

An Oral History of

# JAMES ARNOLD, KEITH BRUECKNER, and STANLEY CHODOROW

On June 10, 1998

1   **ARNOLD:** What I thought might make sense—and you can argue with me—was that we'd start  
2   with how we both came here in chronological order. And then switch; continue with  
3   chronological order in how the departments were built—as Keith got ahead of me—and we see  
4   how far we get.

5   **CHODOROW:** Okay, very good. And I will try to make certain that we stay on point. And ask  
6   what questions occur to me as a historian and as an intellectual historian, okay?

7   **ARNOLD:**     Okay. Well, then, I launch. Right, I was at the University of Chicago in the great  
8   years, and in the course of that time I took two trips west to the Mississippi. It's amazing to think  
9   how little one traveled in those days. And by the time I had done that, I decided I want to be in  
10  the West. I really like the West Coast much better than the East, where I grew up, or the Middle  
11  West.

12  When I went to Princeton, in '56, I'd been working with Libby on radio carbon dating, and that  
13  involved the synthesis of isotopes by cosmic rays in the atmosphere. So, the things I was doing  
14  took off from there, and by '57-'56, rather—I was working on an interesting isotope from that  
15  point of view, beryllium 10, which has a half-life of 1.5 million years in contrast to the thousands  
16  of years for carbon 14. And the best place to look for that was, clearly, deep sea sediments. And  
17  that got me corresponding with several old buddies at Chicago. Ed Goldberg was the first such  
18  person to arrive here, then Hans Suess and Harmon Craig in '56. And so, I was corresponding  
19  with them and calling them about deep sea sediments, and they were sending me deep sea  
20  sediments, and I got to know Gustaf Arrhenius at that time. Then somewhere along the line, two  
21  things happened at once. One was that Roger and friends started talking about UCSD, what  
22  became UCSD, and the other was that my name began to be bandied about.

23  So, I'd like to begin a little more detailed account. In early '57, when these people invited me to  
24  come out here and give a seminar because we had just about discovered beryllium 10 in the  
25  deep sea sediments, at that time—and I'll just take a minute to describe an event that was, for

26 me, quite important then. Came out here, scheduled for a seminar at 4 o'clock. Ten minutes  
27 late, Roger Revelle walked in. That's not much of a surprise to those who knew Roger Revelle.  
28 And when Roger walked in, Harmon Craig handed him a manuscript of a paper that Ernie  
29 Anderson and I—Ernie was my other young friend from carbon 14 days—were writing about the  
30 distribution of carbon 14 in nature. And we had already agreed with Hans Seuss and Revelle,  
31 though I hadn't met Revelle, to exchange manuscripts. So, there's Roger, came in late, sitting  
32 with a manuscript on another topic in his hands. When I had finished the seminar, Roger asked  
33 the best questions, a couple of very penetrating questions. Then I went down and shook his  
34 hand, got introduced, and as we were going out of the lecture room, Roger said to me, "There  
35 are two serious mistakes in your paper." "Huh?" So, we talked about it. One he had  
36 misunderstood, he hadn't read carefully enough, but the other was a serious mistake. And I said  
37 to myself, "Who is this guy, you know, comes in late, does all this sort of thing? And, you know, I  
38 know other smart people, but this guy is the equivalent of the president of the university." I tried  
39 to picture the president of Princeton operating at that level—blew my mind. So, I started with  
40 that impression of Roger. We talked a little bit then, didn't go terribly far. Yes, I was interested—  
41 it was all just talk at that time.

42 So now, coming forward, Roger and Ed Goldberg came to Princeton on their way somewhere,  
43 called me up, and we had a long talk. And it was still mostly conversation, but I was certainly  
44 more interested, and I agreed to come out here and spend the summer of '57 doing some  
45 research that I needed to do with a graduate student named John Merrill—it was basically his  
46 Ph.D. thesis—and then we'd see. So, the end of the summer, I remember, Roger called me into  
47 his office and asked me what I wanted to talk about first, and I said money. And he laughed, he  
48 said, "That's all professors ever want to talk about." But we talked about a good many things. I  
49 was still very uncertain, because the decision to locate the campus here was still up in the air,  
50 way up in the air. And also, if it didn't go that way, I would find myself in a lovely but isolated  
51 situation, lots of friends to talk to but not a full university.

52 Okay, now I go back to Princeton and we started corresponding—and somewhere in your files,  
53 Brad, if it isn't, it should be—is a letter I wrote Roger, probably in the spring of '58, with 12  
54 numbered questions in it, da da da da da. The only one I remember particularly was the  
55 question of whether political activity was possible for a professor on this campus. Because I had  
56 always been rather active politically and there was still an oath at that time. Watered down, but  
57 still an oath. So, Roger replied to these 12 questions, and his answer to that one was that, very  
58 recently, a professor at the University of California—some other, maybe UCLA, I don't

59 remember—had been elected to \_\_\_\_\_ [*inaudible*] had been elected to Congress and not on the  
60 Republican ticket. So, I took that as a good sign, and we went on from there.

61 I then went to my chairman and told him that if I got an offer, I was coming. And in the fall of '57,  
62 we came. Harold and Frieda Urey beat us here by two weeks, or something like that. And just  
63 for the record, I had nothing whatever to do with bringing Harold here. It was the University of  
64 Chicago which accomplished that feat by telling him that at 65 it was time for him to retire. And  
65 Roger, hearing the rumor, flew to Chicago, I understand, and closed the deal very quickly. And  
66 so, we were actually welcomed by the Ureys, who had partly settled in. And we settled down in  
67 a cottage that's now T31 above the main buildings, one of the little rickety cottages that they  
68 house summer visitors in. And the cottage T29 was then inhabited by Hans Seuss and family,  
69 but they were in Germany, so the Ureys were camping there. And the Goldbergs in between.  
70 So, that's how we arrived and where we arrived. And I came with a mandate not to be chairman  
71 but to find a chairman. And I think I'll drop the story at that point.

72 **CHODOROW:** Let me ask one follow-up question from your story. It's very clear that, in the  
73 mid- to late-'50s, you had built a set of scientific relationships with people who ended up here, or  
74 who were in the process of ending up here.

75 **ARNOLD:** Yes, correct.

76 **CHODOROW:** So that, in part, the attraction of the place was that you would join a research  
77 community with which you already were associated.

78 **ARNOLD:** That is correct, and the double advantage that Roger was also a part of that  
79 research community. We were working on similar things. So, there was the sense I could  
80 evaluate what he was made of, in that respect, and was very impressed with what I saw.

81 **BRUECKNER:** In 1958, I was a professor at the University of Pennsylvania, and in  
82 December of '58, I came to do some consulting work at General Atomic. I don't remember  
83 exactly the sequence, but Leonard Lieberman and Carl Eckart, I think they heard me give a  
84 colloquium. And they met me for lunch with Revelle—they drove up to General Atomic to pick  
85 me up. And we went and had lunch down at the Valencia, and Revelle started giving me his  
86 description of what was going to happen. By that time, I believe the land had been passed—the  
87 bulk of the land had been passed to the university—

88 **ARNOLD:** When in 1958 was that? It was the fall of '58 was when the -

89 **BRUECKNER:** December '58.

90 **ARNOLD:** Yeah, okay—when the land was voted.

91 **BRUECKNER:** That's right. Anyway, I talked to Revelle at lunch, then I think after that we  
92 went to his office, and I talked to him for another few hours. And at the end of that time, I  
93 decided I was going to try to pursue his dream: create a new university here. And he described  
94 its composition and objectives, and that convinced me. He was a very impressive and  
95 compelling man.

96 **ARNOLD:** Indeed.

97 **BRUECKNER:** And then I came from Pennsylvania in September of 1959, and started  
98 recruiting in physics. So, the start here was very simple for me, compared to the much more  
99 involved procedure that Jim had gone through.

100 **ARNOLD:** I might break in and mention one point there—maybe you were coming to it  
101 anyway. The presence of General Atomic was an appreciable plus out here at that time. They  
102 had some very good people and particularly in physics, but cryogenics, all sorts of things. They  
103 had recruited Kurtz [?] and Bob Duffield, a chemist, and all sorts of good folk that they held onto  
104 for a while. And the equipment, certainly – well, you're a theorist but I'm an experimentalist—we  
105 certainly took advantage of their facilities. They were quite generous in the transition period.

106 **BRUECKNER:** But that had very little effect on my coming here.

107 **ARNOLD:** No, I understand that, but it might've had a little effect on your recruitment.

108 **CHODOROW:** You came because there was a dream that had been sufficiently described  
109 by Roger, with the force of his personality, to persuade you to come out.

110 **BRUECKNER:** Yes.

111 **CHODOROW:** And when you came out, there was—were there any physicists on the  
112 campus, at that time?

113 **BRUECKNER:** Well, there were the physicists at Scripps.

114 **CHODOROW:** The geophysicists.

115 **BRUECKNER:** Yeah, Eckart and Lieberman and -

116 **ARNOLD:** Well, Fred Spiess—I guess you could say that.

117 **CHODOROW:** Fred had been trained as a physicist, from what I understand, but had drifted  
118 into oceanography. And so, you came out as chair of this department?

119 **BRUECKNER:** No, I didn't. I came out with the understanding I would try to organize the  
120 department. But in that first few months, I contacted a lot of people I knew, and one of them—  
121 Walter Kohn, who finally did come—he said he would come if I became chairman. So, I hadn't  
122 taken that title or role until some months after I came.

123 **CHODOROW:** Why was it, do you think,—from your personal point of view—was it that  
124 Roger thought that you would be a good person to organize the department of physics? Which  
125 he must have regarded as a premier department at the end of the '50s.

126 **BRUECKNER:** Well, I suppose he did. He never said that directly to me, he just gave me a  
127 great deal of authority, go ahead and recruit in physics. And the people that came here – well,  
128 my first office was the director's office at the aquarium. There were no facilities then. But the first  
129 people that came, there were a lot of them that came \_\_\_\_\_ [*inaudible*] recruiting. I knew  
130 people throughout the United States in physics, experimental as well as theoretical.

131 **CHODOROW:** So, you had a wide network of first-class people.

132 **BRUECKNER:** Yes, yes. So that's how it started.

133 **CHODOROW:** How old were you?

134 **BRUECKNER:** I was 35. \_\_\_\_\_ [*inaudible*]

135 **ARNOLD:** So was I. Well, I was 36 in 1959, but I was 35 when I came.

136 **CHODOROW:** If you think about other institutions that were founded within, let's say, the  
137 next 5 to 10 years, which would have included Irvine and Santa Cruz, but also, University of  
138 North Florida and a couple of other places -

139 **ARNOLD:** State University of New York.

140 **CHODOROW:** Stony Brook was—

141 **ARNOLD:** Stony Brook started out with a bit of a rush—didn't last so well, but.

142 **CHODOROW:** Yeah, but when you think about those other institutions, in your field—and  
143 this is really a question for both of you—what was the difference between the way they formed  
144 their first departments, their early departments in your field, and the way you under the guidance  
145 of Roger did the same here?

146 **BRUECKNER:** Well, we knew that the university—and its regional planning—it was to start  
147 as a graduate school. And accordingly, the kind of positions that I could offer were top  
148 professorships at reasonable salaries. Also, it was known from Revelle that along with the  
149 buildings that were planned, there was a lot of equipment money which came with those  
150 buildings. So then, when the people came here they knew that there was a large backlog of  
151 financing for the laboratories. I think those two features and then the powerful influence of  
152 Revelle and the location here. Also, the fact that it was part of the University of California, a  
153 great institution, and this had been \_\_\_\_\_ [*inaudible*] by the Regents. The University of  
154 California, after all, is one of the great universities of the world, considered as a whole. And with  
155 that powerful backing, and the quality of the established universities—Berkeley and UCLA,  
156 particularly—and the idea that we could be as good as they were. That combination. Going to  
157 Stony Brook—there was really nothing in the state of New York which was comparable to  
158 Berkeley or UCLA or Florida. So, it was that combination of circumstances, I think, that -

159 **ARNOLD:** I agree. I would add one other thing, if I might. Roger, his dream was certainly  
160 very important, and I agree totally with what you said about, that the label "University of  
161 California" and the thought that we were going to really try to reach the level of the other great  
162 branches. But one of the things, in addition—it certainly was very important to me despite my  
163 digging in my heels at the start—was the fact that I could have, potentially, a major effect on this  
164 institution. I was coming from Princeton, which is 200 years old. The chance that I would be  
165 anything but a small footnote in the history of that institution was overwhelming. And here, had a  
166 chance to stretch yourself. It was a challenge, a very interesting challenge.

167 **CHODOROW:** Let me ask a related question about these departments and the founding of  
168 them. I had the experience, early in my career, of being on a team that looked at the history  
169 department at Irvine, obviously from the experience of the foundation of this department. And I  
170 saw a very striking difference of strategy in the way in which the two departments had been  
171 created. At Irvine, in history and, so far as I could understand, other departments, they hired

172 across the full range of ranks of faculty from the very beginning so that in several cases, junior  
173 faculty actually arrived before senior faculty did. Whereas, here, as I understood it—and it  
174 certainly happened in my department – a cadre of senior faculty were hired, they were  
175 established, and then they began to hire junior faculty.

176 **BRUECKNER:** Right, that's true.

177 **CHODOROW:** Is that your perception? And what difference did that make?

178 **ARNOLD:** A lot.

179 **BRUECKNER:** I think a tremendous difference.

180 **CHODOROW:** Explain it.

181 **BRUECKNER:** If you have the half-a-dozen, first key people I hired were world famous. They  
182 were outstanding people. And they already put a clear imprint on the nature that physics would  
183 have here. And if you start with the assistant professors and they're involved in selecting the  
184 others, the first assistant professors don't come the same \_\_\_\_\_ [*inaudible*], they're not fully  
185 established yet. They don't have the names, the reputation \_\_\_\_\_ [*inaudible*] first people I  
186 hired. So, that was very, very important.

187 **ARNOLD:** And equally important, I think—maybe I'm again breaking in too soon—is these  
188 are people who know the field very, very well. When it comes to hiring the junior faculty, they  
189 have a pretty clear picture of where the bodies are buried and what kind of criteria to use. If  
190 someone like Roger Revelle, let's say, with all his abilities and vision would have tried, on his  
191 own, to judge which were the assistant professors to put his money on. Another thing, of course,  
192 we were—I never believed this at the time—but we were really favored over those institutions,  
193 because we had the opportunity. Clark Kerr and Roger, I think it should be said, never had a  
194 very easy relationship. They're very different kinds of people. Nonetheless, Kerr put a lot of  
195 chips on the table at UC San Diego, which he did not do to anything like the same degree at  
196 Irvine and Santa Cruz. So that—I suppose you had a similar number—I had 12 FTEs in my  
197 pocket, at one point, for the chemistry department. Looking back on that, Keith, it's just unheard  
198 of, I mean nobody did -

199 **BRUECKNER:** \_\_\_\_\_ [*inaudible*] but also senior professors.

200 **ARNOLD:** Senior, that's right, that's right. We were being pushed, eventually. Our first  
201 assistant professor was Stan Miller—that wasn't so bad. But just as you said, we, too, had a  
202 cadre of people and that's \_\_\_\_\_ [*inaudible*]. But Kerr actually came down here to see me—  
203 oh, 18 months ago, something like that. Flew down here—and some others, not just me but  
204 three or four others—because he's writing a book on the history of UC and his time. And I was—  
205 jeez, you know, he's in his 90s. Steel trap memory—I wish mine was as good. Anyway, we had  
206 a good talk.

207 **CHODOROW:** Let me ask a question about the intellectual framework in the forming of these  
208 departments. You said that when you had appointed, recruited to the campus a certain number  
209 of senior faculty, they put a stamp on the department that said very clearly to the world what  
210 kind of physics were going to be done here. Situate the kind of physics in the world of that time;  
211 what was going on in the field of physics as a discipline, and what piece of it had you carved  
212 out, and why had you carved that out?

213 **BRUECKNER:** Well, I chose the fields of elementary particle theory, elementary particle  
214 experiments—although they would have to be done somewhere else—plasma physics, and low  
215 temperature physics. I chose those four because, with the exception of the energy physics, they  
216 could develop facilities here. Of course, in that period, I also got Margaret and Geoffrey  
217 Burbidge to come here. But of course, they came not because there were facilities here but  
218 there were facilities close by. They were some of the later recruited by me, after the beginning.

219 **CHODOROW:** Mm-hmm, after the first.

220 **BRUECKNER:** Yeah, so the idea of adding those two astronomers was past the original  
221 pattern that I had chosen.

222 **CHODOROW:** And so, some part of your decision to recruit in those fields was related to  
223 your own interests, obviously, but also what was possible in a new campus in this location.

224 **BRUECKNER:** It was a combination of what I knew about the world of physics, which was  
225 extensive, and then the pattern that was set by being here with no major facilities nearby.

226 **CHODOROW:** What about in chemistry?

227 **ARNOLD:** Well, in chemistry, I think the vision developed a little more slowly, but we got a  
228 tremendous impetus in the direction that Keith was talking about, by the presence of Harold



229 Urey. I mean, he was one of the great men, you know? And at 65 still, as we all remember,  
230 tremendously vigorous. The notion was—to the original people here, and you spoke of Eckart  
231 and Lieberman—the equivalents in chemistry were Hans Seuss and Harold Urey, who certainly  
232 was not an SIO person but who came at the same time. So, the three of us were the core. We  
233 taught, and since physical chemistry was what we'd all originally been in, and since the earth  
234 sciences and oceanography were really strong, that a little bit influenced our early  
235 appointments.

236 But actually, the next appointment, and a very critical one, was one that you initiated, Keith, if  
237 you recall, when you asked me whether, since your department, your group, were interested in  
238 Maria Mayer or whether we were interested in Joe. And that was not a very hard question to  
239 answer. Joe and Maria were, you know, just wonderful people, and I had known them both very  
240 well in Chicago, and Harold Urey had known them better. And so, that was the first move. And if  
241 I may, it's your department but I feel so strongly about this. I don't know that it occurred to you or  
242 to Roger or to me not to offer Maria a professorship. But when you did offer Maria a  
243 professorship—12 years after she had done the shell theory, a woman of 55—that was the first  
244 regular faculty position she'd ever been offered in her life. And what I remember so much, I was  
245 shocked at that—I still am shocked at that. And I remember Willie Zachariasen—another old  
246 member of the circle—was at Caltech for the summer. He flew back to Chicago, made what Joe  
247 Mayer described to me as a princely counteroffer, and they thanked him and came. Which is  
248 exactly what you would've done or I would've done under the same circumstances. And I  
249 particularly wanted to dwell on that a bit because of your mention of Margaret and Geoff  
250 Burbidge. It was not an analogous case, quite. Nonetheless, those very distinguished people  
251 hadn't settled yet, and yet they were the news. I mean, B2FH was a big, big paper, and they  
252 were the news. And again, I'm rambling a bit, but why not? One question—Margaret Burbidge  
253 could not be appointed in physics because of the nepotism rules.

254 **BRUECKNER:** In Chicago.

255 **ARNOLD:** At Chicago or here.

256 **CHODOROW:** Or here.

257 **ARNOLD:** So—you might not remember that little thing—we had all these FTEs. Here was  
258 a woman who had written a paper on the origin of the chemical elements that was good enough  
259 for me and my colleagues. So, we told her that she didn't have to come to faculty meetings, but

260 we would cover—that lasted about a year or something, it gave way very, very fast. Of course, I  
261 knew that paper very well and knew them a little, and it was obvious that a somewhat feminist  
262 bias for the time was very cost-effective in our recruitment, because those two couples were -

263 **CHODOROW:** In the case of the Mayers, had Joe always been on the chemistry side?

264 **ARNOLD:** Yes.

265 **CHODOROW:** Because I always thought of him as on the cusp between chemistry and  
266 physics.

267 **ARNOLD:** Well, of course, he was, he became president of the American Physical Society,  
268 for god's sake. But his faculty appointments, I believe—Johns Hopkins, Columbia, and  
269 Chicago—were all in chemistry and he taught physical chemistry and all that. But yes, of  
270 course, he was on the cusp. That cusp is not a very sharp narrow peak, it's very broad. Lots of  
271 people on both sides. Solid state—I used to say Bernd Matthias was the best synthetic and  
272 organic chemist on the campus. He invented—discovered—whatever you like, new compounds.  
273 Where the hell is the boundary?

274 **CHODOROW:** One of the things that has come out in your conversation, Jim, is the Johns  
275 Hopkins/Chicago/Columbia/Princeton sort of circuit. And that it would appear that many of the  
276 people who ended up here had gone through that circuit. Do you want to comment on that  
277 background, and what was happening?

278 **ARNOLD:** Well, as far as I know, only Urey and the Mayers went through that whole circuit.  
279 Chicago was—not for Keith but for us—very much the center. Though if I come to the next  
280 person that we appointed, which was Bruno Zimm—he had been a student of Joe's at  
281 Columbia. Again, just to create an atmosphere a little bit, that's worth telling. We were in  
282 interested in biochemistry from the beginning. That was part of our plan. Biochemistry: very  
283 exciting—was and is. Looking around, we always tried to find somebody who was kind of a little  
284 bit attached to our world a little bit, so that the person didn't feel completely lost. And Bruno was  
285 a theoretical statistical mechanic of some reputation, and we got a call from Joe one day,  
286 who had found out from Bruno that he was looking around to leave the General Electric  
287 Company where he had been a polymer chemist for them for many years. Theorist and  
288 experimentalist—Bruno was marvelous at both. And the reason being that he was drifting into  
289 biology; he was doing these General Electric type things and he was moving away. And this is

290 another story about the atmosphere at the time, because I called Bruno, I knew him slightly, and  
291 he said, "Yes, indeed." I invited him out, and he said, "Well, I'll only have to charge you for half a  
292 plane fare because I'm going to interview at UCLA, also."

293 Okay, well, I knew the rules—as you did, Keith. It was okay for campuses to compete, but they  
294 had to keep each other informed if you started treading on somebody else's turf. And you had to  
295 not outbid them in salary. Those were the rules. So, I called Bill McMillan, who was the UCLA  
296 person, I told him that I talked—what I just told you. And he said, "Oh, well, I think we kind of  
297 have some sort of preemptive claim. A year ago, I appointed a committee of the department to  
298 study directions and who we could hire, and then two months ago we had a shortlist of  
299 candidates, and last month we selected Bruno Zimm as our first choice. And I've talked to the  
300 dean and the dean has given me tentative approval. So, we touched all the bases." And I said,  
301 "Well, I've talked to all my colleagues in chemistry. I have a unanimous vote that you're the next  
302 person we want to hire. I've talked to the chief campus officer, the equivalent. He's enthusiastic  
303 about it. So, we've done all that this morning." That's what we could do in those days.

304 And Bruno came and was also captured by the vision, and took a part then in part of the story  
305 that'll probably come soon, which is the development of biology/biochemistry. But at any rate,  
306 he came, we knew one thing we had to pay attention to: chemistry is a service [?] department,  
307 as mathematics is. There are lots of people in other fields, so we knew that in addition to going  
308 to what we regarded as hot areas—chemistry traditionally has these subtribes; physiochemistry,  
309 organic chemistry, inorganic chemistry, analytical chemistry, are the four big tribes. And we  
310 thought we could get along without analytical chemistry and that was quite correct, because it  
311 was going completely in the direction of instrumentation and physical chemists can administer  
312 instrumentation. But we put biochemistry in its place. So, we needed an organic chemist. We  
313 tried a couple of famous people and didn't get anyone, and I had a big stack of letters from  
314 people who had got word of what was up. And I don't know, there must've been 20 or 30  
315 organic chemists in the stack, none of whom looked interesting. But we had known Frank  
316 Westheimer from Chicago, here it comes again. Frank was, by this time, at Harvard. So I talked  
317 to Frank. Frank said, "We've got this interesting guy, here." Teddy Traylor had been an  
318 industrial organic chemist for about 15 years after he got his degree, and he decided that that  
319 was not what he really wanted to do. So, he went back, after getting a very good salary, and did  
320 a year with Frank, and Frank said, "Look at this guy." So, brought him out, liked him very much,  
321 and he was our second assistant professor, and for a while covered that area. And he was good  
322 enough to make tenure almost immediately, and give us something of a name. We did later go

323 out and hire some more people at the top. In inorganic chemistry, we didn't start fast at all; that  
324 didn't get going until several years later—maybe it isn't properly part of the story. We didn't do  
325 very well, at first, in that; we're doing just fine now, but in that early period when Keith and I  
326 were putting departments together. One question that I don't—Keith, when was—or how was—  
327 the physics department officially blessed and formally established, so that you could recruit  
328 graduate students and have a program and all that sort of thing?

329 **BRUECKNER:** I don't really recall, I think it must've been some of us had postdoc students  
330 come with us, so it was essentially going immediately.

331 **CHODOROW:** I can remember, from some of the records that I have looked at and things  
332 I've read, that the campus was approved by the Regents, I believe, in November of 1960, as a  
333 full-on campus. And, of course, Scripps had existed, so that you could've been departments  
334 under Scripps. But those degrees, as I remember, were actually offered by UCLA, not by UC—  
335 there was no UCSD, so it was a -

336 **ARNOLD:** Well, even more important and more troublesome, though they were very good,  
337 was that all our appointment files, the people you mentioned, Walter Kohn, Bernd Matthias, they  
338 all went through UCLA budget committee. And -

339 **CHODOROW:** Right, which must've been somewhat awkward.

340 **ARNOLD:** It was somewhat awkward. But I have to say that John Galbraith, who was the  
341 chairman of the committee when I was there—and I thought they were very good, you know,  
342 they played it straight, they weren't jealous and troublesome as someone might have imagined.  
343 I want to go back, if I may -

344 **CHODOROW:** Let me just finish the one thing which is that, I believe that you had students  
345 already enrolled prior to that approval. That in effect, you anticipated, by several months -

346 **BRUECKNER:** I think so. I'm not sure there were students. I know there were postdocs.

347 **ARNOLD:** We had one in our case, who was Don Crothers—came with Bruno Zimm, was in  
348 the National Academy nine years later. He was rather an exceptional student.

349 But I wanted to go back to my first arrival, because of something that I think is important in the  
350 history. Roger took me up to a meeting of the Board of Regents at Lake Arrowhead, toward the

351 end of '58. That was very interesting and very informative. We had the land; as you say, the  
352 formalities didn't come until later. But when the proposition to give us the land passed  
353 overwhelmingly, both in San Diego and La Jolla, we thought we were pretty in the clear, and  
354 you must've thought so, too. Anyway, what struck me there: met Regent Pauley. Regent Pauley  
355 was Roger's great opponent. A little bit senile but still a man of power and you could feel it when  
356 you talked to him. And his idea was to put UCSD in Balboa Park, which looks rather shocking in  
357 retrospect. But at any rate, he was not giving up. And two things struck me. One was that Roger  
358 had supporters and not-supporters in the Board of Regents. And the other was that you could  
359 tell instantly which Regents Roger liked and didn't like. And I said to myself, "Trouble. That's not  
360 a good situation." That was my first inkling of what we all hoped, and I think a good part  
361 assumed, that Roger would become chancellor. And the opposition in the Regents was certainly  
362 what prevented that. But at any rate, at that point I began to understand the politics of the place,  
363 which was not discouraging but motivating, really.

364 One person—I've been rambling much more than Keith—one person that I haven't mentioned is  
365 Martin Kamen. Bruno was getting into biochemistry, but he was not a card-carrying biochemist  
366 at that moment. We looked around for one, and Harold was the one who brought up Martin's  
367 name. But I knew Martin very well. He wrote the classic book on radioactivity, radioactive  
368 tracers in chemistry and biology, and had done a Ph.D. in chemistry at Chicago, not overlapping  
369 us at all—'70s, in the '30s. But anyway, the reason we didn't develop as quickly as Keith in  
370 physics is that we missed more often than we hit in the first year or so. We had all sorts of  
371 distinguished people that we wanted to bring, and some really thought about it but they didn't  
372 come, until Joe. And Martin was certainly a distinguished person. And I went to an American  
373 Chemical Society meeting, probably in '60, in Boston, with a list of possible recruits, most of  
374 them from Harvard and MIT, as you might expect, and got nowhere with them. But Martin called  
375 me. It was one of the characteristics of Martin Kamen, that he always knew the gossip all  
376 around the country. And so, he called me, and I thought, "Ah, this is too good to miss," and I  
377 went out to Brandeis, where he was at that time. And I think it took us about 15 minutes to be  
378 good friends. And he had his reasons for leaving, although he turned around and brought Nate  
379 Kaplan here, who had been his chairman. They were very good friends. But at any rate, that  
380 was an instant winner; he wanted to come, we wanted him to come. And it was at that point that  
381 we felt—I don't remember either exactly at what point we were blessed by the authorities to be a  
382 formal department. But at any rate, it was at that point that, in our opinion, we were ready and  
383 we started acting like a department. So, those things were somewhere about the same.

384 **CHODOROW:** Do you want to talk at all about the decisions that led to the foundation of  
385 biology as a third department?

386 **ARNOLD:** Sure, sure.

387 **BRUECKNER:** That happened before I was here.

388 **CHODOROW:** It had already happened when you got here.

389 **ARNOLD:** That's right.

390 **BRUECKNER:** Yeah, Dave Bonner was here, I'm sure.

391 **CHODOROW:** Oh, he was here?

392 **ARNOLD:** Yeah, that's true. It happened in an interesting way. We had not planned it,  
393 except that we knew there was going to be a biology department. It was obvious. And—oh no,  
394 am I going to forget another name? One of the staff people at Scripps walked into my lab one  
395 day—I didn't have an office, I just had a big lab facing the ocean. That was part of the recruiting  
396 game. Here I go getting distracted again because it was the seawall of the new Scripps building,  
397 and it's all glass, looking out on the pier and the ocean. And I had a technique that I applied  
398 several times with Bruno, for example. You pick up the great man at the airport and you say,  
399 "I'm going to take you to your hotel, but I've gotta drop by the lab for a few minutes." And I did it  
400 with Martin, too. And then you can always find something to do in the lab. And then, you take  
401 them to the hotel and by that time, especially if you did it in January or August, you were making  
402 points. Some of the people who were intending to be rather difficult were much less difficult after  
403 that experience.

404 So, okay, Dave Bonner. Bill Belser—it's come to me now, in fact he is now a professor at  
405 Riverside, for many years—he was a staff person at Scripps. And he walked in and said that he  
406 had heard that Dave Bonner was possibly available. And I said, "Who is Dave Bonner?" I knew  
407 Francis Bonner is his brother, who was a chemist and was, about that time, made chairman of  
408 the chemistry department in SUNY—in the State University of New York in Stony Brook. And I  
409 knew of Jim Bonner, who was at Caltech. But I'd never heard of David. So, he straightened me  
410 out and it sounded interesting and I showed the material to Roger and Roger was interested, so  
411 we invited Dave out. And Dave was a very different—here's not here to defend himself, so I  
412 think I should say something about it—he was a very different character from Keith and me. For

413 one thing, he was quite a lot older, in his 50s. For another thing, what really amazed me about  
414 him, he had Hodgkin's disease, which is, in those days, inevitably fatal. Here he was sitting in a  
415 very good professorship at Yale, but the idea of breaking away and starting a new department  
416 and –

**[END OF PART ONE, BEGIN PART TWO]**

417 **ARNOLD:** ...being shown around by Roger and by me—and others maybe—and also  
418 looking around at the possibilities for snorkeling and other things of that kind. And, in no time at  
419 all, I think his reaction was much like Keith's. He just thought, "This is great." And one of his  
420 great complaints about Yale was that it was stuffy. Oh, Princeton chemistry department was  
421 pretty stuffy too, I must say. But nobody could ever accuse David [Bonner] of being stuffy. The  
422 phrase "piss and vinegar" was a sort of favorite thing—that was something good was, or  
423 somebody good was "full of piss and vinegar."

424 We had a little space which we'd planned to use for ourselves for chemistry on the top of  
425 Sverdrup Hall; we had one floor of Sverdrup Hall. It was not very big; I think it's about 8 or 9000  
426 square feet. And into that we shoehorned the new arrivals in chemistry and the first few  
427 appointments. Dave brought John Singer with him from Yale, or planned to and did. And then  
428 later two assistant professors; Stan Mills is still here, he was one of them. And it was just a  
429 perfect fit as far as I was concerned and as far as our department was concerned, because  
430 when I was recruiting almost anybody I could take them up to the top floor in Sverdrup, and say,  
431 "Now, find the line between the chemistry department and the physics department and the  
432 biology department. It does not exist."

433 There was Bruno, there was Keith. There was Bruno, there was David. And they were talking all  
434 the time. We really made—I think this was true in all three departments—we really made  
435 tremendous strides there, which I'm afraid didn't last too well. But at any rate, in that early period  
436 it worked very well in breaking down the institution departmental barriers, which was part of  
437 Roger's dream. We went through a bottom period, but I think it's gotten better recently.

438 Anyway, that worked just fine. I think David lasted four years. I may be wrong, but it was  
439 something like that. Active till the last. Every once in a while, he'd go off for some radiation  
440 treatment and be down for a week or two, but he was a tremendous driving force. He was also  
441 different, I think, from both of us in another way. He loved a good fight. David—I always

442 compared him—he was not Irish, but you know—there is this Irish sense that a good fight is the  
443 most fun in the world. And that's how he was. And he had a lot of opportunity, because he had a  
444 very significant role in starting the medical school. Oh, that was a bloody business. And he knew  
445 more than the rest of us. And he got into one fight with me, which I didn't ask for. I didn't like it at  
446 all. The fact that we had fought on the same side for—again and again—didn't deter him at all.  
447 There was an anthropology appointment which I was pushing, which I still believe would have  
448 been a wonderful one. David just scuttled it and without a trace of shame to cause me that kind  
449 of trouble. But, of course, with every right to do so.

450 So, that I contrasted him, Keith, in fact, in my mind with Steve Warschawski. I think arguably  
451 Steve Warschawski, in math, was the most successful department chairman we ever had in this  
452 place. And that's one hell of a department. And Steve is about as mild and gentle a person—  
453 was—as I ever knew. Or he seemed that way. And these two people were very successful and  
454 very different. And I guess you and I are very different too, Keith, so... [laughter]

455 **CHODOROW:** Let me ask a question about—the question about biology—I didn't realize  
456 what the timing was, what the chronology was, but there was in the early days of the campus, in  
457 fact, before the campus was actually founded, this was going to be an Institute of Science and  
458 Technology emphasizing the graduate level and in fact, I arrived in 1968. Among the first, I was  
459 in the second year of the junior appointments in history. And at that time, it was still said there  
460 would be 60% graduate students and 40% undergraduates. But at some point, in the end of the  
461 50s and beginning in the 60s, the campus became aimed at becoming a general campus,  
462 therefore you needed to develop really substantial departments in humanities and the social  
463 sciences. And a group, the group of you who were already here, were all scientists. How did you  
464 start talking about that?

465 **BRUECKNER:** Well, I can tell you what happened with me. When Herb York was made the  
466 first Chancellor and Roger Revelle was bypassed, that was a very, very unhappy event for me.  
467 York had no academic administrative experience at all. He had none. He had been Director of  
468 Livermore; he was a graduate student with me at Berkeley; been Director at Livermore, and  
469 then he was first DDR&E—Defense Director of Research and Engineering—in the Pentagon,  
470 then he came here. But the contrast between York and Revelle was so great that Revelle left  
471 the campus to go to—he was scientific advisor to the Secretary of the Interior and I left at the  
472 same time. I went to Washington to the— I became Vice President of IDA. But after about a  
473 year—



**[END OF PART TWO, BEGIN PART THREE]**

474 **CHODOROW:** ...and of course now strengthened by IRPS. But that was a very rare instance  
475 in which the Library actually played a substantive role in the academic planning of the campus.  
476 Because it knew what it could buy, what was on the market, what was possible to build. And so,  
477 the history of the Library becomes a very interesting one as it interweaves with the departments.

478 **ARNOLD:** Well, it's...I certainly feel I'm circling back to where we started this tape. I think my  
479 expectations as to what this place might become were of course not realized. I mean, we all  
480 thought it could be much better than it is. But nonetheless, if I can say the sentence that I  
481 believe, which is that this is the best university founded at any rate in this country since the  
482 University of Chicago in 1896, and maybe in the world. Well that's pretty impressive and—  
483 having had a significant role in that—very satisfying to look back on. It's also very satisfying to  
484 have done it in the company of the kind of people that we did. That was a very interesting, very  
485 dedicated group of people. And it was a hell of a lot of fun.

486 **CHODOROW:** Yeah, it certainly was at the beginning for a junior faculty member.

487 **ARNOLD:** Uh huh, I can believe that.

488 **CHODOROW:** One of the things that was so striking is that in the late 60s, it was not  
489 possible for the campus to relegate its junior faculty to the status they usually have in a  
490 university...

491 **ARNOLD:** \_\_\_\_\_ [*inaudible*]

492 **CHODOROW:** ... “go get your work done, sometime in the next few years, we will decide  
493 whether you belong here or not. But in the meantime, your elders, your seniors will do the work  
494 of the campus. There weren't enough of you. And we were drawn in.”

495 **ARNOLD:** Oh, yeah, the atmosphere—I had a real education in group dynamics as the  
496 chemistry department grew. Up to the time I think we were about 12 or 15, we were a big family.  
497 I mean, we just got together if we had some issues to discuss. Obviously, when the promotions  
498 to tenure take up, you didn't discuss them with the assistant professors, but just about  
499 everything else was discussed. Then that started to go away. Then cleavages—sometimes  
500 because of personality, sometimes because of fields—began to appear. There was a period in

501 the middle to late 70s when it was bad enough to cause some concern in higher quarters.

502 Definitely better now, but it's pretty hard.

503 I think even today, a department that has a roster something like 60—\_\_\_\_\_ [inaudible] biology

504 is probably still bigger. But that's not a department as they traditionally were because people

505 have smaller, more local allegiances, they're not really... The tendency would be for a physical

506 chemist not to give a tinker's damn how the department rates in organic chemistry, and so on.

507 And that kind of thing—and then the constant threat of cleavage or trouble between the

508 biochemists and the rest. Biochemistry has done extremely well \_\_\_\_\_ [inaudible], very well.

509 Better than I think straight chemistry on the whole though, again, that's probably not so true

510 now.

511 Wonderful people, but it's certainly assistant professors...and you know, in the case of Bob

512 [Robert] Hamburger, what status did he have that I mean he was in on all of the discussions as

513 a resource. In fact, somebody once, now that I think of it—Bob knows this—but one

514 distinguished faculty member came around at one point and raised hell about who's this fellow

515 that was sitting in on these discussions, planning discussions, throwing his weight around just

516 on the strength of his M.D degree. Where are his publications, where are... you know, all that

517 kind of thing. We didn't look at it that way. If you were helpful, you were there.

518 **CHODOROW:** Okay, well, let's quit.

**[END OF INTERVIEW]**