of Southern California aerospace. So, that tends to reduce your ORU budgets, so then we had to reduce the ORU budgets $1.1 million in 1991, and $600,000 in ’91-’92. This whole nineties are seeing the ORUs from the front row of who we’re gunning for to reduce budget. So, we had to do that. Now, here, some staff early retirements helped because that period, also, you had the early retirement. Staff could take it, and so, that did help get through that.

Then, in early ’91-’92, our information systems and technology was essentially falling apart. So, we had had a guy who’d been hired in who really just loved supercomputers, and he’d wrangled some great deal to get a Cray supercomputer, which the chemistry theorists just loved, but he didn’t have a plan to pay for it. They were going to put all of their research money in as part of that, but it was running $2-3 million a year, and they might have had $200-300,000 total, so then you had to find the rest. So, that idea collapsed. He left. They put a faculty member from social science in as the head of computing. We needed to make many changes—that didn’t work. So, they said, “Give it to Joe. He’ll think of something.” So, we got that, right? It’s still on the chart, way over on the right.

So, we had a really major search, a national search to find somebody to come in to do it. We had nine people, we had a great search committee, and we had three finalists. The first finalist was a really, really good guy at the University of Washington, but he had such a good deal—only worked four days a week and he liked living up in the mountains the rest of the time, and not coming down. We made him an offer, but I don’t think it would have worked if he’d taken it. The second person on the list, it turned out, by the time all the engineers heard about it, they didn’t like it—even though we had engineers on our committee. So, we kind of removed him. The third person was a woman from the Midwest, and she came in for another final interview, where we were going to offer the job, and it was clear she’d just been on the job market for fun because she had a husband who wasn’t about to leave the Midwest, and she wasn’t about to come here and run it. So, there we were.

11-00:40:18
Burnett: To clarify, this is for all information technology on campus outside of—

11-00:40:25
Cerny: Departments.

11-00:40:28
Burnett: Okay, just for the departments.

11-00:40:31
Cerny: Well, outside the departments. Yeah, it’s general computing, it’s the Internet, it’s everything that gets out of your office. Luckily, this highly regarded, past dean of engineering said, “There’s this really bright, really great guy named Jack McCready coming on the market” because DEC is going into

bankruptcy—although they didn't think it was bankruptcy quite then—and they'd had to get rid of really good people with terrific offers, so he took it. So, Jack McCready came and interviewed; perfect. He had a personality and he knew all this stuff, and so, just like Hoskins, he came about the same time. I didn't mention, Hoskins stayed and retired about 2005. McCready came about the same time, maybe slightly later, and so, he was very, very successful, and he got us through all the stuff we had to get through. So, that was a real plus, and so I could forget about that. We would meet every so often, and he was on my internal cabinet when we had internal cabinet issues.

11-00:41:41
Burnett: So, were there fundamental upgrades, like brand-new servers and the fiber optics? I don't know if that was coming on at that point; it was a little early.

11-00:41:47
Cerny: Everything. Yeah, you had to dig your trenches. The guy with the Cray, which was a good idea, except we couldn't afford it, he started trenching the campus so we could put it in. By the time the whole changeover of the year 2000 was going to occur, Berdahl was chancellor, and he didn't want to spend any money on the changeover. He kept asking me, "Do we have to do this?" McCready kept saying, "Yes, you do." So, we did. So, Jack could handle all that, and anyway, so these two, Hoskins and McCready, and Linda Fabbri, were just great appointments.

11-00:42:40
Burnett: Right, right.

11-00:42:41
Cerny: So, that really worked. So, I was really pleased, and they stayed a long time.

11-00:42:47
Burnett: Having that institutional continuity is a theme because you stayed a long time. The average tenure for, speaking of your other job, the dean of the Graduate Division, is around three to five years.

11-00:43:00
Cerny: Is it?

11-00:43:01
Burnett: Yes, so you know, there's you and there's your predecessor.

11-00:43:08
Cerny: Well, Elberg lasted seventeen.

11-00:43:10
Burnett: Elberg, yeah, but he's highly anomalous.

11-00:43:11
Cerny: But there was a guy in the twenties to thirties, when kind of business was slow, I think he did it 23 years, here.

11-00:43:21
Burnett: Yeah, but that kind of institutional continuity at that level is fairly rare in universities. So, it's an incredibly advantageous to have people like you, and Jud King's contribution was tremendous during that period, and just staying that long period of time has a tremendous impact on institutions.

11-00:43:44
Cerny: Well, King, he was the department chair quite a while, and then he was provost for a while, but then he went system-wide, and was system-wide provost.

11-00:43:57
Burnett: While we're on the subject of Jud King, did you have any interaction with him because of the Center for Studies in Higher Education? I'm switching hats to your other job at this point.

11-00:44:10
Cerny: Well, I knew him anyway because I was chair of Chemistry at the same time and he was chair of Chemical Engineering. I was going to mention, once Tien came in, the system changed a lot. A lot of us provosts actually would have informal lunches, separately. So, I would have lunch with even Heilbron or with Carol Christ or with Jud, just so we could have informal discussions of our problems, so I did that with Jud. Then, he decided, in the organization we're going to get to in a minute, to go to the Office of the President.

11-00:44:57
Burnett: Well, we can pass to the next—I didn't mean to digress too much.

11-00:44:59
Cerny: That's fine. So, in '91-'92, then it was decided with the other two provosts that I better have all the natural history museums. I agree with that because I thought the museums were really important, and I thought they were more important than a number of the people on campus probably think. I thought I could handle their budget hits better. So, we took them. Particularly, the Hearst Museum, we poured money into. It was always getting money poured into it. It used to have an in with the previous budget person who didn't stay on when Tien came in because they could never deal with it. An unbelievable collection and inadequate housing. Everything was getting better there, but there's $2 billion worth of artifacts at the Hearst Museum. If you could just sell a little bit of that stuff—you've never even seen them all. You have to take a tour. You ever get a chance, go take one of the tours.

Yeah, they've got everything, so we appointed the directors—I think they reported to me by then—and all this stuff. So, we brought them in. I thought it'd help because so much pressure, then, on the budgets, and I thought people would be deliberately understaffing the museums. They were all great museums. Again, everything in here gets reviewed. I'm going to conclude with saying at the end of the fifteen years, everything, with one exception, has been reviewed at least once. Some of them never had been reviewed, like the

Essig Museum of Entomology. Some, just never, so they actually got really good reviews, but you should still have reviews. Then, I got them to join together as a coordinating council for Berkeley Natural History Museums to get together, and that worked. Of course, some of them got really, really good space in the new, renovated life sciences building. Have you been in to see the—

11-00:47:03
Burnett: The Valley Life Sciences Complex? I haven't been inside; I've admired it from outside.

11-00:47:08
Cerny: It's good, and you can also go in and see there's a tyrannosaurus rex right in there, that I paid for this little part of the rear end. So, there's a lot in there, and there's a nice pterodactyl in there, too. It's beautiful, and you can go see them, we can just go look at that. It's just great. A lot of really good people, so that worked. So, then they decided I should chair the Richmond Field Station Oversight and Planning Committee. So, the Richmond Field Station, you know where that is, roughly?

11-00:47:39
Burnett: Yeah, it's where the library has its collections housed.

11-00:47:44
Cerny: Right, so it's there. Engineering has an earthquake platform test station there, and some other buildings. It's a great spot. Right now, LBL and the campus have had to deal with the Office of the President and make it a development site. When I was doing this, engineering somehow would have those things out there that couldn't be on campus, like the shake table, but twenty minutes was really too far to go. Engineering doesn't really want to cross the street, if you're in one building and you had to cross the street to the other, that's too far. There is this walkway between two of them—I guess that's acceptable. So, they didn't want to go out there, so anyway, they didn't. You go down, there's a great site at the end, so okay, you put in a small faculty club at the end, be a nice place to have lunches, a beautiful view of the Bay across the thing. Great to develop.

The only thing I did there was a little later on, when our mayor, Bates, was with the legislature, and the state wanted to consolidate all their public health research facilities into one place. He thought a nice place would be to take a large part of the Richmond Field Station. So, he came out and we talked about that a number of times. I don't know if you've ever seen the building, but if you're driving to the Richmond-San Rafael Bridge and you look out to the left, if you're going that way, there's this giant building with all these things on the top? That's it. Well, it would have filled up the space—I mean, we didn't have the space. We'd have hit the height limits. We had to put up with these legislators beating us up, till after a while, they got tired, when we kept proving to them we couldn't do it. Luckily, there were some places that you just couldn't possibly build on. So, we got rid of that idea, but the main thing I did was preserve the Richmond Field Station.

11-00:49:48
Burnett: Just recently, in the last couple of years, LBL is trying to set up a second campus there that is going to have level III or level IV biohazard.

11-00:49:52
Cerny: Yeah, LBL had decided it was looking for a second campus. I don't know about that.

11-00:50:05
Burnett: It's going to be genetic engineering, apparently. This is according to the press.

11-00:50:10
Cerny: They'll never do level IV. I wouldn't even spend my time starting that.

11-00:50:19
Burnett: I don't know how you would get level IV in an earthquake zone like that, unless you had it on one of those shake tables, and you could sort of cushion the blow. Apparently, that's what I've heard and read, that LBL is trying to develop that area.

11-00:50:36
Cerny: Yeah, and I think that's really interesting. I'll say here, for the record, I'm very surprised that the campus would do that. I wouldn't give up that space to LBL, and I'm at LBL. The synergistic parts would really have to be explained to me to make sure it made sense, but it can be true, engineering has not wanted to put any of their new buildings or anything out there. The big ones are all here, and so, maybe that's what you do, but nobody asked me my opinion, so it's not my problem. The other thing I got to chair, kept me busy, was the Memorial Glade—the Memorial Glade out there, right? So, until the mid-eighties, we had six long—you probably weren't ever here then—World War II buildings that were long and they were wooden.

11-00:51:26
Burnett: Like Quonset huts?

11-00:51:27
Cerny: They weren't Quonset. They looked more rectangular than the Quonset hut, but they were temporary buildings left over from World War II that were running right up the hill there, and all but one had to be taken out for the underground library addition. Then, the last one, there were some faculty who wanted to stay in it, and Tien said, "Should we let the people stay in it?" I said, "My opinion is of course not." So, he got out a bulldozer and leveled it. Now, above that, we have the Memorial Glade. We had architects come in, and landscape types. I just wanted something really nice—I think it turned out really well. I just thought some of the alums would want a bandstand with Greek fire, and I wanted to make sure we didn't have that, so we didn't. The trees around the end look good, and the students love it, right?

11-00:52:20
Burnett: They really do.

11-00:52:22
Cerny: Because it's away from Telegraph Avenue, it's not Faculty Glade, where you're not supposed to run around. It's perfect.

11-00:52:28
Burnett: It's open, and it's just this simple, open space.

11-00:52:31
Cerny: Yeah, I liked it, so that was a fun thing to chair.

11-00:52:37
Burnett: There are a lot of un-fun things, I suppose, in all of the building management that people had to—

11-00:52:43
Cerny: The un-fun things are when you don't have enough money and the value engineering people come and rip out all the nice stuff. That happens everywhere, and you say, "Why don't we really have it look nice for the future?" "Oh, well, we can't."

11-00:52:57
Burnett: That happens a lot, I suspect.

11-00:52:59
Cerny: One thing that was fun is somehow, engineering had set up a liaison with the Grande Écoles for engineering in Paris. There was a Symposium I held in Paris. Well, Tien said, "You've got to handle the coordination for the International Symposium II." It was the culture of engineering in a rapidly changing world, and it was a four-day conference, hosted by Tien, in conjunction with the conference of the Grande Écoles. There were 200 participants from 18 countries, and a lot of talks and things, many of which I went to. We had a nice dinner in the student union, but the really nice dinner gets me to another subject. So, there's Blackhawk—do you know about the Blackhawk Auto Museum?

11-00:53:56
Burnett: No, I do not.

11-00:53:57
Cerny: In Danville, there's a Blackhawk Auto Museum, which is first-class. It was built by a guy named Ken Behring. Ken Behring is reviled by many people in the newspapers—he's a developer—but anyway, he developed it and he put an auto museum there. He also developed that section of Blackhawk, made a lot of money. We thought for the banquet that, okay, he had a tie-in with us because this automobile museum, he convinced Heyman that it kind of could be a non-profit adjunct of Berkeley, somehow. Actually, there would be five board directors, and in the first ten years, there would be one director from the

campus and four of them, and then in the next ten years, there'd be two of us; then there'd be three, and then with the three and two, then Berkeley would own it. So, he figured we might have $100 million worth of cars in twenty years.

Now, actually, I got to be on that board. I wasn't on it when there was only one Berkeley rep, but when there were two Berkeley reps, then I was on it for a number of years. Toward the end, I'll return to the museum, Carol Christ and I would go out, and the other board of directors were all appointed by Behring, plus a sidekick, and all the other board members were men. They'd never seen a woman before in the board room, and so it was very different. So, I told Carol, when I'd leave, I said, "Every time, I check I've got my wallet." Anyway, they didn't know what to do with Carol. Ultimately, because Behring didn't want to give that up—who knows how correct it is—another deal was made. So, before the third director got on and we owned it, another deal was made. We got out of the museum and he donated some money to the school of education and advanced programs.

11-00:55:58
Burnett: Okay, so he backed out of the deal?

11-00:56:00
Cerny: Yeah, he changed it. Anyway, it's a great automobile museum. That's where we had our dinner, and the French loved it. They're beautiful cars.

11-00:56:07
Burnett: I bet. It's a history of automobiles, sort of from the earliest cars?

11-00:56:13
Cerny: Yeah, and he has other things, too. So, it's definitely worth going to, and so the French thought that was nice. Now, interestingly, about this time, there was a French physicist who was the scientific attaché in San Francisco to the consulate, and he came over a number of times, since he certainly went to this. Somewhere, right in this period, the France-Berkeley fund was created primarily with French funding. So, that's a fund between France and Berkeley, which we then expanded to include Davis and Santa Cruz. It's joint research projects. So, you have sponsors on both sides and they get to evaluate it, and you do that. So, that's really nice. Then, along with this, you have to have a management meeting every year, so then they alternate. So, then I went to the management meeting in 1996, and it was on the Riviera, which was nice—although we went first to a tech transfer meeting, which was a little inland, but then we actually stayed in Cap d'Antibes, in a place I don't want to think how much it cost. Then, two years later, I went to Toulouse for the meeting, and then my last trip was in 2000, when it was in Paris. So, that was nice.

11-00:57:29
Burnett: Lovely. Yeah, it fits into your travel theme. It worked out well.

11-00:57:34
Cerny: But the fund's still operating, and I was just asked to review one of the joint research proposals last year. It's not France-USA, right, it's France-Berkeley.

11-00:57:42
Burnett: That's what's interesting about it.

11-00:57:44
Cerny: We're our own city-state, right?

11-00:57:46
Burnett: That's right, I guess so. I guess the French university system is so centralized that that's how it would work, if the French educational system cooperates with outside forces, it's going to do so as a whole? I'm guessing.

11-00:58:09
Cerny: Do you know, right now, because the French are rating so poorly in *Shanghai* and *London Times*, *Higher Ed*, you know, evaluations of higher education, they're just not showing up. So, they're really trying to do something to reorganize. There was just something written up, that five of them now are calling themselves the Sorbonne in some part of their title, so you'll have to really worry about which ones are the real Sorbonne.

11-00:58:56
Burnett: Oh, that's interesting, that's interesting. Do you think we should pause to change the tape? I think we're getting late.

11-00:59:01
Cerny: Sure, okay.

[End Audio File 11]

[Begin Audio File 12]

12-00:00:14
Burnett: This is Paul Burnett, interviewing Dr. Joseph Cerny, for the University History series. This is session six, tape twelve, June 6, 2014. So, we were talking about the various responsibilities that you had taken on and then built for yourself, in this long process of taking on the organized research units, and delegating some of them out, but also managing the field stations, managing the research museums, taking those on. So, was all of this smooth sailing, or were there moments when you encountered some difficulties?

12-00:01:05
Cerny: Right, well, we had some difficulties. The first difficulty was this incident involving the Western Consortium for Public Health, and the accusation that a number of faculty in public health fields were making excessive amounts of money. This started around '92, and it ran on for a long period of time, but to

really get this correct, the Western Consortium on Public Health was actually established as a non-profit in 1975, for the purpose of continuing education programs and activities in the field of public health. Its establishment was approved by the Regents for the purpose of continuing education programs and activities in the field of public health. Its directors were comprised of representatives of the UCB and UCLA campuses. Ten years later in ’86, the consortium’s activities were expanded significantly into areas of research. Neither the Chancellor’s Office nor the Regents were aware of or approved these changes.

In 1992, the Berkeley Central Campus Administration became aware that members of the faculty were submitting research proposals to the federal government through the consortium rather than through our Sponsored Projects Office. NIH expressed concern and confusion about the relationship between the consortium and the university. The campus initiated an investigation and NIH was informed of the action. The Sponsored Projects Office was heavily involved in this, and Nancy Caputo, who was the director then and who also helped out on other things later, is an outstanding university employee, really looked into it. It was very time consuming and complicated, I can assure you. This is kind of a report to the chancellor. The central concern of the investigation was the administration of federal research grants through an off-campus entity which used the name and seal of the university in a manner that mischaracterized its affiliation with the university.

It was determined that several members of the faculty received compensation for their federal research activities through the Western Consortium at the same time that they were receiving 100 percent salary from the university. The campus auditors concluded that these activities violated federal research principles. Finally, twenty-four faculty were involved. During the academic year, you get 100 percent of your salary. You could buy yourself out on a federal research grant, so if they agreed, you could buy out 25 percent of your time, do more research, and get that money from the federal government, but you couldn’t get above your normal, 100 percent salary. In the summer, it would be a little different matter. You’re not a university employee in the summer. That’s not what they were doing—they were double-dipping. They were reporting through the consortium, and some of them were making substantial extra money.

So, when we finally had everything prepared, we thought the best way to get ahead of this was that public affairs arranged for me to speak to a couple of people on the *San Francisco Chronicle* and explain the situation. So, it had taken a very long time to sort through, but it was January ’95, and so I did that, and then a piece was run in the *Chronicle* and was picked up, nationally. These twenty-four faculty were involved, and then the process just kind of continued. All funding through the consortium stopped, but the investigatory process continued, and continued on and on. I’m quoting from something from ’97. Now, I’ve never seen the Office of the President move so fast. It was unbelievable. In December ’94, even before this, the Office of the President issued a new contract and grant policy specifying that all grants must be run through the campus rather than through outside activities. It didn’t take twenty years, it didn’t consult with the Assembly of the Academic Senate, it just did it. I thought that was great.

In addition, OP just did it that the policies on limitations of outside faculty activities were clarified in the fall of ’95. Now, it’s conceivable they talked briefly to the assembly, I don’t know, but I kind of doubt it because it moves slower than a glacier. So, the rules are, you cannot get paid from the federal government during the academic year. If you want to do private consulting, you can privately consult one day a week during the academic year, if you tell your chair and dean about it. That’s it. You can make whatever you want, if it’s private.

12-00:06:44
Burnett: So, was there any lack of clarity? By the Office of the President, specifying this so explicitly, did that imply that the rules weren’t clear before?

12-00:07:06
Cerny: Yeah, I think they were clear. You probably knew you could do a day a week, privately, but I’m not sure it was really formalized very well. That did get formalized, and right now, we have a real document you had to sign every year about what you are and are not doing, and it goes in, in the fall, right? So, that’s what they sort of did with this memo coming out in ’95. In this case, the Public Health faculty had to know, and the science and engineering people thought they were fools. I can talk to any chemist who knows these guys, they’re just cheating the system.

Privilege and Tenure upheld them, and I had to go to argue with Privilege and Tenure, and I think the chair of that was a humanities professor who, in my opinion, didn’t have a clue of what was going on, but [felt that] the faculty were always right. So, Privilege and Tenure heard complaints from individual faculty members regarding the campus investigation and concluded that the investigation violated the rights of the faculty, that the conclusions of the report were flawed, and that the campus shared information with the federal government in violation of the confidentiality rights of the faculty. Come on! I had to sit there and listen to stuff like that for hours. So, they were in the wrong.

Now, they may, for all I know—I think some of them almost had health problems from it—have not been fined heavily, they might even have gotten away with it. I don’t know. We had an assistant DA come over from San Francisco and talk to us, and then she really wasn’t up to this task, it was kind of clear, and then we heard from her office, very indirectly, well, she’d missed some deadline for filing some kinds of things. So, I think it wasn’t quite what the DAs normally do, and it was all murky as could get out. That’s what happened, and it went away. The faculty stuck around—I don't know if they ever paid anything back. It was outrageous.

12-00:09:25
Burnett: You were concerned for the reputation of the university and you needed to manage that?

12-00:09:29
Cerny: Yeah, but anybody that just reads what's on the federal rules about faculty compensation and federal grants, it's clear, perfectly clear. The Western Consortium for Public Health may have been one of the outlier units, for all I know, but it was very, very good that we got out in front of this. There wasn't anything about Berkeley's hiding zip because I was out as early as I possibly could be. We weren't naming any names, but absolutely early, and so, that was fine. So, I'm glad we did that.

12-00:10:08
Burnett: The alert came from the NIH?

12-00:10:10
Cerny: The alert came from the NIH. We were trying to remember that, but it did, and I was actually talking to Nancy Caputo because I see her regularly. Actually, Linda and Nancy and I have lunch every three months. So, she couldn't remember exactly what the trigger was, but the triggers here are, I think, NIH noticed these grants were all coming through this other outfit, somehow. I think our reputation was okay. It was a mess, but nobody blamed us for not keeping up with the Western Consortium, and who knows whether Berkeley and UCLA were still on the board or not, ten years later. Anyway, so, that went away.

It comes to be 1994, and the university reorganizes a bit because now, the third VERIP has happened, and Heilbron decides to take it. John Heilbron retires from being the vice chancellor, goes to England, Jud King decides to go to the Office of the President and be the vice provost for research. So, Tien thought, and we had a budget crisis, we had an extra layer of administrative bureaucracy, and so we should remove the provost level. So, we should remove the provost of the college of letters and science because you've got deans of letters and science, and the deans of professional schools and colleges don't need to go through a provost. They can just come directly up to the vice chancellor. So, those were just removed. My title was changed to "Vice Chancellor for Research." Carol Christ becomes the executive vice chancellor. They'll say I report directly to Tien, I'm sure I did, but Carol ran all the academic planning stuff, so it didn't really matter who you reported to. Now, is when we get for sure the Office of Laboratory Animal Care, and the Animal Care and Use Committee, and we get the Lawrence Hall of Science coming in.

12-00:12:31
Burnett: So, you had extra resources to deal with this?

12-00:12:37
Cerny: Didn't get any extra resources when I did any of these things. Nothing came.

12-00:12:40
Burnett: So, you've lost Jud King, you've lost John Heilbron.

12-00:12:44
Cerny: Yeah, well, you get two big slots because the two provost slots disappear. You have a vice chancellor position, yeah, so the campus saved some money and they had some small staff, yeah, so you saved money.

12-00:12:57
Burnett: Yeah, you save money, but you lose brainpower, experience, resources.

12-00:13:03
Cerny: Well, it kind of was an unnecessary layer. I don't know how it came in. It worked.

12-00:13:10
Burnett: So, you didn't feel it too much?

12-00:13:12
Cerny: No. No, because I was interacting with all these people anyway, and we were all collegial, and I was always reporting to the executive vice chancellor. The good staff stayed.

12-00:13:32
Burnett: So, that's '94 that that takes place.

12-00:13:34
Cerny: Right. Then, Tien didn't want the University Art Museum, to report to him, and the University Art Museum always wants to report to the chancellor. In desperation, it will report to the executive vice chancellor, but it will not report to the vice chancellor for research—but they had to for a while. We'll return to them, briefly. Now, faculty pressure, back in one of the big budget cuts, the Center for the Study of Higher Education, which has its own history that I'm not elaborating on here, but I certainly can, and I did a lot of stuff with them and Maresi came from there, and they were very cooperative, but they always reviewed very badly, very badly. They didn't raise any outside grants and they just treated it as a sinecure. You do a review where you've got to cut the budgets and then the committee comes in and says, "What?" So, they returned to me. They had gone to the Senate somewhere, to report, to escape from my grasp, but Tien wrecked that and returned them to me.

So, we again had to review all the ORUs in '95-'96. In this case—and by the way, any time there was any of these reviews, I went to all the reviews, all of them, of everything—twenty-eight ORUs were now reviewed in a two-and-a-half day period by a committee chaired by Professor Cal Moore, who's a very distinguished mathematician and who's been an administrator at Berkeley and all over the place, and still is very active in retirement. So, he did that, with a mixture of, like, six senior faculty at Berkeley, and I asked Dick Attiyeh to come up from UC San Diego, who was the vice chancellor for research and the graduate dean and an economist, and so, they reviewed all these ORUs. They had to find $750,000 in budget cuts, about 8 percent. The Center for the Study of Higher Education got cut $180,000 of the $750,000, and the guy chairing it wasn't very happy.

It's always been my fault because here, the committee decides it and I implement it, but it's my fault. Previously, I think President Gardner, the other time CSHE'd been cut, he gave them some more money, so they had some money. They got reviewed again, and it got cut again. The director that did the review, I won't mention his name because I like him, he made a terrible presentation. Just terrible.

12-00:16:19
Burnett: So, it didn't sell anyone on it. Presumably, you would be a defender for the Center for Studies of Higher Education?

12-00:16:32
Cerny: I'm attending these meetings to get informed. On this, I just review their report, and then I implemented their report.

12-00:16:45
Burnett: Well, I understand that as now vice chancellor for research, you are playing straight on all of this. In terms of your feelings about this, it's exactly in line with the work that you're doing as dean of the Graduate Division, and you used some of their resources, and in other words, you understood the value of, at least in principle, what this center was doing.

12-00:17:24
Cerny: But they weren't proactive—ages ago, I guess it had been used as a place to get grants in higher education, and somebody did that a lot, and they didn't like that, the other faculty didn't. They didn't get any grants. They didn't have a high profile. They kind of treated it just like a local center where people were welcome, but they weren't really proactive in getting out and doing a lot of research.

12-00:17:52
Burnett: Were they closely tied with the education faculty?

12-00:17:56
Cerny: No, they weren't. Well, here it said, the comments were: "Mission's not clear," "Productivity is relatively low," they left the core budget to be $70,000, but they had a place. A big engineering unit got cut $175,000, so here, they said the traditional lines of research in the unit have run their course and they have not successfully redefined their mission.

12-00:18:33
Burnett: This is of the engineering unit?

12-00:18:35
Cerny: This was one of the engineering ORUs, hunting for the $750,000, so they got hit $175,000, and CSHE got hit $180,000. I didn't discuss that with the College of Engineering, but I couldn't do anything about it. We had to find $750,000.

12-00:18:58
Burnett: I suppose this is a shock to the ORUs, in that they haven't had to hustle much before, or if they did, it was under their own steam—they felt that the entrepreneurial ones would be entrepreneurial, and the ones who weren't as much, but there was no outside pressure as much because when the money was flush, it was flush, but in the context of these budget crises, ongoing budget crises, coupled with a kind of entrepreneurial reorientation of the university? Just by default, right, because there's less money, you have to do something.

12-00:19:42
Cerny: We'll see it later in the big Sunset to Dawn review; the humanities faculty have zero experience with a review committee coming and reviewing them. They do not know how to cope with it. The social sciences can not be very good, either. Engineering, they're all set, right? That's what they do for a living.

12-00:20:04
Burnett: That's what they do for a living, cost-benefit analysis.

12-00:20:06
Cerny: They come in, they thank you for listening to them, they're well prepared, they lay it all out, and they go, right?

12-00:20:12
Burnett: They give you the numbers. Well, I wanted to ask you about this because I don't know if you've got data off the top of your head, but the representation of scientists and engineers in administration at universities in general, I would think, would have to be quite high. Just anecdotally, as you talk about all of these reviews conducted by a mathematician, an economist, a physicist, a chemist, an engineer, all of these people have—well, perhaps to the exception of an economist; we'd have to see on a case-by-case basis—experience running budgets, running large budgets, managing, in some cases, large technological systems. Is that a fair claim to make? I don't have any evidence, I'm not citing any report, but do you find that that's the case?

12-00:21:12
Cerny: Like Bowen and Rudenstine, Bowen was a labor economist and he was president of Princeton, I don't know, fifteen, sixteen years, and then had the Mellon Foundation, which makes a lot of sense. Rudenstine was an English professor.

12-00:21:26
Burnett: Our current chancellor is a historian.

12-00:21:32
Cerny: That's what Berdahl was, German historian.

12-00:21:37
Burnett: Those are senior positions, but in terms of nitty-gritty, these are budget reviews, largely.

12-00:21:50
Cerny: It's also this dichotomous thing, we're talking about the times to degree and success rate, and the style is so different in the humanities and part of the social sciences. You're a loner, you're doing your own thing, you're not being reviewed.

12-00:22:02
Burnett: [Contrasted with] team research in the sciences.

12-00:22:03
Cerny: Right, team research. For a while, LBL wasn't reviewed, but I mean, when I was an assistant professor, later on, you're reviewed all the time, right? The visiting committees come through. You better do it right. In the humanities and some of the social sciences, the humanities are rarely reviewed in that context.

12-00:22:29
Burnett: I don't know. I don't know exactly the case at Berkeley, but at other institutions now that I've been a part of as a professor, I was at a liberal arts college, and we had to do an external review. We had to bring in people and we had a very, very thick document to prepare. We had a day of visits by the reviewers and interviewed students and interviewed us, so it was quite an elaborate process at a tiny, little liberal arts college. So, I think that practice has now become so engrained in university cultures across the board, it's just considered best practice in 2014. Whether you're humanities or social sciences or sciences, you have to do it this way. The humanities may not have been familiar [with the process], but they had to learn.

12-00:23:22
Cerny: I think that you're quite right. I think it's changed, and in this case, it was more senior people that I'm thinking of. One senior historian said last time he showed an overhead was when he was in the Army.

12-00:23:34
Burnett: Didn't you tell me a story—this was off-camera—once about when Heyman came in, he launched this major capital campaign, and I think he asked some professor to do something—is that that story?

12-00:23:59
Cerny: I didn't tell you that story; somebody else did, I think.

12-00:24:01
Burnett: He asked a professor to do something and he said—oh, I think it might have been Linda Fabbri, actually [who told this story]— [the professor] said,"I *don't do* fundraising." It's hard to imagine. Of course, the fundraising has this terrible tinge to it, but in a sense, today, in 2014, faculty across the board do fundraising of one manner or another.

12-00:24:29
Cerny: Right, that's changed a lot, I agree.

12-00:24:33
Burnett: That illustrates the change, I think.

12-00:24:38
Cerny: We have the Humanities Institute, and that's really well funded. That was fundraising, and that was really great, and so, everybody recognizes that more.

12-00:24:55
Burnett: You almost seem to relish it, the process. Well, I don't think you relished necessarily going through the reviews when you were chair of the department of chemistry, but you have a particular knack for it. I can see part of the appeal being needing to justify the meaning of what you do to outsiders, that it's an incredibly validating process, and it's healthy. It's not healthy if you get all your budget cut, but it can be healthy to justify to different stakeholders the value of the institution that you are a part of, on an ongoing basis. That serves everyone well because everyone is thinking about excellence in different ways, right. So, what you're experiencing in this period is that defining, researching, and presenting excellence is a challenge for some folks. They're struggling with this.

12-00:26:04
Cerny: We'll see at the Sunset to Dawn reviews that we actually create two ORUs in the arts and humanities, at which time there were zero.

12-00:26:17
Burnett: Is that communicated over time, that there are new possibilities for, say, once you reorganize the organized research units, there will be opportunities, and people start to think about how they could become entrepreneurial in that respect?

12-00:26:40
Cerny: Well, that, I don't know. I think it was a surprise when we did this review, but I wanted to have new ORUs created, and so we did it. That hasn't happened since.

12-00:26:52
Burnett: Right. Well, before we get to that, I think you want to go in the sequence.

12-00:27:03
Cerny: Okay, so then, it's '96-'97, College of Natural Resources decides to transfer the [Berkeley] Botanical Garden to us, and I think that just about concludes our empire. It's out of control. We get several field stations also transferred in, and they're also fun, by the way, to go to. I went to three of them after that. There's an Angelo one, up where Highway 1 goes across to 101, and then you go up north in there. It's really interesting.

12-00:27:34
Burnett: Around Bodega Bay, as in Point Reyes?

12-00:27:36
Cerny: No, no, way on up above Mendocino and Highway 1 curves back. It's way up. There's one in Carmel Valley that's nice, and then some others. There's one up in the Donner Region I've been to.

12-00:27:50
Burnett: They're ecosystem research stations?

12-00:27:51
Cerny: Yeah, they were given to the university for different things, and one had one of these really good tree walks, so that was nice. That also put me on the UCOP committee on the fiscal status of the Natural Reserve System. So, we now have three rogue units, and so, immediately, sort of after this, in late '95, Lawrence Hall of Science Director Marian Diamond, whom I know very well, abruptly resigns. So, we now have two problems, and so, I want to talk about first the Botanical Garden.

12-00:28:47
Cerny: What I did was I put Ian Carmichael in as acting director, and then Nancy Caputo had taken VERIP III, and we re-hired her to be chief analyst. So, they went there. What was wrong with the garden? The garden did recognize itself as part of the university, but not of the campus. It was probably under-funded by University of California, which led to inadequate leadership and a power vacuum, happily filled by a fundraising group called The Friends. The Friends, actually, had a high-level, really nice guy, retired UCB administrator who loved botany, but he was there, lending a patina of something to The Friends. The Friends had their own bank accounts, where donations were deposited, invested, and spent as determined by The Friends' board. They actually had their own employee sitting up there, administering the donations, paid from those funds, and paid more generously than the university employees. So, it can't work, right? So, Ian and Nancy said, "This can't work."

So, The Friends, they essentially were running the place, so they disbanded themselves. We went through a low point, and then we worked with integrative biology and brought in a new faculty member from outside, named Ellen Simms, who was the director from '99 to 2003, but I think she was more interested in her research than being the director of the garden.

12-00:30:21
Burnett: As can happen with those kinds of appointments.

12-00:30:23
Cerny: Paul Licht, who's a retired integrative biology professor, is great. He's been the director since 2003, and the place is just humming. It's made incredible progress. He loves it and he's still energetic. I know him pretty well. They still have a community of 250 volunteers, but there's one of these units that did not think it was part of the university—just like the art museum doesn't, but it has managed to escape my reviews. In the most recent message from the chancellor, the announcement came out yesterday, the art museum has now gotten back to where they want to be. They want to report directly to the chancellor, and that's what they're doing, just like the Zellerbach Auditorium person is to the performing arts, they're both now direct reports to the chancellor. The art museum [Berkeley Art Museum] had gone down to my level, and I interacted with them a lot. We weren't really setting up a review of them. Then, after a while, they got to Carol's level, and now they've gone up.

12-00:31:34
Burnett: What was their rationale?

12-00:31:38
Cerny: They're just independent. They've been out there, they've got a lot of funding money, they have this very fancy board. You're not very far away—the university art museum, but it might be. I don't know its history that well, but no one else seems to want to tackle reviewing, that have ever shown me a report.

12-00:31:56
Burnett: Right, but they receive university funding, the property is owned by the university?

12-00:32:05
Cerny: Yeah, and I guess where they're building their new museum, that property is ours, but they've had to raise all the money for the art museum.

12-00:32:19
Burnett: In those cases, they've had some kind of arrangement?

12-00:32:22
Cerny: Oh, yeah. It started, and actually, for modern architecture, the current art museum is really very nice. It was earthquake-unstable, but they've stabilized it enough. So, I don't know what will happen to it, but for architecture for the seventies, it's actually really nice when you look at it. The part that was so dangerous was the theater. They felt it was very dangerous in terms of an earthquake, and so, that's why the film archive is in this kind of temporary building.

12-00:32:54
Burnett: Right, the corrugated building.

12-00:32:56
Cerny: That's why it's there.

12-00:32:59
Burnett: I don't know if you know this, but I've never understood why a building that was built so recently could be so earthquake vulnerable.

12-00:33:15
Cerny: People say to things like that, that earthquake standards changed all the time. So, I really don't know. We had a large number of buildings that were very earthquake unstable, and a bunch of them built in the sixties, like the chemistry building, Hildebrand, had to be heavily buttressed. Even Latimer, they've had all this concrete put on these things.

12-00:33:41
Burnett: And separate buttresses, yeah.

12-00:33:43
Cerny: All that's had to be really buttressed.

12-00:33:47
Burnett: I guess part of it is that they didn't know until fairly recently how active the Hayward Fault was. I think that's part of it. I'm not speaking out of certainty, so I won't add to that.

12-00:34:03
Cerny: Yeah, they don't know anything about the Hayward Fault, except 1860 or whenever it was. So, actually, that solved the Botanical Garden, so that was good. Then, the [Lawrence] Hall [of Science], so we have the director quit. Again, it's way off campus, and just because you're off-campus, doesn't mean you can't be run well, right? The space sciences lab is way off campus—it's run really, really well. The hall had a strange beginning, and so, we put Ian up there as acting director, and Nancy. There had been a big review around 1982 that just said the place was out of control. Seaborg, I guess, got the money for it, somehow, when he was with the AEC [Atomic Energy Commission]. So, what one would say about them—because I was getting this from Nancy because she was up there—it saw itself as a separate entity, not as part of the university or the campus.

It had little sense of any participation in the university mission, it had developed into a collection of separate project-oriented sub-organizations, each with its own acronyms, goals, funding sources, and ideology. They didn't even identify with LHS itself. We had quite a battle just getting LHS into their logos, and I remember Nancy telling me that I had to insist that UCB be put on the new sign over the building. Some groups had funding from the university, others had none, and their success in supporting themselves varied widely. This created jealousy. Several groups created income-producing products; the successful ones did not share with any other groups. It was like Balkanized.

So, Ian got them really organized, and he set up three associate directors, and he made sure that they knew they were part of the university. These associate directors were all good people. Ian even brought them down two or three times a year to my office in California Hall because they were reporting to me, right? They were reporting to the university, and then he had them tell me what all was going on up there, and what did I think about that? He made an instantaneous, dramatic, and indelible impact on the Lawrence Hall of Science within weeks of being appointed acting director, initiating a major reorganization that commenced a new era of internal collaboration, coherence, and programmatic involvement that had previously not been possible on an institution-wide basis.

Now, I don't know how many directors they've had, and I didn't have the time to try to find it, but it doesn't show up easily. So, I thought he did a great job. We had an initial review committee, they thought he did a great job, too, and another review in '98-'99, that said, "Don't search for a new director yet; he's doing a good job. Give him a few more years." So, he actually was acting director till 2003. He also brought in money from a big geology outdoor project called The Forces that Shape the Bay. So, that worked out really well, so it got under control. But in addition, you had Seaborg involved in this. Have you been to the hall?

12-00:37:39
Burnett: I have never been, no.

12-00:37:41
Cerny: Well, Seaborg had gotten the money for it. He was director from '82-'84. That's right after this other report. Then, he decided he wanted to be the chairman, so he was the chairman from 1984 until he died. He died in February '99, and Ian would have arrived in '96. So, according to Nancy, Ian was required to meet with Seaborg on a regular basis, to tell him what he was doing, because he was the chairman. Finally, they talked it through. Anyway, it's in good shape, and so, that's a rogue outfit fixed.

12-00:38:31
Burnett: It's been tamed to some degree. They were not misusing funds or anything? It was just that the culture was really bad.

12-00:38:48
Cerny: Yeah, they weren't misusing funds. It was a culture issue. Apparently, this succession of people could not solve—I mean, W. M. Laetsch for a while was up there, Marian Diamond. I really like Marian and I've known her for years because her husband was a very well known nuclear chemist at LBL, but she hasn't been a department chair or anything, so she might have been frustrated. I don't know why; she just came in and resigned and she didn't say why.

12-00:39:20
Burnett: If there's this incredible autonomy that people are used to, and rivalry, that's not a positive culture. I don't know what Ian Carmichael did, and we don't have access, but perhaps just coming in with support and saying, "We'll support you in making your transition to a more effective organization."

12-00:39:45
Cerny: Well, my guess is he and Nancy had everybody come and tell them what they did, and you start with that. At the end of that, Ian had a meeting and said, "You know, we've got chaos here, and I'm thinking that we're going to have an organization chart, and I'll get you a draft version of what I think it looks like."

12-00:40:03
Burnett: Right, that might be what you would do. Start with consultation, and then try to make some organizational sense out of it, and then communicate that back.

12-00:40:16
Cerny: It was interesting. I think it's going well.

12-00:40:27
Burnett: So, you're given these, you call them rogue institutions, that need to be tamed.

12-00:40:37
Cerny: I'll skip some minor things. I'll talk about Novartis to get at least a couple of things done. So, we did publish one more campus-wide publication in 2000, which I really liked, and it was called *Framing the Questions: New Visions for the Arts and Humanities at Berkeley*. I just looked at it again, and it's really nice. We just asked a group of humanists, and they put it together and we produce it. It had an editorial committee that had the dean of the arts and the humanities, the director and the associate director of the Townsend Center for the Humanities, and the faculty liaison from South and Southeast Asian studies, so they had a really good group of people. The other big issue in the newspapers, so to speak, is the Novartis business. I'm not going to spend too long on it because first place, if you look on the Internet, it's covered all over the place, but there's something really interesting in one of the reports. Certainly, I was involved in it.

12-00:42:11
Burnett: Sure, sure. I guess you signed the agreement.

12-00:42:16
Cerny: Well, the chancellor did and I did, or whatever. So, the department of plant and microbial biology was looking for research money, and particularly the chair, Wilhelm Gruissem, was interested. The dean of CNR was from the ag and resource economics department, Gordon Rausser, but he was certainly keen to get money. The Office of Technology Licensing knew what was going on and was watching other kinds of people have these tentative deals with universities, and Hoskins had reservations about some of these other things that he'd heard about. Anyway, so, in my little experience with this, and this is

what worked for them, you've got to have somebody hot in some industry that really wants to be with your faculty or your department, and then it's going to work. If you don't have that, it is probably not going to work—and when that person goes away, it's probably dead. That's my limited experience.

12-00:43:20
Burnett: Right. You want to have something that's going to be a sustainable collaboration even after, so it leaves behind some vestige of a research program? Is that what you're hoping for, really?

12-00:43:30
Cerny: Well, yeah. I'll read some things from it. So, there are different names for the Novartis Group, but anyone who's really interested should just go read their choice on the Internet, but a guy working for them named Steve Briggs really wanted to fund some new fundamental research, and he knew Gruissem for a long time. So, he got familiar with the Plant and Microbial Biology faculty, and the department was willing to cooperate, as a department, with this, with the idea that any faculty members who wanted could be in this deal, and those who didn't want to be did not have to be. They were in a hurry, and the department wanted a quick agreement. I'm quoted in these things, and that's true, I said, "Well, if this is going to have a draft agreement, Hoskins should do it." I was not going to have the contract and grant office do it because they didn't know how to do it. Hoskins knew how to do all these things, so we had Hoskins do it, and I insisted on that.

They spent a lot of time putting a draft together and talking with the faculty. Carol Mimura was his associate head, and she now is the head of the Office of Technology Licensing, and she was also from biology. She knew a lot about how to do these things, too. So, I'm going to quote from a book, *Universities in the Age of Corporate Science: The UCB Novartis Controversy*. So, at the end of October 1998, Hoskins left for a two-week trip to Hungary, I remember that, and Carol Mimura handled the final rounds of the negotiations. They sort of had to do something in a hurry because otherwise it was going to take forever—either you did it in a hurry or not, and Hoskins said if we stopped and went to everybody that we needed to get approval for in writing, probably the agreement wouldn't be done by today.

As it turned out, there were only three minor amendments, so the agreement was more or less ready to sign in early November, but perhaps in response to the emerging controversy, UCOP sent new attorneys to review the agreement. They were not versed in IP law, and also had to be brought up to speed on the history of the negotiations, points of law, and the background in agreement. So, that's the kind of help you would get from OP in all this stuff, right? So, that pushed it back a few weeks. At 11:00 a.m. on Monday, November 23, 1998, the institutional representatives of Berkeley and Novartis held a joint press conference in Koshland Hall, the home of Plant and Microbial Biology, at which the agreement was officially signed. UC Public Information Office, 1998, it says. Attending for Berkeley were Robert Berdahl, chancellor, Joseph Cerny, vice chancellor for research, Gordon Rausser, dean of CNR, Douglas Watson, president and CEO of Novartis Corporation, and Steven Briggs, president of a group representing the company. Various people from print and radio media were present to cover this event. They were able to report on the opposition's response as well, which came in the form of a pie thrown at Rausser and Cerny, that hit only Cerny.

Now, there are several problems with this. I wasn't on the podium. I was sitting in the front row, right next to a university policeman, it turned out, with a thing in his ear. Carol Christ, the executive vice chancellor, was on the podium. Rausser, having been a boxer when he was young, did dodge the pie, but it hit her. She was *really* pissed.

12-00:47:43
Burnett: I bet!

12-00:47:46
Cerny: So, this is so much for accuracy in writing things up.

12-00:47:49
Burnett: Yeah, well, it's a good lesson.

12-00:47:52
Cerny: Her really nice clothes were wrecked and she was really unhappy. So, how this can be so wrong is beyond me.

12-00:47:59
Burnett: There was a student group that had organized.

12-00:48:06
Cerny: I don't know who it was. There were enough plainclothes people to grab the guy really quickly. When I was going through Novartis and Cerny, this came out, I just couldn't believe it. So, how's that for historical accuracy? Anyway, the whole thing certainly didn't violate anything by the time it ended. The PMB faculty were entrepreneurs; they overwhelmingly supported all forms of the university-industry funding arrangements. They were the only department—at Berkeley—at which the majority of the faculty members considered the use of university facilities by industry scientists appropriate. Not surprisingly, they were most supportive of the agreement, and expected to have mostly positive consequences.

Now, what were the controls? The controls were: research was guided by a committee of three Novartis scientists and three UC Berkeley faculty members, not necessarily all from that department. Another committee, which determined which projects to fund, consisted of three UC Berkeley faculty and two Novartis scientists, so there was a control at the university, and I know there was an outside guy on that. I can't find it, but the contract, I'm sure, said that either Novartis or Berkeley could cancel the agreement with three months' notice, so I could have gone at any point to the chancellor and said, "This isn't working. All this stuff's being violated. We want out of this deal." So, as far as I was concerned, there were plenty of safeguards in place. Things were also changing in Novartis, but Novartis nonetheless paid for five years as this contract specified. So, we got $5 million a year, two-thirds going to research, and one-third to overhead and infrastructure. They had access to Novartis proprietary tools, et cetera, and Novartis would have first rights to 30-40 percent of the research products, and I think that was independently of whether they paid for them or not.

12-00:50:15
Burnett: Yeah, and it was a right of first refusal, so they had the right to enter first into negotiations to license.

12-00:50:23
Cerny: Right, yeah, so the university's learned to only do first refusals really fast after getting into this stuff. So, in the five years, apparently twenty disclosures were patented, ten related to Novartis money. Six were pursued by Novartis, but no exclusive license was executed, and they left. So, at the end of it, there was a big tempest in a teapot, but nothing really horrible certainly happened. They had some extra research money and the university got out of it. I personally thought it was okay.

12-00:51:13
Burnett: What unfolds after you leave has become, I guess, historically more important, almost. I think the controversy over Ignacio Chapela, who was opposed, he's a member of the College of Natural Resources, and he opposed this. He led the college's opposition, stating that 41 percent of the college supported the deal as signed, but 50 percent had grave concerns of the effect on academic freedom. So, he felt that the college was divided as to how this was being undertaken.

12-00:51:56
Cerny: I think that's true; I was just quoting the department.

12-00:51:59
Burnett: There's a question about that, I suppose. I think there are questions more about the potential rather than what actually happened, and the potential for the exercise of control over—

12-00:52:19
Cerny: Well, I don't think so because if you have three faculty members on this committee who I don't think it was prescribed where they came from, except I'm certain that we had one from outside that department, and the fact that, I mean, I also was involved in conversations with this group. The fact that I could, at any point, have gone to the chancellor and said, "This is violating what we understand are the conditions or what ought to be going on in a university," we had a withdrawal clause, and I could just tell the dean and the faculty, "Look, this is the way I see it, we need to get this straight, whether

I'm right or wrong, and then we just withdraw." So, we weren't for life, and we didn't have to do five years. I thought it had plenty of safeguards.

12-00:53:12
Burnett: I guess the controversy that emerged later—and this is after you're gone, and so it's not something that you would—

12-00:53:23
Cerny: No, yeah, it's had a life of its own. I'm not too emotionally involved in it. It just seemed to me okay, to be okay. Hoskins was a smart guy or we would have just said no.

12-00:53:37
Burnett: I guess subsequently, there's this *New Yorker* article about Tyrone Hayes, who did the research on atrazine. His lab got funding from Novartis, actually. It became Syngenta later—in the press, it's always about Syngenta—but at the time that he was doing his research, it was Novartis. He had done research and had raised concerns that the pesticide was causing defects, reproductive defects, in frogs that he was studying, at levels of exposure, apparently, that were lower than the EPA standard for exposure to humans. There's this long story, and apparently because of class-action suits that were lodged against Syngenta by twenty-three different cities, it has emerged that according to the records of the company, Hayes was targeted by the company to be discredited, that they had all kinds of efforts to really go after him. People thought, at the time, that he was quite paranoid, but as it turns out, they *were* out to get him.

I guess perhaps some of the anxiety is less about the details of the arrangement, and perhaps more about the very fact of the collaboration, that it opens up potential for kinds of pressure to be brought to bear on scientists not to publish research, or to give the green light to research that they have misgivings about, because they're fearful that the relationship will not continue, or that if they discontinue the relationship, this guy is experiencing consequences years after the relationship that he ended with Syngenta. So, some of the concerns might be about these gray areas—sometimes they're explicit, that there are contractual agreements not to reveal information in an enterprise that is supposed to be about open sharing of information. The sociologist Robert Merton's famous claim about the communalism or communism of scientific research, that it's supposed to be about sharing information, and that's how the enterprise advances. Do you have any feelings about that, or do you feel that those kinds of allegations are exaggerated? At least as far as you found in your experience with this.

12-00:56:35
Cerny: Our contract was published, right? It was a public document. I thought it was fine. It doesn't have any of those hidden things in it, so yeah, you have to worry about all those other things, but here in this document, it seemed okay.

12-00:57:03
Burnett: From ’85 to 2000, this is that key period in the transition of the American university towards industrial or support from the private sector in the face of declining public funding. Do you have any opinions about the changes that have taken place? Is it case-by-case?

12-00:57:40
Cerny: I’m not up on how much money is flooding into these areas. There’s so much NIH money that’s also gone out, so much in all these years. The Berkeley faculty realize their career is focused on publishing. The NIH is going up more than all the other agencies. There’s lots of research money, and I don’t worry about it. I mean, I worry about all these other things, okay, for sure, but this contract looked fine. I read it, Hoskins was a tough guy.

12-00:58:28
Burnett: And experienced.

12-00:58:30
Cerny: If we had a contract out there that people didn’t like, I’d have heard about it in the remaining time, constantly.

12-00:58:35
Burnett: You said he himself had come from industry, so he understood the legal aspects of the ramifications on the research, so the goal was to provide sort of mutual protection.

12-00:58:49
Cerny: Yeah, I’m just blanking right now on where he came from, but he had a lot of experience.

12-00:58:58
Burnett: So, probably not expecting in your career to encounter a pie, even if it wasn’t directed at you, or to be in subsequent history books—?

12-00:59:07
Cerny: Right, and Rausser was right in front of her, so if he let the pie hit him, it would have been okay, but then when he dodged it, she was right behind him.

12-00:59:18
Burnett: I bet he felt bad for his boxing instincts.

12-00:59:26
Cerny: It was quite a close.

12-00:59:30
Burnett: We probably shouldn’t get into another subject, and that one, that subject, could go on for a very, very long time. So, we’ll conclude and we’ll pick up in our next issue, next session, with the remainder of your work in the Office of Research, and the postdocs.

12-00:59:51
Cerny: So, I think yeah, I want to talk about postdocs and the Sunset to Dawn review, and then I can switch into what I did after I stopped being an administrator.

12-01:00:01
Burnett: Sounds great.

[End of Interview]

Interview #7: June 24, 2014
[Begin Audio File 13]

13-00:00:12
Burnett: This is Paul Burnett, interviewing Dr. Joseph Cerny for the University History Series. It's June 24, 2014. This is our seventh session, and this is tape thirteen. So, Dr. Cerny, we were talking about your career as vice chancellor of research for UC Berkeley. In my background interviews, I spoke with Joe Duggan, and he said that one of the things you really did was to work tirelessly to improve the lot of postdocs at UC Berkeley. Can you tell me a little bit about the genesis for that and why you felt that was an important priority for you and how that developed?

13-00:01:00
Cerny: Yes, I would be glad to. Originally, when I started as graduate dean, I knew that from my experiences with campus, that there was nothing organized whatsoever on the Berkeley campus to deal with the postdoc situation—starting with worker's compensation—or anything. As I was sort of getting into all the top priority things that I thought I needed to do as graduate dean, I just didn't have the time, myself, to do it. I couldn't really find somebody I thought could carry that ball, so we kind of went on for a while. I do remember a report I'd read somewhere in this period that was put out by the National Academy of Sciences, called *Postdoctoral Appointments and Disappointments*. It was one of these NAS-type reports that are really nice, but they don't have any action items or they're not very well laid-out, so nothing whatsoever had happened. Now, I actually knew at Lawrence Berkeley Lab, where I do my research, because we're a government lab, there's not this problem with postdocs. They have regular appointments, there's a pay structure, benefits, but the campus was very different. It turned out in 1994—and the committee ran till '98—that the Association of American Universities decided to appoint a committee on postdocs. President Steve Sample of USC was the chair, and he was a very good chair. I was put on this committee.

It was a very high-powered committee for sure, and he really started it all off with contrasting the situation of postdocs in 1994 to graduate students in 1890, where no one really knew what a graduate student was, there were no rules, and it was just a free market. You kind of got a graduate student from somebody else at some other university. One reason the Association of American Universities got started in 1900 was to deal with the graduate student situation. So, if you looked at postdocs, they were really neglected. There was no quality control, no time limits, no campus-wide compensation policies, few policies differing foreign from domestic students, no benefits, no job placement, no worker's comp. Sample had, because the AAU committee actually had some studies done, picked biochemistry, math, physics, and psychology. In biochemistry and physics, it was almost mandatory to have a postdoc, and yet, you still have no rules or concerns about this group. You even need a postdoc to become an assistant professor.

So, it was certainly true the campus was very different from LBL. LBL didn't do things like create a postdoc community or have placement services or things like that, that you also expect you should have, which we ultimately brought into our stuff. We always had LBL postdocs welcome as we moved forward. So, I was on that committee, and we made a number of decisions, one of which was define a postdoc to be separate from people in a clinical training program. A number of these university presidents or provosts who were on this committee had hospitals, and so those were going to be different. When you tabulated "science" postdocs in the year they surveyed it—and I'm surprised to this day—Harvard says 1,124, and we're actually with 690, and Stanford at 585 was third, so we had a lot of postdocs. This is on campus, not counting postdocs at LBL. Harvard, I don't know how they deal with 1,124, but anyway, that was their number.

The report said, "We really need to shape up on all these areas. We've got to define a total time as a postdoc," and they compromised into wanting it not to exceed six years anywhere. When we did our thing at Berkeley, we made it be five years cumulative anywhere. It took a while for this AAU report to come out, but it touched on everything I knew we needed to fix, and the AAU having blessed this report meant the chancellor was behind what I wanted to do, so I could right away say we got the go ahead. Then, the other thing I decided to do, in '96-'97, I was the chair of the UC Council of Graduate Deans again because it rotated on a cycle. Originally, I had told the group, "Please, let's have a theme for each year. Let's just not have meetings where Office of the President yells at us and we yell back." So, we had themes for the year. I said, "Okay, the postdoc situation, every campus has got a problem," so that was our theme. So, we did that. We did a lot of stuff starting to just get the word out and soften the people who might be unhappy. So, I didn't have to wait for the report to come out.

Then, at Berkeley, I set up a somewhat larger-than-normal invitational seminar on the postdoc situation, where we got faculty from UC Davis, UC San Francisco, Berkeley, and some administrators, and some postdocs, and some people from the Office of the President. I asked Steve Sample to come up and give the introductory lecture, and he really did, which was just wonderful. It couldn't have been better. The president of USC's got time to come out and do this, must be important. So, that got us started. Then, from my other experience, I knew if you really have to do something complex like this, you have to have a high-level team, a postdoc team, and you have to chair it. So, I set up a postdoc team with the lead people—because I was using my vice chancellor for research hat for all this—and we had the top people in every group that we needed on this team, no subordinates, and they had to come because I was chairing it.

Now, there's another group running around in here I should just mention called the visiting scholars. The visiting scholars are people, faculty, coming from other universities and come visit a friend at Berkeley for a semester or a year or whatever, and come from abroad. The Graduate Division sort of had to take care of that to some extent, so I have a guy named Sam Castañeda, who for years had been doing that, and he'd also branched out into helping postdocs even before we got any of these other things fixed. Like, we would give a party for postdocs just to let them meet one another, and so, those were quite a success. This VSPA, I will mention again, but that sort of helped. Once we had something going, that was the group that was going to help him carry it out. VSPA stands for Visiting Scholars and Postdoctoral Appointments.

13-00:08:26
Burnett: On this committee, when you said these are high-level people, for example, from engineering, you'd have—

13-00:08:31
Cerny: Well, this was more fix what you needed, say, who was in charge of worker's comp? All these practical things, appointments, payroll, health insurance, not so much engineering. We just had to get them not to complain.

13-00:08:44
Burnett: Who would be most effective at helping with each specific point of, well, it's not really a grievance because it wasn't brought by the postdocs; it was a study that solicited, nationwide, feedback from postdocs about their experience?

13-00:09:02
Cerny: We just all knew change was needed. I knew that from my surveys, and I think President Sample knew that, too. What the committee got was just a lot of data on how many postdocs there really were, and then looked at it in terms of the career of these people and how it was important that way.

13-00:09:22
Burnett: This was emerging as a kind of new problem, right? The species of the six-year postdoc was not around in the 1960s, I imagine, right? I think this is something that becomes a problem in the eighties, I guess?

13-00:09:42
Cerny: Becomes a problem in the eighties, and well, it's the more than one postdoc adding up to six years.

13-00:09:56
Burnett: This is a subject of conversation when I was in graduate school, about people postdoc-ing, bouncing around from postdoc to postdoc, and not landing a tenure track position, and being in a holding pattern. The postdoc became a kind of limbo or holding pattern for people. This is a phenomenon that comes to the attention through this study in the 1990s, as a problem that begins to be defined with some kind of clarity.

13-00:10:30

Cerny: I mean, I was clear on all this, Steve Sample must have been clear on all this, but I'm not sure how many of the other members from these other universities were. There were a couple of other graduate deans; they were surely aware of it because there were plenty of postdoc laments, but we didn't read anything about much going on in a way to set anything up. There was some university in the East that had just sort of set up a postdoc office, about the middle nineties. I kept having my staff bringing up to me issues like why does chemistry have all these poorly paid people, letters of agreement for their salary, what's going on here, and really low? It was something that just has to be solved, and so a lot of us knew that, and I was just ready to take off with it. By January 1, '98, worker's comp turned out to be easy. You have huge liability problems if that's not solved.

You constantly had a problem—you had postdocs on the payroll because there were postdoc-type titles, there were postdocs on fellowships from the American Heart Association or foreign governments, you have some people come on their own. They're not in any payroll system at all. You constantly have three different buckets of people. You've ultimately got to solve all these buckets. You'd like to get them registered. As a campus, this working group agreed we were going to have a five-year limit on total postdocs. So, it's not five years just Berkeley, it's total. So, if you're going to go beyond five years, we don't necessarily throw you off the campus, but you've got to get into a regular job title, get in career progress, so you can be a whatever the title is—a junior specialist. So, we went to that.

So, by August '98, I wrote a memo I was very proud of. I rewrote it six times, just to get what I thought were basic needs, and then I had the team review it. I sent it out to some deans, and then I got it back, and then I rewrote it three more times. Then, I sent it out. It says, "The Berkeley campus is now going to require 100 percent appointments with a minimum annual compensation." Unfortunately, originally it was set at the NIH lowest level because that was a guideline other people had used, but we rapidly changed that later. "There will be no by-agreement appointments." We also required a consistent postdoc title in the database, the total time can't exceed five years, and everybody should get involved, postdocs, in this VSPA group. So, I got that out, and that was our policy. I think maybe it started January 1, '99, but that was really important. By then, having had enough stuff out on campus, and accommodating some things as I could, it went through.

By September '99, we actually had all postdoc pay through one payroll system. Believe it or not, they didn't have health insurance yet, forgot to mention that, so by then, they also had to have health insurance, comparable to the university plan. By May 2000, we moved postdocs to a higher pay level, consistent with our lowest kind of junior staff position, called postgraduate research step one. Then, meanwhile, this VSPA group was working on the collegiality groups, and we tried to set up, what did the postdocs really think was still wrong, and what was going on? But we basically had solved almost all of those other problems.

13-00:14:35
Burnett: When you said "paid through one source," that's for the postdocs who are paid by Berkeley?

13-00:14:42
Cerny: No, we had to fix the way the money would come in.

13-00:14:45
Burnett: So, if someone's supported on an NIH or whatever, the NIH would pay Berkeley, and Berkeley would pay the person? It's probably too much detail.

13-00:14:55
Cerny: I can't remember now, but basically, you either had to have a decanal-level signoff that this was comparable, but really, all we wanted is as many as possible in one payroll system. We somehow worked the system. It may be with the system, with little marks in it saying where it was coming from, but we could go look at it and see chemistry didn't have any people anymore on by-agreement salaries for $1,000 a month.

13-00:15:19
Burnett: "By-agreement" is just between the postdoc and the faculty member, saying at-will?

13-00:15:23
Cerny: Yeah, there were guys kind of from China that were desperate to come here and do anything.

13-00:15:30
Burnett: Right, and they could be let go at any time, and the contract was not much of a contract.

13-00:15:37
Cerny: Yeah, it was a letter. By 1999 everybody did have real letters of appointment. I know, having had postdocs on campus when we got some campus money, that this is all true. So, actually, from our Ph.D. ten-year study, we had an invited opinion piece. Just to show the amount of time all this took, in September 1999, *Science* magazine had an issue highlighting the postdoc plight, and Maresi and I had this invited opinion piece, just giving our data, particularly focused on biochemistry and math. It was postdoctoral patterns, career advancement, and problems. In math, if you're going to progress, they don't call them postdocs in math, they're called visiting assistant professors, but that's very important. If you get a good visiting assistant professorship, I think you'll actually do some teaching on it, then got a really good line on a tenure-track position.

13-00:16:43
Burnett: It's a good leg up, right.

13-00:16:46

Cerny: Biochemistry, it was clear that the postdoc was a proving ground for excellence where you were going, I think even to industry, but I think if you go into academia, you were much more likely to do more than one postdoc and probably in industry, you'd only have to do one. When we looked at all our data in the Ph.D.s ten years later study, and we looked at since it was so late, tenured faculty, ten to thirteen years after getting their Ph.D., the lowest percent of tenured faculty in the six fields we looked at was biochemistry: 40 percent of these people were tenured, and the other 60 percent were still tenure track because they'd had all these postdocs. So, I think it's a shame.

13-00:17:43

Burnett: Why do you think that was the case in that field?

13-00:17:52

Cerny: Well, after 1987 somehow the floodgates got opened up in biology fields for foreign graduate students coming in, now I think there's a huge excess supply. We looked at people graduating from '83 to '85, and even by then, the number of postdocs were going up and the number of faculty positions wasn't, and then many people just finally drifted off to biotech. We had people with three postdocs in six years—lots of people.

There was a third National Postdoc Network meeting in 2003, so then postdocs were starting really networking, and they're a national group now, and I was asked to give a plenary talk called "Starting The Ball Rolling," which is how do you solve these problems at universities? I said there were three key elements, and I'm just going to be repetitive, first thing you need is buy-in by the senior administration and key faculty members. Second, you need a dedicated, high-level faculty administrator's responsibility for postdocs, and a high-level staff working group. Now, so either the graduate dean has to do it or the vice chancellor for research has to do it, but since I was both, it was clear to me that I had to do it. Third, which I haven't talked about so much but you need to do, is you need to work to develop a postdoctoral community. We did encourage one that virtually was a Berkeley Postdoc Association. We provided them some money. We actually hired a fresh Berkeley Ph.D. in biology, who had a postdoc husband, who was interested in family issues, so she talked to a lot of people about that as peer-to-peer stuff. We held a lot of workshops, we held job fairs, whatever. So, we just got going on a lot of these things, and with Sam Castañeda running this VSPA, set all those things up, and it largely worked.

13-00:20:54

Burnett: Well, it goes back to your initial review of the chemistry department in '77. One of the complaints was about how postdocs were faring in the department. I can only imagine that things got so much worse across the board in the eighties and nineties, precisely because of these supply and demand problems, as one of the ways of dealing with a budget crisis was that faculty lines would not be continued, and there's just this tremendous oversupply, as you suggested, that it leads to a rift, inevitably. To some extent, that's still happening, or to a large extent, perhaps, that's happening in the Academy. Your position was that there has to be a way for the institution to recognize it and then to deal with it because they weren't recognizing it at all, it seems.

13-00:22:03
Cerny: Central campus would have to drive something like this. Now, I thought that Steve Sample at USC, with their medical school, probably had a lot of really good postdocs because their graduate program doesn't rank all that highly. He was really particularly interested in that community and trying to do right by it as an additional motivation. I'm sure he must have encouraged having the AAU set this committee up. That was invaluable to me.

13-00:22:42
Burnett: Right, absolutely. So, there was another piece that you wanted to discuss as part of the last piece, the Sunset to Dawn reviews that you undertook.

13-00:23:07
Cerny: The other thing that I really wanted to deal with over the years, I've talked about how all the organized research units kept coming to us, except the ones in the international and area studies went under a dean. We were constantly having reviews. There was a 1982 policy at the Office of the President that mandated reviews of all ORUs on all campuses by 1986. Then, they assumed it was done. So, when I started in this job in January 1, '86, and never saw anything there, so anyway, so they ignored it but they said, "It should be done," and nobody probably did it. At that point, all our ORUs were granted a fifteen-year lifespan. Now, that didn't mean that they couldn't get their budget cut, but they had a lifespan. So, I said, "Okay, it's nearly 2001," we've got to do this, "so why don't I view this as an opportunity at Berkeley to review our organized research unit enterprise and to create opportunities for new ORUs." They all grew up by some individuals proposing something and then getting, years ago, funded by the state, and when that stopped, not too many came.

So, I asked Chancellor Berdahl, "Wouldn't it be nice to do this? Could I have $1 million a year in perpetuity of new money to do this?" He agreed. So, then we wanted to set up a fair process for the campus to review all the ORUs and try for new ones. The first thing was a call for proposed new ORUs during '98 and '99, with the best of these continuing to a second phase, which would be a competitive evaluation for ORU status with the 32 existing ORUs.

13-00:25:25
Burnett: When you said there's a fifteen-year lifespan, they need to be renewed, or are they just discontinued?

13-00:25:38
Cerny: This is a mandated Sunset to Dawn review, so my attitude is at the end of this, they would be renewed for fifteen years. There would be some new ones, and some old ones might go away. The first phase, then, I thought this was all

intellectually very interesting, so we sent out a call for new ORUs and got sixty letters of intent and forty-seven pre-proposals by a February '99 deadline. This was an extraordinary opportunity since the State of California had stopped funding new ORUs in 1971. So, the proposals were reviewed for quality and strategic advantage, and quality was research excellence, likelihood to leverage extramural resources, evaluation of the track record of core faculty. Strategic advantage was, does the proposed ORU truly utilize unique Berkeley faculty research strengths, and is it likely to emerge as a state or national leader in opening up new research areas? So, those were our two criteria. For this review, we did internally, but complexly. We set up a complex, confidential mail review process, involving junior and senior Berkeley faculty, which resulted in 26 faculty reviewers who evaluated six to nine proposals to give four separate reviews on each proposal. So, we had that set up to get four completely independent, completely confidential, reviews. These reviews were sent to the master review committee.

13-00:27:31
Burnett: That was the nature of the competition?

13-00:27:32
Cerny: That was the nature of it, and it was all written. While this was going on, people were plucked off all the appropriate Academic Senate committees to be on the master review committee. So, they were all Academic Senate appointees, and the vice chair of the Academic Senate came to the meetings ex officio, nonvoting, and I would go, nonvoting. Out of the forty-seven, twelve were selected. So, these were three proposals in the arts and humanities, which had had none so far. In this whole process, we'd never had an organized research unit in arts and humanities. Three in biological science, one in international and area studies, three in physical sciences, and two in social sciences. Then, requests for proposals for creation and renew of the ORUs were issued in summer '99 with a mid-December deadline. So, we were talking about twenty-four existing ORUs that reported to the vice chancellor for research, eight that reported to the dean of international and area studies, plus the twelve new ones, so we had to review forty-four proposals. So, the review areas were biosciences, engineering, international and area studies, physical sciences, and social sciences—and social sciences had so many of these that we had to make them be groups A and B to be roughly comparable in size, because the social sciences had loads of ORUs.

13-00:29:01
Burnett: How big are these proposals you'd have to wade through? Like an executive summary?

13-00:29:08
Cerny: They had limits to how much they could write, for sure. I didn't write that down here. We had limits on exactly how much you could write and what you had to do.

13-00:29:18
Burnett: Still, forty-four of anything would be a significant amount.

13-00:29:22
Cerny: They were constrained for sure, absolutely, as had been the pre-proposals. We had those constraints, I forgot to mention that. Engineering was reviewed, but at that time, engineering was in a very complicated fight with the federal government about changes in federal funding rules on how they'd been doing budgets, and this, that, and the other. We were going nowhere with that, and so, we just decided we're going to review them all because they needed to be renewed, but we weren't going to change their budgets either way because for the federal fight, it might be changed, too, but they had to be reviewed. So, the reviewers, then, were put into panels, and we included seventeen from other UC campuses, four from other West Coast universities, and ten from universities across the nation. So, we had thirty-one in all, and they were selected from nominations by organized research unit directors and proposers and cognizant deans. So, we had no finger in this, since the administration's constantly accused of weighting—

13-00:30:32
Burnett: With having an agenda, right.

13-00:30:36
Cerny: So, each panel, then, had a half-hour presentation and a half-hour discussion from each of their ORUs, and we got really, really good people. I mean, I had John D'Arms come out for arts and humanities, the one who was my friend from Michigan, and he came. That was wonderful. So, actually, I attended all the presentations, as did my assistant vice chancellor, Linda Fabbri, and two assigned faculty from the review committee went, so they were all covered. I was there and heard and saw everything. So, this is *Sunset to Dawn*. I'm just going to give the highlights. Main highlight was, well, what did happen? It was dawn for some groups, and actually, we got eight new ORUs out of the twelve. In the arts and humanities, we got a Consortium for the Arts and the Center for the Study of Sexual Culture. In the biosciences, we got the Center for the Study of California Environments and Biological Diversity, and a functional genomics laboratory. In international and area studies, we got a Center for African Studies, and when you looked at the roster in area studies, it was one that was obviously missing.

13-00:31:56
Burnett: There was only one proposal for area studies, that's right.

13-00:32:03
Cerny: For the physical sciences, we got a Center for Atmospheric Sciences, which had obviously become big, and with people in all kinds of departments working on it, and a Center for Integrated Planetary Science, which had become big. We had hired an outstanding planet-hunter away from San Francisco State. Finally, the social sciences, just one more, a Center for Child and Youth Policy. Now, as a segue, the committee really liked something that

we decided to call an organized research project. This was a Center for the Tebtunis Papyri. The Tebtunis Papyri were collected under the sponsorship of Phoebe Hearst in 1899-1900 by two English anthropologists who went to Tebtunis, collected all kinds of stuff, no one had ever been there, and had languished in Oxford until 1938. Finally came to Berkeley, lots of them hardly had been looked at, and yet, it's the largest collection of papyrus text in the Americas, and it's in the Bancroft.

13-00:33:13
Burnett: Right here on this floor, down the road.

13-00:33:16
Cerny: This really turned on the science members of the review committee. We all thought it was really great. One high-level administrator didn't, but we thought it was really great. It was for ten years, they would get only $50,000 a year, and was renewable up to ten years. It's worked like a charm. They hired a person who relatively immediately became an assistant professor and has now gotten tenure, and it's been a real success. So, I guess its ten years are up, but that really worked, too. Almost all existing ORUs were continued, budgets were rearranged, we're not going to go into that. The only ORU that was disestablished was the virus laboratory. The director felt that it had outrun its mission and should be closed. It actually had been serving primarily as an administrative support unit within the Department of Molecular and Cell Biology.

13-00:34:57
Burnett: It's kind of an off-hand remark, but it's maybe worth talking about: were these jobs thankless jobs?

13-00:35:06
Cerny: They weren't thankless. I thought it was great. The Tebtunis project was the only new project that said, "thank you."

13-00:35:09
Burnett: It was rewarding, but in terms of the appreciation for your service as graduate dean, there was some aggravation in some of these jobs, there are some headaches and things. You're known as someone who thanks other people when they help you—did you feel that you'd been appreciated by folks that you'd worked with or the institution that you'd worked for?

13-00:35:46
Cerny: Oh, sure, but academics being academics, they would figure, "We jumped over the hoop and made it into it, got some money, why should I thank anybody for actually setting up something new that wouldn't have existed without this," wouldn't cross their mind.

13-00:36:01
Burnett: Right, so they congratulated themselves.

13-00:36:02
Cerny: Like a new funding proposal out of Washington. I talked with a number of disaffected people who didn't get enough money in the review, and certainly when we had to remove ORU directors, they didn't like that. There's people who come in and say, "Well, you know, we know how to beat the system, so I don't care. I'm no longer the director of this ORU, of course then I'll just be the *eminence grise* in the background and I'll still do what I want to do." So, you just say, "Okay."

13-00:36:57
Burnett: Good for you!

13-00:36:58
Cerny: What can I do about that?

13-00:37:03
Burnett: Just to be clear, you said there were eight new organized research units out of the twelve which had been proposed.

13-00:37:11
Cerny: The twelve proposed ones. Yeah, it was eight out of the twelve that had made it to become ORU. Plus there was one (the Tebtunis Papyri Center) which became a ten-year Organized Research Project.

13-00:37:18
Burnett: Of the existing ORUs you said only one was discontinued. When they were renewed, they were renewed on the basis of the proposal they made, and was the proposal to do things as they'd usually done them, or did they recognize that they needed to kind of change their—

13-00:37:41
Cerny: Oh, I'm sure they had to get with the times with these review panels we had, or their budgets would have been disastrously affected. I mean, we had real experts come in. I did not go read all the proposals and evaluate them myself.

13-00:37:58
Burnett: But the sense was that we needed to update what we have been doing, the original mandate is no longer relevant right now, or we've accomplished what we needed to accomplish, and therefore, we need to justify the next fifteen years and be forward-looking?

13-00:38:16
Cerny: Clearly, I'd learned when I got in the job, that the ORUs were hardly ever properly reviewed, as the Office of the President mandated. The Sunset to Dawn review created new ORUs and charted the course for all ORUs for at least the near future.

13-00:38:40
Burnett: Are there other aspects of the research position that you would like to discuss? Other studies you'd done before you finished your term?

13-00:38:56
Cerny: I mean, certainly other things were going on.

13-00:39:08
Burnett: In an earlier part of this interview, we talked about how it is a pretty long tenure. There are others, Sandy Elberg had had a long tenure, and there are others who have been there for a long time. Maresi Nerad said that it's very difficult in higher education management, administration, policy, to really get anywhere without committed people who can make a kind of long-term commitment to reform. So, if people are cycling in and out of graduate dean positions every three to five years, it's kind of like the president of the United States, you can only get so much done. So, she said that your ability, your staying power over that period in both of those positions, really enabled a lot of reform efforts and really facilitated them. She said otherwise, it's really, really difficult to track what's happening, never mind to do something about it. So, you managed to do both.

13-00:40:29
Cerny: I ended up with very good people working for me. In the vice chancellor for research office, Linda was there and was outstanding, and she went from whatever her staff position was called to assistant vice chancellor, and we hired in good people. In the Graduate Division, I had to make some changes. We didn't have to fire anybody, but a few people took very early retirement. Then, we had crack people, and so you knew Judy Sui, who did all the data analysis, I may have mentioned before, I could call her eight o'clock on Tuesday night, say, "Gee! I wish I knew this." I think the other deans in the UC system also recognized that we were getting things done. Dick Attiyeh lasted a very long time at San Diego. Some of them turned over pretty often.

13-00:41:37
Burnett: If people were turning over more frequently at other institutions or in other periods of history, why were you so committed to fifteen years? You took it one day at a time?

13-00:41:54
Cerny: Yeah. Well, supposedly all administrators were reviewed every five years, right? So, I don't know, five years was when we transitioned between Heyman and Tien as chancellors, and I don't know if I got reviewed or not. Then, Chang-Lin was happy with me continuing and doing what I was doing, and so, as far as I know, he didn't review me.

13-00:42:29
Burnett: Fifteen years is a fairly long time, but you certainly seemed to feel that there was work to be done, and so you pursued until you felt like you were—

13-00:42:44
Cerny: The postdoc thing, I wanted to deal with, and when we finally got at it, we just all worked really hard. There really wasn't anybody who could, like, write that memo for me. I decided I'd been to all these meetings, it wasn't a very

lengthy memo, but you had to write it very carefully in the way that a minimum number of faculty would be unhappy and would actually have to cooperate. I mean, it wasn't something I could even hand to Linda to do because you've got to have all the sensitivity of the faculty. You need somebody who's had postdocs, right, so nobody could come and say, "You've never had a postdoc, what do you know?" I could say, "Well, I've had twenty-six."

13-00:43:17
Burnett: For those who have had, right. So, it's a communications position in addition to all of the other aspects of it. You need to be a diplomat.

13-00:43:32
Cerny: Right, and these invitational seminars served a great purpose, and got the word out.

13-00:43:41
Burnett: So, did you come in with that skill, or is that something you cultivated over time, that you learned how to get buy-in, as you specified in your requirements for how to get things done? You need to get buy-in from people, and you need to be able to get buy-in from people at the top. Did you have some schooling in that, in the process, while you were in both positions? Did you become better at that, as time wore on, or is it something you felt that you were uniquely adept at when you took things on in '85?

13-00:44:29
Cerny: Even Hutchins said that when he was at Chicago, he went too fast, and if he hadn't gone so fast, changes he'd gotten, were gotten by close votes, and if he'd taken longer, the voting would have been better, and they'd have lasted longer.

13-00:44:52
Burnett: Did you read about Hutchins?

13-00:44:53
Cerny: I saw that somewhere.

13-00:44:54
Burnett: Oh, you read it somewhere. I mean, he's a very famous university administrator, right?

13-00:45:00
Cerny: Somewhere, I read that, and it stuck. There was this Milton Mayer writing in *The Progressive* that loved Hutchins, and that was fine, but I hated his writing. I had liked it, but then it became so much the same, I couldn't stand it. He wrote a biography of Hutchins I haven't read for fear that it'd be just a lot of teary tribute and I maybe wouldn't like it. Hutchins, yeah, he wanted to make change in Chicago. He did make change in Chicago. They still don't have a football team.

13-00:45:34
Burnett: I would submit that dealing with Berkeley is a lot more difficult—well, I suppose the faculty at Chicago are quite strong, but Berkeley is legendary for having this question of governance, right, that you have to tread lightly around the faculty because of the way the governance structure is set up; they're quite powerful.

13-00:46:01
Cerny: Almost everything we did that was controversial, we set up a faculty committee that I might chair or not. We visited departments and we got buy-in. There certainly are people who felt entitled on the faculty. One of the most difficult cases was somebody in political science was head of an ORU. When he became the head of an ORU, some people had concerns about it, but he was appointed for five years, he was very distinguished. He then got reappointed, and the grumbling started. So, I figured after ten years he needed to be replaced. I said, "Okay, there's only one way to do this." I had private conversations with each of the senior members in that department who were associated with the ORU, and asked him or her whether or not he should have a third term. I got their votes, I said, "No one's going to know," one at a time, and wrote the votes down, was essentially unanimous he should go. I told the chancellor, "This is what I learned and I'm not going to reappoint him, but I'm only going to do so if you're going to back me up. If you're not, we'll have to do something else."

13-00:47:32
Burnett: Yeah, buy-in works both ways, up and down the chain, and that's one of the most frustrating and impossible situations, is to have responsibility without authority. You need to make sure that if you're going to make this call, especially if it's a difficult call, you need the backing of the institution.

13-00:47:50
Cerny: Right. This was the only one that was going to be really controversial.

13-00:47:58
Burnett: How did it pan out?

13-00:48:02
Cerny: Oh, well, he didn't like it at all. One of his sidekicks hated it (and me). I just couldn't understand it. It was one of the few instances of real faculty hostility, but there were hardly very many of those.

13-00:48:20
Burnett: You said something about yeah, you'd be making enemies for life in the Academy, which is probably a paraphrase of David Lodge, somewhere.

13-00:48:32
Cerny: My other quote was, "Friends come and go; enemies accumulate." [laughter]

13-00:48:40
Burnett: That is a good one, definitely.

13-00:48:43
Cerny: So, I had very nice parties when we stopped. Castaneda knew how to put on a great party, and also, most of the deans came from the UC system, and it was a nice, late fall day outdoors at the Alumni Club.

13-00:49:02
Burnett: Great, and good memories for having accomplished a lot. Certainly the people that I've spoken with about your time there spoke fondly of the work that you did and working with you, so there is that. So, both terms end at the same time?

13-00:49:22
Cerny: Almost. It turned out, since I was finishing the Sunset to Dawn Organized Research Unit review, I needed to go through the end of December 2000, so I didn't stop that till the end of 2000. I stopped being graduate dean August 1, 2000, I guess because Mary Ann Mason, who came in, didn't want to start until then. The postdoc stuff needed its last memos, and so I finished that off also. A couple things didn't work too well. Mary Ann Mason came in and she was a social welfare and law professor, and she had been an acting associate dean while one of the associate deans was on sabbatical. She really wanted to downgrade comprehensive graduate research and she even wanted Maresi's job terminated.

13-00:50:32
Burnett: Institutionally?

13-00:50:34
Cerny: In the Graduate Division. So, Maresi stayed on until the end of December, but then also, somewhere in there, Maresi's husband, who was an engineering professor, had gotten this really, really good offer at Seattle, so that she was going to Seattle. Then, they gave her a position there as an associate professor in education.

13-00:51:03
Burnett: Yeah, she's done very well there.

13-00:51:09
Cerny: Right, and set up her own—I've got it written down here somewhere—group and got funding from Washington. That worked out really well.

13-00:51:19
Burnett: Do you think there should have been some kind of institutionalization of the work she was doing at Berkeley and continued that?

13-00:51:29
Cerny: Well, we were doing it. I was interested in that. That wasn't Mary Ann's, what she wanted to do, but she did keep the main guy doing analyses that were more focused on her career interests. Maresi and I were still trying to do this *Ph.Ds. Ten Years Later* book that I'll probably come back to in my notes, here, in a minute, but then she actually stayed around in the spring semester of 2001 for a while, and then she went to the University of Washington. Okay, so the

chancellor actually hadn't run searches for these two positions. He just decided he was going to do it, and he interviewed a number of people who'd been suggested to him, and picked two women. So, for the vice chancellor of research he picked Beth Burnside, coming from biology. She didn't want to start anyway until January 1, 2001. Her condition of employment was that Linda be removed. So, that wasn't a very happy camper in my office.

13-00:52:51
Burnett: Was there an explanation for that?

13-00:52:53
Cerny: Well, not to me. Maybe she gave an explanation to the chancellor. So, that didn't help our morale any, but we knew Linda was going to stay while I was staying in the job. But, actually, she was crushed. Then, the chancellor got her a position in the office of the president that was pretty good, and within a few years, she'd actually moved up there to be the executive officer and chief of staff to the provost in the president's office. So, she did well, but it made for lousy morale. Two of the really good people (and her good friends) left when she did, if not more. It's possible more did, so that Beth Burnside had an opportunity to start over. I mean, my attitude on these things, I guess, is when I was graduate dean, I just kept all the associate deans. I wasn't going to clean house, didn't know what I wanted to do, and then one of them decided to step down in two years, and that's when I asked Joe Duggan. In the other place, the only person, really, was Linda, essentially, at a high level. So, I do not know why Burnside didn't want to work with Linda. I can philosophize on this with you over lunch sometime.

13-00:54:31
Burnett: Well, I mean, Linda did talk about the position or the stuff surrounding the research; the chancellor for research position, there was some controversy. Faculty weren't happy over a number of small things. Nothing to do with the performance of the office, but I think they were upset about other things beyond the office's control, and that may have been reflected. I don't know. These can be very political positions, and if the incoming vice chancellor or chancellor for research has a very strong idea of what they want to do, they may want to have a clean slate, and that sounds like part of what happened or may have happened.

13-00:55:32
Cerny: Could be, but I find it a really tough approach. It wouldn't be what I would do.

13-00:55:39
Burnett: To start fresh, right.

13-00:55:42
Cerny: It's a big enterprise, to discover that you've fired the key person and the others are leaving with her.

13-00:55:46
Burnett: With all that institutional memory, yeah, absolutely. Well, we'll have to get Linda Fabbri's oral history, then. [laughter]

13-00:55:57
Cerny: Right.

13-00:56:02
Burnett: So, those two [positions] are winding down roughly the same time, and you are still a professor of nuclear chemistry. I don't know if you want to dive in—I think we're running close to the time, so we can maybe switch the tape out and start to talk about what it's like to return to being a professor of nuclear chemistry, and some of the things that you had kept working on while you had these administrative positions.

[Audio File 14]

14-00:00:10
Burnett: This is Paul Burnett, interviewing Dr. Joseph Cerny for the University History Series. It's June 24th, 2014. This is our seventh interview session, and this is tape fourteen. So, we are winding down your administrative functions at the end of the 1990s, and you are returning to be a professor. You have some personal anecdotes you'd like to recount about how to manage stressful jobs. This is "tips for the wise."

14-00:00:54
Cerny: The thing I do want to say is luckily, to keep my sanity in these jobs where you talked forty hours a week, is that, well, my wife grew up in Kentfield in Marin County and she'd always liked particularly Inverness. We'd been looking for a small place in Inverness as a weekend retreat, and we luckily found, in '87, a cabin in central Inverness, just going up the hill to where Manka's is, and we have an architect-designed cabin. It started as a tent cabin in the 1910s, and then the architect who inherited that was unemployed during the Depression, so he built a nice little cabin out of found materials and from stuff from some buildings that were taken down on the beaches, and it's very nice. It's redwood, and it's kind of an eight-hundred-square foot cabin and with a very large deck and a perfect view of Tomales Bay. So, we'd just go out there weekends and have dinner and do five-to-ten-mile hikes and not talk to anybody except family. It worked just fine to get through.

14-00:02:02
Burnett: At this point, your sons are grown, almost, and so, this is after they've left the nest.

14-00:02:09
Cerny: Yeah, they're grown.

14-00:02:11
Burnett: And they've gone off to college. Do you want to say something? Your sons are quite accomplished, aren't they?

14-00:02:18
Cerny: They are. I don't know what, if I say anything about them or not, but—

14-00:02:22
Burnett: You're very proud of them.

14-00:02:23
Cerny: I'm very proud. Well, the older son started liking music at a young age—Keith—and he would go on to join the Boys' Chorus in San Francisco, and also played piano very well, and did schoolwork very well. When we were in Australia for a quarter, he doubled up on his reading. By then, he was going to College Prep. Then, he said, "You know, I'm supposed to be in tenth grade, but I think I should be in the eleventh." The headmaster of College Prep said, "Okay." He was the youngest freshman at Berkeley at fifteen and a half, but we did not let the *Daily Cal* have an article on him. So, he did a music and physics double-major in five years, with highest honors in both, and then he had a Fulbright to London, and then a Hearst Music Fellowship to London. Then, did two more years of music in London and found his future wife and had Sir Stephen Spender's wife teach him piano. Then, his future mother-in-law called him up and said, "Keith, I'm not going to have my only daughter marry an unemployed musician," so he immediately came back here, found a job, started working for an accounting firm. They got married a year later. He went to Harvard Business School after that. Worked for McKinsey and Accenture, and then had four children, but he really wanted to do music. He always kept up his piano.

So, an opportunity opened up in 2004 at San Francisco Opera, and the board really needed a business-type to help run things, and sixteen interviews later, followed by an interview from the head of the headhunters group to come out and see if he could possibly do such a job, they gave him a job. So, he did very well as executive director and CFO at San Francisco Opera, but when Gockley came in from Houston to replace Pamela, he started cleaning house slowly, and Keith went back to the private sector. But then, in 2010 at the age of 47, he became the general director of the Dallas Opera. He's doing very well at it and he loves it. He got new operas designed. He went into a place where the cash flow was just about shot, and the Harvard MBA has been a real help to doing all this. So, he's got a lot of music going on and he's really loving it.

His brother, Mark, wasn't very musically inclined, but he was almost a mathematical genius. So, my first wife, having been a physics major, could help him with accelerated math. He also went to College Prep young, but then, once they ran through all the math there, he started going to Berkeley. By the time he started Berkeley as a freshman at sixteen, he'd already done all of freshman and sophomore math and physics, and so he lasted a year and two-thirds. By then, he'd done the physics curriculum, and meanwhile, he'd been playing arcade video games. So, he got very interested in video, and so, not quite eighteen, he decided to go to work for Atari. So, he went to work for Atari right away, in two or so years, got out a game called *Marble Madness* that he did that was quite a hit, an arcade game. Then, he went to Japan and worked for Sega for a while, married a Japanese girl—not a war bride, but her father was a well-known physician and she'd gone to college in Vermont for four years. They got married and he came back here to work for Crystal Dynamics, then somebody thought he'd do really well if he went to Universal Studios and was their vice president for videogames. He did that for a while, with the Naughty Dog group. They published a lot of hits.

I was down there one day and he showed me the facsimile of a $20 million check that Universal was paying to Sony, or vice-versa. Anyway, he didn't get a bonus or anything. He said, "It's time to go on my own." So, since then, he's been a high-level consultant for Sony. So, they live in LA and he's just loved it. Brought out a lot of games, and he has his own method, and he just was the main architect for PlayStation 4, and also designed a game for it. So, by now, he's probably recovering. Someone the other day said he'd become something like an icon in the videogame business. Well, actually, there are two awards you can get for lifetime accomplishments, and he's already gotten them. He'll be fifty this summer.

14-00:07:41
Burnett: He's had almost a thirty-five-year career, and to be almost fifty is kind of—

14-00:07:46
Cerny: Well, thirty-two.

14-00:07:52
Burnett: So, they both showed early promise. You're an academic and you value education, clearly. How did you create these geniuses?

14-00:08:16
Cerny: My first wife, actually, was completely Czech in the sense that when her mother was born here and her father came over when he was two, and on my side, my father was born here, of Bohemian and Moravian parents. So, the kids are three-quarters Czech, so it's probably unrealized Central European potential. [laughter] I remember, they could play bridge at eleven and twelve, so when we were on these trips, we could all play decent games of bridge.

14-00:08:56
Burnett: You didn't have to worry about pushing them, in other words?

14-00:09:01
Cerny: No. They knew what they wanted to do. They did a lot of *Dungeons & Dragons* and wrote a lot of computer code, and I would take them up to the cyclotron so they could see the oscilloscopes and talk to an early graduate

student of mine named Creve Maples, who did all kinds of interesting things with computers. They just were stimulated.

14-00:09:19
Burnett: Right, right, surrounded by all of that. That's a good digression, I think.

14-00:09:25
Cerny: So, I think they've done very well, and it's very neat.

14-00:09:28
Burnett: So, you had rest and relaxation to just deal with the challenges?

14-00:09:38
Cerny: Well, I had a year administratively, so after all these jobs, you got a year. So, that was good. We traveled a lot, and caught up on your research, and worked on this project BEARS we talk about. Then, they had me go back to teaching, and to punish me for having been gone for fifteen years, not doing any teaching, they had me teach the introductory junior level physical chemistry course, which I really like, called Introduction to Quantum Mechanics Applied to Atoms and Molecules. That's all fine, except I hadn't taught it for nearly twenty years. It was Monday, Wednesday, and Friday at eight, was up to seventy-four students. I never had seventy-four students before. I had taught it, but when I was younger. Anyway, that worked out, but in this, I had a really nice trip to Switzerland. In that spring semester, somehow, they'd heard what Maresi and I were up to because in March of 2002, I was asked to be the plenary lecturer at the faculty retreat of the Swiss Federal Institute of Technology in Lausanne, which is like the second-best school, held in Interlochen. My subject was to do, how do US Ph.D.s rate the quality and effectiveness of their doctoral education during the process and ten years after? Well, of course, that's what I claim information on, so it wasn't hard to get my talk ready. They couldn't give you an honorarium, it turned out, but they could give you two round-trip business class tickets.

14-00:11:10
Burnett: Well, that's good enough?

14-00:11:11
Cerny: That's good enough. Susan went, and we had a quick visit to Paris. I worked it out just perfectly to hardly miss any class and be gone, you know, I think I had an exam and a review and whatever. So, that worked out. Got to go to Paris, and went to the Interlochen retreat. That was really nice. Then, we went to Geneva, and two of my friends were working on the big accelerator at CERN, doing different things. So, my first French postdoc, Claude Detraz, by then had risen and he was the operations director of CERN, so the second-in-charge for CERN. That's kind of the apex of his career. My Finnish friend, Yuha Äystö, who'd been first a postdoc with me and then a visiting senior person, was there for three years because the low energy nuclear physics people have an online isotope separator that's been very, very productive in Europe, and heads are appointed for three years. So, he was there doing that,

so I got to see both of them, and then we flew back. So, that was a really great interlude, and they were in great positions, and really nice people. After that, I went back to teaching nuclear chemistry and physics, and teaching in the fall semester and the spring semester. I teach a course on an introduction to nuclear chemistry and technology, a laboratory, so I would do that to small classes because there's not much equipment.

14-00:12:43
Burnett: Is that a kind of lab?

14-00:12:46
Cerny: Yes, it's on campus. It was put together by my colleagues. I didn't actually put it together; I just started teaching it after. But they did put it together, and just now, it's gotten some wonderful new money, and Heino Nitsche's running it, it's really got great, modern equipment. We didn't have that. So, my main faculty committee, post-administration, has to be on a postdoc affairs committee—makes sense—appointed by the vice chancellor for research. I've been on it from 2005 to 2013, but it doesn't work very hard. I did some work with BEARS that I can talk about.

So, experimentally, I had actually started something that I was very interested in while I was an administrator, so I can back up to it. During the 1990s, not everybody, but all of the low energy nuclear physics types had gotten really interested in why can't we have radioactive beams out of our accelerators? There were different methods to do it. Some of them required building quite complicated things. We might already have one major cyclotron, and then you get another major cyclotron, probably bigger, that the first one can inject into, and you can get to higher energies. What you really want to do is get these radioactive beams because they have unusual neutron-to-proton ratios. If you pick, say, something that's already got an extra number of protons compared to neutrons and you hit something appropriate, that can get you even further off to the proton-rich side. So, there's been a lot of interest in that. I mean, there still is. Michigan State has this beautiful kind of set-up like this.

So, I had mentioned this at a review, but I hadn't done much about it. In April 1996, I was given a surprise two-night, overnight, festschrift at Bodega Bay, where my colleague Dennis Moltz, who's worked with me for many years, had arranged it. It was a surprise—I was faked out—and a large number of my prior graduate students and postdocs and colleagues came. Obviously, Canada, England, France, Finland, and Germany, and I was really surprised. It was sort of structured as a conference, so people would give talks during the days and have parties at night. So, that was really nice, but I then gave a talk on, well, I knew how we could get radioactive beams at Berkeley, but I sort of then really, again, got motivated into doing that. So, the lab has money. At LBL, there's a program called Laboratory-Directed Research and Development Funds, which are laboratory funds that you can apply for to do new projects. In the spring of '97, I made a pitch, then, to couple another cyclotron at LBL to our eighty-eight-inch cyclotron. The other cyclotron was up the hill—and still is—and it produced eleven MeV protons, and it produced them at high intensity to make radioisotopes. So, like you might make fluorine-18 for those purposes.

I was saying, "Well, why couldn't we use that cyclotron, have a transfer line that took us from that cyclotron down to our cyclotron, make some radioactive beams, and inject them into our cyclotron?" That way, we would have that. It was only 350 meters away. So, we got some money. A graduate student was very good at naming things, so he named it BEARS, Berkeley Experiments with Accelerated Radioactive Species, got it out there early. This project, so, what you do is you use the small cyclotron to bombard a target, a high-pressure gas target with a high intensity of these protons. Then, you want to make a radioisotope of interest, and you can transfer it down the hill somehow. We actually do it in capillaries to our cyclotron.

14-00:17:07
Burnett: Those are three millimeter? They're tiny, tiny.

14-00:17:11
Cerny: Yeah. They're tiny; it's just amazing. So, what you actually have to do is start up there, you have to put all this stuff in a pipe for safety reasons, then we ran a whole bunch of capillary tubes. Up there, then, we got out of their building, and then you had to go into a parking lot, and then dig a hole under the parking lot, across the side, and then put this thing along the side of a hill, and then bring it into our building and take it up. We had a lot of experience with helium jet systems, so we knew that you could actually transfer activity really fast in a helium jet, and we'd done that a lot, where you just blow high-pressure helium through something. Or, in our normal cases, you bombard a target in an atmosphere of helium, have a little capillary come out, point it at a collector, the detectors look at the collector, with a big pump. We knew that kind of thing would work. So, we put all that together, and then you can trap it at the big cyclotron in liquid nitrogen, get that trapped, get rid of everything else, warm it up, and inject it into the regular cyclotron, then you'll have a radioactive beam. That was easier said than done, and you have to do a lot of tests, but it all worked.

14-00:18:33
Burnett: I have here some of the diagrams that they did.

14-00:18:40
Cerny: Actually, the three-millimeter ID, little capillary, worked like a charm. I had a postdoc named James Powell who was extremely good at doing this kind of stuff. So, what we wanted to make was carbon-11, and so, carbon-11 is radioactive, carbon-12-and-13 are normal, and it has a half-life of twenty minutes. So, it's easily made. You put nitrogen up in this proton accelerator, hit it with a proton, knock an alpha particle out, and you have carbon-11. We're going to do everything like it's in a batch process with a five-minute

cycle time. You want to make it come down as carbon dioxide, and so, you're going to have to put a little oxygen in this nitrogen you're bombarding, so then you have to play around with that. You decide that 0.2 percent oxygen is enough, and so, then you can make carbon dioxide. So, we would irradiate it for five minutes and then transfer it, then what you do was some very complicated pumping and boosting. You actually pump the capillary line down, let the carbon dioxide in, push it with helium at high pressure, and you can get it all the way in the 350 meters in twelve seconds, which is pretty good. It comes through in a spike at two seconds, so you could actually stand at the cyclotron and look at the transfer line, if you held the radiation meter up to it, you could see it come through, and you could just see the radiation on the meter.

So, that worked, and then you trap the carbon dioxide in liquid nitrogen, then you warm at a controlled rate, and you feed it into the big accelerator. You have to do a lot of stuff and you have to do a lot of safety precautions, so I won't go into all this. You need a lot of automating computer control, but it all ran smoothly, and we did it—he did it. It required a lot of stuff. By August '99, in two years, we actually had a beam of ($2 * 10^8$) carbon-11 at 110 MeV on target. That may be a world's record, still. Now, how do we put that in any context?

A typical experiment, you would run something called a tenth of a microamp, and a tenth of a microamp would be, like, ($6 * 10^{11}$). This is ($2 * 10^8$), so it's a one third of a thousandth of that. Actually, that's enough to easily detect, and so, you can easily manipulate that. Now, you're going to take it out of the main cyclotron and run it through all your beam pipes and your magnets, get it out to an experimental area, irradiate some target, and do that. So, that worked very well, and it was unbelievably inexpensive to have this capability. It was just unbelievably. The CERN *Courier*, actually wrote this up in November '99, that we have been able to do this, and what our first experiment was. So, we did get our first experiment published in *Physical Review Letters*, and we had done a simpler kind of experiment I don't need to go into. By now, it's May 2000. I'm about to go back to our next steps.

14-00:22:32
Burnett: Does this become a kind of model that is replicated, as far as you know?

14-00:22:38
Cerny: Well, you need the two cyclotrons somehow, right? There'd be no reason a place couldn't buy a simple one because the others, you know, had been at fancy places that already had one big one, let's have a bigger one, and go on. You certainly could do that. I think we showed all the steps that made it interesting. What we also wanted to do was to try to look at the oxygen isotopes and see if we could make the neutron-deficient oxygen isotopes and look at some proton-rich nuclei. So, we wanted to make oxygen-14, so now, the stable isotopes are oxygen-16-17-18. Oxygen-14 has a half-life of seventy-

one seconds, so now you really are going to be running against the clock as you try to move it down the line, catch it in liquid nitrogen, warm it up, get it in. Also, you've got to figure out a way to make it, so this took us some time. The reaction's easy enough; the protons on nitrogen-14 hit it, while the proton sticks, a neutron comes out, and you have oxygen-14. It had been there all along, but with seventy seconds, it was gone, compared to twenty minutes, and so we didn't worry about it then. So, what we actually had to do was very complicated, and Powell worked on it very hard.

14-00:24:09
Burnett: Powell?

14-00:24:10
Cerny: James Powell, yeah, I mentioned the postdoc.

14-00:24:12
Burnett: Yeah, he's the first author on this.

14-00:24:15
Cerny: It turned out you had to do chemistry to do it, so it's amazing we could get it to work. What you did is, so, you're bombarding this nitrogen, mainly nitrogen, with a proton, and then giving you the ^{14}O. You put a little bit of hydrogen in it and you'll make water. Then, you separate this water really fast by running through a trap and taking it down to -40 degrees centigrade, and getting everything else to blow over the top of it. Then, you rapidly heat this $H_2{}^{14}O$ and put it into a small tube furnace containing carbon granules running at 1,000 degrees centigrade, or 1,100, which liked to burn out a lot, we learned, when you're doing that. That converts the water vapor to carbon monoxide, so you actually have carbon plus water, H_20, goes to carbon monoxide and hydrogen. This actually is the famous water-gas reaction, so if you told a chemist that, they'd know what it was, except ours has radioactive isotopes in it.

So, then you've got carbon monoxide, then you put it over a platinum catalyst, 180 degrees centigrade, and you get carbon dioxide. That's what you want because that's what we already know how to run down the pipe. So, it worked, but we had to do a cycle, like, of 100 seconds, and not five or ten minutes. Now, on the cyclotrons, there are all kinds of things running around, so we got, like, $(1 * 10^4)$, not $(2 * 10^8)$, in part because the half-life is so short. There are lots of things in the cyclotron running around at $(1 * 10^4)$. Also, these experiments required anyway that we had to do them in a different kind of kinematics and actually look at zero degrees. So, we had some detectors that would really look carefully at the main beam coming into it because it's so low. As you'd see, all these things, you'd have to try to figure out which one of them is oxygen-14, and which is some other junk that happens to be in the cyclotron. Well, we could actually get that to work well, and then you do experiments which are really simple, in terms of the experiments, so after you've found the oxygen-14, then you put it on a hydrogen target, right at zero degrees. So, the beam comes in, you're in the vacuum, and everything. Beam comes in, hits, and you use polyethylene, which is carbon and hydrogen, and that's the hydrogen target, and part of it doesn't care about the carbon.

Then, there are a lot of complicated things with this I won't go into, but you can do it and it really worked well. So, you can look very carefully now at a very proton-rich system, fluorine-15, because by the time you take one proton, add it to oxygen-14, that gets you to fluorine, and it's a very proton-rich nucleus. You can look at the structure of its energy levels, and you can actually fit it with something called r-matrix theory and do kinds of stuff we hadn't done before, so that was nice. Then, having done this, we thought we'd do oxygen-15, half-life 122 seconds, so a little longer. Now, you just bombard nitrogen-15 instead of nitrogen-14, but since it's expensive—because nitrogen-15 is stable but it's only 0.4 percent of nitrogen—you have to recycle your target gas rather than throw it away. That worked, and as you'd expect, you got ($4.5 * 10^4$), rather than ($1 * 10^4$).

So, we're able to do the same experiment then with oxygen-15, and look at fluorine-16, which had four levels that you could actually separate. We did that, and it looked really nice. In fact, almost for nothing, we got better results than this big French accelerator called GANIL with their two cyclotrons. We had more beam intensity and we could see all four groups, rather than three. So, we were pleased. We did other things with BEARS, but that turned out to be a project, over a number of years, that we learned a lot and did some things.

14-00:28:35
Burnett: So, it was a finite project, or is it still running in some sense?

14-00:28:37
Cerny: No, it's not, because at that point, things got more difficult and my group was kind of slowly waning. So, no, we did a few more, certainly, experiments, but those were the only three beams we developed. We showed it could be done, and I was really pleased. I mean, we didn't submit anything to the *CERN Courier*, so I'm very pleased that somebody did.

14-00:29:11
Burnett: Right. Well, it's fairly unusual because after fifteen years in senior administration at the university, of course you're expected to continue to do research to some degree, but it's very hard to be involved. So, when you return, not only do you resume your research but you seek out funding for and build a new apparatus that ends up being really useful and in your field.

14-00:29:46
Cerny: Right, and we had an experiment with a group from Oak Ridge that showed a really important resonance in carbon-11 plus proton, which was really neat, and a thing you could hardly see. In all of this, now, all my equipment got torn down because at a very poor juncture, my cave had to be earthquake-proofed. While that was going on, my key postdoc quit and decided he'd rather do bio-

rad stuff. So, at that point, we would have to go to different caves and set up—I mean, all at the cyclotron, but different experimental areas—but there were enough postdocs and graduate students coming through.

14-00:30:26
Burnett: It is frightening to consider that the Berkeley Laboratory was not earthquake-proof to start with.

14-00:30:35
Cerny: Oh, well, the standards changed. This is out in experimental areas. I mean, the central part where the cyclotron is definitely was okay, but they decided that some of the big, concrete shielding blocks weren't strong enough in certain caves.

14-00:30:56
Burnett: Yeah, that would be the biggest risk to you, is having your own safety equipment fall on you, right?

14-00:31:00
Cerny: Right, so they were having to do that. In the Loma Prieta earthquake, the cyclotron went off because it jiggled, but actually, they had the cyclotron back on in thirty minutes, I was told.

14-00:31:24
Burnett: It was fine, right. I guess because it's up in the hills, I mean, it's right on the Hayward Fault, in a sense.

14-00:31:33
Cerny: Right, but it's a different fault.

14-00:31:36
Burnett: Yes, that's true, it was a long way away. So, that was great, to have had that achievement as you returned to your research. You were teaching as well. I guess it's hard, once you've sort of made a name for yourself as a social scientist, right, you're doing this research in gathering quantitative and qualitative data and processing it on trends in higher education, so they don't leave you alone as far as that goes, right?

14-00:32:24
Cerny: Right. They didn't for a while.

14-00:32:27
Burnett: Right, you got a little hiatus. So, can you tell me a little bit about education in nuclear science and how that comes about?

14-00:32:28
Cerny: Yes. So, in addition to finishing up some things with Maresi, we'd actually been asked by a program manager at the Getty Museum in LA to do a similar study on Ph.D.s in art history, which we did do. It got published in 2003 as an Internet publication. Maresi was gone, but a colleague of hers from Germany

named Renate Sadrozinski, was the lead person on it here. She worked on that, and we luckily still had an experienced carryover staff member. I don't need to go deeply into that. It was a national survey looking at everybody that did art history or architectural history for the Ph.D. over the seven years from 1985 to 1991, and how did their careers go between academic appointments and museum appointments? It was a mixed, mail and email, study. So, that's available on the Internet. It got talked about.

14-00:34:09
Burnett: Were there some surprising findings from that report?

14-00:34:16
Cerny: No, in the sense that, you know, a lot of these results paralleled English, although there certainly are two fields in Art History. You can do museums or academe, whereas if you were doing English, you might think, "Well, there's only academe, and then there's what we call being business/government and non-profits." So, there were two fields. A lot of people did get their preferences, but the same kind of pattern of faculty neglect, not much help later on in helping look for a job or a lot of these things. Good satisfaction with the field, but disappointment. Almost everybody we surveyed, high percentages would do a Ph.D. again, but percentages would drop on the number that would do the Ph.D. at the same university, and that shows up in a lot of these studies.

As part of this, anyway, it's a little complicated, but years ago, high energy physics decided that it had to get its act together and it had to have a community consensus on what they wanted to do or they would tear themselves apart fighting. So, they set up a High Energy Physics Advisory Program. That was very successful, and so the nuclear scientists copied that, and there's a Nuclear Sciences Advisory Committee, called NSAC. The agencies meet with this committee, and so, in both cases, high energy physics and nuclear physics, to different proportions, the Department of Energy and the National Science Foundation are both involved. So, the NSF department of physics, nuclear physics would appoint people to NSAC in conjunction with the Office of Science, Office of Nuclear Physics of the Department of Energy. So, NSAC decided, in April 2003, to have a subcommittee on education in nuclear science. They wanted a status report and recommendations for the beginning of the twenty-first century. I got asked to chair that subcommittee.

So, the program managers tended to come to these. The NSF one always came; the DOE one, if he didn't come, a subordinate did. The chair of NSAC, though not a member, attended all the meetings. So, again, I thought if we were really going to do this—and they say, "What does a demographic picture look like? What can we say about improving the workforce? Education's important"—our report should document the status and effectiveness of our present educational activities, et cetera, et cetera. Basically, anyway, there are a lot of things to be concerned about, and so, basically what we did—this was a complete web survey—what I did is I decided we should survey nuclear science Ph.D.s five to ten years out, so I said I would do that. DOE would send me some money to take care of that. We also did a survey of all the postdocs because we could get the postdoc email addresses rapidly, and a lot of my survey could be tailored also for the postdoc survey by the person asked to do that. Then, we did a survey of graduate students separately, and so, we really knew what was happening in the field, and progress through it.

A lot of the same kind of things. Worrisome things are like the time to degree is seven years: well, seven years is a really long median time to degree, and you're stuck. Why can't you do better than that? Physics and astronomy has the worst record for women Ph.D.s, and still does, I know it may have changed a little bit, at 15 percent. Nuclear science, which is nuclear chemistry and physics, is 14 percent, then. Virtually no ethnic minorities. To find the Ph.D.s—the postdoctoral fellows were easy; they were working somewhere—I had to contact all the PIs for the NSF and DOE. We had to write them, "Who are your recent graduates, can you please tell me their email addresses?" To get the high levels of response in all these things we've done, you have to have really a tie in the field. Somebody in your field has to believe you care. So, then I had an official letter that I wrote, some more people signing it, saying it's our field and we need to know this.

14-00:39:58
Burnett: Right, get buy-in, as you say.

14-00:40:04
Cerny: We looked at things like the postdocs, well, how does it compare to biochemistry: two postdocs, 31 percent of the people do two postdocs, four and a half years, virtually identical numbers for nuclear science. This is after you've spent seven years. Then, only 26 percent are faculty, and in nuclear science, you tend to be either in the national labs or academe, and so, a reasonable number are in the national labs. Theorists don't do so well, but a lot of people go into other business non-profit things, that we saw what they did. This report went on the web in 2004.

14-00:41:05
Burnett: Well, it's an incredibly fine-grained and wide-ranging report. It looks at a whole bunch of different social and economic dimensions to different groups, different populations. So, how are you surveying underrepresented populations? The gender gap, trying to come to terms or understand the dimensions of the gender gap, there's really great data in here. Sixty percent of women who are postdocs had a spouse in nuclear science, or with Ph.D.s in nuclear science, or were working for national labs. Only 10 percent of male postdocs had spouses in nuclear science. So, there's this sense of this kind of two-body problem, but it was also—and rightly pointed out in your recommendations—an opportunity to capitalize on. If you have these nuclear sciences couples, right, you can better serve them and you also get the talent

of both. How can you better utilize that? That's just one example. It's not just this question of representation of women. You look at childrearing responsibilities, attitudes, experience in the graduate programs—it looks at a whole range of social phenomena.

14-00:42:49
Cerny: Right. That was heavily due to Maresi when we first started because she was very concerned about that. That has been embedded in every one of these, so it makes it very invaluable. There are interesting things about all these studies. Here's a quote from Roman, I don't know how to pronounce his last name, Czujko, who was and probably still is director of the Statistical Research Center of the American Institute of Physics: "Physics departments are isolated from the world outside of academe. Many physics departments are still driven by the dominant goal of adding to the knowledge base, that is, conducting basic research and preparing students to become the next generation of basic researchers. Too few faculty understand the remarkable diversity of careers commonly presumed by people with physics degrees." (You can substitute art history, you can substitute biochemistry, English.)

14-00:43:50
Burnett: It's a common problem.

14-00:43:51
Cerny: "Too few departments have modified their curriculum to address the needs of the majority of their students, that is, those students who do not become Ph.D.s conducting basic research." The open-ended questions are just worth doing the surveys, so you know, we had a dissatisfied group—24 percent said to strongly reconsider a Ph.D. in nuclear physics as advice to beginning doctoral students. Some of them said continue only if you love it, which is what also, I learned from art history, "Don't; choose alternative field; bad job market." "Be interdisciplinary" shows up in almost all these surveys. "If you don't absolutely love this stuff, do something else." "Academic research is all about sacrifice." "You'll work less and find more job openings, money, flexibility, et cetera, doing just about anything else." "If you are smart enough for nuclear physics, you can find something else that will give you a much better life."

Then, just an example for the need for improved mentoring, this is just so wonderful: "What would have helped you with your first job search as you completed your Ph.D. or postdoctoral position?" "It would have helped to talk to other women who had been through the same process. At the time, I did not know how to respond to remarks from the faculty that were interviewing me, such as, 'What's a pretty girl like you going to do for fun in a place like this?' Or, 'How many children do you plan to have? You look like you probably will have about three.'" So, this shows that physics faculty have barely gotten out of the Neanderthal Age.

So, we had a number of recommendations which I think hardly any of them were followed up on. Nuclear science, actually, right now is running one of its five-year plans, and I'm actually going to at least try and look up and see if the number of nuclear physicists has continued to go down. The number of nuclear chemists went down so much that they quit recording them as nuclear chemists in the NSF surveys, which is a shame. So, the overall demographic situation is there's a slowly declining Ph.D. production, low, slowly increasing percentage of women, abnormally low percentage of ethnic minorities, too long time to first job, inadequate career advice, job help, overall mentoring, unrealistic career expectations for too many, poor preparation for careers outside academe and the national labs.

14-00:46:58
Burnett: How do you feel about those kinds of criticisms? When you're talking about a highly specialized training, I mean, this is exactly the kind of conversation that I've heard about any discipline when I was in graduate school in the 2000s, that across the board, we need professional development both within the field and preparation for alternative careers. There was none, and there are blogs that are quite successful that deal with this question of life outside of the Academy. What would that look like, to have a highly specialized program that nonetheless trains you for something else that is ill-defined?

14-00:47:56
Cerny: Well, I'm spoiled by being in a national lab/university, where we have really good students. You're not so isolated, with really few faculty—one of the nice things about, I've probably said before somewhere in this, being a nuclear chemist at Berkeley is there's several fellow nuclear chemists on the faculty, but also, a lot of practicing nuclear physicists and chemists at LBL. So, it's not like a small university department with three people, all growing old together. I think we just know there are more job opportunities, that the well-trained people from here might have to go have a postdoc before going to the national labs, without a doubt. The national labs are desperate. So, I don't quite understand because these people are all just set to go to the national labs.

14-00:48:58
Burnett: One can be sympathetic to it, but if the whole beast is about specialization for a career track that has evolved—

14-00:49:11
Cerny: The nuclear scientists have learned a lot about computers and teamwork and setting up difficult experiments and all this.

14-00:49:18
Burnett: Critical thinking, right?

14-00:49:20
Cerny: So, it's all there and going on. I mean, my last graduate student now, postdoc, helped me on this other big experiment, too, and took a postdoc at Los Alamos but finally, you know, has gotten a job with Halliburton in Houston.

Probably used his computer and program stuff, he's extremely good at, say, pulling computer programs off an Internet and getting them to work the way you want them to modify and get them the way you want.

14-00:49:47
Burnett: Right, hot-rodding programs.

14-00:49:50
Cerny: Or finding new wells in Texas.

14-00:49:55
Burnett: Yeah, I remember that in one of the downturns after the dot bomb [2000 crash in the Internet sector and related software industries], physicists, there was a contraction in the market for physics Ph.D.s, or a perennial contraction. A number of them say that financial engineering was something that physicists were highly sought-after because they had the math, so they could become the quants on Wall Street.

14-00:50:27
Cerny: They did that for a while, right.

14-00:50:31
Burnett: So, a number of them did that, and of course, did have a much more lucrative career, if less fulfilling, in terms of scientific research. So, yeah, if you have those basic skills, you can travel to a lot of places that use math.

14-00:50:50
Cerny: I mean, a number of our nuclear science students have gotten this two-year postdoc to get trained as a health physicist or radiation physicist type, you know, for irradiating patients and designing all the stuff to shield them and protect them. That's great, and some of the people in my survey who had gotten this training said, "I'm happy; every other week, somebody offers me a job somewhere else and more money just because I'm trained in this." One of our best woman students got a postdoc like that at San Diego, and I'm sure she's off to an excellent job. I've had two of my weaker students do this, and get into great jobs.

14-00:51:30
Burnett: So, there are lots of choices. I mean, it is Berkeley, but definitely it seems like there are alternatives. The basic issue is adaptation and the need to adapt and what can the university do to facilitate those kinds of adaptations?

14-00:51:51
Cerny: I think if you've been in a small, isolated university, it could well be different, right? So, that would be too bad. We're probably nearly out of time. It's interesting that our major recommendation—which was ignored—was that the highest priority for new investment in education be creating a center for nuclear science outreach. We thought that—and it was a majority vote; not a unanimous vote, but enough—that public misconceptions happen in our field. There's the nuclear problem, the radiation problem, nobody can deal with that.

We need effective outreach that can engage the public from K-12 to adult; space sciences, and the genome project have done it. You need outreach to K-12 students, stimulate an increasing national understanding of the nuclear world. That was the number one interesting recommendation. That didn't go anywhere. I don't think any of the recommendations we made—but I'm planning actually to check—went anywhere.

14-00:53:01
Burnett: I think that's also a political thing. They might have felt that it's best not to touch that rail at all. But then if they don't, who is going to do that?

14-00:53:14
Cerny: Yeah, I mean, because of this fear of radiation. It's just part of it.

14-00:53:30
Burnett: I think I may have missed something, actually. I don't know if we can go back, here. The designated emphases, this was interdisciplinary graduate minors, this was ascribed to you, that this was your baby. Is that true?

14-00:53:55
Cerny: I think it was proposed by somebody else and I endorsed it. I don't remember it as one of mine, no.

14-00:54:09
Burnett: Well, there are questions from the people I interviewed, so we just tie off this part of your life history. When you reflect on those fifteen years, and even the years immediately after, what do you feel is your fondest achievement in terms of service to UC Berkeley? If there's a single one.

14-00:54:48
Cerny: Well, there'd be two. I mean, one is the fact, somebody had to solve the postdoc problem, and it got solved. The other is, somebody had to really take a look at completion rates and trying to change them, particularly in the social sciences and the humanities, and try to shorten time to degree in them, and try to set up mechanisms that would continue. I have no idea whether the Graduate Division has followed up on that, whether things have fallen back some, or maybe, by the time we put enough money in the right places, at least they would coast. I think that that was really important. I mean, we tried to do something important for the graduate students and we tried to do something important for the postdocs.

14-00:55:36
Burnett: Right, those are the two populations you were most concerned about. So, why don't we stop there? We have a few more minutes; we could talk about Merced.

14-00:55:53
Cerny: Do we have a few more minutes?

14-00:55:54
Burnett: We have a few.

14-00:55:55
Cerny: Let's talk about Merced. I can talk really quick about Merced. Okay, so, the other thing I did, which I found very interesting, was to help Merced. The University of California, Merced, when Merced first started, it wasn't an independent campus. It was sort of being run out of the Office of the President and the Academic Council. It had to do a number of things to actually become an independent division and an independent campus. Until then, the Office of the President set up the members of the Committee on Academic Personnel. Now, the Committee on Academic Personnel (CAP) is a very important Academic Senate committee, which reviews every appointment of the entire professorial series, every single one. It's the quality control for Berkeley and the whole UC system because it's independent of the administration and advises the administration, and so, that's great.

In 1999, UC President Atkinson appointed Carol Tomlinson-Keasey to be the chancellor for the new Merced campus. She'd actually been a UC Riverside faculty member, and then a UC Davis dean, and then she'd been working in the President's Office. So, they needed a system-wide Academic Council Committee on Academic Personnel for UC Merced. I don't know when it started, but I was appointed, starting in 2002. So, what we really did was look at personnel cases for the faculty they were beginning to hire. We did all this by telephone interviews. Every so often, we'd meet in the Office of the President. Basically, you are quality control for the faculty, and then also for decanal appointments and things like that. So, this committee will meet and write a letter to the vice provost for academic affairs, and it'll kind of go from there. In June 2005, Merced actually became an official independent division (campus), so they appointed their own committee. Up till then, there wasn't a single Merced faculty member on it. So, beginning in summer 2005, they had a committee of eight or nine people, of which four might be from Merced and five would be from other campuses. So, I was on that committee from 2005 to 2011.

We would meet every several weeks in the fall, and weekly in the spring, looking at personnel cases via teleconferences and the chair of the committee would write up the cases and talk about them with the Merced executive vice chancellor and the vice provost for academic affairs. For three years, I was the chair of CAP, from 2008 to 2011, where I would do all that. So, I would chair these meetings, we would talk about the quality of the faculty, agree on what level they ought to be appointed, look at various things in the academic personnel process that we didn't think were going well. I got a lot of technical advice here from our Berkeley people, so I had really good help if I had tricky questions. Since we were doing this, in the fall and the spring, we would go to Merced in the fall and we would meet with all the faculty, and particularly the new faculty, and explain, "Here we are, we're trying to help you, we're not trying to be difficult. This is what our job is, et cetera." We would separately meet with the assistant professors to try to say "It's okay, this is what we're doing, this is the process, these are the hoops you have to jump, where available," and we'd go back in the spring.

Every so often, we'd disagree with what the administration would want to do, and then CAP would have to talk about it, and then I as chair would talk to them about it. We always finally agreed. The important thing is you ultimately agree—even at Berkeley, you agree 99 percent of the time. The 1 percent, you got to really worry about, but you do that. I really thought that was a big help because then there were some retired professors from Irvine who were very useful, and we had a lot of other campuses willing to cooperate. So, we had high-level people who'd been on the CAPs at other campuses looking at the Merced faculty.

I was impressed, actually, with the faculty hires at Merced. They got a lot of tenured faculty couples coming from the Arizona schools, for example, that seemed to be working well. We had excellent assistant professors at schools in the Midwest and the East Coast who would say, "Ah, I'd like to come back to California, I'll go to Merced and I'll gamble that I'll get tenure there, rather than stay where I am." They looked good. One case I remember was that the senior faculty wanted to hire this particular person in English, but the assistant professors, we were told, really didn't like that person and thought it was just not going to work. So, CAP agreed that we should go with their opinion, and so we finally said, "Look, if you were a mature campus, we wouldn't get into this. It's not a mature campus. You really can't bring in someone who most of the assistant professors are afraid of or unhappy with or whatever."

14-01:01:41
Burnett: Right, yeah, so you set the tone.

14-01:01:42
Cerny: Yeah, and so we did that. I really enjoyed it. Three years, I've never heard of anybody serving as chair of that committee for three years. Maybe there are some other people, but anyway, I thought even though it's not a heavy workload compared to here at all, it's still a lot of work, and I thought three years was enough. I enjoyed that and I felt I did real service for the University of California, broadly speaking.

14-01:02:07
Burnett: Well, it's a unique opportunity because how long had it been since they created a new campus in the UC system? It was a really unique opportunity, once in a career, to do that.

14-01:02:14
Cerny: Late sixties, right.

14-01:02:17
Burnett: So, that was a really great service and a great opportunity.

[End of Interview]

[Added by Cerny during editing: I retired July 1, 2013. I received the Berkeley Citation from Chancellor Birgeneau in 2013 and the Berkeley Faculty Service Awared from the Academic Senate in 2014.]

Appendix

A. Personal

I. Curriculum Vitae

II. LBNL Retirement Write-up

III. Remarks from his younger brother Jan re: his 60th birthday party

IV. Publication List 1960-2011

V. Scientific American reprint on "Exotic Light Nuclei"

VI. Conference report on "Delayed Proton Emission from Nuclei: A Historical Perspective"

VII. An East African Journal, by Keith Cerny

B. Materials related to his appointment as Dean of the UCB Graduate Division

I. "Graduate Division Report 1985-1997" deposited in the Bancroft Library, not included in published oral history

II. A letter of commendation from the UCB Academic Senate

III. Reprints from the Council of Graduate Schools Communicator: 1. "From Facts to Action: Expanding the Traditional Role of the Graduate Division" 2. "From Rumors to Facts: Career Outcomes of English Ph.D.s"

IV. Viewport article in Science magazine: "Postdoctoral Patterns, Career Advancement, and Problems"

C. Materials related to his appointment as Provost/Vice Chancellor for Research

I. An organization chart for the Vice Chancellor for Research and the Graduate Division in 1999

II. The Academic Senate/Administrative Committee Report of "The Sunset to Dawn Review of Berkeley's Organized Research Units, July 2000"

A.I.

Joseph Cerny
Curriculum Vitae
October 2015

Born Montgomery, Alabama, April 24, 1936
B.S., Chemical Engineering, University of Mississippi, 1957
Fulbright Scholar, University of Manchester, England, 1957-58
Ph.D. Chemistry, University of California, Berkeley, 1961
First Lieutenant, Ordnance Corps, U.S. Army, 1962-63
University of California, Berkeley:
    Assistant Professor of Chemistry, 1961-62, 1963-67
    Associate Professor, 1967-71
    Professor, 1971 – 2013
    Professor Emeritus 2013-14; Professor of the Graduate School, 2014-
    Chairman, Department of Chemistry, 1975-79
    Dean of the Graduate Division, 1985-2000
    Provost/Vice Chancellor for Research, 1986-2000
    Lawrence Berkeley National Laboratory, 1961-
    Associate Director, LBNL, and Head, Nuclear Science Division, 1979-84

Guggenheim Fellow, University of Oxford, 1969-70
Fellow, American Physical Society, 1971
E.O. Lawrence Memorial Award, U.S. Atomic Energy Commission, 1974
Fellow, American Association for the Advancement of Science, 1980
Nuclear Chemistry Award, American Chemical Society, 1984
Alexander von Humboldt Senior Scientist Award, 1985
University of Mississippi Alumni Hall of Fame 1988
Honorary Ph.D. in Physics, University of Jyväskylä, Finland, 1990
The Berkeley Citation, University of California, Berkeley, 2013
Berkeley Division of the Academic Senate's Faculty Service Award, 2014

American Physical Society Division of Nuclear Physics:
    Program Committee, 1974-76
    Executive Committee, 1975-77
Member, Physics Division Review Committee, Argonne National Laboratory, 1973-75
Member, DOE/NSF Nuclear Science Advisory Committee (NSAC), 1980-82
    Chairman, NSAC Subcommittee on Manpower, 1981-82
Member, Tom W. Bonner Prize Committee, American Physical Society, 1982-84
Member, Panel on Electron Accelerator Facilities, DOE/NSF Nuclear Science Advisory Committee, 1983
Member, Department of Chemistry Evaluation Committee, Rensselaer Polytechnic Inst., 1983
Member, Oak Ridge National Laboratory Technical Policy Committee for the Electromagnetic Isotope Facility, 1983-85
Member, National Academy of Sciences Physics Survey Committee, and Chairman of its Nuclear Physics Panel, 1983-86
Member, Nuclear Physics Advisory Committee, Stanford Linear Accelerator Complex, 1983-87
Member, Annual Visiting Committee, Isotopes and Nuclear Chemistry Division, LANL, 1984-87

Member, Task Force for Restructuring the Physical Review for the APS, 1987-88
Member, National Science Foundation's Advisory Committee for Physics, 1987-89, and
Chairman, Subcommittee on Program Balance within the Physics Division, 1988-89
Member, NASA Advisory Council's University Relations Task Force, 1991-93
Member, Executive Committee, Association of Graduate Schools (of the Association of
American Universities), 1991-95; Vice President, 1992-93; President, 1993-94
Member, National Research Council Committee for the Study of Research-Doctorate Programs
in the United States, 1991-95
Member, Association of American Universities Committee on Postdoctoral Education, 1995-98
Member, Department of Chemistry Review Committee, Simon Fraser University, 2001
Chair, the Subcommittee on Education of the Nuclear Science Advisory Committee, 2003-2004
"Education in Nuclear Science: A Status Report and Recommendations for the Beginning of the 21st Century" November 2004
Member, UC Merced Committee on Academic Personnel 2003-08, Chair 2008-11

Edited Books:
J.D.Anderson, S.D.Bloom, J.Cerny, W.W.True (eds.), Nuclear Isospin, Academic Press, New York, 1969 (conference proceedings)
J.Cerny, ed., Nuclear Spectroscopy and Reactions, Parts A, B, and C (1974) and D (1975), Academic Press, London-New York

One hundred and eighty-nine scientific publications in refereed journals.
Publications while Graduate Dean (1985-2000) or inspired thereby (after 2000).

A.II.

Joseph Cerny Retires With Honors From Lab and Campus

LBNL June 2013

Joseph Cerny is retiring after more than half a century of research and leadership at Berkeley Lab and UC Berkeley. The former head of the Nuclear Science Division and Associate Laboratory Director at Berkeley Lab, Professor of Chemistry and former Chemistry Department chair, Graduate Division Dean, and Provost and Vice Chancellor for Research at UC Berkeley, Cerny leaves with another singular honor to add to a long list: the Berkeley Citation, awarded to those "whose attainments significantly exceed the standards of excellence in their fields" and whose contributions are "above and beyond the call of duty."

Joseph Cerny

Born at the height of the depression, Cerny got his B.S. in Chemical Engineering from the University of Mississippi in 1957 with support from the ROTC, and during 1957-58 attended the University of Manchester, England on a Fulbright Scholarship. He earned his Ph.D. in nuclear chemistry from UC Berkeley in 1961 and immediately started work as an assistant professor at the university, simultaneously joining the Nuclear Science Division (then the Nuclear Chemistry Division) at Berkeley Lab (then the Radiation Laboratory, or Rad Lab).

But in August the East German government began building the Berlin Wall, and by early 1962 Cerny was on active duty as an army first lieutenant. For most of the next 16 months he was in New Jersey evaluating techniques for studying explosive detonations. Once back at Berkeley, he wasted no time catching up with nuclear science.

"Russian theorists had suggested some interesting ideas about experiments that could be done to study light nuclei very far from stability," Cerny recalls. These were isotopes of elements like carbon whose nuclei had more protons than neutrons; most carbon is stable carbon-12, with six protons and six neutrons. "For example, we wanted to know the lightest carbon nucleus that could hold together on the order of a hundred milliseconds."

He had a stellar new instrument to work with. His graduate work had been done with Ernest Lawrence's 60-inch cyclotron, still operating on campus, but upon his return from the army in 1963 the Rad Lab's 88-Inch Cyclotron was up and running; it would be pivotal in Cerny's research throughout his career. Using state-of-the-art detectors and electronics developed by Fred Goulding and Don Landis at the Lab, Cerny found the answer to the carbon stability question – carbon-9, with six protons and three neutrons, has a half-life of 126 thousandths of a second, whereas the lighter carbon-8 lasts only about 100 septillionths of a second – "a huge dividing line," he says.

Cerny, right, and students confirm proton radioactivity, ca 1970

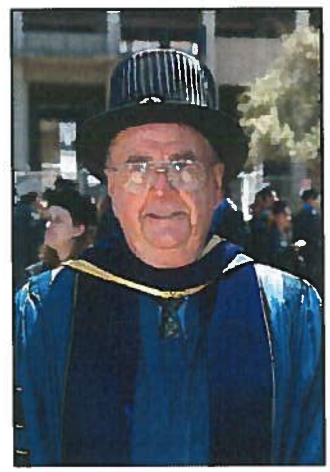
Cerny continued experiments on very proton-rich nuclei while on sabbatical at Oxford University in 1969-70, using a heavy-ion cyclotron at the Harwell Laboratory – "Everywhere I went on sabbatical I was doing experiments" – but he completed these studies at the 88-Inch. The result was the discovery of a new radioactive decay mode, direct proton radioactivity – the first mode of single-step radioactive transmutation to be discovered since alpha decay, beta decay, and spontaneous fission.

Cerny received the Ernest Orlando Lawrence Memorial Award of the Atomic Energy Commission (predecessor of the U.S. Department of Energy) in 1974, for his "discovery of proton emission as a mode of radioactive decay, for investigation of the limits of nuclear stability of a number of light elements" – and, significantly – "for ingenious instruments that made these discoveries possible."

In 1975 Cerny became Chair of the UC Berkeley Department of Chemistry. One of his major acts was a first for the department: the appointment of a woman, Judith P. Klinman, as a tenured Associate Professor of Bio-organic and Biophysical Chemistry.

In 1979 Cerny was appointed head of the Nuclear Science Division and an Associate Lab Director at Berkeley Lab, a time when the Lab was operating three national accelerator facilities, the 88-Inch Cyclotron, SuperHilac, and Bevalac, with a distinct taste for heavy-ion physics.

Cerny and his group continued research on radioactive decay modes, adding another first: beta-delayed two-proton emission, which had been predicted by Russian theorist V. Gol'danskii. Among other honors, in 1984 Cerny received the American Chemical Society's Award in Nuclear Chemistry for work leading to the discovery of "two new modes of radioactive decay: proton emission and beta-delayed two-proton emission."



Cerny at 2013 Chemistry Department commencement ceremonies in distinctive hat from University of Jyväskylä (photo Michael Barnes)

In 1985 Cerny was appointed Dean of UC Berkeley's Graduate Division, serving in that post until 2000. From 1986 to 1994 he also was Provost for Research, and from 1994 to 2000 was the University's Vice Chancellor for Research. And in 1990 he additionally became a nuclear physicist, when the University of Jyväskylä in Finland awarded him an honorary Ph.D. in Physics.

At a *festschrift* on his 60th birthday in 1996, Cerny presented a proposal for equipping the 88-Inch Cyclotron to handle radioactive beams of light ions. Radioactive isotopes of carbon, nitrogen, and oxygen would be made by the cyclotron of the Berkeley Isotope Facility in Building 56, part of the imaging facilities of the Life Sciences Division. The radioactive ions would be transported 350 feet through a capillary down the steep slope of Blackberry Canyon to the 88-Inch.

Dubbed BEARS (for Berkeley Experiments with Accelerated Radioactive Species), the transport system was in operation just three years later, enabling the 88-Inch Cyclotron to produce a world record beam of radioactive carbon-11. That isotope's 20-minute half-life was easily long enough, once it was created, to mix it with oxygen to make carbon dioxide and send the gas through the pipeline to the 88-Inch, where it was trapped and fed into an ion source at the cyclotron.

Cerny's research, teaching, and service work for DOE, NSF, and the UC system are contining from his base in Berkeley, where he and his family have been longtime residents. It's not unlikely that Joe Cerny will be seen around the 88-Inch, a mainstay of his work since his Berkeley beginnings, for many days to come.

—*Paul Preuss*

A. III.

[A Few Words Addressed to the Audience on the Occasion of Joseph Cerny's 60th Birthday Party at the University Art Museum, Berkeley, California, Friday Evening, May 10, 1996, by his brother Jan Cerny]

Good evening, ladies and gentlemen. I'm Joe's brother Jan and I've been asked to say a few words of introduction to the film [*Intruder in the Dust*], but first I'd like to say that although most of us are *not* acquainted, I've lived in Berkeley and the Bay Area for the last 25 years or so and I feel that I've met some of you before because, occasionally, someone waves at me from a passing car or says "Good Morning," across a crowded intersection and I'll glance up and over at a total stranger knowing that I've been mistaken for my brother once again.

In *Absalom, Absalom*, a work that in some respects may be likened only to the most extravagant expressions of the English Renaissance, Faulkner had this to say about his native land: "Tell about the South. What's it like there. What do they do there. Why do they live there. Why do they live at all."

Why do they live at all?

In Faulkner's important work, despite whatever comic or romantic flourishes it may have, the author persistently asks this question of his characters and by implication asks this question of this readers too.

Ah, yes, why do they live at all?

Joseph and I grew up in Oxford, Mississippi, Faulkner's hometown and the home of the University of Mississippi, in the last of what was then called the New South, but which seems to me now some 40 years later as actually the last of the Old South, the old haunted tragic land, still really quite an isolated, poor, and proverbially backward place in the 1940s and 1950s, the Age — after all — of Eisenhower.

At that time in the South , or at least in Mississippi, the myth of a glorious past still reigned supreme and many people who should have known much better still believed that the evils visited upon the South generally by the War Between The States and Reconstruction were the true source of the region's social ills and not some infamy that existed long before the War began. In such a setting, the War was never truly lost and figures like Albert Sidney Johnston and Robert E. Lee still held legendary status, indeed were likened unto plaster saints in some imaginary Southern heaven, while children were taught from an early age that Ulysses S. Grant had been an habitual drunkard and that Sherman's March to the Sea was never to be mentioned.

Thus, it's not so awfully surprising that the Confederate Dead were honored annually with holidays from school — Lincoln's birthday was not observed — and every other week or so in little towns throughout the South the United Daughters of the Confederacy (the UDC) would meet in musty parlors on weekday afternoons to mull over the errors of the past.

And so perhaps it's not so awfully surprising either what my 5th grade teacher, Miss Robbie Eades — Miss Robbie, the same woman who taught me in all due seriousness always to address her (and any other Southern lady) as "ma'am" — "yes, ma'am" —

what Miss Robbie had to say to me one day about the War Between The States that really, Jan, there was nothing very *civil* about it.

But our Old South in Oxford, Mississippi, was a little bit different from that of thousands of other Southerners living below the Mason-Dixon Line. *Not* perhaps just so much because the color line in Mississippi at that time rivaled South Africa under apartheid (you might find just as ugly a situation, say, in Alabama).

And *not* perhaps just because in Mississippi the War — not World Wars One or Two or the Korean Conflict — but the Civil War was still such a hot topic.

No, our southern experience in Mississippi was different from that of thousands of other Southerners because Oxford — our little postage stamp of native earth and its inhabitants — had been interpreted, closely and carefully and to devastating effect by a singular brooding presence who still lived then in our childhood less than a quarter of a mile away from our family home through Bailey's Woods and whose Nobel Prize was on display at the Mary Buie Museum just a short walk down the street.

For many years during my childhood and adolescence, this Prize (the Gold Medal featuring Alfred Nobel in profile and the illustrated citation) sat on a lower shelf in a glass display case at the Mary Buie Museum in between a matchbox presentation of a flea bride & groom made in Mexico and a pair of Confederate dolls. As a boy, the dolls were of no interest to me, but I would always ask the curator, Miss Roland for a magnifying glass so that I might examine the fleas more closely in their infinitely miniature, but immaculate, starched black and white.

As for the Nobel Prize, I regarded it and all of its surrounding paraphernalia with mixed emotion, a bit of idle wonder and curiosity, but mostly circumspection and a certain grave dubiety regarding Faulkner's character that I had learned from my father, the people in town, and the parents of my friends.

The film that you will see tonight is *not* from an important novel. However, it was shot in Oxford in 1948 or thereabouts and many local people, alive and active then, that Joseph and I knew as boys growing up in town, appear in bit parts and crowd scenes of the movie — for instance, the Lowe twins, Ed and Eph, on the mule, and Harry Campbell, an English professor at Ole Miss, who wrote the first book-length treatment of Faulkner's work and who forms part of the lynch mob outside the jail. The lawyer in the story, Gavin Stevens, was modeled on Faulkner's mentor and best friend, Phil Stone, a lawyer himself, to whom the Snopes trilogy is dedicated.

Intruder in the Dust had its world premiere in Oxford in 1949. Faulkner has been quoted as saying, "All in all I think it is a good movie," and Pauline Kael remarked about the film in her collection of essays entitled *Kiss Kiss Bang Bang* that "Had *Intruder in the Dust* been directed by a young unknown it might have been called a masterpiece." She goes on to say that "if this movie had been produced in Europe, it would probably be widely acclaimed among American students of the film as a subtle, sensitive, neorealist work." The performances are described as "astonishing."

As for me, I have always enjoyed the movie, but then it's easy to enjoy things that remind us of our past. Of course, I could identify with Chick the young white boy in the movie, but it was hard to imagine yourself really involved in his dilemma, particularly given the hostile tenor of the times then in Mississippi, and so to that extent the movie may be sentimental.

Juan Hernandez does a very good job indeed in the leading role and received two European awards for his performance. I have also always liked Elizabeth Patterson as the little old lady in the movie and thought indeed that she was not unlike many other courageous Southern women I have known. However, I have also always thought the portrayal of Crawford Gowrie a bit overstated and not in the right way, smacking more of Erksine Caldwell than of Faulkner.

In another context, Faulkner has this to say about his fictional home: ". . . Safe in Jefferson where life lived too with all its incomprehensible passion and turmoil and grief and fury and despair, but here at six o'clock you could close the covers on it and even the weightless hand of a child could put it back among its unfeatured kindred on the quiet eternal shelves and turn the key upon it for the whole and dreamless night . . .."

In closing, I would like to thank each one of you personally for coming to the theater this evening. We are honored by your presence and we hope you enjoy the movie and the reception afterwards. Happy Birthday, Joe, and again, thank you one and all.

A. IV.

Joseph Cerny Publication List, 1960-2011

Scientific Publications[1]

189. *Study of the $^{11}C(p, \gamma)$ reaction via the indirect $d(^{11}C,^{12}N)n$ transfer reaction.* D. W. Lee, J. Powell, K. Peräjärvi, F. Q. Guo, D. M. Moltz, and Joseph Cerny, J. Phys. G: Nucl. Part. Phys. **38,** 075201 (10pp) (2011)

188. *Reinvestigation of the Direct Two-Proton Decay of the Long-Lived Isomer $^{94}Ag^{m}$ [0.4 s, 6.7 MeV, (21+)].* J. Cerny, D. M. Moltz, D. W. Lee, K. Peräjärvi, B. R. Barquest, L. E. Grossman, W. Jeong, and C. C. Jewett, Phys. Rev. Lett. **103,** 152502 (1-4) (2009).

187. *Neutron Beams from deuteron breakup at the 88-Inch Cyclotron at LBNL.* M.A. McMahan, L. Ahle, D.L. Bleuel, L. Bernstein, B.R. Barquest, J. Cerny, L.H. Heilbronn, C.C. Jewett, I. Thompson, and B. Wilson, in Proceedings of the International Conference on Nuclear Data for Science and Technology, April 22-27, 2007, Nice, France, editors O.Bersillon, F.Gunsing, E.Bauge, R.Jacqmin, and S.Leray, EDP Sciences, pp 411-414 (2008).

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182. New ion-guide for the production of beams of neutron-rich nuclei between Z = 20-28. K. Peräjärvi, J. Cerny, J. Hakala, J. Huikari, A. Jokinen, P. Karvonen, J. Kurpeta, D. Lee, I. Moore, H. Penttilä, A. Popov, and J. Äystö, *Nucl. Instr. And Meth. Phys. Res.*A, 546, 418-425 (2005).

181. *Education in Nuclear Science: A Status Report and Recommendations for the Beginning of the 21st Century.* J. Cerny, C. Beausang, J. Cizewski, T. Hallman, C. Howell, A Palounek,W. Rogers, B. Sherrill, R. Welsh, S. Yennello, and R. Casten, A Report of the DOE/NSF Nuclear Science Advisory Committee Subcommittee on Education, November 2004

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Exotic Light Nuclei

by Joseph Cerny and Arthur M. Poskanzer

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Exotic Light Nuclei

Among the light elements nuclei with unequal numbers of protons and neutrons are highly unstable. Some survive just long enough to be detected and exhibit unusual regimes of radioactive decay

by Joseph Cerny and Arthur M. Poskanzer

In principle, protons and neutrons can be brought together in a great many combinations to form atomic nuclei. The nuclei representing the majority of these combinations, however, do not exist; even if they could be created they would decay too quickly to be observed directly. Altogether some 8,000 nuclei are thought to be capable of surviving long enough that they can be said to exist. Of these about 300 are stable indefinitely, and they are therefore by far the commonest nuclei in nature. Another 1,600 nuclei are known that are not stable but decay by the various processes grouped under the term radioactivity. That leaves some 6,000 nuclei that should exist but that have not yet been discovered.

The nuclei that have been investigated most completely are those of the lightest elements, the first 20 or so in the periodic table. Stable nuclei in this region have roughly equal numbers of protons and neutrons, and as a rule the further a nucleus departs from this ratio of equality the shorter its lifetime becomes. Those that are very far from stability have been called exotic light nuclei.

Such exotic nuclei are not observed among the natural elements on the earth today; their lifetimes are in many cases less than a second and some are much less. The exotic nuclei must be created in the laboratory by bombarding stable nuclei with accelerated protons or with heavier projectiles. Even then the short lifetimes hamper the experimental investigation, since all the information that can be obtained about an exotic nucleus must be gathered in the few moments that elapse between its creation and its disintegration.

Among the 20 lightest elements (up to calcium in the periodic table) some 200 nuclear species have been identified, of which perhaps 40 percent could be considered exotic. Theoretical calculations suggest that there should be about 110 other isotopes of elements in this region that are even further from stability. Extreme ratios of protons to neutrons are most easily achieved in light nuclei, simply because a given ratio requires a smaller numerical excess. For this reason the light elements provide an excellent opportunity for exploring the properties of exotic nuclei, many of which should have unusual sizes, shapes and spectra of energy levels. Many of them also exhibit unusual modes of radioactive decay, and the investigation of those decays is under way.

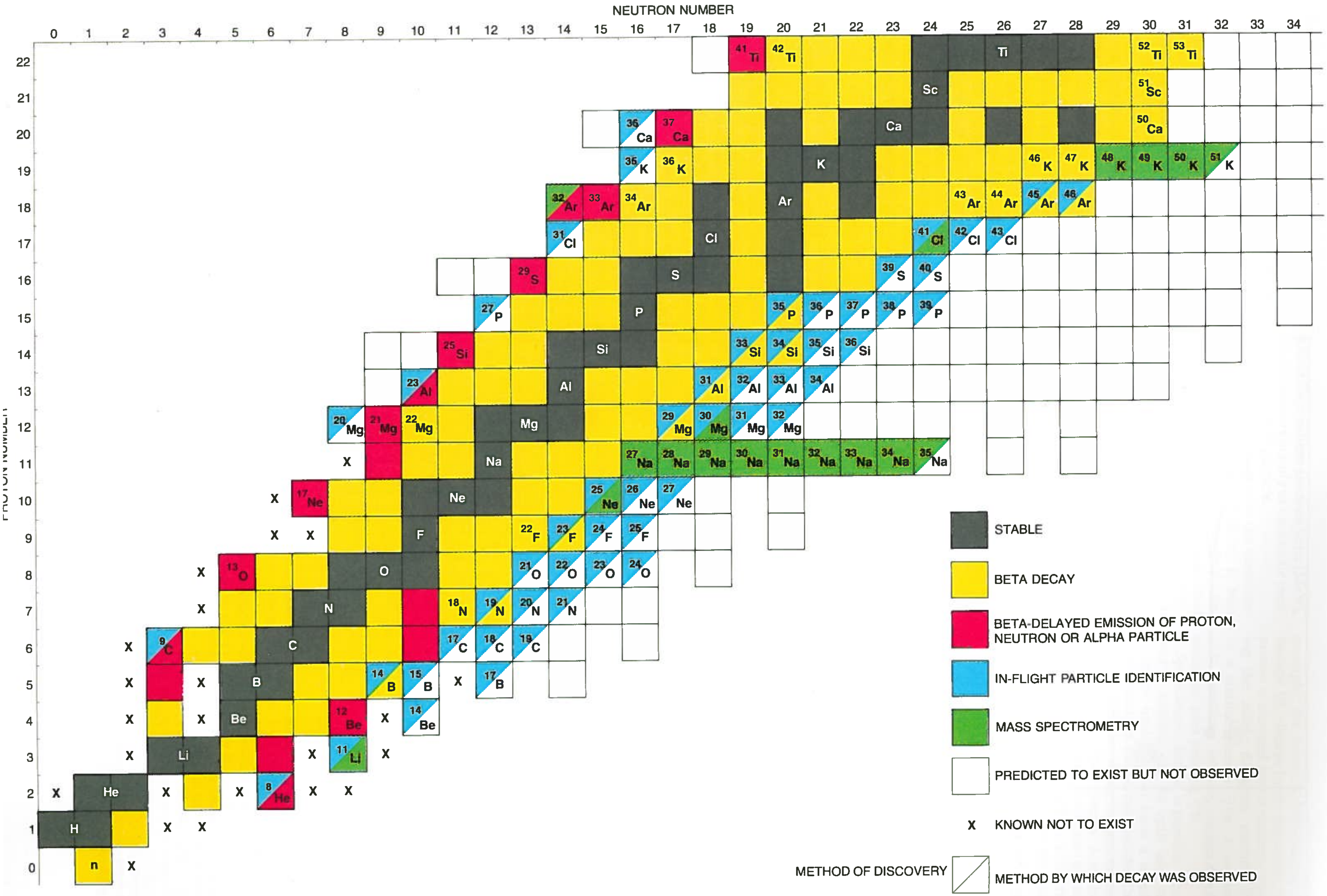
In order to identify a nucleus it is necessary to specify both the number of protons and the number of neutrons. By convention that is usually done by stating the number of protons (the atomic number) and also giving the sum of the proton and neutron numbers, a quantity called the mass number. When these numbers are written with the symbol for a chemical element the mass number appears as a superscript and the proton number as a subscript. (The proton number is often omitted since each element corresponds to a unique proton number.) Thus a nucleus of boron with five protons and five neutrons is denoted by the symbol $^{10}_{5}B$.

The proton and the neutron differ principally in their electrical properties: the proton carries a positive electric charge of one unit and the neutron is electrically neutral. The two kinds of particle are collectively called nucleons. Because the proton number determines the electric charge of the nucleus it also controls the configuration of the electron cloud surrounding the nucleus and hence the chemical properties of the atom. The proton number alone therefore determines the identity of the elements. A nucleus with five protons is a boron nucleus no matter how many neutrons it includes. Nuclei with the same number of protons but different numbers of neutrons (and hence different mass numbers) are isotopes. The search for exotic light nuclei is essentially a search for the lightest and the heaviest isotopes of those elements that have proton numbers ranging from one to about 20.

Nuclei can exist only because a tightly bound aggregate of several nucleons can have a lower mass than the total mass of the same nucleons when they are all considered in isolation. The small "missing" mass, or mass defect, is the binding energy with which the nucleons are held together. Like the electrons in an atom, the nucleons in a nucleus can be regarded as occupying discrete and well-defined energy levels. The lowest level can accommodate two nucleons, and as additional particles are added they must be allotted to levels of progressively higher energy. An important feature of this energy structure is that the levels for protons and those for neutrons can be considered independently. Thus in the helium-4 nucleus ($^{4}_{2}He$) the two protons can occupy their lowest-energy level, and so can the two neutrons. If all four were protons, then two would have to occupy a higher energy level, resulting in a nucleus with a larger mass. Thus, among the light nuclei those with equal numbers of protons and neutrons tend to be the most stable.

If the balance of proton and neutron numbers were the only influence on

CHART OF THE NUCLIDES lying on its side on the opposite page arranges nuclei according to their composition; each square represents a unique combination of protons and neutrons. The color of the upper left half of the square indicates by what method a nuclide was shown to exist; the color of the lower right half indicates by what method its decay was observed. Among the light elements (the first 22 are shown here) the stablest nuclei tend to have roughly equal numbers of protons and neutrons. The exotic nuclei are those that depart substantially from that ratio. They include both neutron-rich species, along the right-hand edge of the sequence, and neutron-deficient ones, along the left-hand edge. Nuclides with labels inside their square have been discovered since 1961, when one of the comprehensive charts of nuclides was made.


NEUTRON NUMBER
PROTON NUMBER
STABLE
BETA DECAY
BETA-DELAYED EMISSION OF PROTON, NEUTRON OR ALPHA PARTICLE
IN-FLIGHT PARTICLE IDENTIFICATION
MASS SPECTROMETRY
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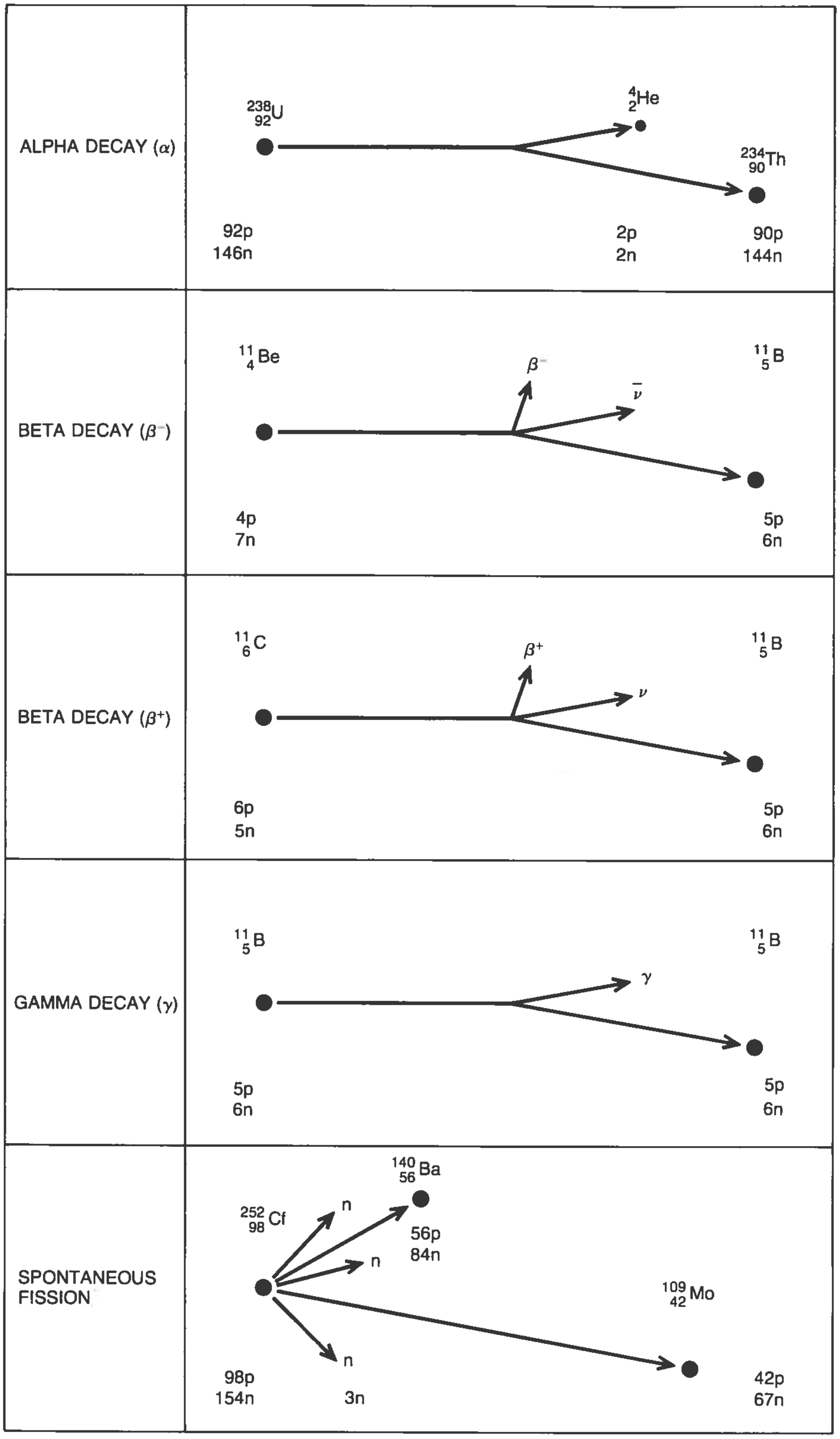
X KNOWN NOT TO EXIST
METHOD OF DISCOVERY
METHOD BY WHICH DECAY WAS OBSERVED



RADIOACTIVE DECAY transforms a nucleus into a more stable configuration of lower energy. Four modes of decay have long been recognized: alpha, beta and gamma emission and spontaneous fission; all of these except gamma emission convert one chemical element into another. Alpha decay and spontaneous fission are predominantly found in the heavy elements. There are two kinds of beta decay. In β^- emission an electron and an antineutrino are emitted when a neutron in the nucleus is converted into a proton; in β^+ decay the emitted particles are a positron (a positive electron) and a neutrino when a proton in the nucleus is converted into a neutron. The neutrino and the antineutrino are massless neutral particles with an extraordinarily weak interaction with nuclei. In gamma decay the composition of the nucleus does not change; only the energy level is affected. The emitted gamma ray is a high-energy photon, or quantum of electromagnetic energy. Some exotic nuclei have modes of decay more complicated than those shown here. A feature of all decays is that the total mass of the products is less than the mass of the parent nucleus; it is this reduction in mass that makes the decay possible.

nuclear stability, then all stable nuclei would have roughly equal numbers of protons and neutrons. In fact there is another factor, which becomes more important as the proton number increases: the growing electrostatic repulsion between protons also diminishes the binding energy of the nucleus. As a result, very heavy nuclei tend to be most stable when they have an excess of neutrons; the longest-lived isotope of uranium, for example, $^{238}_{92}U$, has about 1.6 times as many neutrons as protons.

The relative stability of the nuclides can readily be perceived in a graph that presents the mass defect per nucleon for each of the nuclides. Those nuclei with the largest mass defect, and hence those with the greatest binding energy, form a "valley of stability." Among the light elements the valley runs through those nuclides with roughly equal numbers of protons and neutrons; then among the heavier ones it bends toward nuclear species that are progressively richer in neutrons. On each side of the valley the less stable nuclei have a smaller mass defect and form steep slopes. The exotic nuclei occupy the upper slopes on both the neutron-rich and the neutron-deficient sides of the valley.

The ratio of protons to neutrons and the absolute number of protons determine the major features of the valley of stability, but there is one other influence on nuclear stability that cannot be neglected: nuclides with an even number of protons or of neutrons or of both have a slightly enhanced stability. The small enhancement becomes particularly important among the exotic light nuclei, where it is sometimes possible to add two nucleons to a nucleus to create a new species, although the intermediate nuclide with one extra nucleon does not exist.

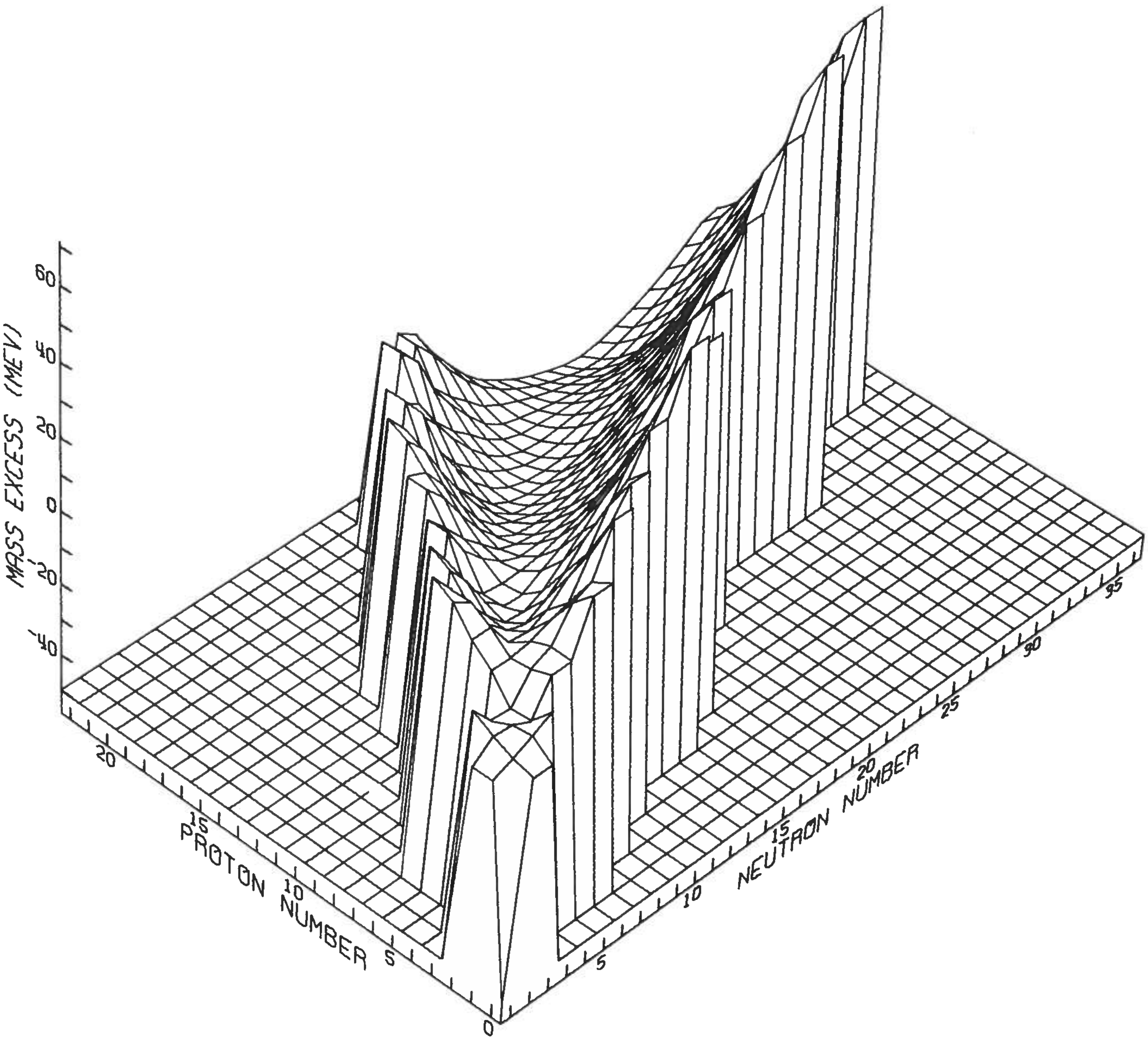
It should also be pointed out that all the nucleons do not always occupy the lowest possible energy levels. When a nucleon or several nucleons have been promoted to a higher energy level, the nucleus is said to be in an excited state. The excited state must of course have the same complement of protons and neutrons as the corresponding ground state, but it may differ in size and shape. The excited state can also differ in angular momentum: each nucleon has an intrinsic spin, equal to 1/2 when measured in natural units, and in the nucleus the nucleons can also have orbital angular momentum, reflecting their state of motion, which must always assume integer values (again in natural units). These quantities must be added or subtracted, depending on the orientation of the momenta, to find the total angular momentum of the nucleus.

Any nuclide that has a small mass defect, or a small binding energy, tends to decay to nuclei further down the slope toward stability. Three modes of radioactive decay that transmute one element into another have long been recognized: alpha and beta decay and spontaneous fission. Several additional modes of decay have been observed among the exotic nuclei, and a few more decay schemes have been predicted.

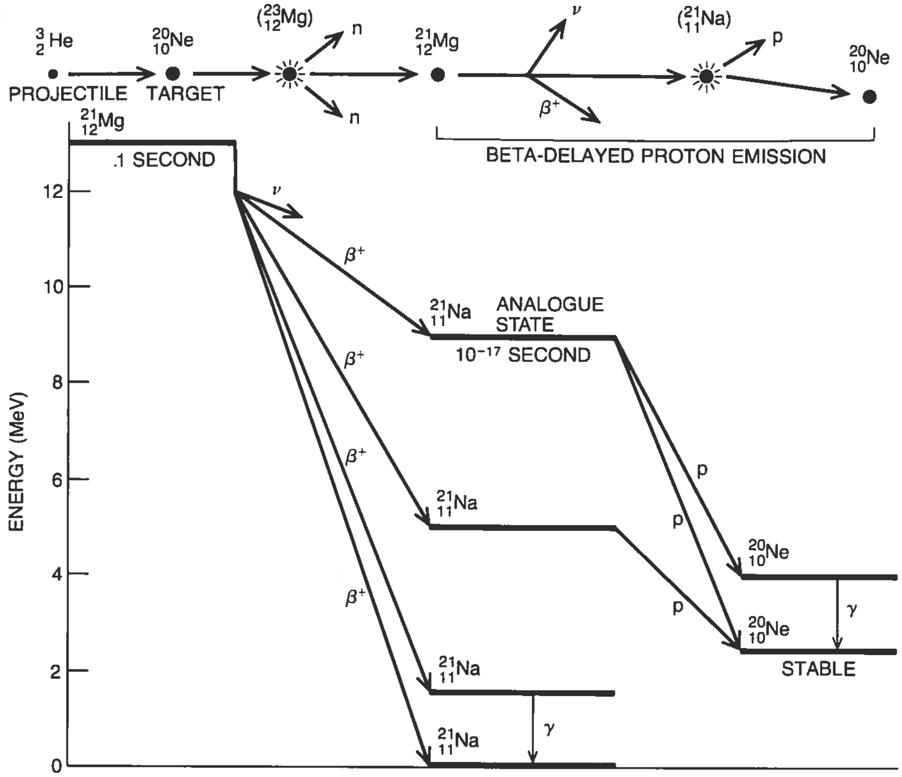
Alpha decay is the emission of a helium nucleus ($^4_2$He). When uranium 238 ejects an alpha particle, the $^{238}_{92}$U is converted to the thorium isotope $^{234}_{90}$Th. The difference between the mass of $^{238}_{92}$U and the combined mass of $^{234}_{90}$Th and $^4_2$He is equivalent to an energy of about four million electron volts (MeV), which appears mainly as the kinetic energy of the alpha particle.

Among the light nuclei the commonest mode of radioactive decay is beta decay, which takes two forms. In β^- decay a neutron in the nucleus is converted to a proton and two particles are emitted: an electron and an antineutrino. In β^+ decay a proton in the nucleus is converted into a neutron and the emitted particles are a positron and a neutrino. In neither case does the total number of nucleons change but the balance of protons and neutrons and the chemical identity of the nuclide are altered. For example, the neutron-deficient carbon isotope $^{11}_6$C decays by β^+ emission; one proton is changed into a neutron, yielding the stable daughter nuclide $^{11}_5$B. In a similar manner $^{11}_4$Be, a neutron-rich beryllium isotope, decays by β^- emission, also yielding $^{11}_5$B.

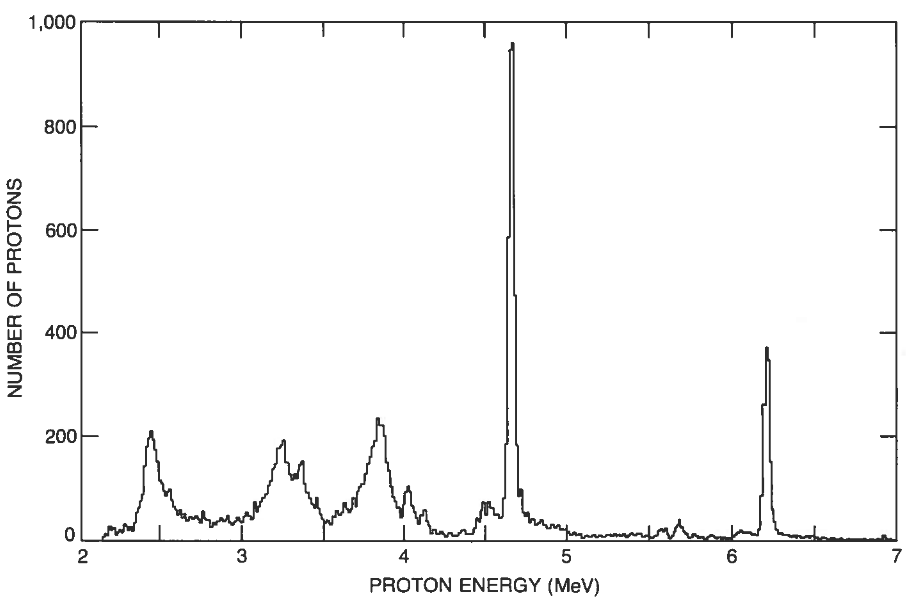
Spontaneous fission is possible whenever a nucleus can be split into two daughter nuclides that have a smaller total mass than the parent nuclide. Californium 252 can decay by spontaneous fission to yield, for example, a nucleus of molybdenum and a nucleus of barium, usually accompanied by a few free neutrons. The fission products are them-



VALLEY OF STABILITY is occupied by nuclides that have minimal mass and are stable. The valley is mapped by graphing the "mass excess" for each nuclide. The mass excess is related to the negative of the binding energy, with a mass excess of zero assigned to a reference nuclide: the carbon isotope with mass number 12($^{12}_6$C). Nuclei on the slopes above the valley can reduce their mass excess by decaying into a more stable nuclide. The exotic nuclei, which occupy the upper slopes on both sides of the valley, have the largest mass excess, and they generally decay the fastest. The graph was constructed with the aid of a computer by Jef Poskanzer, the son of one of the authors.



NEUTRON-DEFICIENT ISOTOPE of magnesium, $^{21}_{12}Mg$, sometimes decays by the unusual process of beta-delayed proton emission. The isotope is prepared by bombarding neon atoms with accelerated ions of helium 3. The nucleus formed is highly excited and decays by emitting two neutrons to yield $^{21}_{12}Mg$. That nucleus in turn decays by β^+ emission; the product is a sodium isotope, $^{21}_{11}Na$. The sodium isotope is often created in the ground state or in a low-lying excited state that can decay by gamma emission to the ground state. When a higher excited state is formed, however, it quickly expels a proton, yielding the stable neon isotope $^{20}_{10}Ne$. Because the excited $^{21}_{11}Na$ nucleus decays almost instantaneously, the entire process must be regarded as the two-stage decay of $^{21}_{12}Mg$. A highly favored beta decay occurs between $^{21}_{12}Mg$ and a state in $^{21}_{11}Na$ called the analogue state; in the analogue state all the nucleons have same relative motions as nucleons in the parent state, except that one proton has become a neutron.



ENERGY SPECTRUM of the protons emitted by decaying $^{21}_{12}Mg$ nuclei includes a number of broad peaks and two narrow peaks. The narrow peaks are at the energies of 4.7 and 6.2 million electron volts (MeV) and arise from a highly favored beta decay between the $^{21}_{12}Mg$ parent state and its analogue in the $^{21}_{11}Na$ daughter state. There are two groups because in the decay of the analogue state both the ground state and the first excited state of the $^{20}_{10}Ne$ nucleus are populated. The proton groups are sharply defined because the protons emitted in the decay process have a definite energy. For this reason the protons provide more information about the nucleus than do the positrons of the initial beta decay. Positrons have a continuous energy spectrum because energy of beta decay can be shared in any way by the positron and the neutrino.

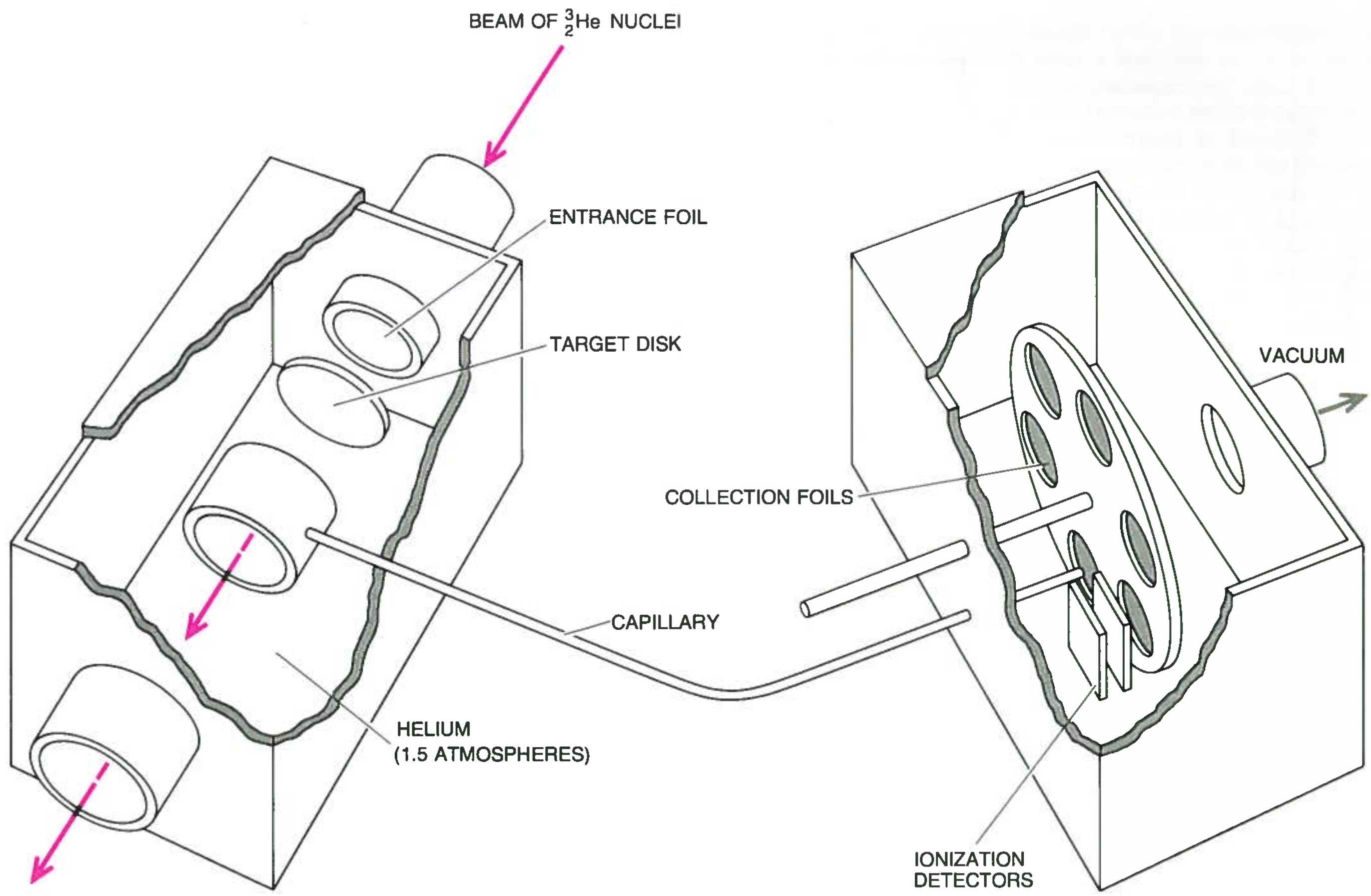
selves unstable and are subject to further decays, but in all cases the sum of their masses is less than the mass of the parent $^{252}_{98}Cf$ nucleus. (The energy released in this way appears as the kinetic energy of the fission fragments.)

There is one other common form of radioactive decay in which the chemical identity of a nucleus is not changed; indeed, neither the number of protons nor the number of neutrons is altered. This is gamma emission, and it affects only the energy state of the nucleus. The gamma ray is a high-energy photon, or quantum of electromagnetic radiation. The energy of the gamma ray is equal to the difference in mass between the parent state and the daughter state. Many of the products of radioactive decays are formed in excited states, so that alpha or beta decay is often followed promptly by gamma emission.

The rate of a radioactive decay is usually expressed in terms of a half-life: the time required for half the atoms of a particular species to decay. The observed time scale of beta decay ranges from about 10^{-3} second to more than 10^5 years; even the minimum lifetime is comparatively long in the realm of subatomic events. The delay is caused mainly by the weakness of the fundamental process that converts one kind of nucleon into the other kind in the nucleus. Certain other nuclear events, which do not involve such a transformation, can take place much faster. One that is of particular importance in the study of exotic nuclei is the spontaneous emission of a nucleon.

A nuclide is susceptible to decaying by emitting a proton or a neutron whenever the mass of the parent is greater than the total mass of the daughter nuclide and the emitted nucleon. The minimum time required for this decay is readily calculated: it is simply the time required for a nucleon moving at a typical speed of 10^9 centimeters per second to cross the nucleus, which has a typical diameter of 10^{-13} to 10^{-12} centimeter. The time is some 10^{-21} to 10^{-22} second. The existence of various inhibiting barriers can increase the time substantially, but only in exceptional cases should it be greater than about 10^{-16} second.

The enormous gap between the typical lifetime for beta decay (10^{-3} second or more) and that for prompt nucleon emission (10^{-16} second or less) provides a convenient experimental boundary separating those nuclides that can be considered to exist from those that cannot. It is possible to calculate theoretically which nuclides are bound against prompt nucleon emission and should exist by this criterion and which should not. One such set of predictions was made by Gerald T. Garvey and I. Kelson, who were then of Yale University. Their method was to calculate the mass



HELIUM JET is employed to transport beta-delayed proton-emitting nuclei from an accelerator target to a detector in the few hundred milliseconds available before most of the nuclei have decayed. The target is enclosed in a vessel that contains helium at about 1.5 times atmospheric pressure. The nuclei produced in reactions with the accelerated $^3_2$He beam recoil from the target; they are then entrained by the helium jet and are carried with it through a capillary tube into another vessel, which is continuously evacuated. In the particular case of $^{21}_{12}$Mg production the solid target was removed and $^{20}_{10}$Ne gas was mixed with the helium. In the second vessel the nuclei are deposited on a series of foils, which are rapidly stepped past a pair of detectors that are sensitive to electrically charged particles. The detectors measure the energy of the protons and can thus distinguish them from the electrons and the positrons, which are also emitted copiously.

of an unobserved nuclide by extrapolating from the masses of known nuclides in the context of an independent-particle model of nuclear structure.

The theoretical limits of stability against prompt nucleon emission and the present experimental limits can be illustrated by the isotopes of boron, an element that has two stable isotopes, $^{10}_5$B and $^{11}_5$B. Among lighter boron isotopes, $^9_5$B is known to be unstable to very rapid proton emission and therefore does not exist. The isotope with still one less neutron, however, $^8_5$B, does exist; it decays by β^+ emission with a half-life of .8 second. On the other side of the valley of stability five neutron-rich boron isotopes have been observed, the heaviest being $^{17}_5$B, which is expected to decay by β^- emission, although its half-life has not been measured. The isotope $^{18}_5$B is not expected to exist, but $^{19}_5$B should be stable to neutron emission. It has not yet been detected.

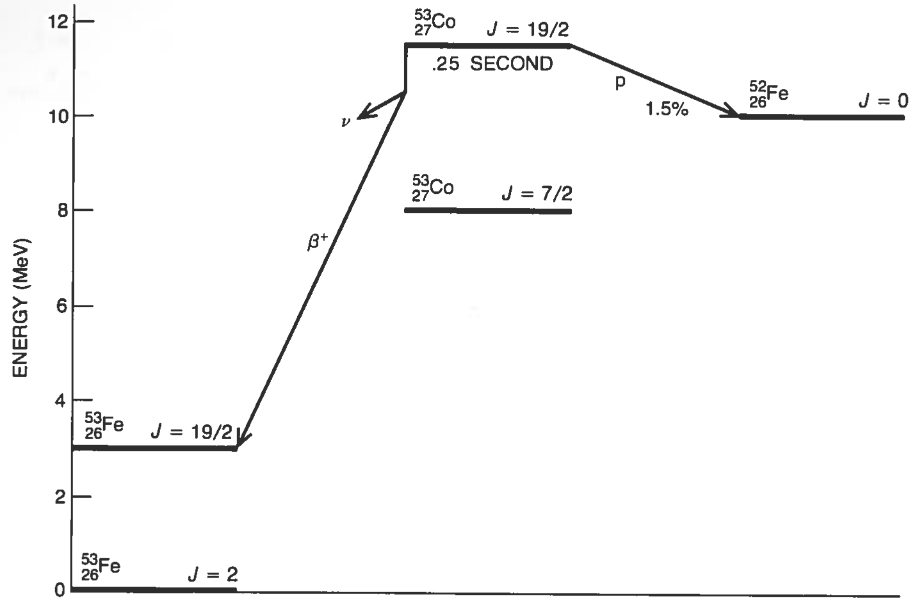
A chart of the known and predicted nuclides reveals several general features of their distribution. The most conspicuous trend is that the number of isotopes increases dramatically with increasing atomic number. Hydrogen (atomic number 1) has only three isotopes; sodium (atomic number 11) has 16 known isotopes and two more predicted ones; calcium (atomic number 20) is expected to have 31.

Another general feature of the distribution of nuclides is that there are more neutron-rich isotopes of all the elements than there are neutron-deficient ones. This effect is another consequence of the proton's electric charge. Adding a proton to a nucleus that is already neutron-deficient not only exacerbates the imbalance of protons and neutrons but also increases the electrostatic repulsion between the protons. Adding a neutron to a neutron-rich species affects only the balance of nucleon types.

The region of greatest interest in the study of exotic light nuclei is that lying between the nuclei that decay by conventional beta emission and those that are unstable to prompt nucleon emission. Among the neutron-deficient examples of these nuclei several decay by the two-stage process called beta-delayed proton emission.

An example of such a nuclide is $^{21}_{12}$Mg, which has three neutrons fewer than the lightest stable isotope of magnesium, $^{24}_{12}$Mg. Because of its neutron deficiency $^{21}_{12}$Mg is highly unstable; it decays with a half-life of .12 second by β^+ emission, yielding the nucleus $^{21}_{11}$Na. In its ground state $^{21}_{11}$Na also decays by the conventional mode of β^+ emission, but the nuclei formed by the decay of the magnesium isotope are not only in the ground state but also in many excited states. The additional energy of excitation is enough to make the $^{21}_{11}$Na nucleus unstable to the prompt emission of a proton, yielding as a granddaughter nuclide the stable neon isotope $^{20}_{10}$Ne. To the experimenter this decay sequence appears as the essentially simultaneous emission of a positron and a proton. (The neutrino that is also emitted cannot be detected.)

In the analysis of these events the protons arising from the secondary decay carry more information than the primary positrons, because the protons emitted in decays to each energy level have a definite energy. In beta decay the energy available can be partitioned in any way between the positron and the



PROTON RADIOACTIVITY has been observed from a long-lived excited state of a neutron-deficient isotope of cobalt: $^{53}_{27}Co$. By far the commonest decay mode of this excited state is conventional beta emission to $^{53}_{26}Fe$; about 1.5 percent of the time, however, the cobalt nucleus simply expels a proton to yield the iron isotope $^{52}_{26}Fe$. What is remarkable is the lifetime of the nuclide. Most nuclei that decay by the direct emission of a proton or a neutron do so very quickly, in as little as 10^{-21} second. Cobalt 53, on the other hand, has a half-life of .25 second, and if proton radioactivity were its only mode of decay, it would survive some 17 seconds. The symbol *J* represents the intrinsic-angular momentum of nuclides, measured in fundamental units.

neutrino. From the proton spectrum it is possible to deduce which states of the $^{21}_{11}Na$ nucleus have been populated. Not all the beta decays, however, leave the nucleus in a state that can decay by proton emission; the rest return to the ground state of $^{21}_{11}Na$ by gamma emission. The gamma rays also have unique energies (equal to the energy difference between the excited state and the ground state), and they convey information about the states populated.

Beta-delayed proton emission was first observed in 1963 by V. A. Karnaukhov, G. M. Ter-Akopian and V. G. Subbotin of the Joint Institute for Nuclear Research at Dubna, near Moscow, and by Richard D. Barton, Ross McPherson, Robert E. Bell, William R. Frisken, William T. Link and Robert B. Moore of McGill University in Montreal. Many light nuclei that decay in this way have since been identified, largely through the work of three groups of investigators: one led by John C. Hardy of McGill, another that included one of us (Poskanzer), then at the Brookhaven National Laboratory, and most recently a third group that included the other one of us (Cerny) at the Lawrence Berkeley Laboratory of the University of California. The 10 lightest nuclei in which this decay sequence has been observed, beginning with the carbon isotope $^{9}_{6}C$ and continuing through the titanium isotope $^{41}_{22}Ti$, have half-lives ranging from nine milliseconds to 470 milliseconds.

The nuclide $^{21}_{12}Mg$ was produced by employing as an accelerator target a gas containing atoms of $^{20}_{10}Ne$. (Since this nuclide is also the end product of the decay sequence, the entire reaction is cyclical.) The gas was bombarded by nuclei of the neutron-deficient isotope $^{3}_{2}He$ that had been accelerated to an energy of 30 MeV. In some of these interactions the two nuclei fuse, forming $^{23}_{12}Mg$ in a highly excited state. A fraction of the excited nuclei promptly eject two neutrons to form $^{21}_{12}Mg$.

Because of the short half-life of this isotope (.12 second) and of other beta-delayed proton emitters, special means are required for transporting the nuclide from the target area of the accelerator to the apparatus employed to detect its decay. In early studies of man-made isotopes the means of transport was somctimes a runner, who carried the specimen by hand from one room to another. As interest shifted to nuclei with shorter lifetimes a pneumatic shuttle, or "rabbit," was often adopted. For studies of the beta-delayed proton emitters even that method is too slow. The $^{21}_{12}Mg$ nuclei were carried to the detector by a helium jet. The neon target nuclei were mixed with a large volume of helium gas at 1.5 times atmospheric pressure. The product nuclei were entrained by the helium and swept through a capillary tube to another vessel that was continuously evacuated by a high-speed pump. There the magnesium nuclei (and others) were deposited on thin foils that were stepped several times per second past the detector.

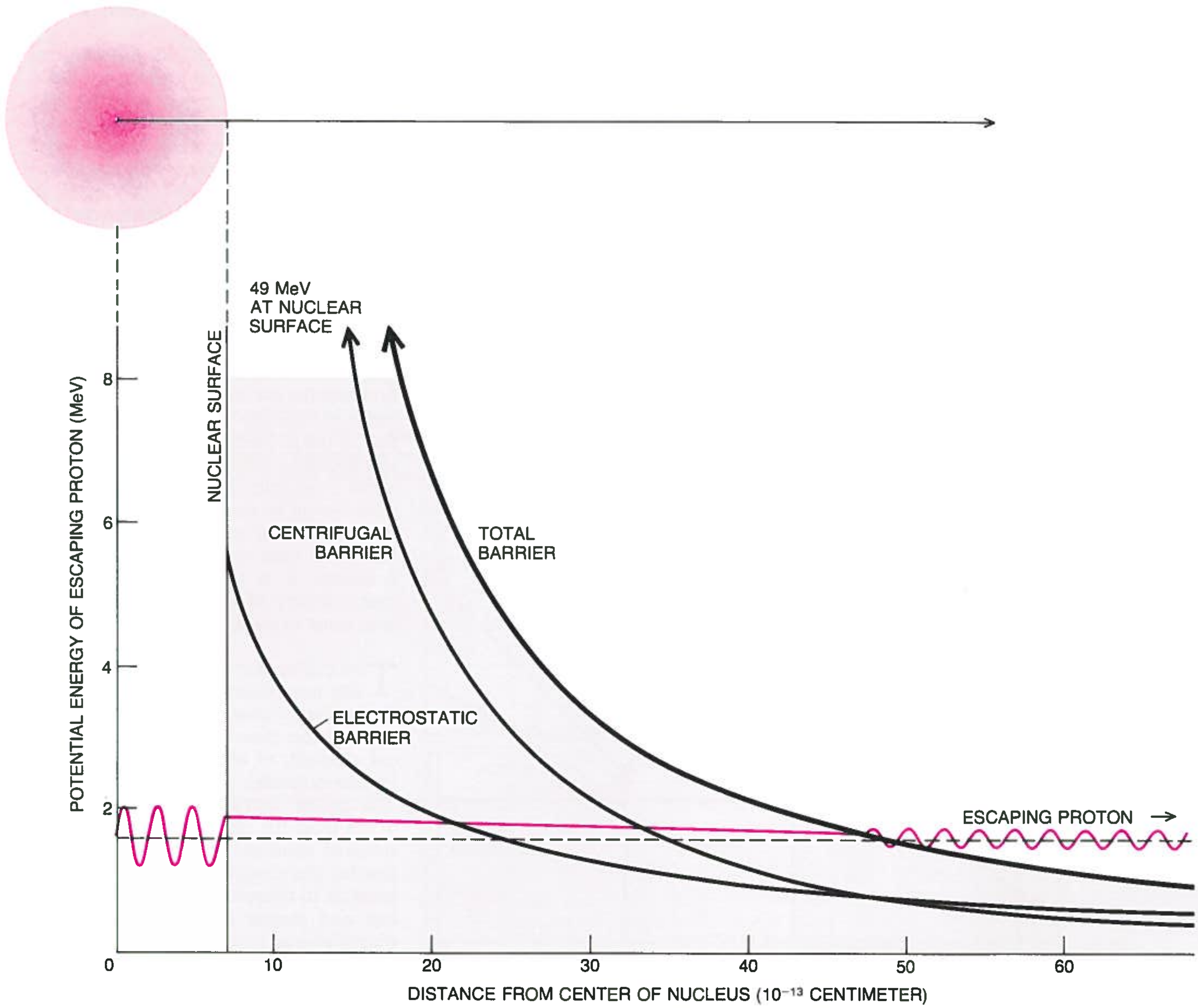
Nuclei of $^{21}_{12}Mg$ were not the only radioactive species formed in this reaction; indeed, they were a small minority. Most of the nuclides decayed by β^+ or β^- emission, and so the detector was required to recognize a small flux of protons in an intense background of electrons and positrons. In general particle detectors operate by sensing the ionization of atoms caused by the passage of a charged particle. Positrons, electrons and protons all bear an electric charge, but because the proton is much more massive it can be recognized. Protons lose energy more quickly as they pass through matter, and by arranging for the particles emitted to pass through two detectors in sequence the rate of energy loss can be measured.

The decay of a specimen of magnesium 21 yields a great many protons, which can be arranged in groups according to their energies. Each of the groups represents decay events to a specific energy level in the intermediate daughter nucleus, $^{21}_{11}Na$. A particularly interesting group of protons arises from a favored transition between the parent nucleus and an energy level in the daughter species that is called the analogue state of the parent. The concept of an analogue state is based on the theory of the charge independence of nuclear forces. This theory holds that if electromagnetic forces can be neglected, then the interaction between a proton and a proton, between a proton and a neutron and between a neutron and a neutron should all be the same. Hence if two nuclei have the same number of nucleons and those nucleons are in the same state of motion, then the two nuclei should be identical, even though their nucleons are apportioned differently between protons and neutrons. In this case both nuclei have 21 nucleons, and the theory of charge independence predicts that they have the same motion in the favored excited state of $^{21}_{11}Na$ as they have in the ground state of $^{21}_{12}Mg$. Actually, of course, electromagnetic interactions cannot be neglected entirely, and small differences between the states of the nuclei should be observed. The extent of the differences can be determined by studying the proton spectrum. For $^{21}_{12}Mg$ and for several other beta-delayed proton emitters the discrepancies have been found to be quite small; the analogue state and the parent nucleus resemble each other quite closely, typically to about 90 percent.

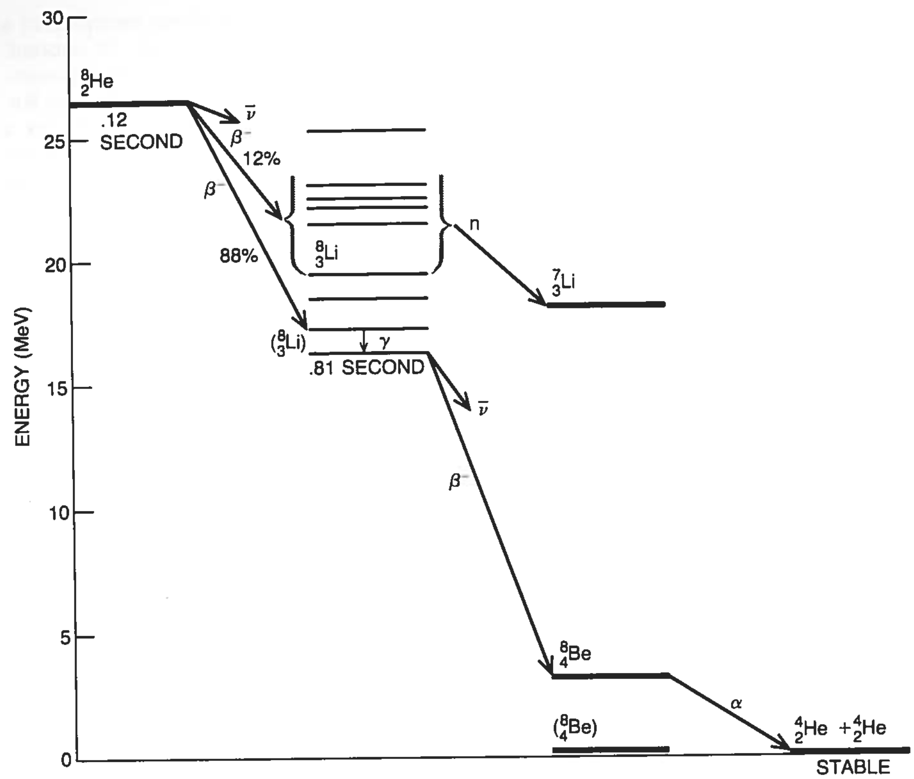
These experiments established the existence of $^{21}_{12}Mg$, along with its half-life and its decay modes, but they did not reveal its exact mass. The mass has been measured through another technique by one of us (Cerny) and his colleagues and by Walter Benenson and Edwin Kashy and their colleagues at Michigan State University. Again the accelerated projectile was $^3_2$He, but the target employed was the stable isotope $^{24}_{12}$Mg. Collisions between these nuclides include very rare events in which the helium nucleus picks up three additional neutrons, creating the isotope $^6_2$He and leaving behind a nucleus of $^{21}_{12}$Mg. The $^{21}_{12}$Mg nucleus goes undetected, but the energy of the $^6_2$He nucleus is measured. Since the energies and the masses of three of the four nuclei involved in the reaction are known, the exact mass of the fourth one ($^{21}_{12}$Mg) can be deduced by applying the laws of conservation of energy and momentum.

Direct nucleon emission was discussed above as a decay mode so fast that it excludes certain nuclei from existence. A nuclide could decay by the emission of a proton or a neutron with a comparatively long lifetime, however, if the emission were strongly hindered by a large barrier. A nuclide of this kind was discovered in 1970: it is a long-lived excited state of the cobalt isotope $^{53}_{27}$Co, which decays by the direct emission of a proton with a half-life of .25 second. The proton radioactivity was discovered at the Harwell Atomic Energy Research Establishment in England by a group of investigators from the University of Oxford made up of one of us (Cerny) and K. P. Jackson, C. U. Cardinal, H. C. Evans and N. A. Jelley. The discovery was confirmed by additional experiments at the Lawrence Berkeley Laboratory by one of us (Cerny) and J. E. Esterl, R. A. Gough and R. G. Sextro.

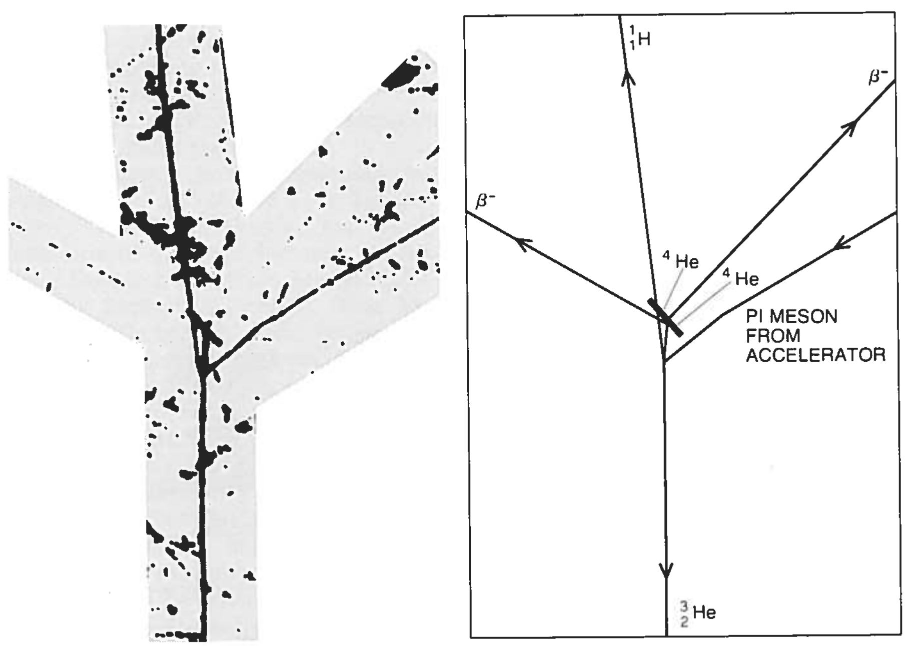
In the latter experiments the cobalt



EXTENDED LIFETIME of the $^{53}_{27}$Co nucleus can be explained in terms of barriers that a proton must be able to penetrate in order to escape the nucleus. There are two such barriers for $^{53}_{27}$Co: an electrostatic one, generated by the electrical repulsion between protons, and a centrifugal one, arising from the requirement that the emerging proton carry off the large angular momentum of the parent nucleus. The total barrier is some 49 MeV high at the surface of the nucleus; since the protons emitted have an energy of only about 1.6 MeV, they clearly cannot surmount it. There is a small probability, however, that the protons can pass through the barrier by the quantum-mechanical process called tunneling. The proton can be regarded as a wave that strikes the barrier and appears again on the other side of it. The emergent wave has the same wavelength as the wave inside the nucleus, indicating that the proton has the same energy, but the amplitude of the wave is vastly reduced, reflecting the smaller probability of finding the proton outside the nucleus. The amplitude is reduced by a factor of about 10^7, which is much smaller than can be shown in the illustration. It is the very small probability of such tunneling, along with an improbable nuclear rearrangement that is also required, that accounts for the long lifetime of the cobalt isotope.



NEUTRON-RICH ISOTOPE of helium, $^8_2$He, can decay through either of two unusual pathways. In each case the initial event is a beta emission, forming the lithium isotope $^8_3$Li. If the $^8_3$Li is created in a highly excited state, it promptly emits a neutron, giving rise to the stable product $^7_3$Li. The overall process is beta-delayed neutron emission. If the $^8_3$Li is formed in its lower excited states, those states decay to the ground state by gamma radiation; the ground state then beta decays to yield an unstable excited state of $^8_4$Be. The beryllium nuclide in turn promptly emits an alpha particle, or in other words splits into two nuclei of $^4_2$He, the commonest isotope of helium. In the latter pathway decay of $^8_3$Li can be described as beta-delayed alpha emission.



DECAY OF A HELIUM-8 NUCLEUS is recorded in a photographic emulsion at the left; the same events are diagrammed at the right. The $^8_2$He nucleus was one of three fragments created when a low-energy pi meson was captured by a carbon nucleus in the emulsion. The other two fragments, a proton ($^1_1$H) and a helium-3 nucleus ($^3_2$He) are shown leaving the field of view. The $^8_2$He nucleus moved only a short distance before it came to rest, and then it underwent two successive beta decays to $^8_4$Be, which split into two $^4_2$He nuclei. Tracks of the two $^4_2$He nuclei go in opposite directions because $^8_4$Be nucleus was essentially at rest when it split up. The photograph was supplied by Yu. Batusov of the Joint Institute for Nuclear Research in the U.S.S.R.

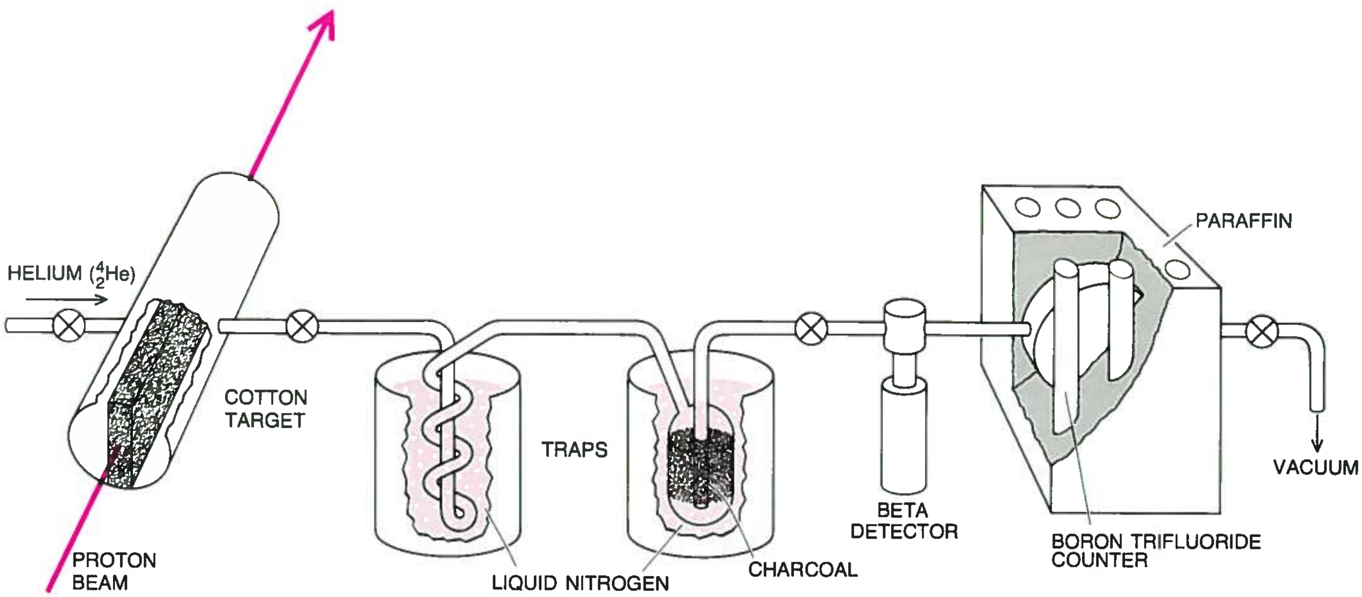
nuclide was made by bombarding the iron isotope $^{54}_{26}$Fe with protons accelerated to an energy of 35 MeV. The unobserved intermediate product was the cobalt isotope $^{55}_{27}$Co, which immediately ejected two neutrons to yield an excited state of $^{53}_{27}$Co. Unlike most excited states this one is inhibited from returning to the ground state by gamma decay and for that reason is called a nuclear isomer. Most of the time it decays by $\beta+$ emission, but in about 1.5 percent of the decays proton emission is observed. Unlike the protons emitted in beta-delayed decays, which come from several excited states and therefore have several characteristic energies, all the protons emitted by $^{53}_{27}$Co are clustered at the same energy, 1.59 MeV.

The $\beta+$ decay of $^{53}_{27}$Co is a highly favored mode because it leads to the creation of a "mirror" nucleus, which has the composition $^{53}_{26}$Fe and which occupies the same excited state as the parent cobalt nucleus. Mirror nuclei are a special case of analogue states in nuclei; in mirror nuclei the only difference between the two nuclides is that the number of protons and neutrons is interchanged. In this case the parent cobalt isotope has 27 protons and 26 neutrons and the daughter iron isotope has 26 protons and 27 neutrons. Because transitions between such states are highly favored, the $\beta+$ decay mode is the major influence on the lifetime of $^{53}_{27}$Co. In fact, if the $\beta+$ decay could be turned off, and proton emission were the only decay mode available, the half-life of the nuclide would be some 17 seconds, roughly 20 orders of magnitude longer than the usual time scale of direct nucleon emission. It is this "partial" half-life that a theory of the slow proton emission must explain.

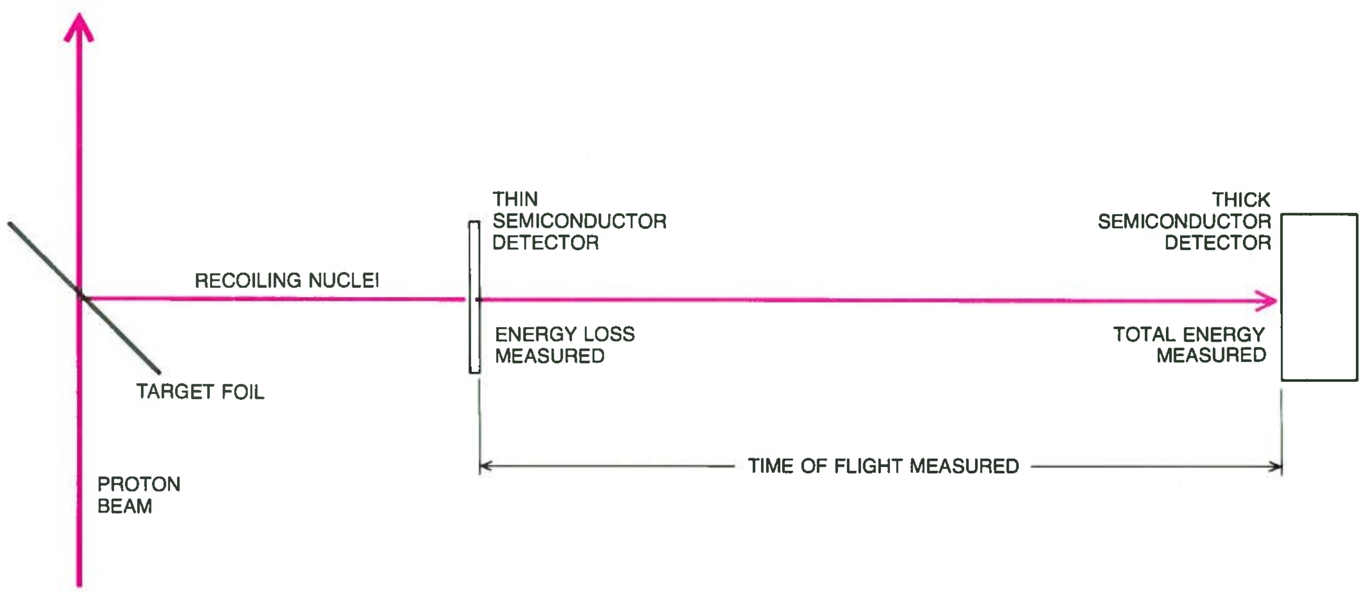
The explanation lies in the exceptionally large barrier to proton emission that must be overcome. Moreover, the explanation closely resembles theoretical accounts of alpha-particle emission by heavy nuclei, such as uranium 238. The major barrier to alpha emission arises from the electromagnetic interactions of protons, following Coulomb's law for the electric force. For an alpha particle to overcome the Coulomb barrier and escape from the nucleus requires an energy of about 25 MeV. According to the laws of classical physics only particles with that energy or more should be able to escape the nucleus, but in fact alpha particles are emitted by uranium with an energy of 4.2 MeV. These lower-energy particles are able to get out by the quantum-mechanical process of tunneling through the Coulomb barrier.

In quantum mechanics particles can be regarded as waves, and a wave has a small probability of penetrating a barrier. In the decay of the uranium nucleus the alpha particle has about one chance in 10^{38} of escaping on any given attempt. Although that is an extremely small likelihood, the alpha particle collides with the "walls" of the nucleus some 10^{21} times per second. It can therefore be expected to escape after some 10^{17} seconds, or three billion years. The measured half-life of uranium 238 is 4.5 billion years.

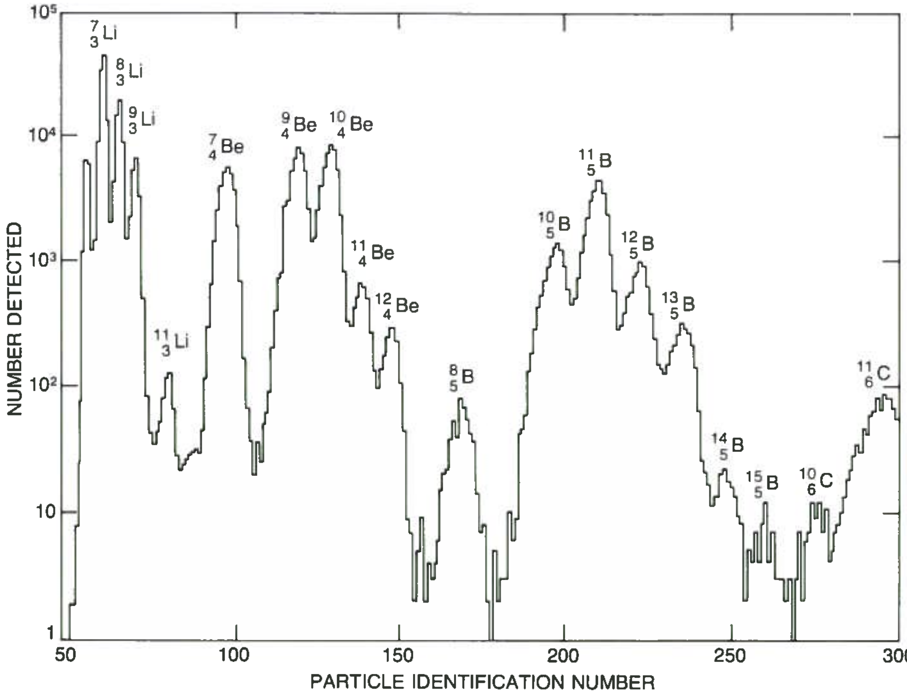
A similar Coulomb barrier inhibits proton emission in $^{53}_{27}Co$, but on calculating the probability that the proton will tunnel through the barrier the half-life turns out to be only 10^{-19} second, still many orders of magnitude short of the experimentally observed partial half-life. Part of the discrepancy can be attributed to an additional barrier, called a centrifugal barrier. The $^{53}_{27}Co$ nucle-



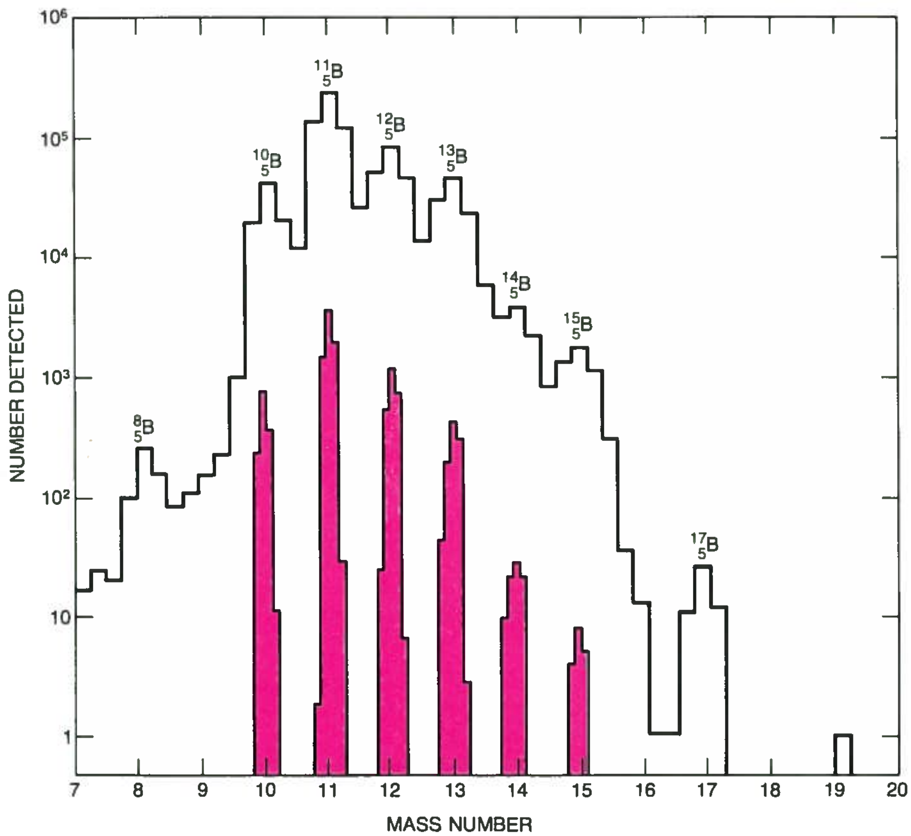
DETECTION OF HELIUM 8 relies on the distinctive chemical and physical properties of helium to separate ^8_2He from the numerous other nuclides created in collisions of accelerated particles with target nuclei. The target employed is cotton, which for the nuclear reactions of interest here can be regarded as being carbon. Nuclei of ^8_2He diffuse out of the cotton fibers and are swept up by a stream of ordinary helium gas (^4_2He). Other elements are condensed out of the stream in two cryogenic traps, which helium is able to pass through because of its extremely low boiling point. The residual gas then flows by a beta-ray scintillation detector into a tank embedded in a large block of paraffin. The neutrons from ^8_2He decay are slowed down in the paraffin so that they react with boron in the ionization detectors.



NUCLEAR TELESCOPE identifies nuclides recoiling from a metal target foil placed in the beam of an accelerator by measuring some of the nuclides' physical properties. The telescope consists of two semiconductor detectors lined up so that recoiling nuclei can pass through both of them in sequence. The amount of energy deposited by a nucleus in each detector can be determined from the number of ions created. The first detector is thin and measures the rate at which the nucleus loses energy in passing through matter; the second detector is thicker and stops the nucleus and measures its remaining energy. From these data a dimensionless quantity called the particle identification number can be determined; it is proportional to the product of the mass number and the square of the nuclear charge. In a refinement of the same technique the time of flight of the nucleus between the two detectors is also measured. This determines the velocity of the nucleus, which together with the nucleus' kinetic energy provides an independent and more accurate result for mass number of nucleus.



RECOILING NUCLEI from a heavy-element target irradiated by high-energy protons were identified entirely by measuring their kinetic energy and rate of energy loss, without measuring their velocity. The peaks represent the yields of the many isotopes produced in this reaction. The neutron-rich nuclides $^{11}_{3}$Li, $^{14}_{5}$B and $^{15}_{5}$B were first observed in such an experiment. On the other hand, the heaviest known boron isotope, $^{17}_{5}$B, cannot be observed because of interference from the more abundant carbon isotopes. The interference arises because the particle identification number is not unambiguous: product of mass number and square of nuclear charge is nearly the same for certain heavy isotopes of boron and certain light ones of carbon.



SPECTRUM OF BORON ISOTOPES is fully resolved in experiments where velocities of recoiling nuclei are also measured. Boron 17 was detected in such an experiment (*black*), in which time of flight was measured between first and second detectors. Even greater resolution is achieved (*color*) by measuring time of flight over a path between target and first detector.

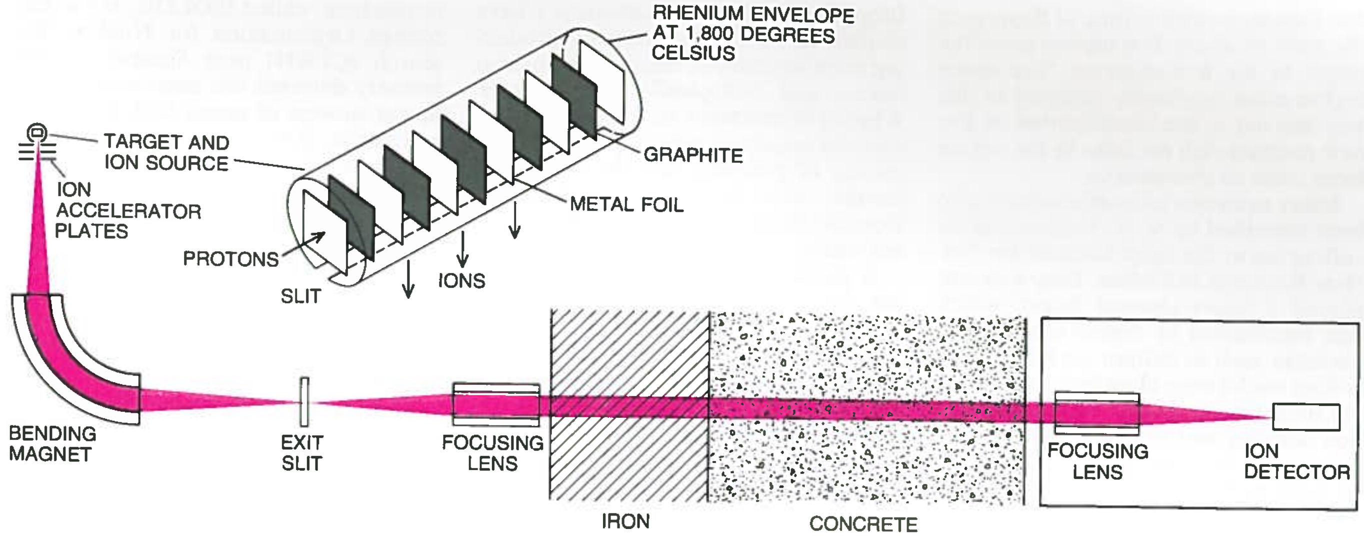
us has an exceptionally high angular momentum of 19/2 units, whereas the daughter nucleus, $^{52}_{26}$Fe, is produced in its ground state and has zero angular momentum. The emitted proton must therefore carry off all 19/2 units of angular momentum. One-half unit is accounted for by the intrinsic spin of the proton; the other nine units must appear as orbital angular momentum in the system made up of the daughter nucleus and the emitted proton.

The half-life expected for a proton to tunnel through both the Coulomb barrier and the centrifugal barrier is about 6×10^{-8} second. The remaining factor of 3×10^{8} can be accounted for by the exceptionally complicated and improbable rearrangement of the nuclear material that is required for the excited state of $^{53}_{27}$Co to decay into the ground state of $^{52}_{26}$Fe. Hence two separate probabilities must be satisfied before the decay can take place, the first for the emerging proton to tunnel through both barriers, and the second for the residual nucleus to have a structure that resembles $^{52}_{26}$Fe.

Another mode of decay might be available to some nuclei that are extremely deficient in neutrons. This mode, first suggested by V. I. Goldanskii of the Institute of Chemical Physics in Moscow, is the simultaneous emission of two protons. It might arise in an even-even nucleus where the decay to the next lighter (and therefore odd-even) nucleus was energetically impossible; in such a nucleus the enhanced stability of even-even nuclei might favor double proton emission.

On the neutron-rich side of the valley of stability the characteristic phenomenon of proton emission is not available. Beta-delayed neutron emission does take place, and indeed it has been known much longer, since 1939. The first light element that decays by delayed neutron emission was discovered in 1948 by Ernest O. Lawrence and was identified as $^{17}_{7}$N by Luis W. Alvarez. Because neutrons are electrically neutral they are not as readily detected as protons and different experimental techniques are required.

Where the nuclide of interest is an isotope of a gaseous element it is still possible to transport it from an accelerator target to a detector. Such a technique was employed by Alvarez for $^{17}_{7}$N and later by one of us (Poskanzer) in the study of helium 8, the heaviest isotope of helium and the only nuclide that has three times as many neutrons as protons. The $^{8}_{2}$He nuclei were created by irradiating the carbon nuclei in cotton with high-energy protons, which occasionally broke up the carbon nuclei. The helium atoms diffused out of the thin cotton fibers and were then swept by a stream of ordinary helium through



MASS SPECTROMETER linked directly to a particle accelerator separates short-lived nuclei according to their mass. The device in the detail drawing at the top is both the target of the accelerator and the ion source for the mass spectrometer. Nuclei created by collisions in the metal foils are stopped in the graphite, where they acquire electrons and become electrically neutral atoms. The atoms of the alkali metals diffuse out of the graphite and are ionized by contact with the hot rhenium envelope; they are then accelerated into the magnet of the spectrometer by an electrostatic potential of 10,000 volts. In the magnetic field the ions are deflected through various angles according to their mass. A particular mass is selected for study by an exit slit, and ions with that mass pass through an iron-and-concrete shield to a detector. The on-line mass spectrometer was developed at the Laboratory for Nuclear and Mass Spectroscopy at Orsay in France.

a series of cryogenic traps, which condensed all elements other than helium, and then on to the detection equipment. There beta and gamma emissions were monitored with scintillation detectors and neutrons were slowed by paraffin and counted with a boron trifluoride detector. The latter relies on the fact that a nucleus of ${}^{10}_{5}B$, after absorbing a slow neutron, sometimes decays by alpha emission; the ionization caused by the alpha particles can be detected. The detector only counts the neutrons, however; it cannot measure their energy.

These studies revealed an unusual decay sequence for ${}^{8}_{2}He$. About 12 percent of the time the nucleus decays by β^- emission to a highly excited state of the lithium isotope ${}^{8}_{3}Li$; that state then promptly decays by neutron emission to ${}^{7}_{3}Li$. In other words, the decay is an example of beta-delayed neutron emission. In the remainder of the decays the initial β^- emission leads to a lower excited state of ${}^{8}_{3}Li$, which is not capable of neutron emission but instead decays to the ground state by gamma radiation. The ground state of ${}^{8}_{3}Li$ is not stable either, however, but undergoes a β^- decay to ${}^{8}_{4}Be$, which promptly breaks up into two nuclei of ${}^{4}_{2}He$. (The last decay can be regarded as an example of beta-delayed alpha emission.) The overall reaction can therefore be described as one atom of helium (${}^{8}_{2}He$) breaking up into two atoms of helium (${}^{4}_{2}He$) plus two electrons and two antineutrinos.

An efficient method of creating many neutron-rich species is by bombarding a heavy element, such as uranium, with high-energy protons capable of breaking off small fragments of the nucleus. In the uranium nucleus the ratio of 1.6 neutrons for each proton provides optimum stability, but when the same ratio is conferred on a light fragment it represents an extreme excess. Such a procedure has the one disadvantage that an enormous variety of nuclides are created at once, and they must somehow be distinguished from one another. In spite of this difficulty most of the extremely neutron-rich nuclides have been discovered by some variant of this method.

One way of identifying the various nuclides created makes use of the kinetic energy with which they are knocked out of the target. The identification can be accomplished by a simple "telescope" made up of two ionization detectors that the recoiling nuclei strike in sequence. The first detector is thin and the nuclei pass through it, depositing only a part of their energy; the second detector is thick enough to stop the nuclei and to measure the total remaining energy. The product of those energies is known to be proportional to the product of the mass number and the square of the nuclear charge of the fragment. Although there are a few nuclei that cannot be distinguished from one another in this way most leave a unique signature.

By this method the extremely neutron-rich nuclide ${}^{11}_{3}Li$ was discovered by us and Earl K. Hyde and S. W. Cosper at the Lawrence Berkeley Laboratory. The nuclide appeared as a small bump in a curve recording the production of many commoner nuclei. The bump revealed nothing more than that the nucleus lived long enough to reach the two detectors, about 10^{-8} second. That in itself was a surprise, however, because theoretical calculations had indicated that ${}^{11}_{3}Li$ should be a prompt neutron emitter. Other prompt neutron emitters were not represented by bumps in the curve.

In a refinement of this technique developed by Gilbert W. Butler, James D. Bowman and one of us (Poskanzer) the differential energy loss in the thin detector is employed only to determine the nuclear charge. The mass number is given by the total kinetic energy and the velocity of the particle, the velocity being determined by measuring the time of flight between the two detectors. Because the velocities are roughly 5 percent of the speed of light and the path over which they are measured is only about 25 centimeters, the timing must have a precision of about 10^{-10} second. In return for making that precise measurement the nuclides are identified unambiguously by both proton number and mass number. By this method a new isotope of boron was discovered, ${}^{17}_{5}B$. It had been obscured by various carbon isotopes in the earlier experiment.

A further refinement of the technique was possible in an experiment conducted at the Los Alamos Meson Physics Facility by Butler, Dennis G. Perry, Louis P. Remsberg, Joseph B. Natowitz, Franz Plasil and one of us (Poskanzer). The high-intensity proton beam produced there is divided into bursts that are only about 10^{-9} second long. By measuring the time of flight of a fragment between the two detectors, both of which are far from the target, it is possible to determine which beam burst produced the fragment. Knowing this, one can then measure the time of flight over the path of about five meters from the target to the first detector. The much higher mass resolution achieved in this way has led to the identification of five new neutron-rich nuclides in the region from neon to phosphorus.

Many neutron-rich nuclides have also been identified by V. V. Volkov and his colleagues at the Joint Institute for Nuclear Research at Dubna. They also employed a heavy-element target, which was bombarded by beams made up of particles such as helium nuclei. The recoiling nuclei were identified by measuring their kinetic energy with an ionization detector and their momentum with a magnetic field. In spite of the success of these techniques a great many neutron-rich nuclei remain to be detected.

At best, the experimental techniques outlined above can reveal only the existence of a nuclide; to determine its mass, lifetime or decay modes more elaborate methods are required. D. R. Goosman and David E. Alburger of the Brookhaven National Laboratory have studied many exotic nuclei by producing them selectively, employing unusual beams and isotopically pure targets. When it is necessary to irradiate heavy-element targets, however, there is such a variety of products that the nuclide of interest must be physically separated from the target and from the other product nuclei.

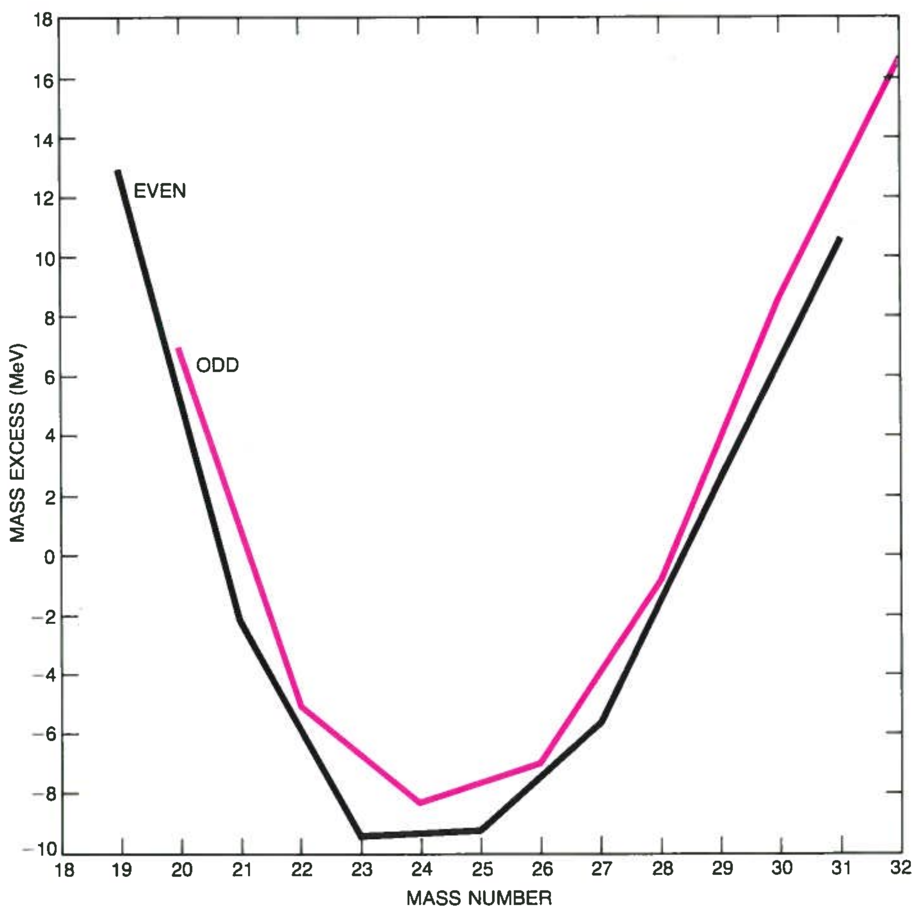
A powerful method for accomplishing the separation with the necessary speed has been developed by R. Klapisch and C. Thibault of the Orsay laboratories in France. In their method the product nuclei are recovered from the target in the form of ionized atoms and injected directly into a mass spectrometer. In the spectrometer the ions are accelerated to a fixed velocity, then passed through a magnetic field. The extent to which their paths are bent by the field is determined by their momentum, and hence by their mass. An exit slit allows only those nuclides of a particular selected mass to reach an ion detector. Similar apparatus is employed in another machine, called ISOLDE, at the European Organization for Nuclear Research (CERN) near Geneva. It has recently detected the most neutron-deficient isotope of argon that is predicted to exist, ${}^{32}_{18}Ar$.

The device built at Orsay is particularly well suited to the study of the alkali metals, and largely as a result of work with that machine more neutron-rich isotopes are known of sodium and potassium than of any other light elements. What is more, the masses, lifetimes and decay modes of most of those isotopes have been determined.

The Orsay group was able to measure the lifetime of ${}^{11}_{3}Li$ by monitoring the rate at which the activity declined after each burst of particles irradiated the target. They found it has a half-life of 8.5 milliseconds. In order to measure other properties of the nuclides the ion detector at the exit slit is replaced by a metal foil on which the ions are deposited. Beta, gamma and neutron emissions can then be monitored with standard detectors, but without the complication of background events from other nuclides. In this way ${}^{11}_{3}Li$ and all the sodium isotopes between ${}^{29}_{11}Na$ and ${}^{32}_{11}Na$ were found to be emitters of beta-delayed neutrons.

Finally, the mass of ${}^{11}_{3}Li$ was measured by the Orsay collaboration and one of us (Poskanzer). The mass spectrometer has an intrinsic resolution of about one part in 500, which is ample for discriminating between isotopes but inadequate for a precise mass measurement. In order to determine the binding energy of the nuclide the mass must be known to an accuracy of about .1 MeV; since the total mass of ${}^{11}_{3}Li$ is almost 11,000 MeV, that requires a resolution of roughly one part in 100,000. The necessary precision was obtained by measuring the mass of tens of thousands of ${}^{11}_{3}Li$ nuclei and averaging the results. It was found that lithium 11 is bound against the prompt emission of a neutron by only .17 MeV ($\pm$.08 MeV).

The study of exotic nuclei continues to generate unexpected results. Although on the neutron-deficient side of the valley of stability all nuclei predicted to exist up to ${}^{20}_{12}Mg$ are known, only for hydrogen, helium, lithium and beryllium have the frontiers been pushed to the edge of existence on both sides of stability. For the other light elements much remains to be learned. New methods of production and ingenious methods of detection will be required. It is of interest to explore nuclei in all their exotic forms and not to be confined simply to the region near the narrow bottom of the valley of stability. Perhaps other modes of radioactive decay will be discovered. Certainly new knowledge of the structure of nuclei will emerge.



MASSES OF SODIUM ISOTOPES were determined out to the high-neutron-excess region with the on-line mass spectrometer in experiments at the European Organization for Nuclear Research (CERN). The graph is a cross section of the valley of stability at the line where the proton number is 11. Again the quantity represented is mass excess. Masses of those isotopes in which the number of neutrons is odd (*color*) are graphed separately from those in which it is even (*black*); the two groups form separate series because nuclei in which particles form pairs have enhanced stability. Masses of two more sodium isotopes (mass numbers 33 and 34) are being determined by experimenters, and the isotope with mass number 35 has been observed.

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A.VI.

Delayed Proton Emission from Nuclei: A Historical Perspective

Joseph Cerny

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Abstract. Early experiments observing proton emission are reviewed, with an emphasis on the initial discovery. Beta-delayed proton emission (1963), direct proton radioactivity (1970), and beta-delayed two-proton emission (1983) have all been observed, while direct two-proton radioactivity is still being sought. This historical overview concludes in 1990.

INTRODUCTION

Though alpha-decay and its related beta-delayed alpha emission have been known for a relatively long time, it was not until the 1960s that decay processes resulting in the emission of a proton began to come under successful experimental observation.

In a series of seminal papers beginning in 1960, Goldanskii [1] described three phenomena involving proton emission: (a) beta-delayed proton emission, (b) proton radioactivity, and (c) his new concept of two-proton radioactivity. Then, in a further paper in 1980 [2], he predicted beta-delayed two-proton emission. What I will emphasize is the progress made in observing these decay modes through 1990, with a focus on the first experiment involving each decay mode. Since there are several other historical talks, I will only allude to the advances that other speakers will present in more detail. Interestingly, the first characterizations of nuclides whose decay process results in the emission of a proton (or protons) have all been observed in the lighter mass region (or are currently being sought there).

Although Karnaukhov and collaborators at the Dubna heavy ion cyclotron are credited with the discovery of beta-delayed proton emission in 1963 [3], from 130 MeV ^{20}Ne on nickel and tantalum targets, the first nuclide to be characterized as a beta-delayed proton emitter was ^{25}Si observed by Barton and collaborators at the McGill synchrocyclotron also in 1963 [4]. The influence of Goldanskii is clearly seen in the Dubna work and John Hardy told me that Professor Bell, leader of the McGill effort, had visited Russia about 1960 and had met Goldanskii. As a further historical note, Alvarez had reported in 1950 [5] attempts to find beta-delayed proton emission using the Berkeley Radiation Laboratory's 32 MeV proton linac, but instead discovered three new beta-delayed alpha-particle emitters, ^{8}B, ^{12}N, and ^{20}Na.

CP518, *Proton-Emitting Nuclei: PROCON '99*, edited by Jon C. Batchelder
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Figure 1 presents the lighter nuclei and the first nuclide to be characterized as decaying by a mode of proton emission. My discussion of beta-delayed proton emission will be relatively brief, since John Hardy will cover this topic later. Subsequently, I will discuss direct proton radioactivity, discovered in 1970 from an isomer in ^{53}Co, and the 1983 discovery of the first beta-delayed two-proton emitter ^{22}Al. Finally, direct two-proton radioactivity has yet to be observed, but I will conclude with the attempt by Détraz and collaborators at GANIL in 1990 to observe the decay of ^{39}Ti by this mode.

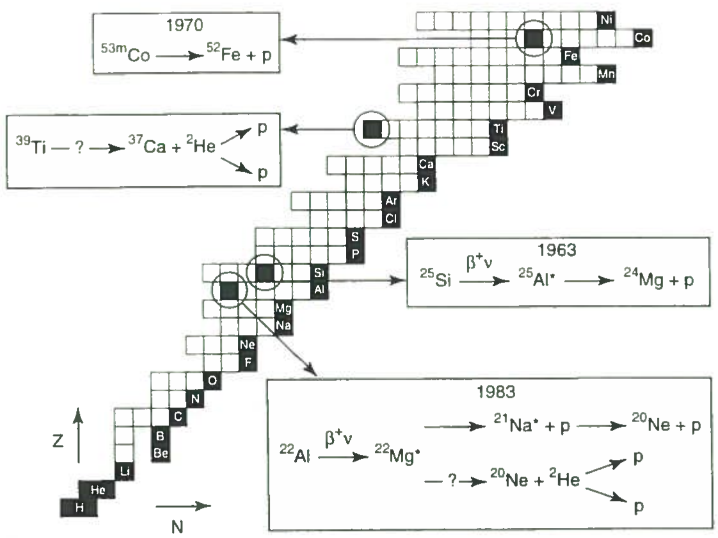

FIGURE 1. A partial chart of the nuclides showing the first nuclide characterized (or the best search) as decaying by a new mode of proton emission. See text.

DECAYS INVOLVING EMISSION OF ONE PROTON

Beta-Delayed Proton Emission

Figure 2 shows a decay scheme for ^{25}Si as a beta-delayed proton emitter. Of particular interest is the superallowed beta-decay to the isobaric analog state (IAS) in the daughter, which is followed by isospin-forbidden proton emission.

The nuclide ^{25}Si was produced at McGill with an external proton beam from their synchrocyclotron via the ^{27}Al(p,3n) reaction. Figure 3 presents the original ^{25}Si proton energy spectrum [4] measured with a single silicon detector which counted between beam bursts. The three observed proton groups were attributed to ^{25}Si by excitation function measurements and cross bombardments.

For comparison, Fig. 4 presents a recent beta-delayed proton spectrum from ^{25}Si produced in the ^{24}Mg(^{3}He,2n) reaction [6]. The region spanned by the McGill data is indicated, which includes one of the decay branches from the IAS. Figure 5 then

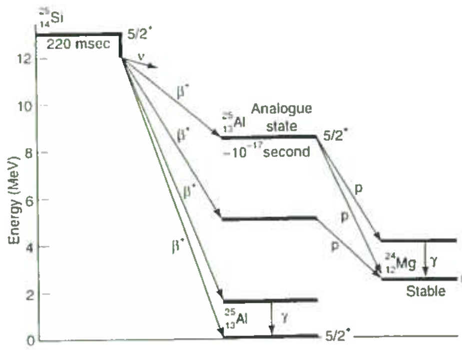

FIGURE 2. A nominal ^{25}Si decay scheme.

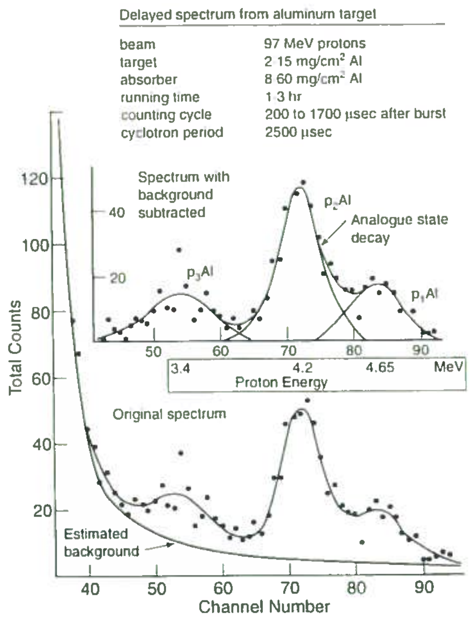

FIGURE 3. The McGill beta-delayed proton spectrum from ^{25}Si (right).

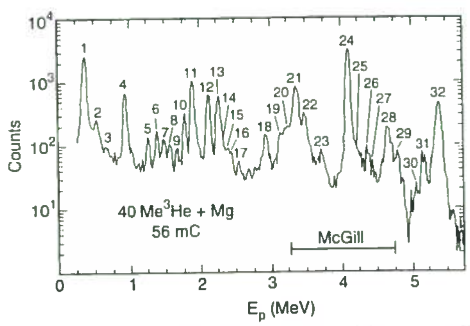

FIGURE 4. The beta-delayed proton spectrum of ^{25}Si obtained with helium-jet techniques and a gas ΔE-Si E detector telescope.

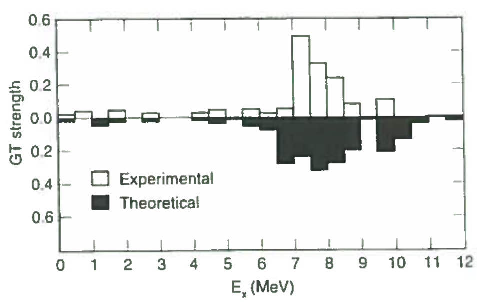

FIGURE 5. Gamow-Teller beta strengths as a function of excitation energy in ^{25}Al.

shows how such beta-delayed proton studies can be used to compare (successfully) experimental Gamow-Teller beta-decay strengths to predictions of large basis shell-model calculations [6].

Proton Radioactivity

I was on sabbatical at Oxford University during 1969–70 and was interested in utilizing both the tandem Van de Graaff at Oxford and the heavy ion cyclotron at Harwell to explore nuclei with $Z > N$ above the titanium isotopes, of which only a very few were known. In particular, the series of strong beta-delayed proton emitters with $A = 4n+1$ and $T_z = -3/2$ had been established through ^{41}Ti by (p,3n) reactions at McGill and (^{3}He,2n) reactions at Brookhaven. I wanted to try to use the (heavy ion, 3n) reaction at Harwell to extend this series.

The initial experiment at Harwell, the ^{40}Ca(^{12}C,3n) reaction with decay products observed in a ΔE-E silicon detector telescope, successfully observed ^{49}Fe [7] with a cross-section ~0.5 μb, a $\tau_{1/2}$ ~ 75 ms, and an $E_{c.m.}$ = 1.96 MeV β-delayed proton group. A subsequent experiment involved an attempt to produce ^{53}Ni via the ^{40}Ca(^{16}O,3n) reaction with an expected beta-delayed proton energy of 2.0 or 2.8 MeV. No proton groups near these energies were seen, but a proton group was observed near 1.5 MeV with a $\tau_{1/2}$ ~ 245 ms and about ten times the yield of the earlier reaction producing ^{49}Fe. Figure 6(a) shows the group in the detector telescope (and uncomfortably close to the telescope cut-off) while Fig. 6(b) again shows this proton group in a single silicon E detector. Measurement of the excitation function for the activity eliminated ^{53}Ni as a possible source. On analysis, the combination of proton energy, half-life and reaction threshold were completely inconsistent with the expected nuclear properties in this mass region.

Luckily, we had developed a setup at Oxford using βγ coincidences to search for other new proton-rich nuclei in the $f_{7/2}$ shell and this was utilized in the 49 MeV ^{16}O on ^{40}Ca reaction. A very high yield was observed for a recently-discovered isomer in ^{53}Fe—an isomer with $J^\pi = 19/2^-$, which was another example of a "spin-gap" isomer involving three nucleons just below or just beyond doubly closed shells. By applying mirror symmetry, with mass predictions and Coulomb displacement energy calculations, the prob-

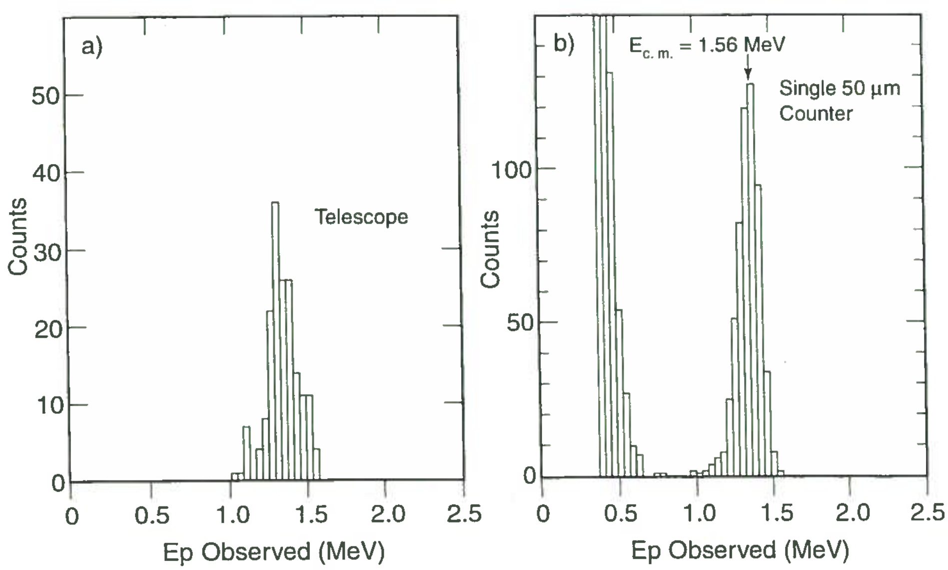

FIGURE 6. Proton energy spectra from the reaction of 80 MeV ^{16}O on ^{40}Ca: (a) using a detector telescope; (b) using a single E detector.

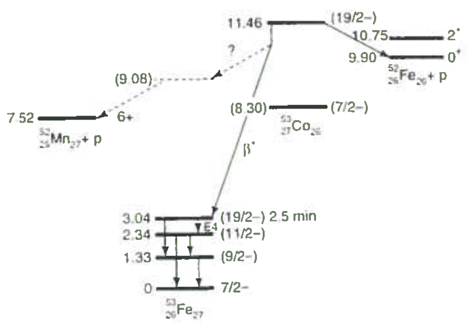

FIGURE 7. Probable decay scheme for ^{53m}Co, $\tau_{1/2} \sim 245$ ms.

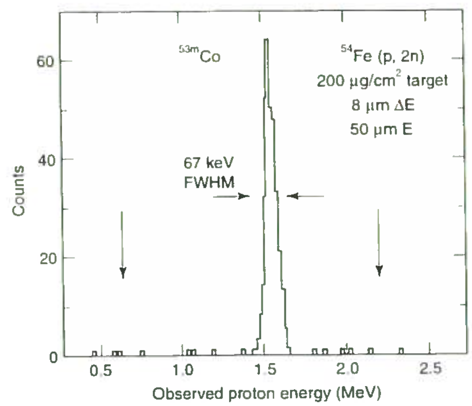

FIGURE 8. The proton decay of ^{53m}Co produced in the $^{54}Fe(p,2n)$ reaction.

able decay of the mirror isomer ^{53m}Co [8] is shown in Fig. 7. The energy systematics shown in this figure indicate that ^{53m}Co would be proton radioactive, emitting an $E_{c.m.}$ = 1.56 MeV proton. Since the 245 ms half-life was only consistent with dominant β-decay, the direct proton radioactivity must be only a weak branch.

Since my sabbatical was over and the Oxford group had broken up, we continued these experiments at the Lawrence Berkeley Laboratory 88-in. Cyclotron. Figure 8 shows ^{53m}Co now produced via the $^{54}Fe(p,2n)$ reaction (as well as a search for a proton decay branch to the ^{52}Fe first excited state, which has never been observed). Experiments at Berkeley also eliminated the possibility that this proton group might arise by beta-delayed proton decay as indicated in Fig. 7; these results confirmed ^{53m}Co to be a direct proton emitter [8].

Figure 9 shows the $^{54}Fe(p,2n)$ ^{53m}Co and $^{54}Fe(p,pn)$ ^{53m}Fe excitation functions—the threshold for the former is in excellent agreement with that required by the proposed decay scheme. Also shown are fits to these excitation functions using the spin-dependent nuclear evaporation code GROGI-2, which permitted us to estimate the direct proton branch to be 1.5%.

These data then lead to the final decay scheme for ^{53m}Co shown in Fig. 10, with a partial half-life for direct proton radioactivity of 17s [9]. Decay of this $(f_{7/2})^{-3}$ configuration, coupled to $19/2^-$, by direct proton transmission through the Coulomb and $\ell = 9$ centrifugal barriers to the ^{52}Fe ground state leads to an expected half-life of ~60 ns, so the ~17s partial half-life implies a reduced width $\gamma_p^2 \sim 4 \times 10^{-9}$. Calculations by Bugrov and collaborators [10] using a multiparticle theory for the proton decay of spherical and deformed nuclei are in reasonable agreement with this long partial half-life.

Proton radioactivity from the ^{151}Lu ground state was discovered by Hofmann and collaborators in 1981 [11] in experiments at GSI. Additional discoveries of proton emitters at GSI and at Munich by 1990 are listed in Table 1. These experiments will be discussed by the next two speakers, Drs. Hofmann and Faestermann. A comprehensive review of proton radioactivity results through December 1, 1992 by Hofmann appears in Ref. 12.

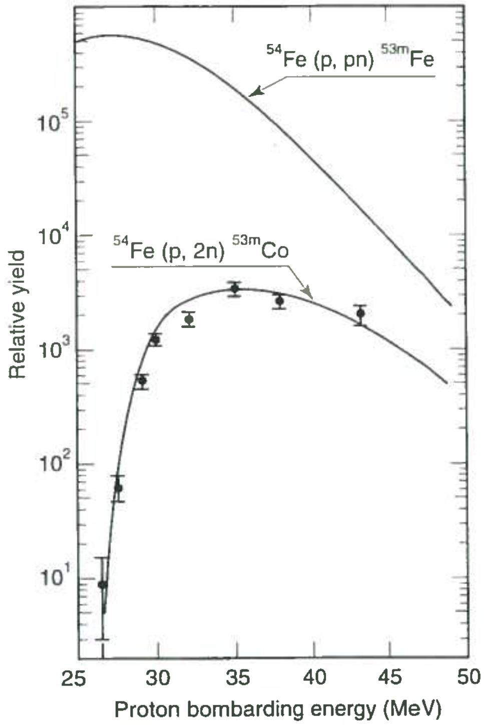

FIGURE 9. Excitation functions for p + ^{54}Fe. See text.

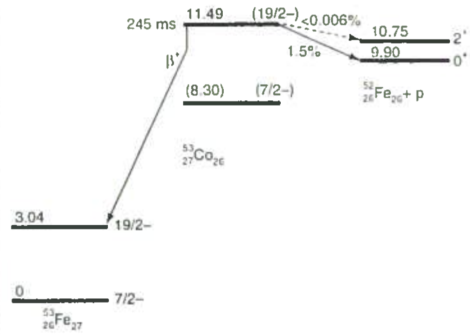

FIGURE 10. Decay scheme for ^{53m}Co.

TABLE 1. Discoveries of proton radioactivity by 1990.

Nuclide	Energy (keV)	Half-Life	Laboratory	Year
^{53m}Co	1590	245 ms	Oxford/LBL	1970
^{151}Lu	1231	85 ms	GSI	1981
^{147}Tm	1044	420 ms	GSI	1982
^{113}Cs	980	33 μs	Munich	1983
^{109}I	830	109 μs	Munich	1984
^{150}Lu	1261	>10 ms	GSI	1984
^{147m}Tm	1117	360 μs	GSI	1984

DECAYS INVOLVING EMISSION OF TWO PROTONS

Beta-Delayed Two-Proton Emission

By the early 1980s, our group had returned to an earlier interest—that of establishing the mass and/or the beta-delayed proton decay mode of the lightest bound member of each $T_z = -3/2$ or -2 mass series—and were characterizing ^{22}Al, $\tau_{1/2} \sim 70$ ms, as the first such member of the $A = 4n + 2$, $T_z = -2$ mass series [13]. With the timely prediction by Goldanskii [2] that nuclei such as ^{22}Al were also good candidates for a new decay process of beta-delayed two-proton emission, we decided to conduct a search for this decay mode.

The decay scheme for ^{22}Al shown in Fig. 11 indicates that the observed delayed proton decays to ^{21}Na from the ^{22}Mg isobaric analog state were of relatively high energy and that this IAS is unbound to two-proton emission to the ground state of ^{20}Ne by 6.1 MeV. Figure 12 shows the experimental setup used to observe this latter decay mode of ^{22}Al produced via the ^{24}Mg(^{3}He,p4n) reaction with 110 MeV ^{3}He beams from the 88-in. Cyclotron. A helium-jet system was used to transport the activity to a slowly rotating catcher wheel, so that ^{22}Al as a thin source could be observed by two high solid-angle detector telescopes. These three-element particle telescopes (with detectors denoted ΔE1, ΔE2, E) observed and identified decays involving two coincident protons and established ^{22}Al as the first known beta-delayed two-proton emitter [14].

Figure 13(a) shows the summed proton energy spectrum from the ^{22}Mg IAS to the ^{20}Ne ground state (g) and first excited state (x). Figure 13(c) shows the individual proton energy spectrum for protons forming group (g) in Fig. 13(a), while Fig. 13(b) shows the equivalent spectrum for protons forming group (x).

Clearly of much interest in the study of such two-proton emission processes is whether the decay is (a) sequential emission, (b) simultaneous but uncoupled emission, or (c) ^{2}He emission—decay through an $L = 0$ final-state interaction between the two protons [1,2]. Comparison of these small angle measurements (~45° separation of the two

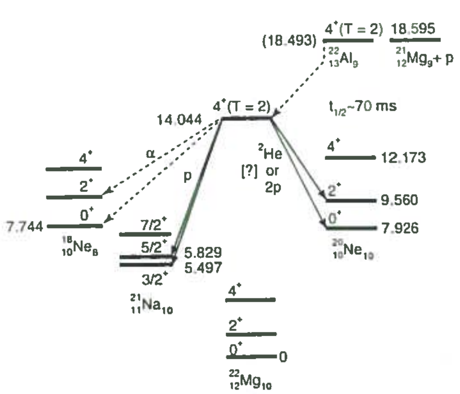

FIGURE 11. An initial decay scheme for ^{22}Al.

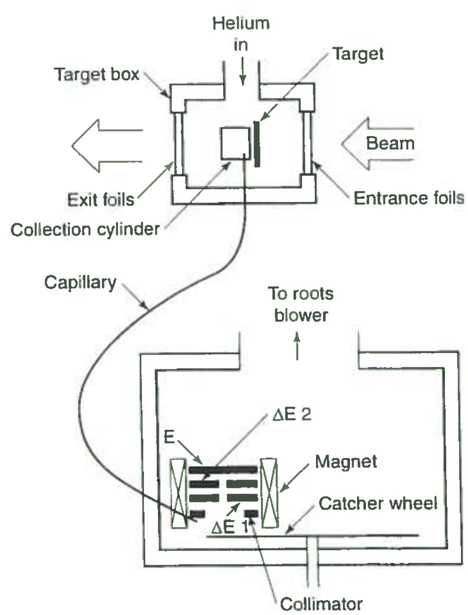

FIGURE 12. Schematic diagram of the experimental setup for observing two-proton decay at small relative angles.

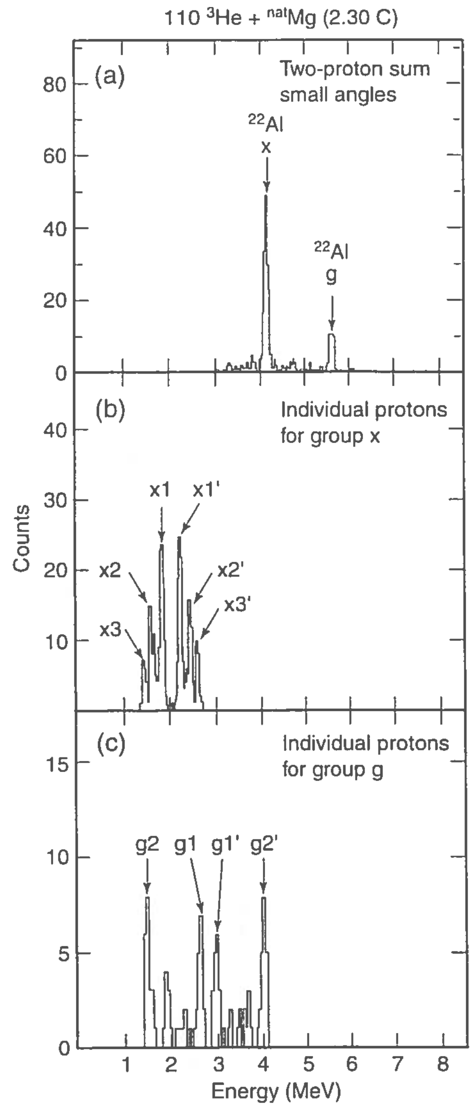

FIGURE 13. Proton-proton coincidence spectra obtained with the small angle detector system following the decay of ^{22}Al. See text.

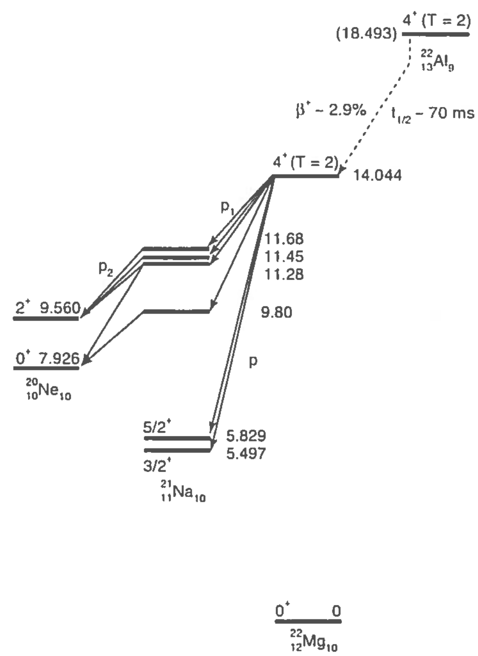

FIGURE 14. Partial decay scheme for the βp and β2p emission from ^{22}Al.

protons) to large angle measurements (~120° separation) established the ^{22}Al decay mechanism to be sequential. Figure 14 presents the details of this sequential decay [15].

Table 2 lists the discoveries of the known beta-delayed two-proton emitters by 1990 (for references to all but ^{31}Ar, see [16]). The nuclide ^{31}Ar was discovered and shown to be a beta-delayed two-proton emitter in [17]. So far, only predominantly sequential proton decay of these nuclides has been observed.

TABLE 2. Discoveries of beta-delayed two-proton emission by 1990.

Nuclide	Observed IAS 2p Decay Energy (MeV)	Half-Life	Laboratory	Year
$T_z = -2$				
^{22}Al	5.6	~70 ms	LBL	1983
^{26}P	4.9	~20 ms	LBL	1983
$T_z = -5/2$				
^{35}Ca	4.1	~50 ms	LBL	1985
^{31}Ar	7.5	~15 ms	GANIL LBL	1987 1989

The Search for Two-Proton Radioactivity

By 1990 (and even by the time of this conference) there has been no experimental observation of this new decay mode of two-proton radioactivity as defined by Goldanskii [1]. Also see the review in Ref. [18]. The most promising search for this decay mode was by Détraz et al. [19 and Refs. therein] in observing the decay of ^{39}Ti with the LISE spectrometer at GANIL. The nuclide ^{39}Ti was expected to have a two-proton decay energy ~750 keV.

Figure 15 is from the GANIL experiment and shows the beta-delayed proton spectrum obtained from 75 decays of ^{39}Ti; it shows primarily one peak at ~3.6 MeV. Three independent results in the experiment ruled out ^{39}Ti as a significant direct 2p emitter from its ground state: (a) its observed half-life, ~26 ms, agreed with that expected from beta-decay theory; (b) the energy spectrum in Fig. 15 shows no evidence for beta-delayed protons from ^{37}Ca, which would be produced as the daughter nucleus in this decay mode; and (c) a search for correlated low-energy protons (several hundred keV each) in a digital-storage oscilloscope triggered by the implantation of ^{39}Ti nuclei showed no events.

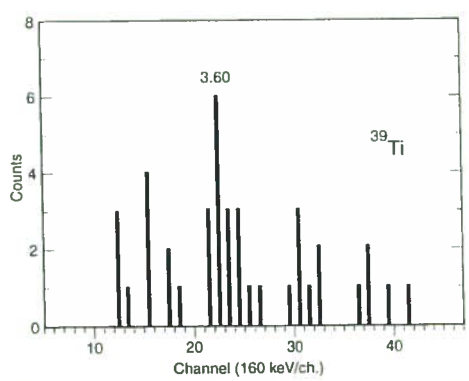

FIGURE 15. The energy spectrum of beta-delayed protons from ^{39}Ti.

When Détraz et al. [19] corrected for the Thomas-Ehrmann shift in this $T_z = -5/2$ nucleus, they found a significantly lowered two-proton decay energy of ~570 keV to be expected. A slightly later experiment [20] observed the beta-delayed two-proton decay of ^{39}Ti from the IAS and used Coulomb displacement energy calculations to estimate the mass of ^{39}Ti; results attributable to both experiments are shown in Fig. 16. The ground state of ^{39}Ti is found to be unbound to two-proton emission by 530 ± 65 keV (in agreement with the Thomas-Ehrmann shift calculations). Though a small direct two-proton decay branch from the ^{39}Ti ground state is not precluded by this lower mass, it is constrained to be less than 0.1%.

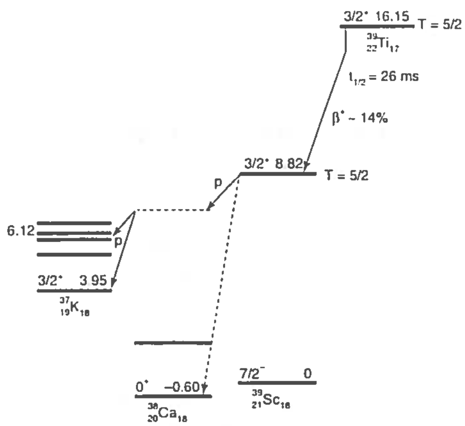

FIGURE 16. Proposed partial decay scheme for ^{39}Ti. The intermediate state in ^{38}Ca is not known.

CONCLUSIONS

This review has outlined the progress made in the observation of the emission of delayed protons from nuclei through 1990.

By the early 1990s, with the advent of double-sided silicon strip detectors, second generation recoil mass spectrometers, etc., the stage had been set for (a) a quintupling of known proton emitters; (b) a doubling of known beta-delayed two-proton emitters; and (c) a continuing search for direct ground-state two-proton radioactivity. Many of these newer results will be discussed throughout this conference.

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An East African Journal

Text and Photography by Keith Cerny

The author (right), his brother, Mark, and his father with their new African friends at their home in Kenya.

Keith (left) and Mark at Tsavo National Park, Kenya.

Lake Manyara, Tanzania.

A small African termite mound, Tanzania.

Last year at this time our family was preparing for an East African safari. As I watched the lions at the San Francisco zoo, the last of several Bay Area places we visited trying to obtain a feel for East Africa, I closed my eyes and tried to imagine these animals on the grassy plains. We also visited the African artifacts at the de Young Museum, the Simpson and the African Hall and the Origins of Man exhibit at the Academy of Sciences, and the African animals at Marine World.

Of these four, my favorite was the Academy of Sciences with its beautiful dioramas. These are stuffed animals displayed with vegetation and scenery showing the terrain in which the animal lives. After our safari, when I revisited these dioramas, I was struck with how accurately the scenes were done. I could almost imagine myself staring out of our camper once again.

To me, animals in zoos always seemed unhappy with too little space. Yet, when I did see vast herds of elephants, gnu and zebra, it seemed unreal. Try to extrapolate from the few flamingos at the zoo to the flocks of thousands we saw in Africa whose discarded feathers covered the earth with a pink blanket. And then imagine a hippo emerging from the river and lumbering through the feathers, scattering them in all directions!

My father, a scientist on leave from UC, would be visiting physicists in Europe and India. In between, our family — my parents, my ten year old brother and I (just turned twelve) — could vacation in East Africa. I was so intrigued with the idea that I kept a diary. What follows are excerpts from it.

February 1. We arrived in Nairobi to equip ourselves for the safari. Our rented camper contained a sink, a stove, a table and room for four people to sleep. I was so excited I wanted to leave immediately.

In the afternoon we visited the Nairobi museum. The most impressive exhibit was the remains of early man. It seems strange to see bones at least three million years old. Even now, with technology and medicine, we seldom live over a century. The number of generations that span these two ages must be incredible.

The museum also contains an excellent collection of East African tribal artifacts. Here, as in many of the exhibits at the de Young, the emphasis is upon the spiritual world. The actual art differs from area to area but is similar with its functional, religious and political purposes. Face masks, costumes, and objects charged with magic show how important the spirits were in controlling day to day life.

We spent the twilight hours in the Nairobi Game Park. We did not see many animals but our short excursion outside the city began to give me a feeling for the vastness of the African plains.

February 3. Today as we left Nairobi I knew I was going to like our safari. We headed to Machakos, a large town approximately forty miles outside Nairobi, and then to Wamanyu, the traditional center of African carving and drum making. The journey took us down some incredibly dusty dirt tracks, but it was worth it.

Wamanyu existed long before the British came to Kenya. The carving trade is passed on from the parents to the children; people also come from other parts of Kenya to work here. The carvers sit under small bamboo and straw huts which keep the scorching sun off the workers. The work itself is done with a crude pick and a saw, yet it is so fine it needs very little sanding. It was fascinating to watch the animals emerge from the wood.

February 4. In the space of a few hours' drive we left the green hilly countryside and arrived in the volcanic bleakness of Tsavo National Game Reserve. The horizon is dominated by Mount Kilimanjaro, whose 19,000 foot peak is perpetually covered by snow even though it is on the equator and the temperature on the plain often exceeds one hundred degrees.

Yet even here in this seemingly unchanging environment, variation occurs. At Mzima Springs, collected rainwater flows out of the ground allowing trees and large plants to grow. This attracts animals looking for shade and water. The vegetation extends only several hundred yards from the river but in this distance we saw a surprising number of animals. The pool formed by the springs could more appropriately be called the Hippo Tank. We watched several hippos fighting, but they soon became exhausted and submerged into the quiet depths of the deep pool. It seems ironic that such large animals have skin so delicate that they must remain in the water all day, venturing out only in the evening to look for food.

Later we visited Poacher's Lookout to see the varying terrain rather than to spot animals for a future kill and then retired to the lodge to watch the animals in an artificially created waterhole, built to attract both animals and tourists. Feeling satiated with our day's activities, we went to the campground, which turned out to be a cleared area in the immense lava field. The cinder cones in the distance and

"*When I finally saw vast herds of elephants, gnu and zebra, it seemed unreal.*"

porous volcanic ash made Tsavo look very much like Lava Beds National Park in California. This was a luxurious campsite because it had a shower (the only one we found on the whole safari). In our more fastidious days we might have objected to the monkey feces on the floor, but we were so very hot we were thankful for the cold water, under any circumstances.

The next morning we saw several baboons drinking from the overflowing water tower that served the campsite. When I moved closer in an unsuccessful attempt to get a picture, they immediately climbed down and hid a short distance away. I felt sure they had seen humans before but they were extremely wary and uneasy, unlike almost all the plains animals. The zebra and gnu stay in immense herds, completely unconcerned with cars. They move only when one is several feet from hitting them; often a whole herd will stand on a road jumping off it two at a time.

February 6. From Tsavo we drove along the unpaved road to the Amboselli Game Park. This 100 mile trip took us five hours. As we left Tsavo the terrain became less volcanic and eventually the lava disappeared completely. However, at Amboselli we found the plains equally bleak and barren as Tsavo.

The only break in the desolate expanse is the river. This water gives life to plants and animals as well as to the Masai tribe, who still live as they have for centuries. The Kenyan government created Amboselli in an attempt to preserve their tribal heritage; inside the park one cannot interact with them in any way. However, outside of it the government has no control and the Masai constantly try to get you to take their pictures and buy their artifacts made for the tourist trade. They wear long red robes and decorate their short curly hair with ochre. The women often wear necklaces of small brightly colored beads similar to those in some of the displays at the African section of the de Young.

The Masai are a nomadic tribe widely spread throughout Kenya. Their diet is the meat, milk and blood of their cows. The herds vary in size from a few to several hundred animals, depending on the wealth of the owner. When the missionaries came to Africa they brought money to the Masai tribes. This was a completely new idea to them and they could not understand why people would want to use money, for unlike cows, it would not grow or reproduce.

In the afternoon we hired a guide to take us to see some lions. He directed us to a tree where six lionesses were resting, hiding from the heat of the sun. A large number of zebra and gnu walked quite unconcernedly one hundred feet from these natural enemies. To catch these animals the lions must trap them so they cannot escape, so the zebra and gnu stand out in the blazing heat for safety.

We then went to a very dusty campsite, which, if nothing else, was certainly "back to nature." We seemed to be camped in some giant animals' latrine, for there were a large number of dung balls fifteen inches in diameter. We hoped we wouldn't meet whatever had left them.

February 7. We left Amboselli and started driving along the red road towards Marangu, at the base of Kilimanjaro. To get there one had to enter Tanzania, the second of the three East African states. (The third is Uganda.)

When we arrived at the Tanzanian border we learned that we were the only car that had passed through this day; the day before there had been only three. After we filled out the immigration forms we asked about changing money. The immigration officer closed the door and took off his offical hat.

"Dollars?" he asked.

We replied, "Travelers cheques."

"Sorry," and he put his official hat on and opened the door.

Next a large "Diversion" sign pointed to a track to the left of the main road. This was the only time the direction towards Marangu was marked, so at every one of the numerous side roads we had to stop and ask directions. Five hours after we passed the sign we arrived at the Marangu Hotel. The fifty mile diversion covered perhaps ten miles along the main road.

February 8. Marangu is a large town by African standards. It has two stores, a gas station and a hotel which is a base camp for climbing expeditions up Kilimanjaro. We had planned to spend another day at the campsite here but we needed to buy more food for the next leg of our trip. The stores in Marangu sold medicine, eggs, china and milk, but no canned food. Because it was Saturday we had to drive

continued

Travel continued

to Moshi, the second largest town in Northern Tanzania, where we managed to buy a few dented cans of peas and carrots and some canned fruit which had been packed in China.

Then we started driving towards Lake Manyara. Here we found a $10 camping fee for nothing but water and a toilet — which consisted of four sides around a hole in the ground with lizards living in it.

The campsite was a cleared area in a thick forest and the insects in and around the trees emitted a loud steady hum. We could see monkeys and many colorful birds in the near proximity of the campsite and wondered what was hiding in the thick vegetation. But by far the most spectacular feature was a twenty foot high termite mound. It was as close to a rain forest as we found in East Africa, but I was slightly disappointed. I felt that we were just beginning to get into the Africa I had envisioned. I had read Alan Moorehead's book *The White Nile*, and seeing this relatively thin jungle made me appreciate his descriptions of the explorers in Central Africa who had to hack their way through the impenetrable rain forests.

February 9. In the early morning we explored the park. Lake Manyara has tremendous variation contained within it, including an alkaline lake, a forest, and a plain within several miles of each other.

The forest is fascinating. Its thickness gives a sense of mystery which is not found on the plains and this, to me, makes the animals much more interesting. Almost a line separates the lush forest from the alkaline lake shore with its bleak color and dead trees; in another direction the plain extends to the horizon. Between the forest and the plain are trees whose branches provide a home for lions. Lake Manyara is the only place in the world where lions drape themselves over trees as if they were pretending to be leopards. Unfortunately we didn't find any.

We left the park and began a steep ascension along the bare rocky rift. From the top we could see for miles. The distances in East Africa are immense, and we could especially appreciate this from the top of the rift.

The barren surroundings stayed with us for several hours, until we entered the forest on the final stretch of twisting road to the rim of Ngorongoro Crater Game Park. We had zebra steak for dinner, which was a special treat. So many animals live at the bottom of the crater that the hotel is allowed to shoot a few. I was disappointed that it tasted so much like beef, due to the way it was cured.

February 10. We rented a four-wheel drive with a driver and descended several thousand feet to the bottom of the crater along an unbelievably steep road. The game on the floor is incredible. Great herds of animals congregated around the numerous water holes. I wondered what kept the animal population down, since the only predators we saw were a few lions and hyenas.

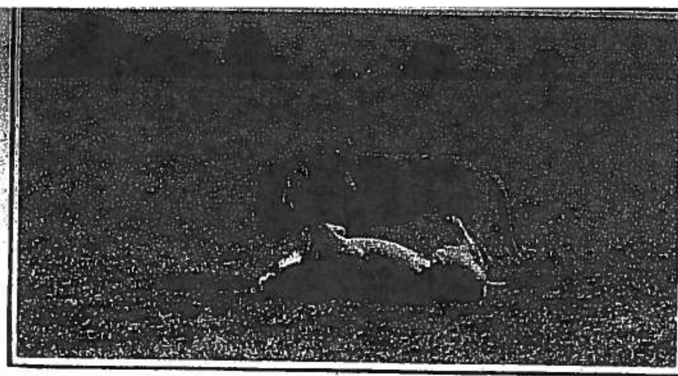
The animals in the crater are surprisingly tame. We found a pair of lions mating out in the open and they moved under the shade of our camper when they saw us stop there to watch them. A curious monkey with a baby clinging to her stomach came up to us. We threw her a banana which she carefully peeled and ate, skin and all.

We saw a newborn zebra with its mother. The umbilical cord was still attached and many vultures, drawn by the smell of the afterbirth, nipped at its legs. If the zebra had not been completely healthy it would have been eaten by the vultures. It's a hard life out on the plains.

We continued up the steep road to the rim and returned to our camper. The only gas station was out of gas, but we thought we could barely make it to Serengeti. So once again we were bouncing along not knowing what adventure awaited us down the hot, dirty, corrugated road. As we coasted down the rift to save gas, I noticed that the land became drier as the altitude decreased. At the base of the rift it is almost desert; the only plants that grow are grasses. The area is completely uninhabited, due to the Tsetse fly, yet animals not affected by it live here in the thousands. There were as many antelope and gazelle in this desert as there would be wildflowers on a grassy hill.

February 11. It was raining in Serengeti so animal watching was very poor. We did, however, see a leopard. By Ngorongoro Crater we had seen four of the "big five," a b ffalo, a rhino, an elephant and a lion. Here we finally saw a leopard. Being nocturnal it was sleeping and its body was draped over a tree ninety yards away. I was glad I had seen leopards at the San Francisco zoo, for even with binoculars he was hard to spot.

At the Serengeti campsite, the lightning through the intermittent rain lit the plain with an eerie glow, bright enough to illuminate the horizon for the brief seconds it lasted. Its intensity made us very uneasy. This "campsite" was a wide open waterless area miles from anywhere. The deso-



late plain, the ominous storm and the screams of the animals made it a very frightening night.

February 14. As we approached the Aberdare Mountains the terrain became very rocky, but as we started to climb the familiar lush vegetation reappeared. Higher yet these plants were slowly replaced by bamboo which at the equator needs a very high altitude to grow. The bamboo thinned as we neared the peaks (13,000 feet), but then a forest replaced it. There was always some type of vegetation and this surprised me, for in the Sierra 13,000 feet is above the timberline and all that can grow are very small shrubs.

Later, as we drove along the thick groves of trees, I spotted a group of white and black Colobus monkeys with long furry tails. I barely recognized them as the animals I had seen in the zoo because, being in their natural habitat, they hid as soon as they caught sight of us. A short distance further we saw an unusual antelope which turned out to be the rare Bongo. I remembered the Bongo diorama in the Simpson African Hall and how well it caught the feeling for this beautiful scenery.

This night was our favorite camping spot, sleeping by the side of the main road where it cut through an immense forest of bamboo plants. From a distance the tall green plants looked like a giant grass. There were no cars or people, the animals kept their distance and a cool wind whistled gently through the bamboo as we watched the sun set behind the mountains.

February 16. Our safari ended where it began — in Nairobi — but I felt very glad that I had seen East Africa. Any films or books about this part of Africa would be much more meaningful. But I also felt slightly disappointed. The sense of wonder that had existed in my imagination and my unrealistic expectations were now replaced with less impressive real events. However, being an incurable optimist, I felt sure that if I traveled throughout other parts of Africa there would be places beyond my greatest dreams. Even the name still breathes mystery and adventure.□

B. II.

ACADEMIC
SENATE
BERKELEY

320 STEPHENS HALL
UNIVERSITY OF CALIFORNIA

November 6, 2000

PROFESSOR JOSEPH CERNY

The Academic Senate wishes to express its gratitude to you on the occasion of your stepping down after having served for 15 years as Dean of the UC Berkeley Graduate Division, and to record its admiration for your accomplishments. We salute your devotion to the cause of graduate education at UC Berkeley, your personal concern for student welfare, your sense of fairness, your commitment to research as a tool to enhance the quality and effectiveness of the graduate experience, and your energy in promoting academic excellence at UC Berkeley.

With thanks and warm wishes,

Christina Maslach
Chair, Academic Senate

Michael Hanemann
Chair, Graduate Council

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B.III. #1

FROM FACTS TO ACTION: EXPANDING THE EDUCATIONAL ROLE OF THE GRADUATE DIVISION

by

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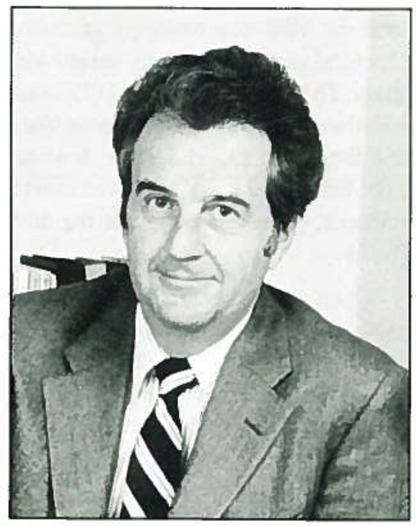
Institutional Research
University of California at Berkeley

Joseph Cerny
Dean, Graduate Division
and Provost for Research
University of California at Berkeley

At the 1990 annual meeting of CGS, Maresi Nerad discussed much of the work described in this paper. Her presentation generated so much interest that we decided to publish this paper as a special issue of the Communicator. This work provides an excellent example of how a graduate school can play an active role in improving graduate education.

Maresi Nerad

Mounting concern over the anticipated shortage of college teachers, scientists, and engineers, and the societal need to diversify faculty has made time-to-doctoral-degree and doctoral completion major issues for graduate deans, funding agencies, and government officials. As a result, for the last several years the Graduate Division at the University of California at Berkeley has studied doctoral completion times and rates. Breaking out of its traditional administrative role, the Graduate Division undertook research and used its findings to design and implement programs that encourage students to complete their degrees and to do so in a reasonable amount of time.



Joseph Cerny

Our Berkeley study proceeded in five steps: first, we developed a number of statistical analyses based on demographic data on our graduate students to determine the average time-to-degree in our many Ph.D. programs, the completion rates, and the points in these programs at which students tended to leave without completing their doctoral degrees. Second, the Berkeley data were compared with national trends and similar analyses at comparable institutions. Third, we interviewed and surveyed students in an attempt to find the reasons for long time-to-degree and low completion rates in certain disciplines. Fourth, by combining the knowledge accumulated from quantitative and qualitative findings, a conceptual model was developed to determine the conditions under which students completed their degrees in good time and with a low rate of attrition. Fifth, this model was used as the basis for developing policies and making recommendations to the Graduate Council of the Berkeley Academic Senate, faculty, graduate students, and graduate assistants/secretaries, on ways to shorten time-to-degree and increase completion rates.

Time-to-Doctoral-Degree and Retention

How long do graduate students take to complete doctoral programs? How many students actually receive doctoral degrees, and how many fail to complete the doctorate? These were the questions pursued in the first step of our study.

Time to Doctoral Degree

The average time-to-degree from entrance to Berkeley for all our doctoral recipients between July 1980 and June 1987

(4,949) was 6.9 years.[1] This period included the time spent earning a master's degree, if it was required for the Ph.D. The time during which students were not registered and were perhaps away from the campus was also included in the total time-to-degree.

As expected, we found that time-to-degree varied widely by field of study.[2] The most substantial differences in mean time-to-doctoral-degree occurred between students in engineering (5.5 years) and the natural sciences (6.0-6.2 years), and students in the social sciences (8.4 years), arts (8.6 years), and languages and literature (8.9 years), not between minority and non-minority students or between men and women. Foreign students across disciplines completed their degrees more quickly than domestic students.

Completion Rates

Our most recent analysis of completion rates utilizes the cohorts entering in 1978 and 1979 as measured in November 1989. Most of the students, by then, should have had sufficient time to complete their doctoral programs. Only students who identified themselves as working toward the doctoral degree were included. Fifty-eight percent of the students in the 1978-79 cohorts completed doctoral degrees. Another 20% changed their plans, earned master's degrees, and left graduate school, so that a total of 78% completed a graduate degree of some kind. Doctoral completion rates varied markedly by major field of study. The biological sciences (72%) and physical sciences (69%) had the highest completion rates; the arts (39%) and languages and literatures (37%), had the lowest. Percentages in the other four fields were: for the professional schools, (48%); social sciences, (49%); and engineering and natural resources, each (65%).

When analyzing data by sex, race and ethnicity, one has to be aware that women and non-Asian minority students are more heavily represented in fields where long time-to-degree and low completion rates are the norm—the professional schools, social sciences, and humanities (see Figure 1 for men/women comparison).[3] When women as a group are compared to men as a group, the completion rates by major fields differ significantly between the compared groups (Figure 2). Overall women had a completion rate of 47% while the rate for men was 63%. More research is in progress to analyze the differences that may be attributable to sex and ethnicity.

Time of Attrition

Contrary to popular belief, the majority of the graduate students who failed to earn their doctorates left the program before advancement to candidacy for the Ph.D., not after. Although 24% of the students in the 1978-79 cohorts left during their first three years of graduate study, most of these students (83%) earned master's degrees. An additional 10% left after advancement to candidacy, and another 8% were pending at the time we analyzed the data.

Comparison with Other Universities

Is this situation unique to Berkeley? In our second step we compared our current sample over time and with other universities. The National Research Council data show that during the last 20 years time-to-degree, both at Berkeley and nationally, appears to be increasing. We asked the University of Michigan at Ann Arbor to work with us on a comparison of our doctoral time-to-degree and completion rates. In this case using 1975-77 cohorts, both Berkeley and Michigan found that slightly more than half of all their doctoral students completed their degrees in a period of seven years. Ellen Benkin's study (1984) at the University of California at Los Angeles showed that about 30% of UCLA students leave during the early period of the doctoral studies.[4] These comparisons showed that Berkeley was not atypical, at least among public universities.

Reasons for Lengthy Time to Degree

Third we asked, why do some students leave doctoral programs? Why do some take longer than seems appropriate? To answer these questions, we began qualitative research, mainly in-depth interviews.

Initially 40 UC Berkeley students from history, English, French, and sociology were interviewed.[5] These departments were chosen because our analysis had shown that these were departments in which students historically took a long time and had low completion rates. For comparison students from psychology and biochemistry were interviewed. All these students had nearly completed their dissertations or had just filed their theses. About half of the students took at least one year longer than the average departmental time-to-degree; the other half completed in average time.

During the interviews the students were "walked through" the five major stages of the doctoral program: (1) course work; (2) preparation for the oral qualifying exam; (3) finding a dissertation topic, selecting a dissertation adviser, and writing a prospectus; (4) the actual dissertation research and writing; and (5) applying for professional employment. These students were asked how they moved from one stage to the next, what financial and moral support they had, what would have helped them at each stage, and whether they had recommendations for what the university could do to help students finish more quickly.

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[1] The data used in this study were produced by the staff of the Information and Technology unit of the Berkeley Graduate Division: Betty Liu, Bob Tidd, and Dennis Anderson, under the direction of Judi Sui.

[2] At Berkeley Ph.D. programs are grouped into eight major fields of study: arts, biological sciences, engineering, languages and literature, natural resources, physical sciences, professional schools, and social sciences. No law (J.D.) data are included.

[3] Statistically meaningful data on minorities are not available at this point.

[4] Ellen Benkin, "Where Have All the Doctoral Students Gone: A Study of Doctoral Attrition at UCLA," Doctoral Dissertation, UCLA, 1984.

[5] Each of these students was interviewed individually for one and one-half hours.

Figure 1

DISTRIBUTION OF DOCTORAL STUDENTS
BY 8 MAJOR FIELDS OF STUDY
FOR DEGREES AWARDED 1980 TO 1987

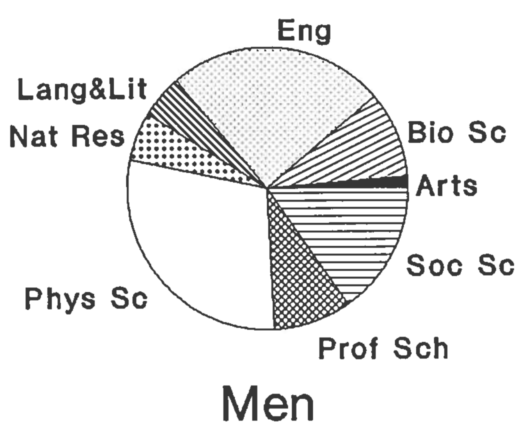
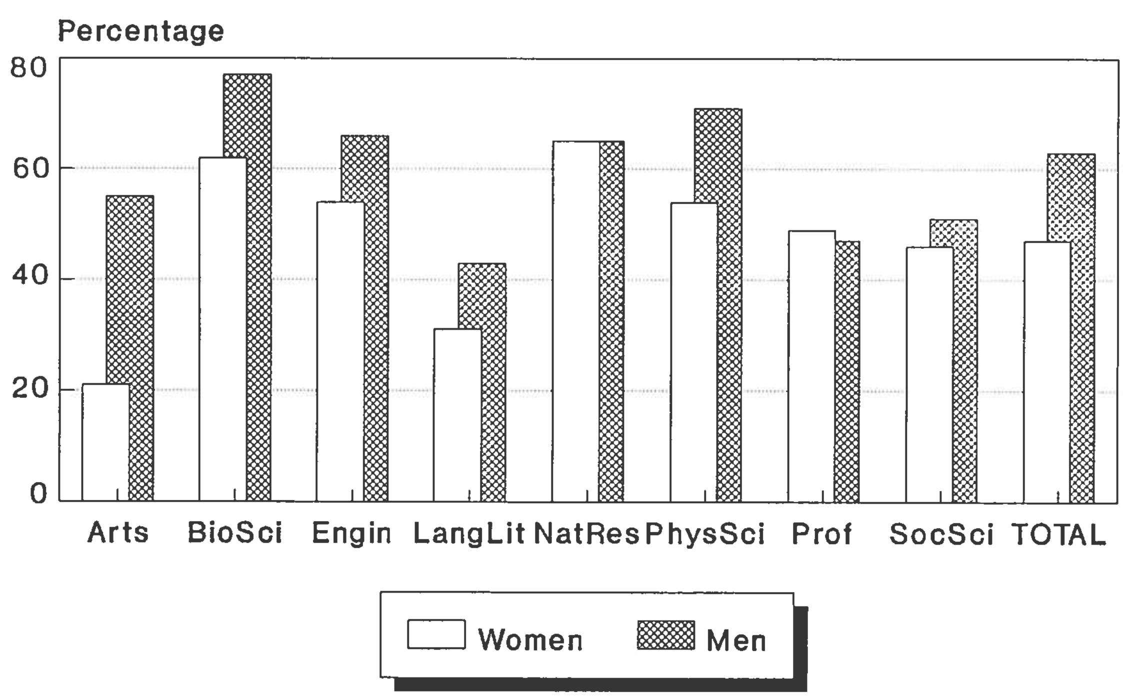


Figure 2

DOCTORAL COMPLETION RATES
1978-79 COHORT, MEN / WOMEN



Results

We found six major patterns for humanities and social sciences students with long time-to-degree.[6]

(1) Students in departments that require an M.A. thesis spent an excessive amount of time polishing their master's theses. They seemed to receive mixed messages from the faculty. They were told to do a simple thesis, and simultaneously they were told to choose a topic with potential so they could get their feet wet in "real research." Under pressure to produce, they hoped to increase their chances for receiving a fellowship by writing an impressive master's thesis.

(2) Students in the humanities and social sciences over-prepared for their orals. They spent, on the average, from six months to one year studying, usually in isolation and withdrawn from the department. Their ideas about the structure, scope, and standards of the exam were vague. The aura of mystery which surrounded the exam seemed to lead to a self-imposed perfectionism. They were disappointed, after taking the exam, to discover that they were given the opportunity to demonstrate only a small part of their knowledge.

(3) After having passed the qualifying exam, these students spent between one and two years searching for a dissertation topic and writing a dissertation prospectus. They did not have information about the prospectus format; they knew only how long it should be. They had difficutly deciding which topics were feasible and which goals were achievable within a certain time limit.

(4) Students in the humanities and social sciences wrote their dissertations in total isolation. They felt lost in the transition from being what they called "a class-taking person" to a "book-writing person." During this period they completely withdrew from departmental activities. Most said, "No one on the faculty knows about my topic, so why should I meet with them." During the actual writing period they found it very difficult to work as a teaching assistant or at another unrelated on-or off-campus job.

(5) These students perceived the course work, orals, and prospectus-writing stages of their doctoral studies as *hurdles* that needed to be jumped, but not as steps leading to the completion of their dissertations. The curriculum structure was not seen as an opportunity to develop an overview of one's field and to decide on a research project. As a result, the students focused too narrowly on getting the course requirements out of the way or on taking the orals for orals' sake. After passing their orals, they had trouble mustering enough energy to write a dissertation proposal. In fact, they felt that they were starting from scratch as they approached writing a proposal. Once their proposal was approved, often they realized that they needed to take more courses in the area of their dissertation topic.[7]

(6) Many students in the humanities and social sciences complained that the department and faculty failed to assist them in preparing for orals, in developing a dissertation prospectus, in applying for grants, and in the actual writing of their dissertations. Further, students in those fields found that after advancement to candidacy it was very difficult to find work; and if they were employed as teaching or research assistants, they found it distracting to be doing work unrelated to their theses.

Working Model of Factors Determining Time-to-Degree and Attrition

After gaining insight into some of the reasons why students in the humanities and social sciences took a long time, we asked why students left the doctoral program before completing the degree. As expected, substantial numbers of students left for both personal and institutional reasons.

Personal Reasons

Frequently students who left graduate school after one or two years reported that their expectations about the general field of study, graduate student life, or the focus of the program were not met. Students, particularly in the professional schools and engineering who already had master's degrees, rethought their career goals and chose to leave, often after the first year. These students could return to well-paying jobs as an alternative to graduate school.

Institutional Reasons

By pulling together what we had learned from our qualitative data, the following nine-point model was developed to interpret the conditions, other than personal ones, that contribute to long or short time-to-degree or to high or low attrition rates (Figure 3).[8] (1) research mode; (2) program structure; (3) definition of the dissertation; (4) departmental advising; (5) departmental environment; (6) availability of research money; (7) financial support; (8) campus facilities; and (9) the job market. With the help of this model, the Graduate Division could begin to determine where we could recommend or implement programs to assist doctoral students.

(1) The **research mode** is a field-specific factor. Between the sciences and the humanities there are pronounced differences in the way research is conducted. Graduate students in the sciences and engineering acquire research skills through an apprenticeship mode of research instruction and team work in a laboratory setting where they benefit from frequent social interactions. The laboratory research and the dissertation work often coincide and frequently are supported by a research assistantship under the direction of a faculty investigator. The arts, humanities, social sciences, and professional schools do not have the same structure for involving students as active participants in the research process. In addition, these fields have

[6] A study conducted by M. Nerad (1990), "Graduate Education at the University of California and Factors Affecting Time-to-Degree," for the nine UC campuses revealed similar patterns for students in the humanities and social sciences who took a long time to complete their degree programs.

[7] These were students who took longer than the average.

[8] This model shows the extreme ends of a continuum. Most departments are located somewhere in the middle.

Figure 3

FACTORS DETERMINING TIME TO DEGREE AND ATTRITION

Institutional and Field-Specific Factors

1	**Research Mode**	Apprenticeship Mode Team Work Laboratory	Individualistic Learning Solitariness Library
2	**Structure of Program**	No M.A. / M.S. required QE includes Dissertation Prospectus Annual Evaluation	M.A. / M.S. required QE does not include Dissertation Prospectus Sporadic Evaluations
3	**Dissertation Definition**	Test of Future Ability to do Research	Major Contribution to Knowledge (Book)
4	**Advising**	Faculty Mentoring Departmental Advising	Absence of Faculty Mentoring and Dept. Advising
5	**Departmental Climate**	Sense of Community Students treated as Junior Colleagues	Factions among Faculty Students treated as Adolescents
6	**Research Money**	Many Sources	Few Sources
7	**Type of Financial Support**	Research Assistantship Fellowships	Teaching Assistantship Loans Own Earnings
8	**Campus Facilities** Housing Child-care Space (Office, Meeting) Transportation Library	 Affordable Available Available Efficient, Affordable Long Hours, Year round	 Expensive Overcrowded Overcrowded Slow, Expensive Short Summer Hours
9	**Job Market** Post-doc Academic Industry	Many Openings Well-paid	Few Openings Medium or Low Salaries
		= SHORT TIME LOW ATTRITION	**= LONG TIME HIGH ATTRITION**

few resources to pay for research assistants. Even though the research mode plays an important role in students' staying in doctoral programs and completing them in a timely manner, this factor is not one that can be altered by administrative intervention.

(2) In interviews with students, the **program structure** emerged as a strong determinant. If a master's degree is required in the course of receiving a doctorate, time-to-degree is affected. To investigate this point further, we used the data gathered by the National Research Council (NRC) in its annual survey of earned doctorates. These data were rearranged into the following three groups: (a) students who did not receive the master's; (b) students who received the master's and doctorate at the same institution; and (c) students who received master's degrees at an institution other than the doctoral-granting institution. The findings are not surprising (Figure 4).[9] Students with no master's degree take the shortest time (6.0 years); students with the master's from another institution take the longest time (9.8 years), since the doctoral-granting institution rarely accepts a substantial portion of the prior course work in lieu of its own program. Also students who come with a master's degree from elsewhere will often take more courses voluntarily in order to become familiar with the faculty. Students with master's degrees from the same institution complete the program in less time than those who come with master's degrees from another institution, but take longer (7.4 years) than those with no master's degree. Seventy percent of all UC students acquired master's degrees before the doctorate, half of them (35%) at the same campus as that from which they received the doctorate, and the other half (35%) from a different institution.

A Berkeley survey found that programs requiring a dissertation prospectus as part of the qualifying examination tended to have shorter time-to-degree.[10] Programs with a structure that called for an early start to dissertation research tended to have shorter degree times. Programs that evaluated the progress of their students annually and suggested improvements seemed to inspire student confidence about completing the degree. Students appreciated especially the regular progress meetings with dissertation committees after advancement to candidacy; such students appeared to "drift" less. Students also favored the custom of a dissertation defense (though this practice is no longer common at Berkeley).

(3) The **definition of the dissertation.** Another factor affecting time-to-degree is whether the dissertation is perceived primarily as a test of future ability to do research or whether it should be a book.[11] Science and engineering programs generally seem to perceive the dissertation as a test of future ability to do research; humanities and social sciences programs often expect the dissertation to be a major contribution to the field.

(4) **Advising.** The concept of advising is broad, and we have broken it into two components: (a) advising and mentoring by the dissertation director and (b) advising and guidance by the *department* independent of the individual dissertation adviser. Where department advising activities exist, there is some guarantee that "things just do not happen accidentally or never at all," and that students tend to receive more direction and drift less frequently.

In addition to the information gathered from the student interviews, further insight into student satisfaction with advising came from the Graduate Division's exit questionnaire, which is "required" at the time of filing the dissertation.[12] One question is, *How satisfied have you been with departmental advising?* The results from 1,200 students who completed their dissertations between fall 1987 and fall 1988 show that about half of all students were satisfied, one quarter were very satisfied, and one quarter were dissatisfied (Figure 5).[13] The level of satisfaction varied by major fields. Students in the social sciences and humanities were the least satisfied, and those in the physical sciences, engineering, and natural resources were the most satisfied. Proportionally more women than men were *dissatisfied.*[14]

Another relevant question asked was, *How satisfied have you been with the professional relationship with your dissertation adviser?* (Figure 6). Here the overall satisfaction level was considerably higher, more than 92%. Interestingly, students in the social sciences were more satisfied with their individual advisers than were students in the biological sciences, and those in the humanities were the most satisfied. Again, women were more dissatisfied than men. From the interviews we found that students had good personal relationships with their advisers, but many did not receive enough professional support. Students expected an adviser to be a mentor who would set standards, develop their skills, advise them on appropriate and feasible dissertation topics, and treat them as junior colleagues.

(5) **Departmental environment.** What impact does the environment in the department have on time-to-degree and attrition rates? Some departments were identified as having an impersonal environment, in which there were no professional student support activities or social events, or in which only star students were recognized, leaving many other students with a sense of being failures. These are the departments in which students were likely to take a longer time-to-degree, or from which students may frequently leave before completing the doctoral degree. The climate in a department often ties in with the kind of advising available. In contrast, departments that support their students with programs designed to assist them at

[9] The data are for all nine UC campuses; the NRC data tapes were provided by the UC office of the President.

[10] Some life sciences programs even had students write this prospectus in the form of a grant proposal.

[11] A task force of the Council of Graduate Schools has investigated this point further in a report entitled, "The Role and Nature of the Doctoral Dissertation."

[12] A 95% return rate is obtained.

[13] It should be remembered that these results came from the *successful* students. The differences between the groups' ratings of departmental advising was significant; $X^2 = (7, \underline{N} = 1097) = 31.8$, $p = .002$.

[14] However, the difference between men and women was not statistically significant.

Figure 4

University of California
Nine Campuses
NRC Median Time to Degree
Doctorate Recipients, 1980—1988

BY MASTER'S DEGREE

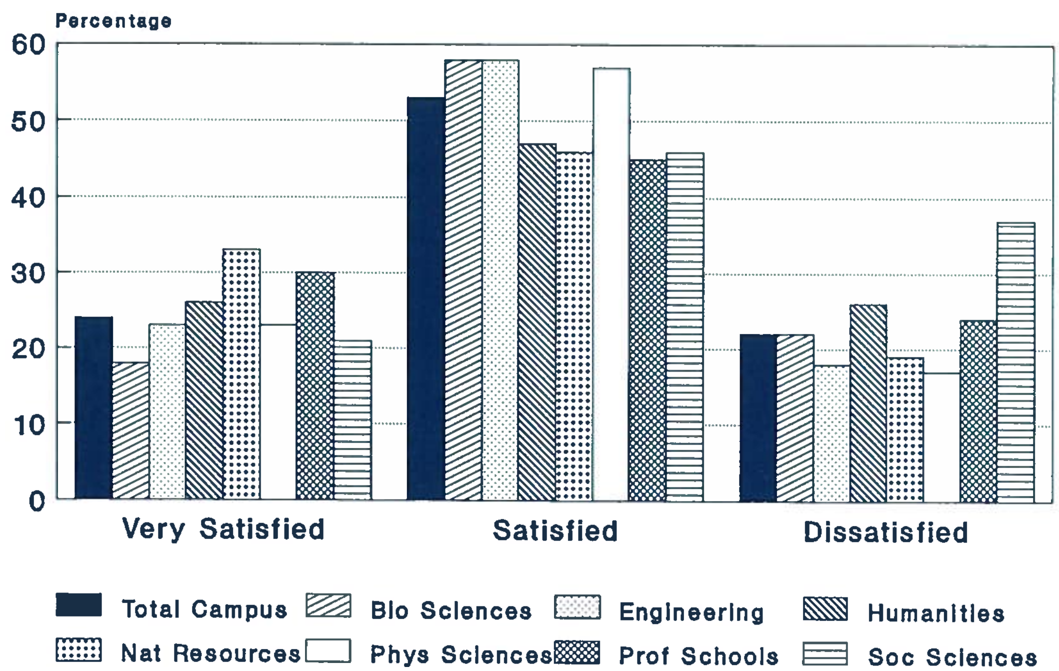
	No Master's GRAD—PHD	**Master's Same Campus** GRAD—PHD	**Master's Other Institution** GRAD—PHD
Arts & Humanities	7.9	9.2	11.6
Engineering	5.5	5.9	8.3
Life Sciences	6.0	7.3	9.1
Physical Sciences	5.5	6.5	8.4
Professional Schools	7.2	9.8	12.1
Social Sciences	7.1	7.8	10.3
All Fields (Years)	**6.0**	**7.4**	**9.8**

GRAD—PHD = Time from entry in any graduate program to Ph.D.

Figure 5

SEVEN MAJOR FIELDS

UCB Doctoral Exit Questionnaire
Doctorate Recipients 1987—1988
How Satisfied Have You Been With Departmental Advising?



each stage of the doctoral program and that have social gatherings may have a lower attrition rate. This is an area in which more research is necessary.

(6) **Availability of research money.** This factor is only too familiar to everyone and is one that is largely field- and somewhat institution-specific. According to students, faculty, and many graduate deans, one of the key factors influencing longer time-to-degree is insufficient financial support for doctoral students. Many students in the arts, humanities and social sciences are adversely affected because in addition the few available faculty research grants are small. Most students in engineering, the physical sciences, and the biological sciences, however, can count on employment as research assistants on the research grants of their faculties. In many cases, that work constitutes their dissertation research as well.

(7) **Type of financial support.** We did a one-time study of the relationship of time-to-degree and financial support *per individual student.* This was a very labor intensive study for which the unit of analysis was the actual expenses and financial support of each student who completed a degree between May 1986 and May 1989 in three departments in the social sciences and two in the humanities. We assume that these five departments had an equal proportion of outstanding students—all five departments rank among the top seven programs in the nation, in their respective disciplines. First, the financial support was calculated *during each student's first five years* by amount and length of time of the various types of support. These figures were then compared with the time the students took to complete their degrees. Students who received between four and five years of support took the shortest time, an average of 7.9 years to degree, while those who received no support took twice as long—16.6 years (Figure 7). As expected, the time decreased with an increase in support.

Second, the annual (12-month) financial support was divided by the annual (12-month) expenses[15] (Figure 8). These findings showed that, on the average, support money could cover between 30% and 90% of a student's expenses during the first five years in the program. Departments varied in the type of support they gave to students, as well as in the length of time the support was provided. Not surprisingly, the department offering the most financial support had the shortest time-to-degree (Department D). The department that offered the most financial support in the form of teaching assistantships had an intermediate time-to-degree (Department B). However, Department E, with the longest time-to-degree, did *not* offer the lowest amount of financial support to its students. Significantly, the department with shortest time-to-degree (Department D) not only provided the *most* financial support, but distributed the support most equally among research assistantships, teaching assistantships, and fellowships. From these results we can reconfirm that time-to-degree is related to the amount and type of support, but also emphasize that factors other than financial support, particularly the inherent structure of the Ph.D. program, also influence significantly time-to-degree in the humanities and social sciences.

We also examined the relationship between time-to-degree and the number of years students were supported by teaching assistantships in these five departments. The study showed that students who taught three or more years took one year longer (9.9) to complete the degree than students who taught less than three years (8.8)[16]. The same differences existed for those who taught four years or more (10.1) as compared with those who taught less than four years (9.0). Given these findings, the Graduate Division would recommend that, if at all possible, many departments in the humanities and social sciences implement a support package that would give students an efficient mix of support for each stage of the doctoral program—fellowships for the first year, teaching assistantships for years two and three, fellowships at the conceptualizing stage of the dissertation, and then, if available, research assistantships and dissertation writing fellowships for the final two years.

(8) **Campus Facilities.** As noted in Figure 3, the quality and effectiveness of the library, the availability of office and meeting space, and the issues of transportation, housing and child care certainly can influence time-to-degree. Of particular concern are housing and child care costs and availability for students with dependents. In order to shed some light on time-to-degree issues for students with dependents, the NRC data on earned doctorates were used, disaggregating doctoral recipients with dependents from those without dependents. Time-to-degree was then correlated with dependent status. Of the 1980-88 doctoral recipients of the nine UC campuses, 42% had one or more dependents.[17] (Figure 9) Men and women with dependents took 1.5 (2.2) years longer than those with no dependents. Figure 9 shows also that a higher percentage of minority graduate students at UC have dependents than do white students. Affordable housing and available, affordable child care are important if these students are to remain in graduate school. Especially given the inadequate child care facilities on most campuses, this is a real problem if we want to attract more women and minority students to our doctoral programs.

(9) **The job market.** Faculty most often cite the lack of academic jobs as a major reason for high attrition and the lengthening time-to-degree in some disciplines. This factor is beyond the control of the university. However, departments, faculty, and administrative units such as career planning and placement centers can actively support students in their job search. Departments can offer seminars that address the various aspects of becoming a professional in one's field and can prepare students for national conferences at which job interviews are held. Departments can also appoint faculty placement officers.

[15] The annual student expenses were taken from the student financial aid budgets of the appropriate year; these budgets have been extrapolated to 12 months.

[16] The difference in time-to-degree between students who taught for two years and students who taught for less than two years was statistically insignificant.

[17] According to the NRC definition, a dependent is someone receiving at least one-half of his or her support from the doctoral student.

Figure 6

SEVEN MAJOR FIELDS

UCB Doctoral Exit Questionnaire
Doctorate Recipients 1987—1988
How Satisfied Have You Been With the Professional Relationship With Your Doctoral Supervisor?

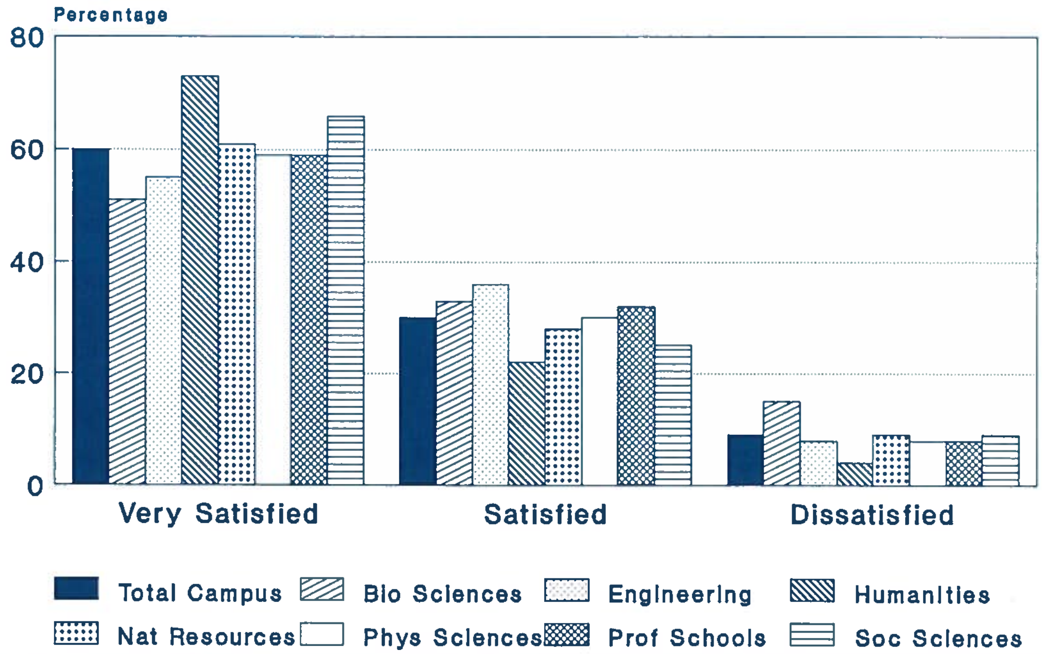


Figure 7

UNIVERSITY OF CALIFORNIA AT BERKELEY

Relationship between Time to Doctoral Degree and Years of Support
During the First Five Years of Doctoral Studies
Doctorate Recipients, May 1986 — May 1989
For Five Selected Departments in the Humanities and Social Sciences

ALL TYPES OF SUPPORT*

MEAN TIME TO PH.D. DEGREE

Years of Support during the First Five Years

0yr		0–1yr		1–2yrs		2–3yrs		3–4yrs		4–5yrs		Overall	
Mean Time	N	Mean Time	N	Mean Time	N	Mean Time	N	Mean Time	N	Mean Time	N	Mean Time	N
16.6	(13)	13.0	(8)	9.8	(16)	9.2	(48)	8.2	(83)	7.9	(54)	9.1	(222)

* Types of support are: Fellowship, teaching assistantship, research assistantship, reader, and other employment. Loans are excluded. Expenses used in calculation of years of support are based on 12 month estimates.

Figure 8

UNIVERSITY OF CALIFORNIA AT BERKELEY

Annual Expenses Covered by Financial Support
During the First Five Years of Doctoral Studies

Doctorate Recipients, May 1986 — May 1989

Five Selected Departments in the Humanities and Social Sciences

BY 5 TYPES OF SUPPORT

		TYPE OF FINANCIAL SUPPORT							
Department	Year	TA %	RA %	Reader %	Other Employ. %	Fellowship %	**TOTAL %**	Mean Time	Students
A	1	0.0	6.9	9.1	4.0	10.1	**30.2**		
	2	11.6	5.3	16.8	3.0	8.9	**45.5**		
	3	57.1	2.2	7.6	0.9	5.2	**73.0**		
	4	63.9	1.7	1.8	1.5	11.6	**80.5**		
	5	55.4	2.8	2.8	2.0	11.8	**74.9**	9.7	(54)
B	1	57.4	0.0	0.0	1.4	6.3	**65.1**		
	2	68.9	1.4	0.0	0.4	19.7	**90.3**		
	3	63.9	0.3	0.0	3.3	9.4	**76.9**		
	4	55.1	1.0	0.0	0.0	1.0	**57.1**		
	5	47.4	0.5	0.0	1.6	6.6	**56.0**	9.4	(12)
C	1	2.5	5.3	11.5	3.0	16.3	**38.4**		
	2	22.2	12.7	11.6	3.8	10.7	**61.0**		
	3	38.1	9.8	3.1	0.9	15.4	**67.3**		
	4	33.3	8.4	2.5	1.1	22.7	**68.1**		
	5	26.4	5.7	2.7	0.7	21.7	**57.2**	9.8	(64)
D	1	29.5	20.3	0.9	0.3	27.5	**78.4**		
	2	30.1	24.6	2.8	1.2	29.0	**87.8**		
	3	33.5	27.7	1.3	1.2	23.1	**87.0**		
	4	29.5	29.7	1.2	0.5	12.0	**72.9**		
	5	25.8	27.7	2.8	0.1	9.3	**65.6**	6.7	(47)
E	1	11.8	12.2	5.2	0.9	22.3	**52.3**		
	2	31.4	13.7	2.7	0.0	21.7	**69.5**		
	3	30.3	15.1	2.2	0.2	25.9	**73.7**		
	4	30.6	9.4	3.5	1.3	21.2	**66.0**		
	5	31.8	11.7	1.5	0.3	17.3	**62.6**	10.2	(45)

Developing Recommendations

As our last step, the Graduate Division developed recommendations and designed and implemented activities to work toward decreasing time-to-degree and lowering attrition. In this process we worked with faculty, graduate students, and graduate assistants/secretaries (the departmental staff who often know most about the difficulties of graduate students, who are the most knowledgeable about formal rules and regulations concerning graduate education, and who often act as counselors and therapists for students). Finally, many of these recommendations have been developed in conjunction with the Graduate Council of Berkeley's Academic Senate.

Faculty

A monthly invitational seminar on graduate education at Berkeley was initiated. Membership in the group of 35 included faculty and department chairs, senior administrators from the Berkeley campus and UC systemwide office, some members from the Graduate Council, graduate students, senior graduate assistants, and the several deans of the Graduate Division. The seminar had several goals: to inform and sensitize a part of the campus community, particularly the faculty, about particular issues of graduate education; to generate ideas on what changes should be made; and to receive feedback on recommendations we had developed.

The administrators in the Graduate Division also met monthly with faculty and students who served on an *ad hoc* subcommittee of the Graduate Council, which is the legislative arm of graduate education at Berkeley, in order to specify appropriate recommendations. Last year, one focus of these meetings was to formulate a new policy requiring students to meet annually with at least two members of their dissertation committees in order to review their progress on their dissertations and to map out a plan for the following year. This annual review is designed to improve communication between students and their committees and to provide students with feedback on their work.

The Dean of the Graduate Division sent to each department a data packet that included: time-to-degree and completion data for each department at Berkeley; the frequency distribution of departmental time-to-degree; a list of department faculty with the average time to Ph.D. of their advisees during the last ten years; and some key results from the doctoral student exit questionnaire. Responses were requested from each department's senior graduate advisers on what steps the department had been taking or could take, if appropriate, to improve the situation for their graduate students.

Graduate Students

Meetings were initiated with interdepartmental student focus groups. *The best ideas emerged from these focus groups.* Each semester a monthly meeting was held with a group of 12-15 doctoral candidates from various departments within one major field of study. These meetings served several purposes: first, they functioned as a support group for the students, giving them a chance to recognize that others shared the same difficulties and worries, and they made students aware of what other departments were doing for their students. Second, it told these students that their problems were being taken seriously, it helped develop possible solutions with them, and it encouraged them to initiate departmental support activities. Third, the Graduate Division developed a better understanding of the specific needs of students, and received ideas about educational activities that we could offer or that we could encourage the department or other campus units to provide.

From these meetings with the humanities student focus group emerged the idea of a workshop, sponsored by the Graduate Division, on practical tips for dissertation writing. Rapidly it became a hit with students in the humanities and social sciences. The information packet that we are now distributing to all doctoral students when they advance to candidacy is another idea developed from one of the focus group meetings. This packet is intended to help students make the transition more easily from taking classes to doing research and writing.[18]

Another event sponsored by the Graduate Division is an annual faculty forum entitled, "The View from the Other Side of the Desk." At this forum faculty were asked to discuss how they saw their role as dissertation advisers and what their opinions were regarding the purpose of the dissertation.[19]

Graduate Assistants

An advisory group of graduate assistants was formed whose function is similar to that of the student focus groups: to exchange information about shared problems, to develop ideas and recommendations, and to reflect upon implementation of these recommendations. Close collegial contact with the graduate assistants is essential since they are important to the actual implementation of our policies. In addition, the Graduate Division is developing at the present time a "generic" resource guide for departments based on successful departmental activities currently offered to graduate students. This guide will provide departments with ideas for support activities (often inexpensive) such as a day-long student-organized research conference or a series of workshops on becoming a professional in one's field.

The activities we have described were possible because of the creation of a professional research position in the Graduate Division; the expansion of the publications unit to include a full range of outreach activities, in addition to publications aimed at helping students to succeed in graduate school; and the ability of

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[18] The packet contains a question and answer sheet relating to major problems that may arise for a doctoral candidate; reprints from our graduate student newsletter on "Choosing your Dissertation Topic," "Writing a Successful Grant Proposal," and "Writing your Thesis;" and a list of services for Ph.D. students seeking academic employment.

[19] Some faculty participants were quite amazed to learn the opinions of their colleagues on these issues.

our information and technology unit to develop ways of using our historical database to address pressing issues in doctoral education.

To summarize, we have described how the Berkeley Graduate Division has used various research activities to address the issues of time-to-doctoral-degree and doctoral student retention with a focus on their improvement. Quantitative analyses have been supplemented with qualitiative methods to develop a basis for designing recommendations and programmatic outreach activities. This approach—working with the Academic Senate, faculty, graduate students, and graduate assistants—has led to more awareness of the issues to be resolved and has increased dedication to their resolution. It has also demonstrated that a graduate division, although part of the administration, can function more as an educational agency and less as a bureaucratic unit. ■

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Figure 9

UNIVERSITY OF CALIFORNIA
Nine Campuses
Doctorate Recipients, 1980 — 1988
MEAN TIME (GRAD-PHD)
BY DEPENDENTS / NO DEPENDENTS

Years	One or More Dependents	No Dependents
Men (all)	9.1	7.6
% with Dependents	47%	
Women (all)	11.3	9.1
% with Dependents	29%	
White	9.8	8.3
% with Dependents	37%	
Asian	9.2	7.5
% with Dependents	45%	
African Am.	12.5	11.4
% with Dependents	48%	
Chicano/Latino	9.7	8.6
% with Dependents	55%	
Total	9.5	8.2
% with Dependents	42%	

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FROM RUMORS TO FACTS: CAREER OUTCOMES OF ENGLISH PH.D.s

Results from the Ph.D.'s.-Ten Years Later Study*

by
Maresi Nerad and Joseph Cerny

For over 20 years the crisis in the academic job market for humanities Ph.D.s has been lamented. There are too many candidates and not enough faculty positions. If there are so few prospects in academia, where have all the Ph.D.s gone? Anecdotes describing doctoral recipients working as taxi drivers have been widely circulated and sometimes accepted as truth. But these distressing stories are based on rumors, not facts, and without facts, we are unable to truly understand the employment outcomes of our humanities Ph.D.s.

With the completion of *Ph.D.'s-Ten Years Later*, a national study of the career paths of doctorates, findings exist to examine the actual employment patterns of Ph.D.s and to provide a realistic basis for developing productive policy responses to the continuing crisis in the academic job market for humanities Ph.D.s.

Study Design and Methodology

The *Ph.D.'s - Ten Years Later* study involved almost 6,000 Ph.D.s from 61 doctoral-granting institutions across the United States.[1] Six disciplines were chosen to represent major fields of study: life science (biochemistry), engineering (computer science, electrical engineering), humanities (English), physical science (mathematics), and social science (political science). The 61 universities were selected based on their participation in the 1982 National Research Council (NRC) doctoral program assessment, the availability of doctoral programs in the selected disciplines, level of Ph.D. production (minimum of six Ph.D. degrees in the three years sampled), geographical distribution, and a representative mix of private and public institutions. Once an institution was selected, all doctoral recipients of the relevant programs were included in the survey population. No subsampling occurred. This survey population accounted for 57% of Ph.D. degrees awarded at all U.S. institutions in the six selected fields between July 1, 1982, and June 30, 1985[2] (Table 1 depicts the basic characteristics of the survey population.).

Table 1

Ph.D. Recipients in Six Fields at 61 Universities: July 1, 1982 – June 30, 1985

Size of Surveyed Population

Field	Men	Women	International	Total
Biochemistry	694	268	97	962
Computer Science	583	69	209	652
Electrical Engineering	966	36	417	1,002
English	567	650	72	1,217
Mathematics	1,005	187	395	1,192
Political Science	630	199	144	829
Total	4,445	1,409	1,334	5,854*

*Excludes deceased (63)

Source: "Ph.D.'s – 10 Years Later" Study, UCB Graduate Division

To ensure the highest possible response rate, addresses for doctorate recipients in the selected fields and institutions were obtained from participating institutions, commercial locator agencies, professional association membership directories, the national faculty directory, the national telephone directory, and online search engines as well as library author searches. Surveys were mailed out between October 1996 and October 1997. There was a total response rate of 66% from domestic Ph.D.s (U.S. citizens and permanent residents) and 52% from international Ph.D.s (temporary visa holders at the time of their doctorate completion).[3]

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Respondents completed a 22-page questionnaire focusing on employment history, the job-search process, and factors important in the decision to accept the first and current positions, as well as a retrospective evaluation of the doctoral program and the usefulness of the doctoral degree. In addition to the survey, in-depth interviews were conducted with 64 respondents to provide information about the context within which career decisions were made. The *Ph.D.'s -Ten Years Later* data set has been integrated with the national Survey of Earned Doctorates data of the same group, allowing comparative analysis by type of institution, time-to-degree, program size, parents' education, and fellowship support. Analyses were run using data weighted to be representative for the entire population of 172 Ph.D.-granting institutions rated in the 1982 NRC doctoral program assessment. Only small differences between weighted and unweighted results were found. Consequently, results of unweighted data analysis that exactly represent respondents' answers are reported here.

Where Have All the English Ph.D.s Gone? The Many Career Paths of a Cohort of English Ph.D.s

The cohorts of English doctorates sampled for this study have been called "the lost generation of humanists."[4] But were they really lost? While it is true that the long-standing crisis in the humanities academic job market appears to have presented doctoral graduates with a bleak reality, is it also true that these same new Ph.D.s remained jobless or underemployed?

This article traces the often difficult transition from receiving the Ph.D. to stable employment, examines the level of satisfaction or dissatisfaction English Ph.D.s have with their current employment, and discusses the value that they place on their doctoral education. Based on an analysis of survey results, it then discusses whether English Ph.D.s 10 to 13 years later had satisfying careers, whether they were within or outside academe, how many languished in non-tenure-track teaching jobs, and what other types of careers these Ph.D.s established. Finally, it offers graduate programs and university administrators some practical recommendations for augmenting the career outcomes of their humanities doctoral students.

English Ph.D.s found jobs, but, as the study shows, these jobs were not solely in the academic sector; they were also in the business, government, and nonprofit sectors (BGN). Unfortunately, in many cases English Ph.D.s spent several years navigating the non-tenure-track faculty route, hoping to finally arrive at the departmentally sanctioned destination--tenure-track professor--before they in fact reached this objective or before they made the difficult but fruitful transition to nonacademic employment.

To the detriment of their graduates, many institutions have neglected these employment realities. Describing the situation in his department, one English Ph.D. explained, "The culture of the department was such that even though everyone knew it was very difficult to get a tenure-track job . . . anything less was considered a failure. It was almost like a little tiny mass psychosis going on among both the faculty and the students." Examining the actual career outcomes of English Ph.D.s is a crucial step toward improving this kind of unfortunate departmental culture and developing institutional and departmental policies that address the continuing dismal realities of the academic job market in the humanities. If you want to test your assumptions about English Ph.D.s' responses before reading on, try to answer the following questions.

TEST YOUR ASSUMPTIONS

1. What percentage of English Ph.D. recipients who graduated between 1983 and 1985 were tenured professors in 1995?

2. What percentage worked as tenured professors in Research I institutions (Carnegie classification) in 1995?

3. What percentage worked as non-tenure-track faculty in 1995?

4. What types of jobs do you think English Ph.D. recipients working in the business, government, and nonprofit sectors (BGN) were doing?

5. Given the following categories:
- autonomy of work
- work environment
- location for spouse
- flexible work situation
- content of work
- career growth
- prestige of organization

would you expect the job satisfaction of English Ph.D. recipients working in the BGN sectors to be higher or lower than those working in the academic sector?

6. Thirteen years later, what percentage of respondents working in the academic sector do you think would still get a Ph.D. in English if they had it to do over again? What percentage of those working in BGN sectors in 1995 would get an English Ph.D. again?

7. What percentage working in the academic sector reported that they work in a team in their current job? What percentage use managerial skills?

8. What do you think our respondents recommended for doctoral programs?

–Source: Ph.D'.s - Ten Years Later *study, Graduate Division, University of California, Berkeley–8/1999*

Answer Key is on page 11

Characteristics of English Ph.D. Respondents

The 814 English Ph.D.s who completed the survey were mainly U.S. citizens, predominantly white, and slightly more than half (53%) of them were women. With an average age of 35, English Ph.D.s were older than those in the other surveyed fields upon completion of doctoral studies. This was primarily due to English Ph.D.s having the longest time-to-degree among the surveyed disciplines. Over half (51%) of the English Ph.D.s took more than 9 years to complete the degree, 44% took between 5 and 9 years, and 5% took 3 to 5 years (Table 2).

Table 2

Time-to-Degree for English Ph.D.s

Years	*Percent*	*N*
3-5	5%	(36)
5-7	21%	(164)
7-9	23%	(182)
9-11	18%	(137)
11+	33%	(257)

Note: Time-to-degree data were only available for 776 individuals

An English doctoral student's journey from the beginning of graduate school to degree completion is long and arduous; for some, equally lengthy is the journey from the time of Ph.D. completion to stable employment. This second stage of the journey, the transition from Ph.D. completion to work, left many individuals with hopes unrealized and a need to rethink career aspirations.

Expectations, Goals, and Realities

Most of the surveyed English Ph.D.s began graduate school planning to become professors and studied in departments in which faculty (73%) also expected them to pursue academic jobs (Table 3). Only 8% of respondents indicated that faculty also encouraged them to look for careers outside academia,

Table 3

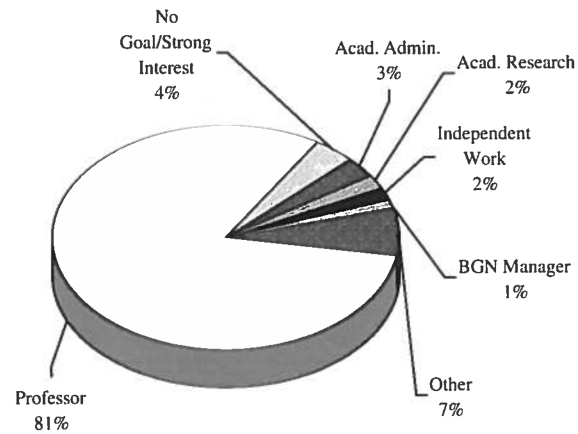
English Ph.D.s' Views on Faculty Expectations for Their Students

Pursue academic jobs	73%
Pursue jobs in both sectors	8%
Pursue BGN jobs	– (3)
No specific ideas/encouragement	19%

and these expectations were consonant with their own career goals. Of respondents who indicated (retrospectively) having a definite career goal at the onset of their doctoral education, nearly three quarters recalled wanting to become professors. The numbers desiring to join the ranks of the professoriate actually increased during the course of graduate school to the point that 81% of respondents recalled wanting to become professors at the time they completed their doctoral studies (Figure 1).

In spite of these career goals and expectations, in 1995 only 53% of English Ph.D.s were tenured and another 5% were in tenure-track positions (Figure 2). Fifteen percent were in non-tenure-track faculty or other academic positions. Thus, altogether 73% worked in the academic sector and another 16%[5]

Figure 1--*Career Goal at End of Doctoral Education*



worked in the BGN sectors.[6] Among those who secured tenured positions, less than one fifth of the Ph.D.s worked at the kinds of Carnegie[7] Research I institutions at which most were trained (Figure 3 on page 4).[8]

Does this gap between expectations and outcomes represent disappointed aspirations? Clearly this depends on the individual, but one way to evaluate this is to examine career outcomes in relation to career goals. As Figure 4 (on page 4) shows, nearly three fourths of those who wanted to become professors at the time of degree completion held tenured or tenure-track faculty positions 10 years later. However, a substantial minority of 14% was made up mostly of untenured year-to-year faculty and a small number of academic support staff. A few (3%) had become academic (nonfaculty) administrators, and 11% were employed in the BGN sectors.

Figure 2--*Employment Status at the End of 1995 (N=814)*

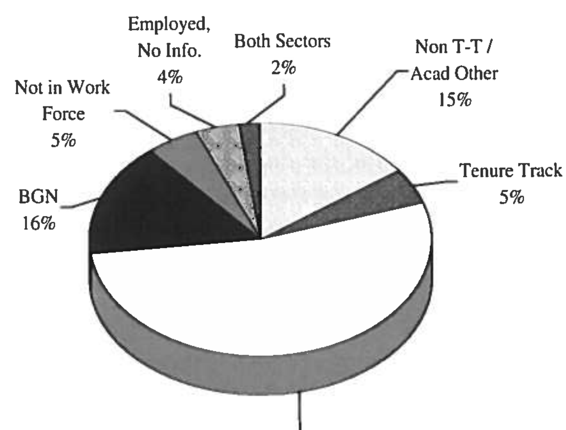
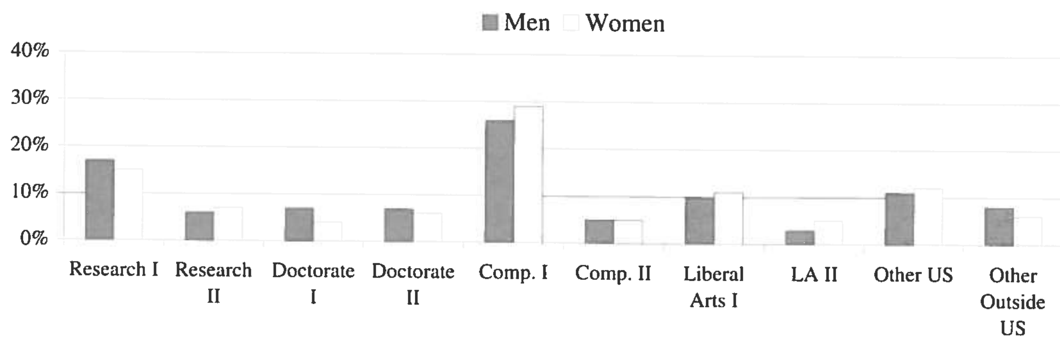


Figure 3--*Tenured Faculty by Institutional Classification and Gender, Dec. 1995*



Holding Patterns on the Way to the Professoriate

These findings might suggest that everything is actually in order, but this positive picture is marred by the relatively lengthy process and the difficulties of attaining career stability. English Ph.D.s spent on average 10 years in their doctoral studies and, as noted earlier, their mean age at completion was 35. Because of the difficult academic job market, many did not move smoothly out of graduate school into tenure-track employment. They entered a "holding pattern" of working in short-term non-tenure-track faculty positions, with some teaching courses at several different universities simultaneously. One out of two (48%) eventually attained tenure after working as non-tenure-track faculty. They spent, on average, 2.8 years in these positions before beginning a tenure-track position. Then, on average, they spent 5.3 years on tenure track before attaining tenure, in comparison to 6.3 years for those who never entered this holding pattern. In all, those who started in non-tenure-track positions spent 8.1 years from the time they earned the doctorate until reaching tenure. Having started an academic career in non-tenure-track faculty positions was a viable route to attain tenure for many in this cohort, and it even seems to be reflected in a shorter tenure clock; but overall both the cost and risk were very high.

Postdoctoral Appointments--Another Holding Pattern?

Holding a postdoctoral position is not a standard component of an academic career in English, nevertheless, a small percentage (8%) of English respondents used this kind of position to stay in the academic "game." Those who assumed a postdoctoral position, in fact, moved in a much higher proportion (73%) to tenured or tenure-track faculty positions in 1995 than those who did not. However, they seemed to view postdoctoral appointments as another kind of "holding pattern." Contrary to the expectation that English doctorates might choose postdoctoral positions in order to revise their dissertations, when asked to specify why they chose such an appointment, most respondents indicated that it was "the only acceptable employment" or a "necessary step." While this holding pattern seems advantageous for those whose goal was a tenure-track position, the average 2.0 years spent in postdoctoral appointments did not seem to be reflected in shorter time to tenure.

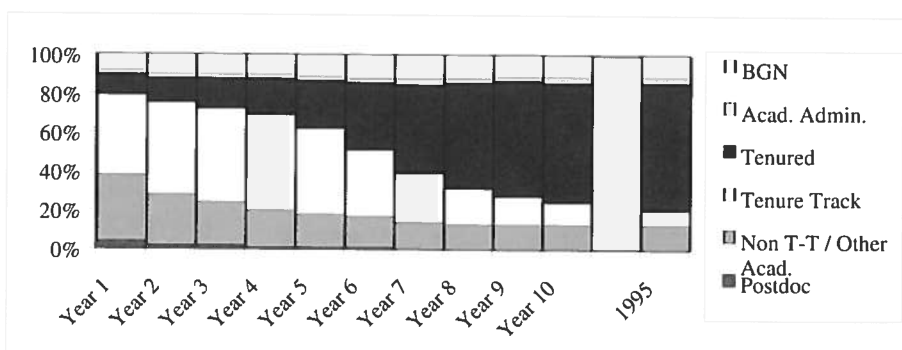
Career Paths Within and Outside Academe

Individuals less focused on academia encountered a very different job market, producing substantially different career paths (Figure 5 on page 5). The "cohort story" for this group--the 19% who stated a career goal other than becoming a professor in their field--also shows gradual movement out of "holding pattern" jobs into more stable employment. Ten years later, almost half of this group (46%) worked in the BGN sectors. Interestingly, 29% of those who did not explicitly state that professor was a career goal actually held tenured faculty positions 10 years later; 16% were in non-tenure-track jobs, most working as temporary faculty; and 9% worked in academic (nonfaculty) administrative positions. Despite the bias of graduate programs towards academic faculty careers, respondents with nonfaculty administrative positions within academe and those whose career paths took them outside the academic milieu reported good salaries and overall job satisfaction, as will be shown later.

English Ph.D.s in Nonprofessorial Positions within Academe

A typical example of the small group of

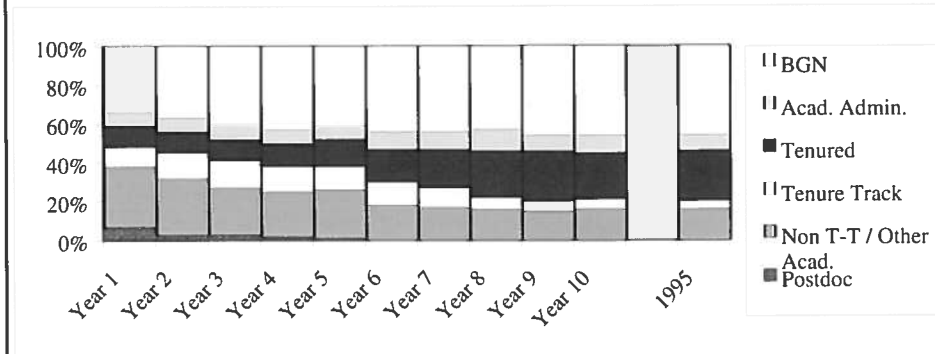
Figure 4--*Career Paths of English Ph.D.s Who Wanted to Become Professors (81%)*



English Ph.D.s in academic administration[9] is one respondent who indicated that she desired a career in academic administration. She gained experience during her last year of graduate study administering a college writing program and after completing her Ph.D., continued to work in this program, eventually accepting a position as assistant dean for special projects. When she was interviewed in 1996 for this study, she was associate dean of student life, was very satisfied with her employment status and her income, and believed strongly in the social value of her work.

In general, academic administrators enjoyed their jobs and found them challenging and rewarding, reporting nearly identical levels of overall job satisfaction as did tenured and tenure-track faculty. Academic administrators had on average a higher salary than tenured faculty. While the median annual salary of tenured faculty was $48,000 (including summer teaching, consulting, and other income), the median annual salary of academic administrators was substantially higher at $62,000.[10]

Figure 5--*Career Paths of English Ph.D.s Who Did Not Want to Become Professors (19%)*



English Ph.D.s in the Business, Government, and Nonprofit Sectors

The 128 surveyed English Ph.D.s employed in business, government, and nonprofit organizations in 1995 were engaged in a wide variety of occupations drawing on their expert knowledge of texts as well as their high-level analytical skills (Table 4); the largest number (35) worked in the writing/editing field. Notably, for several respondents, writing/editing positions led to work in general management, the second largest category (27) of BGN employment for English Ph.D.s. Very few English Ph.D.s worked in primary and secondary education (15), research and development (10), or law (7). Three had advanced to the level of chief executive officer.

The case of one respondent, who began her doctoral studies after a decade as a successful high school English teacher, illustrates a rewarding career outside academe. This individual continued teaching high school during and after attending graduate school at one of the country's top English departments but later worked as a business consultant training people to use computer software. Thereafter, she was employed as an editor and supervising editor in several different major educational publishing houses. Her Ph.D. training plus her years of high school teaching made her a very attractive candidate for these positions. By the time she was interviewed for this study, she had become marketing manager for an educational publisher. She was satisfied with her salary and thoroughly enjoyed her work.

Table 4--*BGN Sectors Employment Distribution, Dec. 1995 (N=128)*

Job Title	*N*	*Job Title*	*N*
Writing / Editing	35	Consultant	4
Management	27	Executive	3
Teaching	15	Administrative Support	3
R&D	10	Finance	2
Communications / Gov. Relations	7	Information Systems	2
Legal	7	Other	13

Table 5--*Not in the Paid Work Force – December 1995*

	Men	*Women*	*Total*
Caretakers	0	2	2
Retired (Age & Other)	0	12	12
Medical Condition	0	1	1
Between jobs / Other	3	1	4
Fringe Employment	2	12	14
Don't know why	0	1	1
Unemployed	1	4	5
Total (N=814)	6	33	39 (5%)

Not in the Paid Work Force

Contrary to rumors of rampant joblessness, only 39, or about 5%, of respondents in English were not in the paid work force in December 1995 (Table 5). However, only five people, less than 1% of respondents, were unemployed in the traditional economic sense of being involuntarily out of work and seeking work. Fourteen of those not working did not give an explanation for this, but it is probable that most of these women were caretakers since, in another section of the survey, they reported having small children. Twelve people, all women, were retired, many of them former high school teachers who started graduate school in midlife because they enjoyed literature.

Satisfaction with Current Employment and Value of the Ph.D.

After making the difficult transition from graduate school to more permanent, stable employment, how satisfied were English Ph.D.s with their work situations? One survey question solicited evaluations of job satisfaction and dissatisfaction by asking respondents to rate 24 aspects of their current employment on a scale from 1 (very satisfied) to 4 (not satisfied). Dimensions of current employment evaluated by respondents included autonomy, content of work, flexibility, salary level, job security, and time for leisure.

Dimensions of Job Satisfaction

Most respondents indicated being "very" or "fairly" satisfied with their jobs overall (Figure 6). Eighty-eight percent of academic administrators and 87% of tenured and tenure-track faculty were very or fairly satisfied with their current employment as well as managers, writers, editors, and teachers in the BGN sectors. However, only 71% of those in non-tenure-track faculty and academic support staff positions reported being very or fairly satisfied with their jobs.

A detailed analysis of selected dimensions of satisfaction yielded some interesting results (Table 6). For this analysis, dimensions of job satisfaction commonly associated with the privileges of academia were selected and compared for respondents employed in the BGN and academic sectors in 1995. For example, 92% of respondents employed in the BGN sectors were satisfied with the autonomy of their work as compared to 90% of academics. Eighty-seven percent of BGN respondents were satisfied with the content of their work, as compared to 89% of academics. Flexibility of working arrangements was rated satisfactory by 82% of those in the BGN sectors as compared to 84% of academics.

Most Often Cited Dimensions of Job Satisfaction

Job satisfaction was also analyzed separately for respondents employed in each of four job categories: faculty, non-tenure-track faculty, academic administration, and the BGN sectors (Table 7 on page 7). Some differences and some similarities were found between these groups in the most commonly cited dimensions of satisfaction and dissatisfaction.

Tenured and tenure-track faculty were most commonly satisfied with job security, opportunities to teach, and the autonomy of their work. While many respondents in non-tenure-track faculty positions were dissatisfied with their jobs overall, they were satisfied with the fact that their jobs allowed them to live in a desirable geographic location, be near job opportunities for a spouse, and be in a good location for raising children. Academic administrators were most commonly satisfied with geographic location, health and retirement benefits, and administrative responsibilities.

Based on the dimensions of job satisfaction most commonly cited by respondents employed in the BGN sectors, it appears that BGN positions combine some of the positive aspects of faculty positions with some of the positive aspects of non-tenure-track and administrative jobs. For example, like those in non-tenure-track and administrative jobs, respondents employed in the BGN were usually satisfied with geographic location and spousal job opportunities. And, like faculty members, they were usually satisfied with the autonomy of their work.

Most Often Cited Dimensions of Job Dissatisfaction

For each of the four job categories, faculty positions, non-tenure-track faculty, academic administration, and BGN employment, dissatisfaction with research opportunities was among the mostly commonly cited aspects of dissatisfaction (See Table 8 on page 7). Although occupying tenured and tenure-track positions, over two fifths of faculty were dissatisfied with research opportunities offered by their current employment. However, respondents differed when identifying other areas of dissatisfaction. Among other most commonly cited dissatisfactions, faculty members indicated not having enough time for leisure and experiencing stress. A picture emerged of non-tenure-track faculty putting up with insecure, dead-end jobs, and stressed academic administrators struggling to operate programs in substandard facilities. Respondents employed in the BGN sectors were dissatisfied with opportunities to teach and with the direct use of their Ph.D. education.

Figure 6--*Overall Satisfaction with Current Job*

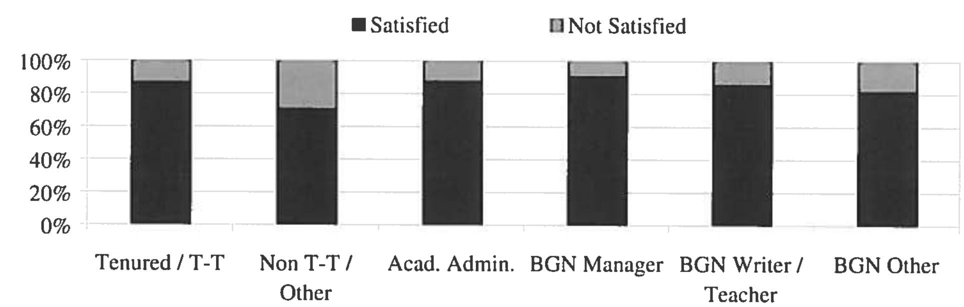


Table 6--*Satisfaction Dimensions of Current Job by Sector*

	BGN	*Academic*
Autonomy of Work	92%	90%
Spouse's Job	91%	75%
Content of Work	87%	89%
Prestige of Organization	83%	68%
Work Environment	83%	73%
Flexible Work Situation	82%	84%
Career Growth	78%	67%

Table 7--*Most Cited Aspects of Current Job Satisfaction*

Tenured / T-T		*BGN*	
Job Security	95%	Autonomy of Work	92%
Opportunity to Teach	95%	Spouse's Job	91%
Autonomy of Work	91%	Geographic Location	89%
Non T-T Faculty		*Academic Administrator*	
Geographic Location	91%	Geographic Location	96%
Spouse's Job	91%	Health and Retirement	96%
Location for Children	90%	Administrative Responsibility	96%

Table 8--*Most Cited Aspects of Current Job Dissatisfaction*

Tenured / T-T		*BGN*	
Leisure	43%	Research Opportunity	49%
Research Opportunity	42%	Opportunity to Teach	42%
Stress	41%	Use of Ph.D.	38%
Non T-T Faculty		*Academic Administrator*	
Career Growth	63%	Stress	48%
Research Opportunity	60%	Equipment	48%
Job Security	54%	Research Opportunity	47%

Median 1995 Total Annual Salary by Sector and Gender
Among the six surveyed disciplines, English Ph.D.s reported the lowest total annual salaries, including summer teaching, overtime, consulting and other income sources. However, BGN salaries were substantially higher than academic salaries, with the median total annual salary being $47,000 in the academic and $56,000 in the BGN sectors. Within the academic sector, there was virtually no difference between the median salaries of men and women in tenured, tenure-track, and non-tenure-track faculty positions (Figure 7). The men among the academic administrators reported a higher annual salary than the women. Also in the BGN sectors the median annual salary of $65,000 reported by men was $15,000 higher than that reported by women.

Usefulness of the Doctoral Education
Despite the difficult transition to stable employment and the disappointed hopes of some who wanted faculty positions, English Ph.D.s overwhelmingly reported valuing the doctoral experience (Table 9 on page 8). Ninety-nine percent of tenured and tenure-track faculty reported that the Ph.D. was "definitely worth" or "probably worth" the effort. Academic administrators were also virtually unanimous in their assessment of the value of the Ph.D., with 96% reporting that it was "definitely worth" or "probably worth" the effort. Somewhat smaller but still very high proportions of those in other categories felt similarly, with 89% of those employed in the BGN sectors considering completing the doctorate as worth the effort.

Furthermore, when respondents were asked to evaluate whether they would get a Ph.D. in the same field again, "knowing what you know now," most English Ph.D.s in each job category responded affirmatively (Table 10 on page 8). Overall, 78% of English Ph.D.s would still get a doctorate in English. However, not surprisingly, those employed in non-tenure-track positions and in the BGN sectors were somewhat less likely to state that they would get a Ph.D. in English again, and somewhat more likely to state that they would obtain a Ph.D. in a different field or study for a professional degree.

The Value of the Ph.D.
What is it about doctoral education in English that Ph.D. recipients value so highly? The many responses to open-ended questions at the end of the 22-page survey (nearly 70% of all English respondents completed these questions) provided valuable insights. For one question, respondents were asked to express the main points they would make "if you were asked to testify before a legislative committee on the value of a doctoral education in your field." Respondents repeatedly emphasized both the personal value of doctoral education and contributions made to society by their teaching, research, and writing activities.

English Ph.D.s eloquently expressed their commitment to the value of scholarly work in the humanities. According to one, doctoral training in English "bolsters self-discipline, trains, and stimulates the mind and imagination, develops the entire character. It is not just a specialized skill." Others emphasized the implications for society. "In a good doctoral

Figure 7--*Median 1995 Total Annual Salary in Thousands by Job Title & Gender in the U.S.*

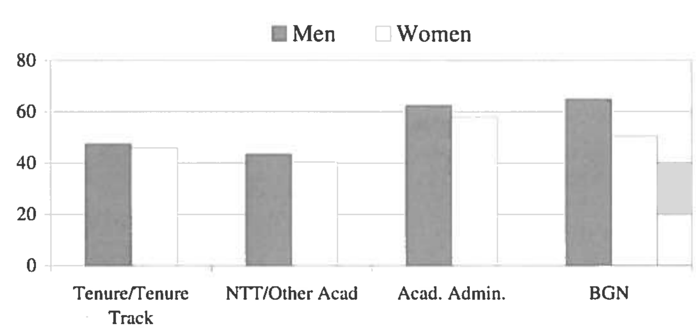


Table 9--*Feelings about Completing Ph.D.*

	Worth	*Not Worth*
Tenured / Tenure Track	99%	1%
Non T-T Faculty	87%	13%
Academic Administrator	96%	4%
BGN	89%	11%

program one learns to solve problems and develop insights in this spirit. All of these virtues are relevant to all of life and an educated electorate in a democracy," one person stated. Another pointed out, "A doctoral education in literary studies prepares an individual to become a tremendous value to any civilization like ours governed increasingly by signs and symbols, where distinctions between 'information' and 'knowledge' have to be made constantly."

Advice from English Ph.D.s-Ten Years Later

Given their love for and commitment to the humanities, do English Ph.D.s have suggestions for improving graduate education? In response to open-ended questions, respondents poured out thoughtful advice and suggestions for beginning doctoral students and existing graduate programs. In particular, they were concerned about the high individual costs in terms of time and money incurred in graduate training in English. In addition, they criticized graduate programs for not teaching students how to survive professionally and for not supporting them in their search for employment.

Recommendations for Current Doctoral Students

The most common piece of advice respondents offered to beginning graduate students was "love it or leave it" (Table 11). One respondent stated, "Do not enter this field unless you feel you would never be happy doing anything else. You should have as strong a sense of vocation as one entering the ministry because the sacrifices required for teaching in this field are as great as those required of a pastor. You will have no time to call your own and you will never be paid what you are worth."

Table 10--*Would you do the Ph.D. again?*

	Tenured / T-T (N=437)	*Non T-T Faculty (N=81)*	*Academic Administrator (N=26)*	*BGN (N=115)*
Yes: Same Field	84%	67%	88%	64%
Yes: Different Field	9%	18%	4%	13%
No: MD / JD	5%	10%	4%	12%
No: Master's	1%	4%	4%	8%
No Graduate Degree	1%	1%	-	3%

Other recurring recommendations included practical advice about what it takes to finish a Ph.D. and have a chance in the job market. Recommendations made most commonly by respondents working in both the academic and BGN sectors included: "focus," "define your goals," and "publish." Academics emphasized the importance of learning how to teach.

Recommendations for Doctoral Programs

In general, English Ph.D.s were highly critical of their doctoral programs for failing to adequately professionalize students and for not supporting them in the difficult job search. Open-ended responses to the survey item requesting recommendations for doctoral programs revealed that the most common piece of advice was that programs be downsized (Table 12 on page 9). For example, some advised graduate programs to "accept fewer students and offer more support." Many respondents were disappointed with the quality of mentoring and support available in graduate school and their negative experiences in the academic job market.

Respondents employed in both the BGN and academic sectors often suggested that graduate programs should improve graduate student teacher training, improve career and placement services, and help students publish and attain professional visibility. Comments included, "Require pedagogical training in multiple contexts," and "Most graduate programs emphasize a narrow specialty and yet most jobs are for generalists" (In recent years, graduate student teacher training has been addressed in most graduate programs). These practical suggestions are all aimed at better preparing current students for available jobs (*i.e.*, teaching as opposed to research positions) and more effectively integrating them into the professional networks where vital job contacts are made.

English Ph.D.s tended to be satisfied with the content of their doctoral programs, with one exception: respondents in both the BGN and academic sectors recommended that graduate programs increase opportunities for interdisciplinary training. This advice reflects the reality that faculty must often teach a broad range of courses that demand interdisciplinary knowledge as well as the respondents' interest in and the workplace's demand for interdisciplinary work.

Table 11--*Most Cited Recommendations for Doctoral Students*

Ranking Order	*BGN (N=79)*	*Academic (N=366)*
Love it or leave it	1	1
Focus, define your goals	2	4
Consider BGN careers	3	10
Publish	4	3
Be aware of poor job market	5	5
Learn how to teach	-	2

Table 12--*Most Cited Recommendations for Doctoral Programs*

	BGN (N=75)	*Academic (N=328)*
	Ranking Order	
Downsize	1	2
Teach how to teach	2	1
Improve career services	3	8
Help students finish	4	-
Help with publishing	5	4
Provide interdisciplinarity	7	3

Workforce Preparation

The survey also invited respondents to answer specific questions about core workplace skills that, according to some employers, doctoral graduates lack: teamwork, collaboration, interdisciplinary work, and organizational and managerial skills. Respondents were asked (a) whether their doctoral education had involved any of these skills, (b) whether they used these skills in their current jobs, and (c) whether instruction in these skills should be an important component in present-day doctoral education. Responses confirmed that positions held by Ph.D.s in both academic and BGN sectors usually require such skills, but that graduate programs do not teach them. Figures 8 and 9 show that fewer than one fifth of respondents gained experience with teamwork, collaboration, or organizational and managerial skills in graduate school, but more than half use all of these in their current jobs! Respondents employed in the BGN sectors were even more likely than academics to use these skills.

Respondents were also asked whether or not it was important for graduate programs to include teamwork, collaboration, interdisciplinary work, and managerial experience. About half of those employed in the academic sector rated working in a team and collaboration as important for inclusion in current doctoral education, while interdisciplinary work and managerial skills were rated as even more important. Except for interdisciplinary work, respondents employed in the BGN sectors were even more likely to consider it important to incorporate all of these skills into doctoral training. These findings strongly suggest that providing experiences in teamwork, collaboration, and organizational and managerial skills would be as relevant to graduate students seeking academic employment as to those who work in business, government, and nonprofit organizations.

The "Culture of Neglect"

The job search is probably one of the most daunting and certainly one of the most important tasks faced by the newly minted English Ph.D. Yet respondents revealed repeatedly that their universities, departments, and advisors did not provide sufficient assistance with the job search. Comments like, "Basically it would have been nice if someone had cared if I got a job" and "Professors actively helping me make contact--'opening doors'--instead of shrugging it onto my shoulders entirely [would have helped]" indicated the level of frustration job-seekers experienced. Although respondents employed in the academic sector were more satisfied than respondents employed in the BGN sectors with the help received in the job search process, the general level of satisfaction with the help in the job search was low.

When seeking the first post-Ph.D. job, 63% of respondents answered job notices in professional journals or the *Chronicle of Higher Education*. As shown in Table 13 (on page 10), slightly more than half of respondents received leads and advice from faculty members and from their Ph.D. advisor. Only one third of respondents used a campus career center in the job search. The percentage of respondents who evaluated each of these methods as useful, however, was much lower.

Figure 8--*Assessment of Workforce Preparation– Employed in Academia 1995*

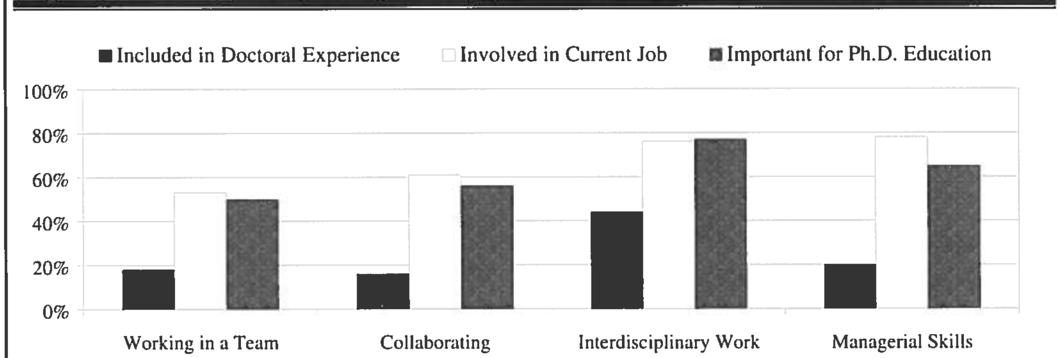
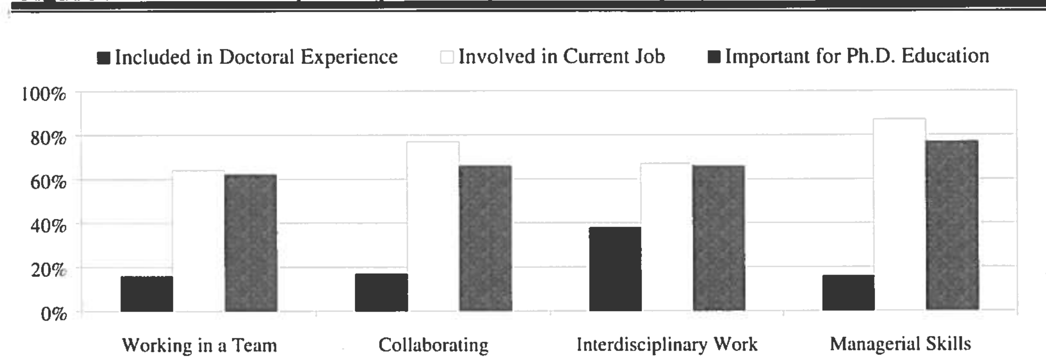


Figure 9--*Assessment of Workforce Preparation – Employed in BGN Sectors 1995*



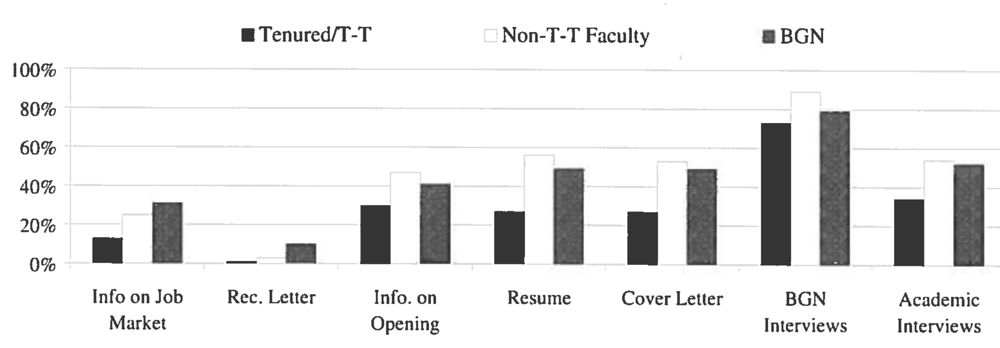
When asked to specify what would have helped most with the job search, respondents most often mentioned "improved career services" and "help from faculty or department." Respondents also explained that they would have been helped by having faculty members actively promote them among colleagues, receiving realistic and honest feedback on their skills and their appropriateness for different kinds of positions, being provided more information about how the job market worked, and getting coaching and advice on interviewing and assembling an application dossier.

Table 13--*Four Most Used and Useful Sources in the Job Search*

Source	*Used*	*Useful*
Job notice in professional journal or the Chronicle of Higher Education	63%	39%
Faculty	53%	26%
Ph.D. Advisor	53%	26%
Campus Career Center	33%	11%

High proportions of respondents reported seeking help of various kinds and getting none of the type or amount they wanted (Figure 10). Not surprisingly, respondents employed in the BGN sectors received the least amount of help, but many who sought academic employment were also neglected. Forty-one percent of the 487 individuals who wanted help preparing for an academic job interview reported that they "never got this help," and 32% did get "some help, but not as much as needed." One third of those wanting advice on résumé-writing, advice about writing cover letters, or suggestions about where to find job openings never received assistance. Almost 20% of those seeking job market information never obtained this from departmental faculty. A small percentage (3%) of the 561 English Ph.D.s who wanted recommendation letters from faculty did not get them.

Figure 10--*Percentage Receiving No Help on the Job Search, By 1995 Job Title*



Includes only those who sought help

Because of the high levels of dissatisfaction and numbers of individuals receiving no help with significant aspects of the job search, the approach taken by English departments to job placement might best be referred to as a "culture of neglect."

Combating the Culture of Neglect: the Career Management Series in English

Clearly, doctoral education in English benefits the individual and equips him or her to contribute to society in a myriad of ways. Graduate education contributes to an individual's growth, fosters creative abilities, helps students mature intellectually, and develops in them the valuable skills of analysis, problem solving, and critical thinking. Society then benefits from the knowledge, skills, capacities, and values of its humanities Ph.D.s. In addition, the widely recognized teaching and research activities of doctoral graduates, as well as their less commonly acknowledged work in business, government, and nonprofit organizations contribute to social, cultural, and political life.

Toward the goal of easing the transition from education to meaningful employment, a Career Management Series devoted to the practical aspects of envisioning, developing, and managing a career for English/humanities doctoral students has been developed at Berkeley.[11] This workshop was preceded by an intensive process of interaction with English department graduate students, staff, and faculty. The process began with a needs assessment--a series of interviews with faculty, students, and staff--that allowed the team to be sensitive to the particular departmental culture. Every aspect of the workshop was designed to respond to the specific concerns and needs of this group.

Over the course of the 2-day workshop, the 37 doctoral students in attendance engaged in a series of student-centered interactive activities designed to develop an enhanced recognition and appreciation of their skills and accomplishments; greater clarity about the broader uses of the skills and capabilities developed in graduate school; greater clarity about core values, work preferences, and strategies for being proactive in the pursuit of a working career; and an increased understanding of current trends in the economy and how they affect career options.

By opening up the entire world of employment options, encouraging students to explore their values and preferences, dismantling the prejudice against employment in the BGN sectors, and helping students identify the concrete skills they already possess, the workshop aimed to help advanced doctoral students more quickly find productive and satisfying careers.

Recommendations

In light of the *Ph.D.'s-Ten Years Later* findings and our practical experience delivering the Career Management Series, we have the following recommendations:

1. If Ph.D. programs in English continue to train their graduates solely for the future professoriate, then doctoral programs need to reduce their enrollment.

2. Universities, graduate schools, and graduate programs should publish the results of alumni placement, as well as statistics on the average time it took their students to complete the doctorate, how much financial support a student can expect for the duration of doctoral study, and the completion rate for the cohort that entered 10 years earlier.

3. Starting at the end of the second year of doctoral study and throughout the remaining years, humanities departments in collaboration with the campus career center should offer opportunities for students to reassess life and career goals, to assess core values and work preferences, and to receive training in proactive career planning skills.

4. Graduate programs in the humanities should include preparation for employment in the BGN sectors as well as in academia. This can be done by communicating the value of successful career outcomes both within and outside the ranks of the professoriate; articulating the multi-applicable skills that are being learned; enhancing training in teamwork/collaboration, interdisciplinary work, managerial and organizational skills; exploring internship opportunities (university administration, BGN); and encouraging faculty and graduate student contact with the BGN sectors to dismantle stereotypes about academic and nonacademic workplaces.

5. Humanities graduate programs and professional associations should track and assess the careers of their Ph.D.s, not just for 1 or 2 years after Ph.D. completion, but for at least 5 and possibly up to 10 years.

6. It is crucial that department chairs, humanities deans and graduate deans, with the help of career centers, take constructive action on career and employment issues for their graduate students.

Notes

* We would like to thank the research team of the *Ph.D.'.s-Ten Years Later* study: Marc Goulden, Deepak Gupta, Renate Sadrozinski, Lucy Sells, Orr Shakked, but specifically Elizabeth Armstrong for her devotion to the English Ph.D.s and Elizabeth C. Rudd for "bringing it together." We also want to thank Debra Sands Miller for her editorial work.

[1] This survey was funded by the Mellon Foundation and selected analysis by the National Science Foundation.

[2] In order to capture longer term career outcomes, doctorate recipients were surveyed at least 10 years after earning the Ph.D.

[3] Biochemists responded at the highest rate of 68% (70% of domestic Ph.D.s), English Ph.D.s and political scientists followed with response rates of 67% (also 67% of domestic English Ph.D.s) and 64% (68% of domestic political scientists), respectively; response rates of 63% of mathematicians (67% of domestic Ph.D.s), 60% of computer scientists (65% of domestic Ph.D.s), and 53% of electrical engineers (57% of domestic Ph.D.s) were achieved.

[4] Baer, Kristine, *Chronicle of Higher Education*, May 25, 1983, p. 64.

[5] Excluding "not in the work force" and those who were employed with incomplete information, 58% were employed as tenured faculty and 6% were on tenure track, 16% non-tenure-track faculty or other academic positions; 80% were employed in the academic sector, 18% in the BGN sectors, and 2% in both sectors. For English Ph.D.s working in both sectors, the academic component is mainly a non-tenure-track faculty position.

[6] 5% were not in the workforce in 1995, 4% were employed, but we have incomplete employment information, and 2% worked in both sectors simultaneously.

[7] See definition according to the Carnegie Commission's report, *A Classification of Institutions of Higher Education*, 1987 edition.

[8] It is noteworthy that one quarter of women in the academic sector held non-tenure-track faculty positions as lecturers or administrators, as compared to only 17% of men. Discussion of gender differences in employment patterns is beyond the scope of this paper and will be examined in subsequent publications.

[9] Among all English Ph.D.s, 3.8% were employed as academic administrators in 1995.

[10] Excluded from the salary calculations are part-time employees and respondents working outside the U.S.

[11] Building on the work of Rose von Thater, Director of Education and Outreach at the Berkeley Center for Particle Astrophysics, the Graduate Division together with von Thater and an outside consultant, Myan Baker, designed this 2-day workshop.

TEST YOUR ASSUMPTIONS
ANSWER KEY

from quiz on page 2

1. 53% were tenured professors. (58% excluding not in the workforce and incomplete information)
2. 8% worked as tenured faculty in Research I institutions. (16% of all tenured faculty worked in Research I institutions)
3. 12% worked as non-tenure-track faculty 13 years later. (13% excluding not in the workforce and incomplete information)
4. Employment distribution in the BGN sectors:

◆ Writing or editing	◆ Communications	◆ Info. systems
◆ Management	◆ Executive work	◆ Finance
◆ Teaching	◆ Legal work	◆ Gov't. relations
◆ R&D	◆ Consulting	◆ Adm. support

5. Satisfaction dimensions of current job:

Satisfaction category	BGN	Academe
Autonomy of work	92%	90%
Location for spouse	91%	75%
Content of work	87%	89%
Prestige of organization	83%	68%
Work environment	83%	73%
Flexible work	82%	84%
Career growth	78%	67%

6. Do the English Ph.D. again? Employed in 1995: Academic 82%; BGN 64%.
7. Academic sector: teamwork 53%; managerial skills 78%.
8. Recommendations for doctoral programs:

	BGN (N=75)	Academe (N=328)
	Ranking Order	
Downsize	1	2
Teach how to teach	2	1
Improve career services	3	8
Help students finish	4	-
Publish, professional visibility	5	4
Provide interdisciplinarity	7	3

–Source: Ph.D'.s - Ten Years Later *study, Graduate Division, University of California, Berkeley–8/1999*

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For Further Information

Further analysis of the *Ph.D.'s-Ten Years Later* survey will examine multiple relationships among graduate school experiences and career outcomes. Questions to be addressed include the relationship between evaluation of the usefulness of the Ph.D. and respondents' employment sector, current job satisfaction, and the impact of family and marital status on objective and subjective measures of career outcome among Ph.D. recipients in this study. The careers of international Ph.D.s and issues of mobility and race/ethnicity and gender will also be addressed.

Forthcoming publications include an article examining postdoctoral appointments appearing in *Science*, September 3, 1999, and a book presenting major disciplinary and general findings of the study, to be published in 2000. Inquiries should be directed to <Phd10yr@uclink4.berkeley.edu>.

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VIEWPOINT

B.IV

Postdoctoral Patterns, Career Advancement, and Problems

Maresi Nerad and Joseph Cerny

Postdoctoral appointments can have different functions and meanings, depending on the field and whether the postdoc is a man or a woman. The *Ph.D.'s—Ten Years Later* study confirmed that in biochemistry, the postdoc, not the Ph.D., has become the general proving ground for excellence both in academia and industry. Because they spent a longer time in these "mandatory" postdocs, biochemists had the largest proportion of untenured faculty 10 to 13 years after the Ph.D. In mathematics, where substantially fewer postdoctoral positions are available, Ph.D.'s taking postdocs are more likely to obtain faculty positions, but this is true only for men. University administrators should be accountable for monitoring the total time spent in these positions and should provide administrative assistance for skills training, career growth, and the job search. In addition, creative solutions concerning the dual-career couple phenomenon are necessary.

Recent reports in the United States have claimed that increasing numbers of Ph.D. scientists are holding postdoctoral appointments for longer periods than ever. Concern about the implications for careers in the life sciences even prompted a warning from a National Research Council committee about a possible overproduction of Ph.D.'s in this area (*1*). However, recent comprehensive data on postdoc appointees and their experiences have not been available, given that the last national postdoc survey was published 15 years ago (*2*). Rectifying this situation was one of the goals of the *Ph.D.'s—Ten Years Later* study (*3*), which collected data on the career paths of scientists and engineers in biochemistry, computer science, electrical engineering, and mathematics, including the role of postdoc appointments (Table 1). Here we highlight some results from this study and discuss some of the implications for policies regarding postdoc positions.

Addressing matters related to the educational and training environment of postdocs in the United States is complicated, because few universities have a central authority overseeing the conditions of postdoc appointments, such as duration, salary structure, benefits, and placement services. Few universities can provide a truly accurate count of the number of postdoc fellows on campus. These deficiencies exist because of the lack of a consistent definition among hiring units in universities and other laboratories of what constitutes a postdoc, and because postdocs are compensated and recorded in several different ways—some are paid as university employees, some are paid through an entirely separate stipend account, and others are paid directly by foundations and foreign governments.

We analyzed the career paths of the 86% of Ph.D. biochemists and 31% of Ph.D. mathematicians responding to the *Ph.D.'s—Ten Years Later* survey who had held postdoc appointments. In computer science and electrical engineering, less than 10% of respondents had a postdoc appointment along their career path.

In biochemistry, for the cohorts who graduated from July 1982 to June 1985, the postdoc was the norm. In this field, a postdoc appointment is regarded as a necessary step after doctoral completion, whether the individual plans a career in academia or in the business, government, or nonprofit (BGN) sectors. Consequently, the postdoc, not the Ph.D., has become the general proving ground for academic excellence, scientific entrepreneurship, and ultimate intellectual independence.

By 1995, about half of all Ph.D. biochemists who had held postdocs (49%) were employed in the BGN sectors, and the other half (51%) worked in various jobs within academia; 34% held a tenured or tenure-track faculty position (Table 2). Not surprisingly, biochemists outside of academia earned almost $22,000 more in median annual total salary (including consulting, overtime, summer research or teaching,

Table 1. Size of the surveyed population and response rates. The data cover Ph.D. recipients in six fields at 61 universities from 1 July 1982 to 30 June 1985.

Major field	Men	Women	International (out of total)	Total	Total responses (*n*)	Response rate (%) Domestic	Response rate (%) International
Biochemistry	694	268	97	962	654	70	50
Computer science	583	69	209	652	388	65	51
Electrical engineering	966	36	417	1002	534	57	48
English	567	650	72	1217	814	67	65
Mathematics	1005	187	395	1192	752	67	57
Political science	630	199	144	829	525	68	47
Total	4445	1409	1334	5854*	3667	66	52

*This number excludes 63 people who were deceased.

Table 2. Employment in 1995 by postdoc history and gender. Data are in percent except where raw numbers are given in parentheses. M, men; F, women.

	Biochemistry				Mathematics			
	Postdoc		No Postdoc		Postdoc		No Postdoc	
	M (376)	F (143)	M (63)	F (20)	M (180)	F (37)	M (395)	F (85)
Tenured faculty	20	18	21	(2)	75	46	56	54
Tenure-track faculty	15	16	16	(5)	9	(2)	6	5
Academic researcher	3	8	(2)	–	1	–	2	4
Academic other	9	17	(4)	(5)	3	(5)	5	3
BGN researcher	35	23	32	(2)	5	30	21	18
BGN manager/executive	12	13	13	(2)	4	(1)	5	8
BGN other	6	4	(4)	(3)	2	(1)	3	8
Both academic and BGN	(1)	1	(2)	(1)	1	–	2	–

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and other income sources) than those employed in the academic sector (where the median salary was $57,000) (*4*).

On average, the biochemists employed in academia in 1995 had spent 7 months longer in postdoc appointments than the 3.5 years of those employed in the BGN sectors. The length of time spent in postdoc training did not appear to be a factor in the decision to appoint a postdoc to a faculty position. However, if the postdoc period was 5 years or less, those who were hired into the faculty had a better chance of being appointed to a position at one of the top quarter (*5*) of doctoral programs. Individuals who received one of the prestigious, portable postdoctoral fellowships from the National Institutes of Health or the National Science Foundation (NSF), as did 12% of the first-time biochemistry postdocs, had an advantage when competing for faculty positions in the top-quarter–ranked doctoral programs.

The results of the study revealed that university administrators and professional societies in the sciences need to be concerned both about the long time it takes to earn a doctoral degree and about long intervals between Ph.D. degree completion and the first non-postdoc position. Biochemists spent 3.8 years in postdoc appointments, whereas mathematicians spent 2.5 years and computer scientists and electrical engineers only 1.6 years. As a result biochemists, who had the shortest time to Ph.D. among these disciplines but essentially faced a mandatory postdoc, had the largest proportion (46%) of untenured faculty 10 to 13 years after completion of the Ph.D. (Table 3).

Fewer postdoc appointments are available in mathematics than in biochemistry. These seemed to be highly sought after by those whose career goal was a faculty position. Just under one-third of the Ph.D.'s in mathematics spent time in postdoc training, and of these, 78% held a tenured or tenure-track faculty position in 1995. However, a large proportion (61%) of mathematicians who did not take postdoc appointments also held a tenured or tenure-track position in 1995, and almost one-third (31%) found employment in the BGN sectors (Table 2). Unlike biochemistry doctorates, 21% of mathematics Ph.D.'s spent a portion of their postdoc appointment abroad (domestic, 14%; international, 36%).

The survey results also revealed two particular positive outcomes for mathematics postdocs. First, the time invested in a postdoc significantly improved the odds of gaining a faculty position in the top quarter (*5*) of research doctorate programs—particularly if the applicant was among the 12% of first-time postdocs (*6*) who received a portable fellowship, such as an NSF fellowship, or had spent a year or more at one of the internationally renowned mathematics institutes. However, this was true only for men, 84% of whom were tenured or tenure-track faculty in 1995, and not for women (Table 2). Second, the experience gained in a postdoc position in mathematics, often called a visiting assistant professorship, seemed to be reflected to a modest extent in a shorter tenure clock. The

Table 3. Age at tenure and percent in tenured and tenure-track (TT) positions in 1995 in the United States, by field.

	Age at grad school entry	Time to Ph.D.	Years between Ph.D. and TT	Years to tenure	Age at tenure	Tenured (%)	TT (%)
Biochemistry	22.8	5.9	4.1	6.1	38.9	18	16
Computer science	23.1	7.6	0.5	6.1	37.3	32	4
Electrical engineering	23.5	6.4	1.3	5.7	36.9	24	3
English	23.7	8.9	1.1	5.9	39.6	57	6
Mathematics	22.6	6.9	1.4	5.6	36.5	59	6
Political science	23.7	8.7	0.5	6.2	39.1	54	8

Table 4. Family, postdoc appointments, and career. Data are in percent except where raw numbers are given in parentheses. M, men; F, women.

Did postdocs	Biochemistry				Mathematics			
	Married at Ph.D.		Not married		Married at Ph.D.		Not married	
	M	F	M	F	M	F	M	F
Goal at end of Ph.D.	(182)	(77)	(192)	(73)	(66)	(20)	(119)	(15)
Wanted to become a professor	37	26	35	32	70	55	58	40
First employment after postdoc*	(184)	(79)	(201)	(76)	(69)	(21)	(113)	(18)
Tenure-track faculty	25	23	27	18	71	29	62	72
Academic researcher/other	24	32	20	38	16	28	19	(2)
BGN researcher/other	45	41	47	39	(2)	43	12	(1)
Spouses' 1995 education	(177)	(73)	(159)	(48)	(64)	(19)	(85)	(10)
Spouse had a Ph.D., J.D., or M.D.	24	75	43	56	25	84	22	80
Employment 1995	(179)	(77)	(197)	(66)	(69)	(20)	(111)	(17)
Tenure-track faculty	15	20	15	12	(4)	(1)	11	(1)
Tenured faculty	21	17	19	18	84	35	69	59
Academic research/other	12	23	13	29	(1)	(1)	6	(4)
BGN researcher/other	39	26	42	29	(5)	50	9	(2)
BGN manager/executive	13	14	11	12	(1)	(1)	5	–

*First employment after postdoc may not total 100% because the small numbers of tenured faculty and BGN managers and executives are not given.

Table 5. Major reasons for choosing postdoc appointments. Data are in percent except where raw numbers are given in parentheses. M, men; F, women.

	Biochemistry				Mathematics			
	First postdoc		Last postdoc		First postdoc		Last postdoc	
	M	F	M	F	M	F	M	F
Necessary employment step	76	76	59	49	57	35	56	(2)
Additional training	38	38	22	11	46	53	25	(2)
Training in another field	42	45	42	44	7	–	6	–
Specific organization	10	6	14	11	16	(3)	29	(3)
Specific person	32	33	36	36	23	(5)	38	(3)
Only acceptable employment	11	9	24	22	31	(5)	22	(2)
Specific geographical area	17	29	30	52	16	35	20	(5)
Location worked for both spouse and self	21	38	38	66	15	50	17	67

same is not true in biochemistry. Like the biochemists, however, mathematicians working in the academic sector in 1995 earned less annually than did their counterparts in the BGN sectors (an average of $53,000 versus $80,300).

A substantial percentage of women in mathematics who did postdoc training in the hope of becoming a professor did not realize this aspiration. Women who were married at the time of Ph.D. completion and who held postdoc positions were more likely to end up in research positions in the BGN sectors than in academia (Table 4). Women postdocs in biochemistry, whether married or not, held tenured or tenure-track positions in 1995 at about the same proportion as men, although women stayed slightly longer in postdoc positions and thus advanced even more slowly to tenured faculty positions than men did. Furthermore, for women in both biochemistry and mathematics, the motivation to enter postdoc positions often seemed to be related to the desire to live in the same location as their partners and to combine family and career (Table 5).

Less has been known, in either discipline, about the careers of international students who studied in the United States. The 1983–1985 Ph.D. cohorts comprised 10% international students in biochemistry (*7*) but 33% in mathematics. International and domestic Ph.D.'s in both disciplines assumed postdoc positions in about the same proportions. Half of the U.S.-trained international mathematicians remained in the United States. For them, postdoc training did not affect the odds of their holding a faculty position—in 1995, with or without postdoc training, 75% of these U.S.-trained international mathematics Ph.D.'s were in tenured or tenure-track positions. Although few of the prestigious U.S. postdoc fellowships are available to non-U.S. citizens, the postdoc gave them a hiring advantage for faculty positions at the top quarter (*5*) of research universities.

In their search for more permanent employment, postdocs used many sources of assistance with varying degrees of utility. The postdoc mentor was certainly important for biochemists in the job search, but less so for mathematicians, who returned to their Ph.D. advisors for this significant support. The second most commonly used source was job notices in relevant journals. Universities should certainly extend the placement services that they offer to doctoral students to postdocs.

In light of the *Ph.D.'s—Ten Years Later* findings (not all of which we could cover here), and from our experience as doctoral and postdoctoral administrators, we recommend that universities designate a central authority for postdoc affairs—either the senior research administrator or the graduate dean. This office should monitor the total length of time graduates spend in postdoc appointments, allowing a maximum of 5 years in these training positions, including time spent at other institutions (*8*). Any subsequent appointments, even if they are by fiscal necessity temporary, should be staff appointments and should reflect career growth and advancement. Adequate salaries and employment benefits should be ensured for postdoc appointees. Administrative assistance should be provided to create a campus-wide postdoc community to combat the frequent experience of isolation, to provide the skills training necessary for becoming a professional in academia or the BGN sectors (including grant writing and presentation and communication skills), and to support career planning and job search activities. Finally, we recommend that a high-level National Research Council (NRC) committee be established to develop creative solutions, especially in the universities, to the widespread phenomenon of dual careers for couples. More spousal accommodation would enable our country to take greater advantage of the proven talent of its men and women scientists.

References and Notes

1. NRC, *Trends in the Early Careers of Life Scientists* (National Academy Press, Washington, DC, 1998).
2. W. Zumeta, *Extending the Educational Ladder. The Changing Quality and Value of Postdoctoral Study* (Lexington Books, Heath, Lexington, MA, 1984). See also these earlier studies: NRC, *Postdoctoral Appointments and Disappointments* (National Academy Press, Washington, DC, 1981); NRC, *The Invisible University: Postdoctoral Education in the U.S.* (National Academy Press, Washington, DC, 1969).
3. *Ph.D.'s—Ten Years Later* is a national study, conducted by us, of the career paths of doctorates, involving almost 6000 Ph.D.'s from six disciplines (biochemistry, computer science, electrical engineering, English, mathematics, and political science) from 61 doctoral-granting institutions across the United States. The Mellon Foundation funded this study, and selected analysis was funded by NSF. The survey population accounted for 57% of Ph.D.'s awarded at all U.S. institutions in the six selected disciplines between 1 July 1982 and 30 June 1985. The study had a total response rate of 66% from domestic Ph.D.'s (U.S. citizens and permanent residents) and a 52% response rate from international Ph.D.'s (temporary visa holders at the time of their doctorate completion). The number of minority respondents was too small for a meaningful analysis.
4. These salaries are those of biochemists employed within the United States.
5. "Top quarter" refers to a 1982 NRC evaluation of doctoral programs [*An Assessment of Research—Doctoral Programs in the United States: Biological Sciences and Mathematical and Physical Sciences* (National Academy Press, Washington, DC, 1982)].
6. Counting all postdocs, 14% of the biochemists and 19% of the mathematicians held portable fellowships.
7. Before 1985, relatively few international students (temporary visa holders) studied biochemistry in the United States.
8. It is difficult to understand why, after a well-organized doctoral program and a 2- or 3-year postdoc position under a thoughtful mentor, a Ph.D. would not have acquired the necessary skills for more permanent employment.

9. We thank the research team of the *Ph.D's—Ten Years Later* study (E. Armstrong, M. Goulden, D. Gupta, R. Sadrozinski, L. Sells, O. Shakked, and E. C. Rudd) and D. S. Miller for editorial suggestions.

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Postdoctoral Patterns, Career Advancement, and Problems

Maresi Nerad and Joseph Cerny

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1999

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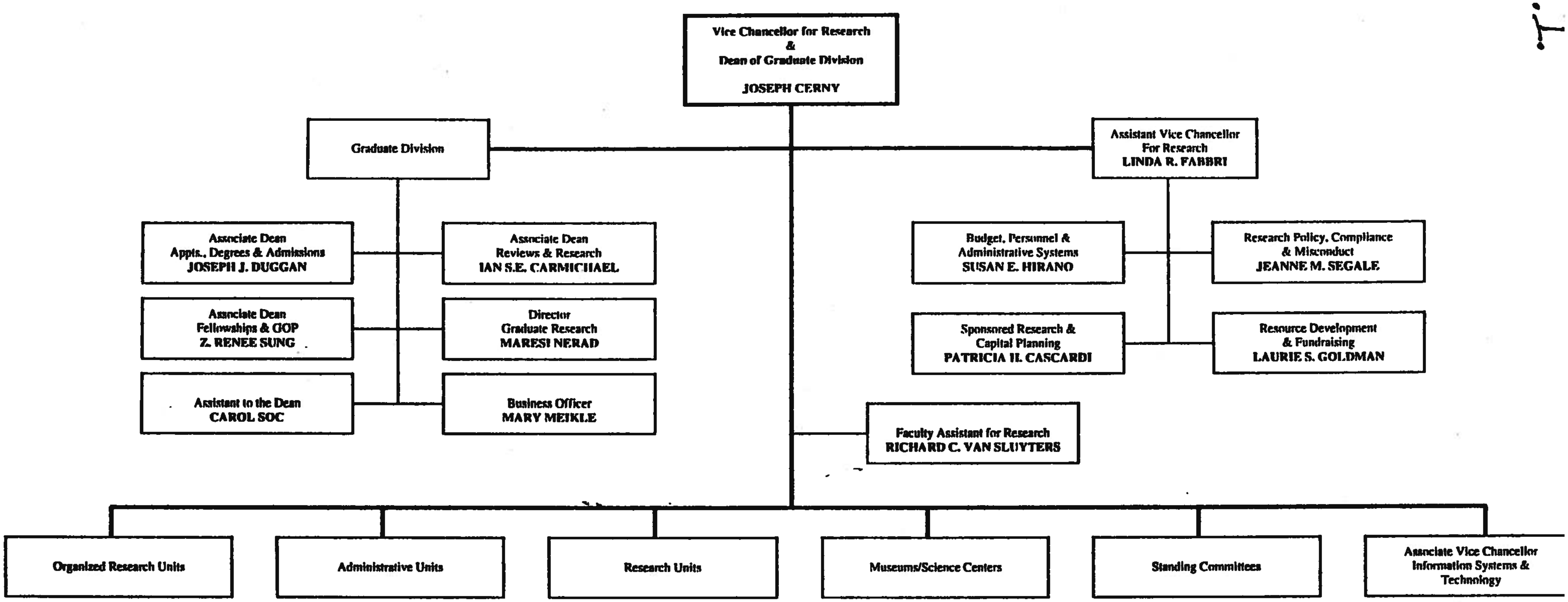

Organized Research Units

Archaeological Research Facility
Berkeley Seismological Laboratory
Field Station for Behavioral Research
Institute of Business & Economic Research
Cancer Research Laboratory
Laboratory of Chemical Biodynamics
Institute of Cognitive Studies
Earthquake Engineering Research Center
Electronics Research Laboratory
Engineering Systems Research Center
Center for Environmental Design Research
Environmental Engineering & Health Sciences Laboratory
Institute of Governmental Studies
Institute of Human Development
Institute of Industrial Relations
Center for Study of Law & Society
Institute of Management, Innovation & Organization
Center for Pure & Applied Mathematics
Institute of Personality & Social Research
Radio Astronomy Laboratory
Institute for the Study of Social Change
Space Sciences Laboratory
Survey Research Center
Theoretical Astrophysics Center
Institute of Transportation Studies
Institute of Urban & Regional Development
Virus Laboratory
Earl Warren Legal Institute

Administrative Units

Animal Care & Use
Office of Laboratory Animal Care
Committee for Protection of Human Subjects
Office of Radiation Safety
Sponsored Projects Office
Office of Technology Licensing

Research Units

Beatrice Bain Research Group
Berkeley Multimedia Research Center
Center for Extreme Ultraviolet Astrophysics
Earth Resources Center
Emma Goldman Papers Project
History of Science & Technology
Miller Institute for Basic Research
University of California Energy Institute
U.C. Data
U.C. Semiconductor Manufacturing Alliance for Research & Training
U.C. Systemwide Biotech. Research & Education Program
University of California Transportation Center

Museums/Science Centers

Angelo Coast Reserve
Blackhawk Museums
Donner Region-Field Research Stations
Essig Museum of Entomology
Lawrence Hall of Science
Phoebe Hearst Museum of Anthropology
U.C. Museum of Paleontology
U.C. Botanical Garden
University & Jepson Herbaria
Museum of Vertebrate Zoology

Standing Committees

Animal Care & Use
Berkeley Natural History Museums
Diving Safety
Hazardous Waste Management
Laboratory & Environmental Biosafety
Laboratory Operations & Safety
Laser Safety
Positive Financial Disclosure
Protection of Human Subjects
Radiation Safety

Associate Vice Chancellor Information Systems & Technology

Administrative Systems
Communications & Network Services
Central Computing Services
Instructional Technology Program
Museums Informatics Project
Social Sciences Computing Laboratory
Strategic Technology Planning
Student Information Systems
Workstation Support Services

L. II.

REPORT OF THE

SUNSET TO DAWN REVIEW OF

BERKELEY'S ORGANIZED RESEARCH UNITS

Submitted by the
Academic Senate/Administration Committee
July 2000

Committee Members:	Representing:
Professor Lewis Feldman Plant and Microbial Biology	Graduate Council (1998-99)
Professor William Clemens Integrative Biology	Graduate Council (1999-2000)
Professor J. Keith Gilless Environmental Science, Policy and Management	Committee on Academic Planning and Resource Allocation
Professor Richard Marrus Physics	Committee on Research
Professor Karlene Roberts Business	Committee on Educational Policy
Professor Mary Ryan History	Committee on Academic Planning and Resource Allocation
Professor Anne Wagner History of Art	Budget and Interdepartmental Relations Committee
Vice Chancellor Joseph Cerny	Office of the Vice Chancellor for Research

The Sunset to Dawn Review of Berkeley's Organized Research Units
Academic Senate/Administration Committee Report
July 2000

I. Introduction

The 1998-2000 Berkeley campus Sunset to Dawn Review of Organized Research Units (ORUs) has its origins in a 1982 policy issued by the University of California Office of the President, which provided that, following completion of mandated reviews of all ORUs on all campuses by 1986, ORUs would be granted a maximum 15-year lifespan. They then would be required to re-compete for ORU status, based on the needs and resources of the University at that time.

For Berkeley, this continues a tradition of planning, evaluation, and oversight of our organized research mission. The campus utilized the mandated review in the mid-1980s to ask an additional fundamental question: how do we best position our organized research capabilities for the 1990s and beyond? Unfortunately, the 1990s brought a serious recession to the State of California, resulting in several years of major budget cuts to the University, and a total reduction of more than 30% in permanent funding for our Organized Research Units, setting back considerably our ability to launch any new initiatives.

We viewed the Sunset to Dawn Review as an opportunity to restructure our existing organized research enterprise, some of which dates back 100 years, and to create opportunities for new Organized Research Units to support the campus as we moved into the next century. The Chancellor agreed, and $1M in new permanent funding was approved for allocation in July 2000 for this venture. The plan consisted of two phases: first, to issue a call for proposed new ORUs during 1998-99, with the best of those proposals continuing to the second phase in 1999-00, a competitive evaluation for ORU status with the 32 existing ORUs.

II. Process for New ORU Candidates: Academic Year 1998-99

In October 1998, a call was distributed to all Berkeley faculty announcing the opportunity for the creation of new Organized Research Units. The call specified criteria and outlined the process for both the required letters of intent and the preproposals. (See Appendix I.) This represented an extraordinary opportunity, since the State of California had stopped funding new ORUs in 1971.

Further, the campus had never held a competition for new ORUs, so this first-ever call generated considerable interest, as measured by the sixty letters of intent received in December, and the 47 preproposals submitted by the February 1999 deadline. (Table I, below, lists all the preproposals and their faculty sponsors.)

Table I: Preproposals Submitted in Response to Call
for Possible New Organized Research Units

Name	Faculty Proposer
Advanced Industrial Nations, Ctr for Comparative Study of	Trond Petersen, Haas/Sociology
Advanced Scientific Instrumentation, Ctr for	Bernard Sadoulet, Physics
African Studies, Ctr for	Gillian Hart, Geography
Americas, Ctr for Research in the	Carolyn Porter, English
Arts, Consortium for the	Charles Altieri, English & Art Practice
Asia Business Center	Michael Gerlach, Haas
Asian Digital Center	Lewis Lancaster, East Asian Languages
Atmospheric Sciences, Ctr for	Inez Fung, Earth & Planetary Science/ESPM
Berkeley Earth Resources Center	George Brimhall, Earth & Planetary Science
Biodiversity, Ethnobiology, & Medicine	Brent Mishler, Integrative Bio/Univ & Jepson Herbaria
Bodies, Global Markets & Bio-Power, Ctr for Study of	Nancy Scheper-Hughes & Paul Rabinow, Anthropology
Brain Imaging Center	Richard Ivry, Psychology
CA Environments & Biological Diversity, Ctr for Study of	David Lindberg & Mary Power, Integrative Biology
Central Asian Studies, Ctr for	David Stronach, Near Eastern Studies
Child and Youth Policy, Berkeley Ctr for	Mary Ann Mason, Social Welfare
Competition Policy ORU	Joseph Farrell, Economics
Cultural Identity & New Technology, ORU on	Michael Buckland, Information Management & Systems
Culture, Organization, and Politics, Ctr for	Neil Fligstein, Sociology
Design of Urban Places, Ctr for the	Peter Bosselmann, City & Regional Planning
Design Visualization, Ctr for	Jean-Pierre Protzen, Architecture
Environmental Law, Economics & Policy, Ctr for	Michael Hanemann, Ag & Resource Economics & John Dwyer, Law
Environmental Science and Policy, Ctr for	David Zilberman, Ag and Resource Economics
Ethics, Law and Public Affairs, Ctr for	Eric Rakowski, Law
Functional Genomics Lab, UCB	John Ngai & Tito Serafini, Molecular & Cell Biology
Gender & Globalization, Ctr for the Study of	Norma Alarcon, Women's & Ethnic Studies, Spanish & Portuguese
Genomics Research, Ctr for	Sung-Hou Kim, Chemistry
Geographic Info Science, Ctr for Integrated Research in	Peng Gong, Environmental Science, Policy & Management
Health Policy & Social Sciences, ORU for	Stephen Shortell, Public Health/Haas/Sociology
Human-Centered Computing	John Canny, Electrical Engineering and Computer Science
Information Policy, Ctr for	Peter Lyman, Information Management & Systems
Integrative Planetary Science, Ctr for	Frank Shu, Astronomy
Isotope Biogeochemistry, Ctr for	Ronald Amundson, Ecosystems & Todd Dawson, Integrative Biology
Law & Technology, Berkeley Ctr for	Robert Merges, Law
Literacy & Learning, Ctr for Research in	Glynda Hull, Education
Mechanistic Biomedicine, Ctr for	Tom Alber, Molecular & Cell Biology
Medicine, the Humanities, and Law, Ctr for	Thomas Laqueur, History & Guy Micco, UCB-UCSFJoint Medical Program & Marjorie Shultz, Law
Molecular & Developmental Evolution, Ctr for	Michael Levine, Molecular & Cell Bio & David Lindberg, Integrative B
Nemea Research Center	Stephen Miller, Classics
Nutrition, Food Security & Community Health, Ctr for Study of	Sharon Fleming, Nutritional Sciences
Organizational Research at Berkeley, Ctr for	John Freeman, Haas
Risk Analysis, UCB Ctr for	W.E. Kastenberg, Nuclear Engineering
Sexual Culture, Ctr for the Study of	Michael Lucey, French/Comparative Literature
Social Justice, Ctr for	Rachel Moran, Law
Tebtunis Papyri, Ctr for	Donald Mastronarde, Classics
Technology, Work & Society, Ctr for	Clair Brown, Economics
Tradition & Community, Program on	Ronelle Alexander, Slavic Languages & Literature
Traditional Environments, International Unit for Study of	Nezar AlSayyad, Architecture

As the first step in the evaluative process, a mail review of the preproposals was conducted over the next two months, utilizing Berkeley faculty as confidential reviewers. Nominations for possible reviewers had been solicited from the preproposal sponsors themselves, and these were augmented with additional suggestions from cognizant deans and department chairs. Efforts were made in selecting reviewers to include a representative sampling of junior and senior faculty members.

Each preproposal was to receive four separate reviews, so the process ultimately involved 26 faculty reviewers, each from a different department, who evaluated between six and nine selected preproposals. The preproposals were arranged in broad disciplinary groups with the designations chosen by the proposers. Preproposals were assigned so that each faculty member would review some preproposals which focused on research near their area of expertise and some preproposals outside their field which would draw upon the reviewer's knowledge of scholarship in general as well as their knowledge of the campus.

The two primary review criteria in both this preproposal stage and the final competitive Sunset to Dawn evaluation were "Quality" and "Strategic Advantage."

> "Quality" represents the probable interdisciplinary research excellence underlying the proposed ORU's mission and goals and its graduate student and undergraduate research impact; whether it would be likely to leverage significant extramural resources at the federal, state, foundation or donor level; and an evaluation of the past track record of core faculty.
>
> "Strategic Advantage" assesses whether the proposed ORU provides truly creative synergy of unique UCB faculty strengths in addressing important research directions and whether the proposed ORU's new faculty alliance is likely to provide state or national leadership in opening up new or emerging research areas.

Utilizing these criteria, reviewers were asked to provide a brief (one-half to a full page) narrative analysis, and a two-part rating which asked for (a) an overall "absolute" rating ranging from Excellent to Fair and (b) a "relative" rating on both Quality and Strategic Advantage, requiring the allocation for each preproposal of a number of points between 0 and 9 from a total number of points based on the number of proposals that the reviewer was sent.

Overall, the reviewers felt that the preproposals reflected some exciting new interdisciplinary collaborations and intellectually challenging research plans.

While this preproposal review was under way, the campus appointed the Academic Senate members of the joint Academic Senate/Administration Committee (Committee), which was charged with overseeing the Sunset to Dawn Review process and carrying it to its conclusion. Members were selected from the relevant Academic Senate committees and were to speak on behalf of their parent Senate committee as well as represent their general disciplinary area. The Committee for this phase of the process also included as non-voting members the Vice Chair of the Academic Senate Robert Spear and the Vice Chancellor for Research Joseph Cerny. In late Spring 1999, the Committee discussed the 47 preproposals and the reviews, selecting twelve preproposals (Table II) to continue to the final phase in 1999-2000, the competition with Berkeley's established ORUs.

Table II: PROPOSED ORGANIZED RESEARCH UNITS

ARTS AND HUMANITIES
Consortium for the Arts
Center for the Study of Sexual Culture
Center for the Tebtunis Papyri

BIOLOGICAL SCIENCES
Center for the Study of California Environments and Biological Diversity
Functional Genomics Laboratory
Center for Molecular & Developmental Evolution

INTERNATIONAL AND AREA STUDIES
Center for African Studies

PHYSICAL SCIENCES
Center for Advanced Scientific Instrumentation
Center for Atmospheric Sciences
Center for Integrative Planetary Science

SOCIAL SCIENCES
Center for Child and Youth Policy
Center for Medicine, the Humanities, and Law

The Committee membership is listed below:

Committee Members:	Representing:
Professor Lewis Feldman * Plant and Microbial Biology	Graduate Council
Professor William Clemens * Integrative Biology	Graduate Council
Professor J. Keith Gilless Environmental Science, Policy & Management	Committee on Academic Planning and Resource Allocation
Professor Richard Marrus Physics	Committee on Research
Professor Karlene Roberts Business	Committee on Educational Policy
Professor Mary Ryan History	Committee on Academic Planning and Resource Allocation
Professor Anne Wagner History of Art	Budget and Interdepartmental Relations Committee

- Professor Clemens replaced Professor Feldman for 1999-2000.

III. The Sunset to Dawn Review Including New ORU Candidates: Academic Year 1999-2000

The Requests for Proposals (RFPs) for creation and renewal of ORUs were issued in the Summer 1999, with a deadline for the submission of materials in mid-December. While the focus of the proposal was to be on the future, the RFP for existing ORUs did ask for some historical narrative covering the accomplishments of the past five years. (A sample RFP may be found in Appendix II.)

The existing ORUs to be reviewed included 24 units reporting to the Office of the Vice Chancellor for Research and eight reporting to the Dean of International and Area Studies. The 1999-2000 permanent budgets for these ORUs are shown in Table III (below).

During late July and early August 1999, meetings were held with ORU directors and proposers to discuss the RFP requirements and the review process. Units were encouraged to contact each other to explore potential synergies and joint programs and to consider combining administrative functions for efficiency. They were also encouraged to contact the unsuccessful preproposal sponsors for possible partnerships.

Table III: ORGANIZED RESEARCH UNITS
PERMANENT ADJUSTED BUDGETS
Core Funds, July, 1999

Unit Name	Unit Director	TOTAL Permanent Funds
BIOLOGICAL SCIENCES		
Cancer Research Laboratory	James Allison	405,821
ENGINEERING		
Electronics Research Laboratory	Jeffrey Bokor (Acting)	488,616
Earthquake Engineering Research Center	Jack Moehle	496,185
Engineering Systems Research Center	Masayoshi Tomizuka	268,521
Environmental Engineering & Health Science Lab	James Hunt	203,144
INTERNATIONAL & AREA STUDIES		
Institute of International Studies	Michael Watts	259,472
Center for Latin American Studies	Harley Shaiken	72,511
Center for Middle Eastern Studies	Nezar AlSayyad	0
Center for Slavic and East European Studies	Victoria Bonnell	178,943
Center for South Asia Studies	Thomas Metcalf	74,720
Center for Southeast Asia Studies	Aihwa Ong	63,773
Institute of East Asian Studies	Frederic Wakeman	433,309 †
Center for Chinese Studies	Wen-hsin Yeh	98,703
Center for Japanese Studies (not an ORU)	Andrew Barshay	28,776
Center for Korean Studies (not an ORU)	Hong Yung Lee	16,446
PHYSICAL SCIENCES		
Berkeley Seismological Laboratory	Barbara Romanowicz	543,630
Space Sciences Laboratory	Robert Lin	674,841
Radio Astronomy Laboratory	Leo Blitz	700,512
Center for Pure & Applied Mathematics	Alexander Chorin	69,344
Theoretical Astrophysics Center	Jonathan Arons	93,538
SOCIAL SCIENCES GROUP A ††		
Center for the Study of Law and Society	Robert Kagan	96,830
Institute of Cognitive Studies	Stephen Palmer	127,477
Institute of Personality and Social Research	Robert Levenson	184,984
Institute of Human Development	Philip Cowan	384,049
Earl Warren Legal Institute	Franklin Zimring	91,090
Institute for the Study of Social Change	Pedro Noguera	136,340
SOCIAL SCIENCES GROUP B ††		
Archaeological Research Facility	Margaret Conkey	105,914
Institute of Business and Economic Research	Carl Shapiro	234,000
Center for Environmental Design Research	Edward Arens	119,035
Institute of Urban and Regional Development	Judith Innes	274,026
Institute of Management, Innovation, & Organization	David Teece	142,605
Survey Research Center	Henry Brady	696,201
Institute of Industrial Relations	James Lincoln	1,010,642
Institute of Governmental Studies	Bruce Cain	714,399

† Includes permanently budgeted endowment funds

†† For purposes of the external review, the social sciences were divided into two groups because of their large number.

External Review Process

The units being reviewed were divided into seven groups by discipline. (Given their number, the 16 existing and proposed social science units were split into two groups.) During late January and February 2000, 31 faculty experts, constituting seven separate review panels, were brought in from across the country to hear presentations from each of the established and proposed ORUs. (The format was 30 minutes of formal presentation followed by 30 minutes of discussion.) Membership on the panels was drawn primarily from recommendations received from ORU directors and proposers, as well as from cognizant deans. Overall, 17 of the 31 external panelists were from other UC campuses, four were from other West Coast universities, and ten were from other educational institutions across the nation. (Appendix III lists the names and affiliations of the external reviewers and their panels.) Table IV provides summary detail of the panels.

Table IV: External Review:
Established and Proposed ORUs

Area	Proposals to be Reviewed	Number of External Reviewers	Date(s) of Review
Biological Sciences	4	4	January 28
Social Sciences - Review Group B	8	5	February 8 - 9
Engineering	4	4	February 11
Arts & Humanities	3	3	February 14
Social Sciences - Review Group A †	8	5	February 18 - 19
International & Area Studies †† (plus 2 centers)	10	5	February 22 - 23
Physical Sciences	8	5	February 28 - 29
Total	45	31	

† For the external review, the two proposed ORUs in the social sciences were placed in Group A.
†† The proposal for an Institute of European Studies was also reviewed. A proposal to establish IES as a multicampus ORU was under review at the Office of the President; it was denied after the campus deadline for new ORU proposals. Given this situation, the Committee made the exception to include IES with the other proposed ORUs, but with the caveat that any funding would be the responsibility of the IAS Dean.

In addition, two of the six Academic Senate members of the review Committee were assigned to each panel, such that each member attended approximately one-third of all the presentations. Attending all presentations were the Vice Chancellor for Research, the Assistant Vice Chancellor for Research, and cognizant Deans.

The 31 external reviewers were sent all relevant proposals as well as other background material by mail. When each panel arrived on campus, the Vice Chancellor for Research briefed them on the overall process prior to the start of the individual unit presentations. As in the earlier preproposal phase, the reviewers' charge was to focus primarily on "Quality" and "Strategic Advantage." Additionally, they were asked to consider the extent and impact of junior faculty involvement; prospects for significantly increasing the current levels of extramural support, or for substantial support from new external sources; public service, including ongoing and/or proposed programs for K-12 educational outreach efforts; and administrative efficiencies which could be achieved, e.g., by merging specific ORUs.

Each panel was asked to prepare 1) written evaluations of the major strengths and significant weaknesses of each of the units, along with a qualitative rank ordering; 2) recommendations for establishment of new ORUs and suggested funding levels; and 3) recommendations for possible redistribution of budgets among existing ORUs, based on the total budget for that discipline and the new $1M. Prior to the panel's departure, an exit discussion of its findings and recommendations was held with the Vice Chancellor and the Assistant Vice Chancellor for Research and the two assigned Committee representatives.

Overall, the external panels were impressed with the quality of the great majority of the ORUs, noting the richness, diversity, and excellence of their research. Within that framework, however, the panels were able to suggest some clear evaluative guidelines for the Committee to consider in the final phase of the Sunset to Dawn Review.

As a last step in this part of the process, each ORU director and proposer was sent a copy of the external panel's findings (without any of the specific recommendations) and offered the opportunity to comment for the record and for the benefit of the Committee.

Funding: Guidelines and Issues

Given the limited funding, the following budget guidelines were developed:

(a) Proposed new ORUs could request up to $200k in budgetary support, which would come primarily from the new $1M permanent funding allocation;

(b) In order to provide some "equity of opportunity," established ORUs could also request additional resources. ORUs with budgets above $200k could request up to $100k; ORUs with budgets below $200k could request an increase of $100k, or a total budget of $200k, whichever was more; and

(c) Augmentations to the budgets of existing ORUs should be offset to the extent possible by reductions in budgets of other ORUs within the same disciplinary group. That is, the total current budget of each disciplinary group would at minimum remain intact, so that if funding were withdrawn, for example, from one or more social science ORUs, that funding would be redistributed among the existing and proposed social science units.

The four Engineering ORUs have represented a special budgetary situation for the last several years, continuing through this review process. Their budgets have been in flux due to the fiscal impact of the revisions in the Office of Management and Budget's Circular A-21 *Cost Principles for Educational Institutions for Grants, Contracts and Other Agreements*, and the implementation of the federal Cost Accounting Standards for educational institutions. Prior to 1996, the College of Engineering recouped research management service costs through fee assessments against each contract or grant. Revisions to the A-21 policy disallowed this practice for federal awards. Encouraged by the Department of Health and Human Services, our cognizant audit agency, the College moved to establish each individual ORU as a "major project" in order to direct charge for these administrative services. Unfortunately, this plan was disapproved by that same agency in 1997, and the campus was faced with the situation of having to cover the "red-lined" or disallowed costs. This is a major issue, since all of the sponsored research in the College of Engineering is administered in these ORUs, with awards in 1998-99 exceeding $77M. Temporary campus funds have been allocated each year to cover these expenses for federal awards, which are estimated to be approximately $600k for these units in 1999-2000.

These regulatory changes have severely impacted the Engineering ORUs' abilities to manage their extramural awards, much less encourage their growth. This is most pronounced in the Electronics Research Laboratory, where research funding is increasing by nearly 30% each year. The campus has yet to find a permanent solution to this problem, relying to date on temporary year-end allocations. It was felt that this Sunset to Dawn Review process was not the venue to address these budgetary deficiencies, given their magnitude and uniqueness to Engineering.

IV. Final Review Process: March-June 2000

The Committee met almost weekly from March through May. The Senate members assigned to each of the external panels reported on the presentations and panel recommendations, and the Committee reviewed and discussed the responses received from the ORUs to these findings. The Committee was also concerned about broader issues such as the lack of representation of ORUs in the Division of Arts and Humanities of the College of Letters and Science and the current distribution of resources among excellent, existing ORUs within a disciplinary area. As deliberations proceeded on each disciplinary area, the cognizant dean or deans were invited to meet with the Committee to discuss the tentative findings and recommendations, having previously been sent copies of the external panel comments. Their concerns were considered in the final recommendations that appear in this report. Lastly, the almost-final draft report and relevant disciplinary sections were sent to all campus deans for their information and comment.

Dawn: New ORUs

The eight proposed ORUs recommended for establishment are all of high Quality and Strategic Advantage, with broad representation across disciplines. The Committee is pleased to recommend for inclusion in this group two new ORUs in the arts and humanities, an area where ORUs have not existed previously. Table V shows the eight proposals recommended for ORU status and their core faculty.

With the new funding for ORUs limited to $1M, the budgetary allocations recommended are somewhat spartan. However, the Committee feels that the recommended allocations will enable the new ORUs to get started and should give them the ability to leverage supplemental extramural funding over the coming years. With a few exceptions, the average recommended allocation is $100k, the only specific restriction being that no more than $25k of this budget may be used for release time or summer salary for faculty.

In addition to these new ORUs, the Committee recommends that one other proposal be funded as an Organized Research Project, with support for an initial three-year period. While the proposed Center for the Tebtunis Papyri does not fit the criteria for an ORU, it offers a unique opportunity for a valuable and potentially significant interdisciplinary research project.[1]

Finally, for two of the proposed ORUs not recommended for establishment, the Committee recommends that they be funded as organized research initiatives, with modest, temporary seed monies to enable them to develop programs that could then be sustained by extramural resources.[2]

Sunset to Dawn: Existing ORUs

A number of factors affect the individual unit findings and recommendations that follow in the next section. First, it was clear that some of the existing units do not meet the current University criteria for ORUs, but further discussion with the cognizant deans

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1 Funding is recommended for an initial three years at $50k per year, with a possible project lifespan of ten years at this funding rate, following a successful review at 2 ½ years. Upon termination of the project, those funds will be redirected to ORUs or organized research initiatives in the arts and humanities.
2 Funding will be provided over a three-year period up to an aggregate of $100k, contingent upon an approved program plan.

	Proposer and Core Faculty
ARTS AND HUMANITIES	
Consortium for the Arts	Charles Altieri, English and Art Practice Shawn Brixey, Art Practice Anthony Cascardi, Spanish, Rhetoric & Comp Lit T.J. Clark, Art History Harrison Fraker, Architecture Shannon Jackson, Theater Arts David Wessel, Music
Center for the Study of Sexual Culture	Michael Lucey, French and Comparative Literature Daniel Boyarin, Near Eastern Studies Judith Butler, Comp Lit & Rhetoric Caren Kaplan, Women's Studies Leslie Kurke, Classics & Comp Lit Sharon Marcus, English Linda Williams, Film Studies & Rhetoric
BIOLOGICAL SCIENCES	
Center for the Study of California Environments & Biological Diversity	Mary Power, Integrative Biology David Lindberg, Integrative Biology Rosemary Gillespie, Environ Sci, Policy & Mgmt Brent Mishler, Integrative Biology James Patton, Integrative Biology Ellen Simms, Integrative Biology David Wake, Integrative Biology
Functional Genomics Laboratory	John Ngai, MCB (Neurobiology) Tito Serafini, MCB (Cell & Devel Bio) (left, 6/30/00) Gerald Rubin, MCB (Genetics) Robert Tjian, MCB (Biochem & Molecular Bio)
INTERNATIONAL AND AREA STUDIES	
Center for African Studies	Gillian Hart, Geography Mariane Ferme, Anthropology Louise Fortmann, ESPM and Geography Percy Hintzen, African American Studies Tabitha Kanogo, History Donard Moore, Anthropology Michael Watts, Geography
PHYSICAL SCIENCES	
Center for Atmospheric Sciences	Inez Fung, Earth & Planetary Science & ESPM Dennis Baldocchi, Environ Sci, Policy & Mgmt Kristie Boering, Earth & Planetary Science & Chemistry Ronald Cohen, Earth & Planetary Science & Chemistry Allen Goldstein, Environ Sci, Policy & Mgmt Robert Harley, Civil & Environ Engr John Harte, Energy & Resources Group & ESPM
Center for Integrative Planetary Science	Geoffrey Marcy, Astronomy Frank Shu, Professor, Astronomy Raymond Jeanloz, Earth & Planetary Science Mark Richards, Earth & Planetary Science Jere Lipps, Integrative Biology Richard Saykally, Chemistry Robert Lin, Physics

SOCIAL SCIENCES

Center for Child and Youth Policy	Mary Ann Mason, Social Welfare Mike Austin, Social Welfare Jill Duerr Berrick, Social Welfare Bruce Fuller, Public Health Neil Gilbert, Social Welfare Sylvia Guendelman, Public Health Richard Scheffler, Public Health

FINAL RECOMMENDED NEW ORGANIZED RESEARCH PROJECT

Center for the Tebtunis Papyri	Donald Mastronarde, Classics Susanna Elm, History Mark Griffith, Classics Erich Gruen, History and Classics Cathleen Keller, Near Eastern Studies Robert Knapp, Classics Carol Redmount, New Eastern Studies

revealed certain historical circumstances that must be considered in any recommendations. Second, the Committee felt that it was important to try to achieve a more equitable fiscal distribution by some rearrangement of resources among units within the same disciplinary area, with a goal of approximately $150k minimum for the budgets of all established units that reviewed very positively. Third, as noted earlier, the Committee recognized the special situation faced by the Engineering ORUs with federally disallowed costs, and that the resources available to the Committee could not begin to address these budget needs. Fourth, the Committee identified projects within three existing ORUs of sufficient merit that they are being recommended for temporary budget allocations as organized research initiatives. Finally, two ORUs were special cases. The Virus Laboratory Director felt that a review of the unit would not be useful, and instead recommended its disestablishment. This ORU had long since outrun its mission, instead serving primarily as an administrative support unit within the Department of Molecular and Cell Biology. Resources have been divided between the Department and the Committee's overall ORU budget for reallocation. Also, the Center for Studies in Higher Education was not included in the review due to the serious illness and subsequent death of its Director.

A Summary View:

The summary budgetary recommendations by disciplinary area are shown in Table VI below.

Table VI: Recommendations
Overall Permanent Funding ($k)

Area	Changes to Existing ORUs	New ORUs	Total
Arts & Humanities	--	300	300
Biological Sciences	(100)	275	175
Engineering	Special Case		
Physical Sciences	38	200	238
Social Sciences	75	100	175
International & Area Studies	49	63	112
			1000

Overall, eight new ORUs are being recommended for creation, and one existing ORU will be disestablished. One proposal is being funded as an Organized Research Project, with a fixed period of funding. Temporary organized research initiative funding ($100k total over three years) is being provided to two proposed ORUs not recommended for establishment and to three projects within existing ORUs. It is hoped that this seed funding will enable the faculty to develop programs that would attract substantial outside resources and thereby sustain their activities beyond the period of campus support. The Committee is also recommending the approval of the requested Institute status for the Center for Slavic, Eastern European and Eurasian Studies and ORU status for the Institute of European Studies.[3]

Appendix IV provides a detailed chronology of the overall review process.

V. Specific Recommendations for Individual ORUs

The sections that follow provide the summary findings and recommendations for each of the proposed and existing ORUs:

A. Arts and Humanities

B. Biological Sciences

C. Engineering

D. International and Area Studies

E. Physical Sciences

F. Social Sciences

[3] The proposal to establish IES was under review at the Office of the President as a multicampus ORU; it was denied after the campus deadline for new ORU proposals. Given this situation, the Committee made the exception to include IES with the other proposed ORUs, but with the caveat that any funding would be the responsibility of the IAS Dean.

Academic Senate/Administration ORU Review Committee
1998 - 2000

Respectfully submitted:

William D. Clemens
Professor William Clemens
Department of Integrative Biology
Representing: Graduate Council

6/15/00
(Date)

Professor Keith Gilless
Department of Environmental Science, Policy & Management
Representing: Committee on Academic Planning
& Resource Allocation

6/15/00
(Date)

Richard Marrus
Professor Richard Marrus
Department of Physics
Representing: Committee on Research

6/15/00
(Date)

Karlene H Roberts
Professor Karlene Roberts
School of Business
Representing: Committee on Educational Policy

6/15/00
(Date)

Mary P. Ryan
Professor Mary Ryan
Departments of History and Women's Studies
Representing: Committee on Academic Planning
& Resource Allocation

6/15/00
(Date)

Anne Wagner
Professor Anne Wagner
Department of History of Art
Representing: Budget Committee

15 June 2000
(Date)

Joseph Cerny
Professor Joseph Cerny
Department of Chemistry
Vice Chancellor for Research

June 15, 2000
(Date)

External Review Panels	Panel Coordinator
Arts & Humanities	
Dean Jill Beck School of the Arts University of California, Irvine	
President John D'Arms American Council of Learned Societies New York, New York	*
Professor Carla Freccero Department of Literature University of California, Santa Cruz	
Biological Sciences	
Chair Barry Bowman Department of Biology University of California, Santa Cruz	
Vice Chancellor Zach Hall Research Affairs University of California, San Francisco	*
Associate Dean William McGinnis Natural Sciences & Department of Biology University of California, San Diego	
Chair Gary Polis Division of Environmental Sciences University of California, Davis	
Engineering	
Professor Ian Buckle Department of Civil Engineering University of Nevada, Reno	
Chair Daniel Chang Department of Civil & Environmental Engineering University of California, Davis	
Professor Sanjoy Mitter Laboratory for Information and Decision Systems Massachusetts Institute of Technology	
Professor Robert Skelton Department of Applied Mechanics & Engineering Sciences University of California, San Diego	*

External Review Panels	Panel Coordinator

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Director Jere Bacharach
Jackson School of International Studies
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Director Richard Brecht
National Foreign Language Center
Johns Hopkins University

Professor Stephan Haggard
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Dean Pauline Yu *
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Chair Ferdinand Coroniti
Department of Physics and Astronomy
University of California, Los Angeles

Senior Associate Dean Greg Forest
College of Art and Sciences
University of North Carolina at Chapel Hill

Director Joseph Miller
Lick Observatory
University of California, Santa Cruz

Director John Orcutt
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Scripps Institution of Oceanography
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Dean Mark Thiemens *
Division of Natural Sciences
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External Review Panels	Panel Coordinator
Social Sciences (Review Group A)	
Professor Patricia Churchland Department of Philosophy University of California, San Diego	
Director Kenneth Dodge Center for Child and Family Policy Duke University	
Dean Ronald Feldman Columbia University School of Social Work	
Director Edward Laumann Ogburn Stouffer Center University of Chicago	*
Dean Ray Solomon Rutgers School of Law	
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Senior Vice President Norman Bradburn National Opinion Research Center University of Chicago	*
Director David Sears Institute for Social Science Research University of California, Los Angeles	
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