An Oral History of HUBERT RENÉ HALKIN and STANLEY CHODOROW On May 11, 1999

HALKIN: —somebody like Warschawski had an idea of—its a [*inaudible*] theory of mathematics that he wanted to open. But he had also an idea of boundaries. Okay. And now those ideas of boundaries, especially in a field like mathematics at that time, a department who decided to move the boundaries a little more broad, directed to people who were doing, for instance, things—at that time, they were not, computer science barely existed at that time—but people who were going into that direction -

- 7 CHODOROW: Was it already called combinatorics?
- 8 HALKIN: Oh, no, combinatorics is an old name.
- 9 **CHODOROW:** It's an old name.

10 HALKIN: In mathematics, maybe 50 years ago it meant something possibly a little different than now. Now combinatorics is a very interesting but well-defined topic, which has some 11 12 overlaps with computer science. But this is a field itself. In fact, this is a field in which we are very good. In the whole world there are three departments in combinatorics: University of 13 14 Jerusalem, MIT, and here. And definitely I think that Warschawski would not have been ready in 15 1960 to hire somebody in combinatorics. But combinatorics already existed at that time. So yet, he was broad but, I mean, it was a certain type of perimeter. And within that perimeter, he was a 16 17 person without ego. And he was extremely successful as a recruiter.

CHODOROW: 18 Let me ask a question about the early—as you said—the intellectual life of 19 the department. One of the things that has struck me in it, and especially when I've had to deal 20 as a dean or associate or provost, with files for the mathematics department, is that it is nearly 21 impossible to turn to a colleague of a mathematician to say, "Is this good work?" The answer 22 you usually get from the person whose office is next door is, "I have no clue whatsoever. I don't 23 understand anything that person is doing." It would seem to me that one of the keys to success in the department would be to have at least a couple of people like Errett Bishop, or Garsia, or 24 25 Warschawski, who somehow bridged—understood enough so that they could make a difference. 26

27 **HALKIN:** There, what I say is that we were all in analysis, okay, it means they could talk to 28 each other about problems at their respective boundaries. But I doubt that in the mathematics 29 department anybody-even now-understands more or has read more than a miniscule 30 percentage of Bishop's stuff. And, for instance, the idea, if you like, there is a quality in the mathematics department that we appreciate, and which colors our review: is my neighbor a 31 32 good guy for me to play his ideas with me. And fortunately, the department—we had spectacular persons to play the game. I would say they are, in fact, the most poignant of the 33 34 department, but they are spectacular in their area, [inaudible] pretty good in all the other 35 aspects. And for instance, I imagine in the case of those people, that quality is not something 36 which is measured, or officially cared for either in a department or in a university, but which is 37 extremely valuable.

38 **CHODOROW:** So then, one of the keys to a good department is having one or two or three 39 people or whatever number of people who may not themselves be phenomenally creative or 40 productive but who enable other people to do so because they are willing to be interlocutors?

HALKIN: Yeah. Somehow this might be a help: equivalent to a good psychiatrist or to make sure the secretary is friendly. It is difficult to know exactly where it actually goes. [*inaudible*] I remember a few people in my life who help me. I ask questions and I got answers yet they're supposed to be—but those are rare. Most of them I sort of remember, for example, in the case of Bishop—talking to him a great deal—and I cannot remember something, there was a particular piece of work, and he really gave me the light, the story, but he was extremely valuable.

48 And at that time, some organization that we had, which does not exist anymore, was the 49 analysis seminar. And this was something organized by Garsia, mostly, in which he managed to 50 invite—there were a lot of faculties there—and went virtually all of them. And the topics were 51 extremely—it did not look at all like a colloquium. So, somebody was saying something, was 52 interrupted all the time, and all that with student participation. And with good dinner, somewhere in La Jolla, afterwards, and parties, for the Garsia is a person of great, multiple talents. And all 53 his qualities—is a very excitable mathematician, I imagine he was giving, he was giving one of 54 55 those analysis seminars one out of five times or something like that. Very interesting, very 56 eager, very controversial, but also a very social animal. And in that way, I think this was possibly 57 one of the focus [?] of the intellectual life of the department in the late '60s, and I think it died out 58 when he stopped being interested in combinatorics, in analysis. And he switched to something

else. And there he developed his own narrow group of faculty and the [*inaudible*], but it is more
narrow in mathematics, but it is still very like.

61 CHODOROW: When did he come and how did Warschawski recruit him?

HALKIN: In '65, he came in '65, from Caltech. And he had a very sad history, mathematically. I
forget exactly. But one problem in complex variable was a very hard, hard problem. And in, I
don't know exactly when, the late '50s, early '60s, he made something which was spectacular.
And six months later, somebody by a totally different route made something which covered all
that but made that totally irrelevant. So Garsia was quite a spectacular – I assume he was
already a full professor at Caltech when he came here. An extremely energetic and also a most
difficult person to have in his department.

69 **CHODOROW:** Yes, I understand. Very demanding, very mercurial, excitable.

HALKIN: Yeah, excitable, not reliable, with a lot of characteristics. But still, when I think of my
years at UCSD, they exist of Garsia and of Bishop, for instance—are really the peak
intellectually.

73 CHODOROW: Where did Bishop come from and why did Warschawski bring him here?

74 **HALKIN:** This is my own view. Bishop was at Berkeley. Bishop was an extremely famous quy 75 extremely, extremely early. But Bishop thought he was not quite as good as Steve Smale. The 76 best guess I have that Warschawski was able to recruit him here was because here Smale 77 would not be here. Okay. I mean this is guesswork, and I've talked about that to some of my colleagues and some people say, "You might be right" or something. But definitely Errett Bishop 78 79 was way above our league. When he came here, he was surely the best catch that 80 Warschawski made. You know, there are a lot of theories [?] which had his name on and with 81 what he did in function analysis and all of these things that he did in the '50s and early '60s, and which was really, I mean, he could have gone anywhere he wanted. Some of the reasons he 82 came here had to do with things, some family connections, or liked the place, or something like 83 84 that. But basically, I think we were extremely lucky to get him.

CHODOROW: Talk about that a little bit in mathematics. Do these stars make a huge
difference, that is to say to they make it easier to attract other people of high quality -

87 **HALKIN:** Oh, yeah, oh!

88 CHODOROW: —so that the strategy of catching a star like that is a critically important
89 element of building a department?

90 **HALKIN:** Yeah. And, of course, you have stars who are empire builders, but you have stars 91 like Bishop who was totally the reverse of an empire builder. He was somewhat of a loner, 92 [inaudible]. I do not know if there was anybody that Bishop was the one to think, "Hey, we should hire that guy." He went to Warschawski, "Hire that guy!" It might occur, but I would not 93 94 know that because when he was here, he was talking to a lot of different people, but he did not 95 act like a follower. Okay. And I would say hiring somebody like Bishop, especially somebody of 96 that type is extremely valuable. Because he's a great guy and you know his character. So, you 97 go to a department of mathematics where he is you know that he will not grab and monopolize 98 all the FTE coming down the pipe. Okay. And whether sometimes you can make spectacular 99 hires of people who are known as ambitious empire builders, which is pretty good. Okay. 100 Because actually, in mathematics, in the department now, we still have a few pairs. Okay. And 101 pairs are enormously productive. In a sense, when I see Burton Rodin, for instance, who when 102 he was in his 45, or 50s, started to work a lot with Warschawski. This was the most fruitful, 103 continued collaboration I ever seen my life, because Warschawski was there, still full of energy, 104 but he needed somebody else and all that. And this is really a synergistic collaboration. And among some of our [inaudible], we see that same thing, but let's say within the department 105 106 [inaudible]. And, for instance, people sometimes go to the [inaudible], in probability, we hired 107 quite a few people, because they came here because there was somebody they could exactly 108 work with. And possibly, that would also apply to young topologies or representation theory or 109 something like that.

110 **CHODOROW:** You're describing—it is interesting, because you're describing a department 111 is very different from any other department, we've now listened to, to the stories of different 112 departments. And this is very different. It's very different because it is much more like an arts 113 department than it is like either science or humanities or social science, in this sense that you're 114 describing a group of people of really a group of virtuosos who will collaborate the way that 115 you're saying virtuoso musicians will and very often produce extremely important work through 116 collaboration, but also are themselves incredible performers.

HALKIN: Yeah. Work through collaboration. If you look, for instance, at the entire output of the
department: those who yield collaboration, people writing joint paper regularly—I would say one,
two, there's possibly three pairs of faculty who have written more than three papers with each

other. So, collaboration is sort of rare, but a certain type of conviviality [*inaudible*]. But the structure of the department, for instance—I think our department can go beyond the perceived domain. Okay, to be narrow is affect a lot of things. For instance, if a department decide to be narrow, okay, we are interested in algebra or something like that, but then if you look at the history of the Chicago Department of Mathematics, they be narrows, but by shifting a lot over the years. It means that they miss a lot of opportunities. Those opportunities can be picked up by somebody else. But to the new fields, can it affect how they develop.

127 For instance, I would like to see an intellectual history of computer science at universities, for 128 instance. Computer scientists here, or at Stanford, or at every place-the people in that field in the '60s, when it was really sad, sad, sad stories. There were a few very good people who did 129 130 not get any sort of recognitions. There is also feeling which [inaudible] mediocre people and 131 how a structure reacts to them. But here at UCSD, we are so proud of our alumni. Dr. Irwin 132 Jacobs, you know, great success of giving millions of dollars. But I think for instance, in this 133 case, I think he was surely not liked by his department, when he was here. I do not remember 134 exactly the detail, but the founder of the Department of EECS and was Henry Booker. He has a 135 very, very prominent man but in a very narrow area. Basically, this is applied mathematics-British style, just like John Meyer was also an applied mathematician. British style with slightly 136 137 different interests. John Meyer was interested in elasticity and [inaudible] of fluid mechanics and 138 Henry Booker was interested in electromagnetism.

139 But for instance, none of those people I think had any affinity for new stuff, like, for instance, the new stuff - like, for instance, the new stuff presented by computer science or type of discrete 140 141 communication theory and things of that type. I find it guite curious. But narrowness also makes 142 you escape big mistakes. There are a lot of fashions. For example, there are fashions in 143 mathematics which turned out to be very good, like nonlinear stuff in nonlinear analysis. When it started, it was really tremendous intellectual academic [inaudible] in the '70s. But at the same 144 time, there were other groups in mathematics, doing the same fluff, but there was nothing there. 145 146 And at the moment when this took place, I think it was very difficult for people to know the 147 difference.

148 CHODOROW: How good at that was Warschawski—in guessing what was going to fly and149 what was not?

150 **HALKIN:** Actually, Warschawski was lucky that people that he hired changed. I think 151 Warschawski was very [inaudible] but defined and he was ready to [inaudible] the people who 152 came here definitely changed quite a bit once they were here. And it might very well be because this is a new campus. You know, why would an established Professor decide to move here? 153 154 They must they have, they must be of a—this is a demographic issue [?] of is the population of California different in the population of Ohio or something like that. And, for instance, Errett 155 156 Bishop when he was here, switched totally from functional analysis and his old topic to 157 constructive analysis—something totally rare. Errett Bishop is the only mathematician who did 158 work which led to fistfight among mathematicians. This is extremely, extremely rare. Okay, 159 because it's a borderline between mathematics—the foundation in mathematics and logic and 160 things like that. And, you know, not here. There was quite a famous fistfight at Boston 161 University, over Bishop's idea. Okay, so there's some of you who change totally his mind and went way out! Of course, if Bishop would have done that before producing the Bishop-DeLeeuw 162 163 theorem—this big thing that he did in functional analysis—he would never have gotten tenure, 164 not even been hired anywhere you needed to-

165 CHODOROW: Is there a conclusion that you can draw that the fact that the faculty who
166 came here changed a good deal – that is they gave up certain fields and took on new ones—
167 was a function of Warschawski's perception of who he hired? His hiring people who were willing
168 to do that?

HALKIN: He hired quite a few of the faculty who did not budge an inch! They came here, and
they kept working exactly—very, very well—in what they were doing in the '60s. I think
Warschawski is both [*inaudible*] and the fact that he did not [*inaudible*] and he was able to
recruit very good qualities in what -. I imagine if Warschawski left a diary and I would read it I
might change my opinion. But I sure did know him, I love him, and all—but he did not have that
much of a—

175 CHODOROW: How does one these days—I mean, one knows that Princeton is a great
176 department, that Berkeley is a great department. There are a few really great departments. How
177 does one in mathematics define those departments? What makes them so great? Do they have
178 a bunch of Field Prize winners or is it something else?

HALKIN: The flavor which makes it—for instance, if you would ask people, one cannot
compare in quality Princeton and Harvard. These are totally not comparable quantities. Of

181 course, in the traditional pecking orders. I think Harvard might be number one, depending on the 182 year and Princeton might be three, four, five, six or something like that. But they have a different 183 view of mathematics. For instance, new mathematics is first done at Princeton. And after it has been recognized as a field, it is basically polished at Harvard. Probably this is a gross, gross 184 oversimplification. But even that those are sort of very superficial ratings put up by with the 185 American Council on Education or something like that. For instance, a mathematician—he goes 186 187 and he'd say, "This is a very reasonable sort of like pecking order"—depending in some fields, 188 some people would feel—but myself I find it guite—

189 **CHODOROW:** Where does UCSD rank in this? That's the Research Council.

HALKIN: Yeah, I think we did quite well. But I forget the Research Council or Newsweek. I
would say that they turn up more or less the same pecking order in department. You know, I
think I know that 20 years ago UCSD was number 21 or something like that and recently might
have gone to 15 or something like that. We'll have to check on that. Of course, when I tell that to
my kids, "We are 15," they say, "We are that low?" [*inaudible*]

195 CHODOROW: Let's talk a little bit about relationships with other departments. Mathematics
196 is in one respect a service department: it teaches everybody calculus. But in terms of research,
197 in terms of graduate programs, and so on has there been and was there in the early days a
198 good deal of interaction between your department and other departments on the campus?

199 HALKIN: I would say really to do work, to do research jointly, I know of very few examples of 200 that. I would say all the example I know of is the statisticians. I know some people work with 201 statisticians and physicists, statisticians at the med school and nursing. But for instance, at the 202 very beginning here. This is what for me is the big difference between UCSD in the '60s and 203 UCSD now, the campus was so small that in order to get the good party going, you needed to 204 be very interdisciplinary. So, for instance: the math department in the '60s and economics. I 205 don't know why—possibly there was a very old chair of the economics department, Seymour 206 Harris—I sort of, both my—

207 **CHODOROW:** Yes. He wasn't exactly a mathematician.

HALKIN: No, no, no, no, and both my in-laws say, "Oh, he was such a nice guy," but he was
already in his 30s an old fashioned—he was a perfect gentleman. And it was parties in
economics, all the young faculty in mathematics were there. There were parties in mathematics,

- all the young economists were there. And you know, it was possibly the same thing with
- anthropology. I sort of remember even a party at [*inaudible*], where you were there. I can't think
- 213 why I remember that. But it was in the '60s. Important people were talking about—I sort of knew
- what quite a few people in economics were doing, because possibly I have some interest there.
- But also, the interest—I sort of remember I even read your dissertation once.
- 216 CHODOROW: Right. Poor man!
- 217 HALKIN: It was something on the 14th century?
- 218 **CHODOROW:** The 12th century.

219 **HALKIN:** 12th century but in Spain, and the effect of the collecting of dues as a [*inaudible*]

220 **CHODOROW:** No, that's different, that's somebody else.

HALKIN: Oh, this is? Oh, I see, no. Okay. So, yeah, so I saw the title of this dissertation.
Actually, I had to join Air Force contract with somebody at Ames [?] for a collection of years
there. Actually, he was younger, it was easy for me to [*inaudible*] a grant from the Air Force. He
worked on it, but we never wrote the actual paper together. So, I don't' think there was that
much collaboration, but somewhat interest. Sometimes that interest was pointless. I was
flabbergasted myself by our department of philosophy when they arrived here.

- 227 **CHODOROW:** It was all history.
- 228 **HALKIN:** Yeah...this was one of my worse intellectual disappointments.
- 229 CHODOROW: Was finding a philosophy department that was not -

HALKIN: No. At a party I made a bet with Errett Bishop. We had a nice philosopher who was a 230 231 very pleasant, conversational person. He was then teaching a class and he was talking about his class of great philosophy on Plato and all that. And the irrational number, rational number. 232 233 We are all mathematicians so this was very nice. But then I told Errett Bishop, "I bet you he 234 does not know what an irrational number is." Bishop told me, "This is impossible." Actually, he 235 gave the definition which corresponds to a concept of the 17th century [inaudible] never read 236 the texts of Plato, that because there is nothing to do [*inaudible*] mathematics. For instance, 237 there is one dialogue of Plato where the discussion of what is a rational/irrational number is 238 extremely similar to what I would say in a calculus class. And actually, in my case possibly I

knew it because I sort of summed it up now, but I read it in Greek, but even in translation,[*inaudible*].

CHODOROW: What other aspects of the early years of the department would you pick out
that you haven't talked about that should be paid attention to—things that are important.

WESTBROOK: When you were getting on this notion of the service role of the department l'm starting to get this image of the department of virtuosi talking about problems—mathematical problems—and that there doesn't seem to be much discussion or image of teaching, what was the role of teaching? It's almost -

HALKIN: Oh no, no, no, no. Sorry, this is my own fault. The aims of the department in
mathematics, from early on and still now, all the teaching is not actually by faculty. Just not at
all. In fact, literature was interviewing French literature students to go to Paris for a year on the
EAP [?]. And I asked them if they knew any French professors, they said, "I never, I never saw
one. But I saw a lot of TAs." And in the '60s, teaching was a big part, and people were very
interested in it. And we are talking about it and people would have, I would say, disastrous ideas
about it.

254 **CHODOROW:** They had disastrous ideas about it?

HALKIN: Bishop and Garsia. People at that age you find giants. I mean, especially in the case of Bishop. This was possibly the late 60s, early 70s, he had ideas on personal development. I liked to talk to him, it was very pleasant to me and I'd say he was genuinely interested in the students. But when we taught the fifth course of a sequence, and Bishop had taught the four ones before and there was nothing you could count on. It was a little annoying.

CHODOROW: Bishop had a theory—I remember conversations with him—that it was really
up to the student. And he would go to a class and sit down in front of the class and wait for a
student to ask a question. And if nobody asked the question, nothing happened that day.

WESTBROOK: Well, I sort of get a sense—I'm not surprised by that at all, frankly. That would be the way that the students would [*inaudible*]. I'm getting a sense—and I don't know where it's coming from—but that the student in a mathematics department is a completely different animal than, say, a student in engineering or physics—

267 HALKIN: Oh, no, no, no.

268 **WESTBROOK:** In the sense of probably the professors don't look to their students for help 269 with their own projects. I mean, it sounds like mathematical research is very solo work.

270 **HALKIN:** No—here one has to make distinctions, okay.

271 WESTBROOK: Okay.

HALKIN: All those encounters that you describe about Bishop—those encounters with Bishop
with students in the lower division. Students in lower division, these are 10% of them are
mathematics majors. All of the other one, if it is a lower division in our serious tracks, the one for
engineers, physicists, a physical chemist. And these are all from other departments. And they
are—I do not know exactly how intellectually—it is difficult to get intellectually excited at that
level. If you give a problem to be intellectually exciting, then sometime you run into problem.

278 When first I arrived at UCSD I had never been a TA in my life. It was a beautiful class. And in that class, all my students walked to see Goldberg-he was Provost - and complaining about 279 280 that "It was not the style we were used in high school." Then, I ended up being quite friendly 281 with those students and all that but basically, this is the moment I gave them work on something 282 which is really of sort of like an intellectual challenge appropriate to undergraduate. But this 283 unfortunately is not done anymore in undergraduate education. But if you've talked to a 284 mathematician about their student in upper division. We meet often-and we met at a dinner for 285 the analysis seminars six years ago—or now, when we meet at the faculty club, everybody will 286 talk about what has a smart student done in algebra 100, or math 140. [inaudible], and the 20 to 287 40 student class in which those students who have an extraordinarily close, close interaction.

And I think Warschawski had possibly a lot of influence in the way the curriculum was set up.
And the curriculum was set up, basically, to dovetail nicely with what physics wanted to do. And
what the two sort of pre-engineering department we wanted to do at that time.

291 CHODOROW: Both of them really applied mathematics departments. Or physics, applied
 292 physics -

HALKIN: Yeah. Yeah. In some way, it was a relatively reasonable way of doing things. And the
students we had in the '60s here were so different. There's a student we had, on the scale, a
difficult to imagine [*inaudible*]—.

[END OF PART ONE, BEGIN PART TWO]

Oral History With Hubert René Halkin and Stanley Chodorow

296 **HALKIN:** —exam guestion of 1966, 70s. You know, I know that this is sort of like an old routine 297 where people say the students were good then. I know that, but if I have a set of test answers 298 given back to me about the same problem in the 60s, in the 70s-they're different. It's spectacular. And I tell you this, the reason it is spectacular is because in the 60s, very bright 299 300 students were not yet learning in high school something enabling them to skip Calculus. So those who don't do well there, you are quite capable of giving them a tough class. Whereas now 301 302 people with either—you teach calculus now – either you will have students who are poor 303 because in high school they didn't take Calculus or because now they very much can do it [?]. 304 This might be—I don't think the people—it just happened that these students who are in 305 mathematics class are quite different. I don't know if you are interested in the political history?

306 CHODOROW: That's interesting, especially as it reflects the development of the department307 intellectually.

HALKIN: Oh no, not so much department, but [*inaudible*]. Did you attend the famous lecture ofDan Orr?

310 CHODOROW: No.

HALKIN: Oh, you missed that one. This for me was a piece of cake. I imagine this must havebeen 1968, there about.

313 **CHODOROW:** That's the year that I came. I came in the fall of '68.

314 HALKIN: Dan Orr was -

315 **CHODOROW:** Yes, know. I know Dan.

316 **HALKIN:** Yeah. He was a professor of economics here.

317 **CHODOROW:** That's right. He was one of the early senior people in economics.

318 HALKIN: And I think he gave us some of the provocative. You know that during those years,

319 there were—we had a tradition equivalent to the European tradition of -

320 CHODOROW: Inaugural lectures.

HALKIN: Yeah. Yeah, inaugural lectures. So, it was his own inaugural lecture. He took a topic
that was a little controversial, and he was a studied economist. He was not a pure intellectual
that I know, but he was—what was his topic? It was empirical vilification of Marx's theory and -

324 CHODOROW: He's an anti-Marxist.

325 HALKIN: Yeah.

326 CHODOROW: Very strongly.

327 HALKIN: Yeah. And Dan's lecture turned out to be received like I've never seen. You know, I bet you anything [inaudible] almost died of a heart attack there. He was so-Marcuse was there, 328 329 there was Marcuse's followers, and I found it spectacular. But [inaudible] was upset, and I was 330 the only one that complained, because the lecture was interrupted, the lecture could not take 331 place. And I didn't like that. And it was very interesting for me, because this is my closest 332 contact in the 60's with that part of mathematics. [inaudible] but there were a few sessions in 333 there, with quite—of course, I shouldn't say that. My godfather was a professor of philosophy in 334 France and while here, sometimes conservative Californian complaining about the politics of somebody like Marcuse. My godfather was very [inaudible] about this philosophy. And I imagine 335 336 that Marcuse himself was a very nice gentleman. We share the same [inaudible]. What I also 337 find interesting is new ideas, new trends were started in one country, get accepted in another 338 country, and the marketing of all those ideas get appointed with science studies, for instance. It is extremely difficult for me. You know there are some parts of science studies that I find 339 340 reasonable, but I honestly cannot say that I find most of science studies reasonable. And when I 341 read for instance French intellectual newspapers, any of those that make fun and they own, they 342 say [inaudible].

343 CHODOROW: L'autre [?]

HALKIN: —[*inaudible*] they must not be so bad because Americans like them a lot. And
fortunately, I think in the university environment there is—it's very difficult for the university to
make a distinction between something like a class like science studies or a class like nonlinear
mechanics. In the 70's, those are brand new ideas, both with no cohorts, they have no
followers, you know? And possibly since I feel myself a silly little European, when those things
come from Europe to the US, I feel a little, you know, disappointed.

350 **CHODOROW:** Why do you feel disappointed?

HALKIN: Oh, because you know, I think that will not be [*inaudible*], okay? Those fields could
possibly be directed better, and this is the same type of—there is a certain type of—a good
example in mathematics or pseudo-mathematics is that fuzzy theory. Fuzzy sets.

354 **CHODOROW:** Right, fuzzy sets.

HALKIN: Fuzzy sets. If for instance Japan would not be there, or Japan and the US would be in a different sphere, that topic would have died—not survive—long, long time ago. Okay. But it required this cross-cultural game of ping-pong to sort of keep that more or less alive. And I have the impression that with some [*inaudible*].

359 **CHODOROW:** What you're saying in effect is that some of these ideas are sustained and 360 developed because they perform a functional—a social function or a political function.

361 HALKIN: Yeah.

362 **CHODOROW:** Or a cultural function between two different cultures.

363 HALKIN: Yeah.

364 **CHODOROW:** Not because they're good ideas or productive ideas.

HALKIN: Yeah. Oh, actually, you know this is a difficult point in France—intellectual history. At
one time, I studied the French philosopher Bergson And so for instance, the faith of Bergson
exploded in France and died down and all that cannot be explained by playing ping-pong with
somebody in a faraway country. This was—but [*inaudible*] in France it's always very sort of—
you have to go in the Academy, and you have to get an academic means if—possibly you have
had the history of Scientific Revolution of Kuhn?

371 **CHODOROW:** Oh Kuhn, yeah.

HALKIN: And put in there, there's quite a few examples of in a very structured system – in an intellectual society like France—when you have something very bad at the top, you have to wait until it dies for something good to come. He gave a lot of examples in chemistry. You know, we had a few really good French chemists, we had some very poor ones. Among French chemists, the notion of atoms and all that was delayed in its appreciation much later than it was accepted in Germany or even in England. So, but I think that French structure with a ping-pong across countries [?] like the U.S., especially in all that—in all those borderline topics. Yeah. I find it very curious, on the other hand, it's very difficult to get any semi-decent analysis of that because wehave arrived on that topic [?].

381 CHODOROW: Right.

382 **HALKIN:** It's from one extreme to the other extreme.

383 **CHODOROW:** But do you think? I mean, it's an interesting question whether—how one 384 defines productive. That is to say, it may well be that in the long run, [*inaudible*] ideas, for 385 example, are not productive in one sense. That is that they don't produce readings, let's say, of 386 literary texts or any texts, that are regarded as sustainable and making a substantial contribution 387 to our understanding of something of the period. On the other hand, the argument over 388 [*inaudible*]—the ping-pong -may well have produced an enormous amount of intellectual energy 389 and work which eventually produces something very useful.

HALKIN: Yeah. Oh, you know I'm not so sure I would go that far, but it surely keeps people
mentally occupied on sort of like a big scale. Okay, but then you could say this of some religion
sometimes. I might have a very simple, narrow view [*inaudible*].

393 **CHODOROW:** Actually, take the example you used of fuzzy sets.

394 HALKIN: Yeah.

395 **CHODOROW:** It may well be that in mathematics, fuzzy sets don't—the idea doesn't 396 produce very interesting results but in language and in cognitive science, it appears to produce 397 some very interesting results because language—natural language as opposed to 398 mathematical—seems to operate at least at some level as a series of fuzzy categories.

399 **HALKIN:** —[*inaudible*] mathematics that sort of things, you know, even if you look at quantum 400 mechanics of nonlinear probability. You know, these are all nice topics, of mathematics. So, I'm 401 not told [?] that the idea to be able to put in a concept which is relatively superficial—a veneer— 402 and out of that to create—this is something which makes me very uncomfortable. And possibly, 403 production of it puts me in a nice narrative but then there are other things of the same type, 404 which occur for instance, you know, something a mathematician called Bellman invented a name called dynamic programming in the late 50s, early 60s. And under that, he sort of covered 405 406 this [?] which was good. This is for me also; this is something like fuzzy sets. This is-this is not

science. This is not anything, this is just like a PR campaign. But sometimes, possibly, PRcampaigns are good to give people narratives.

409 CHODOROW: Interesting, interesting set of ideas. Brad, do you have any other questions?

410 **WESTBROOK:** No, I don't. No, I guess not.

411 **CHODOROW:** It's been interesting. Good start on mathematics.

412 HALKIN: Can I—can I ask you—could you—I sort of forget exactly where you fit? You know, I
413 know, Stan.

414 **WESTBROOK:** But where do I fit in this? In this transact—or in this business? I'm the 415 University Archivist and Manuscript Librarian.

416 **HALKIN:** Oh okay, okay.

417 **WESTBROOK:** Stan and I sort of stumbled in this together. I was helping Jim Arnold with his 418 oral histories and he needed an interlocutor for a session, I recommended Stan and out of that it 419 blossomed. But you and I have met.

420 **HALKIN:** Oh, yeah, no, that's exactly what—I sort of know you.

421 **WESTBROOK:** We had—many years ago walked down to the beach from Lynda's house one 422 night and had this conversation about—

423 HALKIN: Yeah. So, like I said before, you would tell me that you were a faculty member in

424 economics, or something like that. They would say to me—you know, I could not—

425 **CHODOROW:** You know, he's more important than that, he's a librarian, an archivist.

[END OF PART TWO, END OF INTERVIEW]