

University of Wisconsin  
University Archives Oral History Project

R. ALEXANDER BRINK

An Interview conducted by  
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BRINK: That's right.

TAYLOR: Now, I understand you are intimately acquainted with the conditions under which Lederberg came to the University. I would like very much to hear about that.

BRINK: Yes. I was chairman of the Department of Genetics at the time Lederberg joined our staff. There was a deep difference of opinion among several members of our staff. We'd grown to eight or nine people on the senior staff by that time.

TAYLOR: About when was that?

BRINK: Just at the end of the war--'46, I think, when Lederberg came--I'm not quite sure about that year.

TAYLOR: Give or take a year, so to speak.

BRINK: Yes, that's right. Cole had retired, having reached the retirement age, and he was in ill health at that time and took no part in the deliberations concerning staffing at that juncture. There was unanimous agreement within the group that in replacing Cole we should seek a man especially qualified in basic research. Basic research has always been stressed in this department since H. L. Russell became dean of the College of Agriculture. One of the things that impressed me when I first joined the staff here was the very liberal attitude toward basic research, providing one's responsibilities to the agricul-

tural industry were not overlooked. This came first. The first mortgage on your time was to serve the agricultural industry; the first mortgage on the college was to serve the agricultural industry. And within that goal there was liberal allowance made for teaching and also basic research in those fields related to agriculture. And I've always felt that Wisconsin's success in this area--and certainly it is one of the leading agricultural research institutions in the world--was due to the men who founded the college, who staffed the college in its early years--men like Henry and Babcock and L. R. Jones, and Cole too, who founded the Department of Genetics. He was a zoologist, with no close interest in agriculture, certainly, but with a broad knowledge and interest in the natural sciences. This is the kind of man H. L. Russell sought and succeeded in recruiting.

TAYLOR: Why do you think he was so successful in getting men like this? .

BRINK: Well, he himself, of course, was well trained in this field. He was a bacteriologist. He'd studied in Germany under Koch, and I think was in Pasteur's laboratory for awhile. And he'd seen enough of agricultural research in Europe to realize what its potentialities were. And being trained in the natural sciences himself

he realized how intimate a relationship there was between the technologies on which agriculture is dependent and science. Russell took his Ph.D. in bacteriology at Johns Hopkins University, which was one of the great early training centers in the United States in the natural sciences. So he came with a strong predisposition to build up a staff here which could actively contribute to the body of knowledge he felt underlying--scientific knowledge--underlying agriculture. That's why we have departments here like genetics and biochemistry, in addition to, say, agronomy, horticulture, soils, that are more directly concerned with the problems as you find them in the field.

This type of organization was a real advantage. These things may be of interest, I think, to other people besides you and me. The type of organization that Russell fostered was favorable to research in the basic sciences in this respect: that departments like genetics were insulated, in some degree, from the pressures growing out of the problems in agriculture as you find them in the field, where demand comes in for help in some particular area--the outbreak of a new pest, or something of this sort--that will consume the time, then, of somebody here in dealing with that particular applied problem. So the

Wisconsin College of Agriculture had, you might say, an outer ring of departments, like agronomy, horticulture and soils, entomology, dealing with these problems as they find them in the field. Behind that was another rank--biochemistry. Feeding problems as you encountered them on the farm were dealt with through animal husbandry and dairy husbandry, but biochemistry concerned itself with basic problems in nutrition--research in this area. So that the pioneer work on vitamin D was done here, and Connie Elvehjem's work on niacin, which wouldn't have been done if, say, all the nutrition research was in the Department of Animal Husbandry, probably.

TAYLOR: I see--because you didn't have to respond to these immediate and temporary crises.

BRINK: Yes. I was much impressed with that organization when I first came here, realizing that it was radically different than that at the University of Illinois, where I'd been a graduate student for one year.

TAYLOR: I wish you'd explain that to me.

BRINK: Well, in the Department of Agronomy at Illinois, for example, they had a little college in itself. They had the crop specialists--corn men, oats men, forage crop men, and so on. Then they had a plant physiologist, they had a plant pathologist, they had a geneticist, serving

this little departmental group. They didn't have a department of plant pathology or a department of genetics. They repeated this pattern over horticulture. Horticulture had its plant physiologist and its geneticist and its entomologist in addition to the men dealing with the particular crops there--orchard crops, and strawberries, and so on.

TAYLOR: What kind of relation would there have been between the two geneticists, say?

BRINK: Well, this was all right, probably, in that these various crop specialists had aid right at their elbows from these people. But it wasn't good for the development of plant pathology or genetics or plant physiology or entomology. These people's time was monopolized, you see, by dealing with problems as you encountered them in the field. And they had less time than we have under our system, our organization, for consideration of the problems in those disciplines themselves.

TAYLOR: Nor does it sound as though they could get together very easily.

BRINK: Yes. So Wisconsin had a pattern of organization that was favorable to the development of the natural sciences underlying agriculture. This was one of the reasons, of course, that prompted us to replace Cole with a

person whose primary interest would be in basic research--not necessarily related in a direct way to agriculture. We'd heard about Josh Lederberg and his discovery in cooperation with E. L. Tatum of sexuality in bacteria. This appeared to open up a whole new area of research in genetics. It turned out, in fact, to be one of the most important developments of the time, and led to Tatum and Lederberg and Beadle being awarded a Nobel laureate some decade later. Lederberg's background had been entirely metropolitan. He'd been born in Montclair, New Jersey. He'd taken his undergraduate work at Columbia University, with two years in the College of Physicians and Surgeons. He had thought, apparently, at that time of qualifying himself for medical research or practice. He had no contact with agriculture at all during his upbringing or during his formal schooling. But he was recommended to us as a brilliant young fellow, and we realized that microbial genetics could become an important area of research, and we were anxious to--some of us at least--to recruit him to our staff. But there was determined opposition on the part of some members of our staff--those mainly concerned with the application of genetics to livestock breeding--determined opposition to bringing Lederberg here as a member of our staff because

his background was a city one exclusively and they saw little hope that he could make himself at home in the College of Agriculture. The fact that he was Jewish, too, probably was a factor in the thinking of some of our members, although that wasn't--

TAYLOR: Oh, really?

BRINK: Yes. I don't think there's any question about that. If you look over the correspondence you can see that as an undertone in the letters. But it didn't become the first, here. Their main objection to him was that he just couldn't fit himself into the College of Agriculture. Well, it was clear to me as chairman of the department that if a vote were taken a majority wouldn't favor inviting Lederberg to come. The administration, on the other hand--Dean Ira Baldwin was our dean at that time--they were supporting Lederberg's candidacy without any reservations. Dean Baldwin had learned something of his work and he supported us valiantly in trying to get him here. Bob Irwin and I carried the main burden in the department in trying to work out an attitude, develop an attitude among our colleagues favorable to inviting Lederberg to come. Two letters--copies of which are in your records now [see Appendix]--turned the tide. One was from E. W. Sinnott, who was then dean of the Graduate School at Yale Univer-



sity, where Lederberg was doing his graduate work. Sinnott had been professor of botany at Yale. He'd become personally acquainted with Lederberg while Lederberg was a student at Yale. Sinnott was also a personal friend of mine. I had great respect for his judgment. He had been on the staff, before he went to Yale, at the Connecticut Agricultural College at Storrs, Connecticut--in the Department of Botany--so that I knew that Sinnott could picture the kind of environment into which a man like Lederberg would come, and could give a judgment as to how well he could likely adjust himself to it. I can remember sitting down deeply discouraged after a staff meeting and writing to Sinnott at some length about the situation we faced here--that there was determined opposition to Lederberg; that some of us wanted him badly. How did Sinnott think he would fit if we brought him here? Were these objections being raised to him as a potential member of the College of Agriculture staff valid? Sinnott replied in a letter that gave us just the information we needed. He told us of Lederberg's extraordinary energy--mental, physical--of some of the difficulties he'd had as a student, he being a little too, perhaps, pushy, forward with his instructors--getting into their hair when he was ahead of them in their thinking. He was an extra-

ordinary person--Josh Lederberg. Really, he had many of the characteristics of a genius and at this time just bursting with mental and physical energy.

TAYLOR: Impatience, probably.

BRINK: That's right--impatience. But he married a fine girl and she tamed him a good bit, and Sinnott encouraged us to interest him in the appointment here. And then the other letter came from Ray Owen, who had been a member of our staff earlier but who had left us for a position in the biology division at Cal Tech in Pasadena. Ray had taken his Ph.D. here, had been appointed then, in the early 1930s, as assistant professor in genetics and zoology, under an arrangement in which zoology joined with us in giving one introductory course in genetics in the University. Previously there had been a separate one in zoology and one separate in genetics. Ray did this job splendidly, but Cal Tech took him away from us. Well, Ray was well known to our staff and highly respected. His letter, too, was distinctly in favor of Lederberg's candidacy. He took the trouble to canvas his associates in the biology division at Cal Tech about Lederberg. Some of them knew him personally, others had known him by his work only. Some people were skeptical about the claims that Lederberg made--was making about sexuality in

bacteria--but other people were convinced of its validity. And so Ray's net recommendation was distinctly in Lederberg's favor. Well, this had a considerable influence on the thinking of our group on the staff when it was presented to them, along with Sinnott's letter, and when a vote was taken a majority had voted in favor of inviting Lederberg to come for a seminar. He came and made a very favorable impression, and was invited, and he accepted the position here. As I noted in this memorandum I gave you, a couple of years later one of the people who had objected most strenuously to bringing him here, on the grounds he couldn't possibly fit into our College of Agriculture, told me how well he thought Lederberg was doing, saying that he was doing so well, in fact, that the methods that were used to bring him here were justified [laughter].

TAYLOR: Oh, I see [laughter].

BRINK: There was a hedge on it. That's right--almost justified, I think. Well, Lederberg's appointment was, of course, a very significant development in genetics. This was a turning point in the development of biology on this campus. Lederberg remained here for twelve years and just turned out one outstanding piece of work after another. As I say, he was awarded the Nobel Laureate the year in which he left, and out of Lederberg's work and his influ-

ence throughout the campus in general grew the present development of molecular biology. Lederberg was one of the founders of molecular biology as such, and of course his influence on this campus was very direct.

And I can say that he never presented any administrative problems. All these dire things that people predicted would happen when he came to our campus didn't happen. We were working under difficult circumstances then--just the end of the war. We were in a flood of students. We didn't have space for them, we didn't have equipment for them. We didn't have staff enough to take care of them properly. We had all kinds of problems that just almost overwhelmed us. There were all kinds of handicaps that new people like Lederberg had to face. He was developing a new area. We needed a new laboratory, we needed new facilities, new equipment. We didn't have a storeroom which would meet his ordinary needs for supplies--we had to make an arrangement with biochemistry. In none of these relationships, where there was plenty of opportunity for friction and trouble, did any trouble occur. Josh was a very thoughtful person. He realized what the situation was and he was determined to take his share of the responsibility in working it out. I always

had great admiration for him. We lost him, to be sure, but--

TAYLOR: How come?

BRINK: Well, he was bombarded with offers from other institutions, particularly after he got the Nobel Laureate. Harvard made him, I think, two successive offers, both flattering. California--they were anxious to recruit him at Berkeley. Perhaps the reason they didn't get him was that they weren't able to offer his wife, who is also a Ph.D.--Esther--in genetics, a satisfactory arrangement. Under their rules two members of the same family couldn't work on the same staff.

TAYLOR: The old nepotism problem.

BRINK: That's right, whereas we'd taken care of that here by having Esther on an army contract involved with state farmers. Well, Stanford came along with a very attractive offer and they were able to work out an arrangement whereby Esther could continue her work, too. And Josh took it, as much I think, just as a result of the pressure that these offers from outside created. He liked Wisconsin; we liked him. I don't think he had any particular criticisms of the institution. But there was the offer to leave, of course, so he moved away, to make a new start under, perhaps, somewhat increased financial support.

TAYLOR: Well, sometimes this is a real inducement.

BRINK: That's right, but I don't think it was. Wisconsin already was providing him with quite acceptable working conditions, and there was really little more that we could do than we did for him. It's not uncommon, of course, for people under those circumstances, it seems to me, to make a move. Most people make along about three moves during their academic career.

TAYLOR: It seems that way, more so now than it used to be, I think.

BRINK: Yes. Josh has donated his Nobel medal to the University of Wisconsin, with the State Historical Society as custodian.

TAYLOR: I didn't know that. That's very nice.

BRINK: His attitude toward the department was really very satisfactory. He had good working terms with everyone with whom he came in contact.

TAYLOR: In spite of the problems in getting him here?

BRINK: Yes, that's right. Yes.

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