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Berkeley, California

MARIAN E. KOSHLAND (1921-1997): RETROSPECTIVES ON A LIFE IN ACADEMIC SCIENCE, FAMILY, AND COMMUNITY ACTIVITIES

James P. Allison
Anne H. Good
Catherine P. Koshland
Daniel E. Koshland, Jr.
Douglas E. Koshland
James M. Koshland
Hugh O. McDevitt
Gail Koshland Wachtel

Introduction by Daniel E. Koshland, Jr.

Interviews Conducted by
Sally Smith Hughes
in 1999 and 2000

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KOSHLAND, Marian Elliott (1921-1997) Immunology Professor

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Retrospective interviews about Marian Elliott Koshland (1921-1997) with eight individuals, friends, family and colleagues. Koshland's support of immunology as a distinct field, role in the reorganization of biology at the University of California, Berkeley; membership and support of the Haverford College board of directors, National Immunology Society, National Science Foundation, Exploratorium board of directors, Jane Coffins Memorial Fund, Marian E. Koshland Integrated Science Center, Graduate Affairs Office in the Department of Molecular and Cellular Biology, Lawrence Hall of Science, National Academy of Sciences, National Institute of Health, Bay Area Immunology Club, American Association of Immunologists; work in the Brookhaven National Laboratory; women in science; husband Dr. Daniel Koshland, Jr., and children; balancing family life and research responsibilities.

Interviews with James P. Allison, Anne H. Good, Catherine P. Koshland, Daniel E. Koshland, Douglas E. Koshland, James M. Koshland, Hugh O. McDevitt, Gail Koshland Wachtel.

Introduction by Daniel Koshland, Jr., Professor Emeritus of the Graduate School of Biochemistry & Molecular Biology, University of California, Berkeley.

Interviewed in 1999 and 2000 by Sally Smith Hughes, Ph.D., the Regional Oral History Office, The Bancroft Library, University of California, Berkeley.

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INTRODUCTION by Daniel E. Koshland, Ph.D.

Marian Elliott Koshland was one of those rare individuals who had to be known to be believed. She had so many diverse qualities that it was hard to believe one person could embody all of them. She started life as the oldest child of two devoted but limited parents, who watched then child develop to heights they had never dreamed. She was an excellent student who put herself through Vassar College on scholarships and a job for her four years there. Her parents were too poor to be able to help her so she sewed her own clothes. She went to the University of Chicago for a Ph.D. because she could afford the railroad fare if she stayed up all night in a coach seat. She was passionately devoted to her family, her work, her garden, her students, and life in general. She became a member of the National Academy of Sciences, a professor of the University of California, a member of the board of the National Science Foundation, and yet managed to raise five children in a home where she cooked for her family of seven every night. She was a pioneering woman, not because she wanted to have confrontations, but because her era had lots of professors who did not want women on their faculties. She overcame the obstacles by ability and determination and a quiet integrity that melted opposition.

Since society likes to stereotype people, the determined career person is not easy to reconcile with the devoted family-loving, garden-loving, housewife but she managed to carry both roles with success and charm. Both her colleagues in the university and her kin in the home saw her as a larger than life vibrant character. These pages will give a little picture of that personality.

Daniel Koshland, Jr., Ph.D.
Professor Emeritus,
Molecular Cell Biology,
University of California, Berkeley

May 19, 2003
Berkeley, California

INTERVIEW HISTORY--by Sally Smith Hughes, Ph.D.

Marian E. Koshland was a professor of immunology at the University of California, Berkeley, who died on October 28, 1997 at the age of seventy-six. Her death was a loss to her family, friends, and the profession of immunology. It also was a loss to history. Several years ago, we had approached Professor Koshland about recording her oral history and had been rebuffed with the explanation that her best research was done before she arrived at Berkeley in 1965. (Professor Allison disagrees, as the reader will discover in his interview herein.) An equally likely explanation for her refusal could be that she was not one to trumpet her accomplishments.

It so happened that around the time of her death, I was negotiating with her husband, Daniel E. Koshland, Jr., professor emeritus of biochemistry at Berkeley, to conduct with him a full oral history. This project eventually came to pass. As the interview sessions drew to a close, Dr. Dan Koshland and I began to discuss an oral history retrospective on his wife, including who might be invited to contribute views of her professional and personal lives. The result, we hope, is a portrait, as full as we could reasonably make it, of an accomplished scientist, teacher, department chairman, wife, mother, and grandmother, who also found time for community involvement, gourmet cooking, and gardening--whew.

Of the many people who could have been invited for interviews, Dan Koshland chose the following because of their close personal and/or professional ties with "Bunny" Koshland, as her friends knew her. Briefly, in alphabetical order, they are:

James P. Allison, Ph.D., Professor, Division of Immunology, University of California, Berkeley [UCB], came to Berkeley as part of Dr. Koshland's effort as chairwoman of immunology to attract top immunologists to the department. Of those interviewed, he and Hugh McDevitt, M.D., provide the greatest insight into Prof. Koshland's contributions to immunology and place them in the context of developments in the discipline. The interview was recorded in Dr. Allison's office in the Life Sciences Annex Building on the Berkeley campus.

Anne H. Good, Ph.D., Senior Lecturer Emerita in Immunology, UCB, also commented on Koshland's immunological research. In addition, as vice chairman of the department of immunology in the years Prof. Koshland was chairman (1982-1989), she was able to speak from first-hand experience of these years, which overlapped with the grand scheme to reorganize biology at Berkeley. The interview was recorded at The Bancroft Library.

Catherine P. Koshland, Ph.D., Professor of Environmental Health Sciences, and Energy and Resources, UCB, is a Koshland daughter-in-law because of her marriage to elder son James Koshland. Of special note are Catherine's comments on her mother-in-law as scientist and mentor and her service on the Board of Overseers, Haverford College, which Catherine, Douglas (the youngest Koshland child), and Jim attended. Catherine tells of eventually following her mother-in-law on the Haverford board. The interview was recorded in Professor Koshland's office in University Hall at Berkeley.

Daniel E. Koshland, Ph.D., Professor Emeritus of Biochemistry, UCB. The interview in this volume is reproduced from one chapter in his oral history in progress. As fellow faculty members in the same umbrella department, Molecular & Cell Biology, and as her husband and father of their five children, he best bridges Marian Koshland's professional and personal lives, lives which she herself strove to keep separate.

Douglas E. Koshland, Ph.D., Associate Investigator, Department of Embryology, Carnegie Institution, and Adjunct Professor, Johns Hopkins University, Baltimore, Maryland, is a basic scientist like his parents. He, too, in his comments about his mother, straddles the scientist/family divide. The interview was recorded in his father's home in Lafayette, California, in the course of a visit, accompanied by his wife and two children.

James M. Koshland, twin of Gail, fourth Koshland child, and husband of Cathy Koshland, is an attorney and partner at Gray Cary Ware, LLP, in Palo Alto, California. He speaks in the interview as a Koshland family member and of his mother's service on the board of Haverford College, his alma mater. The interview was recorded in Mr. Koshland's office at Gray Cary.

Hugh O. McDevitt, M.D., Professor of Immunology at Stanford, speaks as colleague and friend of Prof. Koshland's immunological research and their joint service on committees of the National Institutes of Health and the American Association of Immunologists. He also places her research in the context of developments in immunology after World War II. The interview was recorded in Dr. McDevitt's office in the Fairchild Building at Stanford.

Gail Koshland Wachtel, Ph.D., Assistant Professor, Department of Physiology, University of Arizona, twin of Douglas and third Koshland child, speaks, with occasional interjections by her daughter Nadine, of life in the Koshland family. As scientists and mothers, both she and Cathy Koshland, marvel at Marian Koshland's ability to achieve in science and simultaneously raise five children. The interview was recorded in her father's home in Lafayette, California, in the course of a weekend visit.

All interviews were transcribed and minimally edited by the narrators. We are grateful to Dan Koshland for his support in many ways, including the underwriting of this project, and to the other interviewees who devoted time and thought to this partial reconstruction of the life of a remarkable woman.

The Regional Oral History Office was established in 1954 to augment through tape-recorded memoirs the Library's materials on the history of California and the West. Copies of all interviews are available for research use in The Bancroft Library and in the UCLA Department of Special Collections. The office is under the direction of Richard Cándida Smith, Director, and the administrative direction of Charles B. Faulhaber, James D. Hart Director of The Bancroft Library, University of California, Berkeley.

Sally Smith Hughes, Ph.D.
Historian of Science and Interviewer

Regional Oral History Office
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May 2003

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MARIAN E. KOSHLAND (1921-1997): RETROSPECTIVES ON A LIFE IN ACADEMIC SCIENCE, FAMILY, AND COMMUNITY ACTIVITIES

James P. Allison

A COLLEAGUE'S PERSPECTIVES ON MARIAN KOSHLAND'S CONTRIBUTIONS TO IMMUNOLOGY AND DEPARTMENTAL ADMINISTRATION

Interviews Conducted by
Sally Smith Hughes
in 1999

INTERVIEW WITH JAMES P. ALLISON

Allison's Education and Early Career

[Date of Interview: November 16, 1999]##[1]
[Berkeley, California]

Beginnings in Texas

Allison: I grew up in Texas; was educated there [University of Texas, B.S. 1969; Ph.D. 1973], and then did postdoctoral studies at Scripps Clinic [postdoctoral fellow, 1974-1977], in a lab run by Ralph Reisfeld who, it turned out, was someone that Bunny Koshland knew, had come across in the course of her career. Then I went back to Texas and worked at a small laboratory, called the University of Texas [System Cancer Center,] Science Park, for about eight years [1977-1983]. While I was there, I began to work on identification of the T-cell antigen receptor. The immune system has two kinds of lymphocytes, B-cells and T-cells, and they have different recognition systems. Bunny had worked for a long time on B-cell development and on issues associated with how they recognize antigen. Anyway, when I began doing this work, there wasn't all that much known on T-cells.

Hughes: When was that?

Allison: It was 1982.

Marian Koshland and Allison's Recruitment to Berkeley

Allison: I had published a paper--it was not necessarily immediately accepted--claiming that we had identified the protein that comprised the T-cell antigen receptor. It caught a lot of people's attention. I ended up going to Stanford [Visiting Scholar, Department of Pathology, Stanford University School of Medicine, 1983-1984] to try to clone the genes that encode that protein. I was working in the laboratory of Irv[ing] Weissman. Bunny somehow found out that I was there, I guess. I think it was at her instigation that I was invited to Berkeley

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[1]This symbol (##) indicates that a tape or a segment of tape has begun. For a guide to the tapes, please see the page following the transcripts.

to give a seminar, which I did, and then a few weeks later they offered me a job. She tried to recruit me to come here. I guess that was mid '83, something like that.

Hughes: Is that when she was chairman of immunology [1982-1989]?

Allison: Yes. So that was our relationship. I of course knew her by reputation and had been seeing her at meetings for years. She contacted Ralph, my postdoc supervisor, to get a recommendation, and I assume it was favorable. With Dan's [Koshland] help, she began to recruit me here. After almost a year of thinking about it, I agreed to come here.

Hughes: Why was Dan involved?

Allison: It was during the reorganization of biology, and I had an interest in cancer research as well. He was involved-- indirectly, I guess--in recruiting a new director for the Cancer Research Lab, which was an organized research unit. So I ended up being recruited for half a position in the soon-to-be Division of Immunology and then half as director of the Cancer Research Lab.

Hughes: Were you looked upon as representing a molecular approach to immunology?

Allison: Yes.

Hughes: That was one of the themes of the reorganization, trying to move Berkeley forward in terms of the molecular approach.

Marian Koshland's Scientific Contributions

A Molecular Approach to Immunology

Allison: Not that long before that Bunny had gone to David Baltimore's lab and actually cloned the gene encoding the J-chain, so in a way what I was doing was paralleling what she had just done. I had gone on sabbatical to Stanford to try to clone the genes encoding the T-cell antigen receptor. The difference was she was successful, and I wasn't. [laughs]

Hughes: During the Baltimore period, Dr. Koshland's research took a more concerted molecular direction. Is that right?

Allison: Yes.

Hughes: And before that it had been what?

Allison: Before that I guess what would pass for molecular in those days was more biochemical rather than genes and data. But she had always been more biochemical than a lot of immunologists. So in that sense she was always more molecular.

Hughes: Biochemical as opposed to biological?

Allison: Purely cellular; a lot of people would do things just in animals or looking at phenomena by mixing cells. She always did biochemical analysis.

Work on Antibody Specificity

Allison: One of her early marks, for example, in the early sixties was actually looking at the structure of antibody molecules at different specificities.

Hughes: That was Brookhaven work, wasn't it?

Allison: I believe so.

Hughes: I had one brief conversation with her maybe six years ago. I was hoping to do an oral history with her. And she said, "Oh, you don't want to talk to me"--or words to that effect--"because I came to Berkeley late in my career, and my best work was done before I came."

Allison: For me, that's certainly not true. Well, she published a paper and gave some talks in the early sixties when the controversy was still raging about the origin of the specificity of antibody molecules. She did some biochemical analysis that convinced her that there were different genes involved. It wasn't just folding. One idea was that antibodies folded around proteins.

Hughes: The template idea.

Allison: And the other idea was that there was a genetic basis for antibody structure.

Work on the J-chain and Regulation of Gene Expression

Allison: I wouldn't say the J-chain was all she worked on, but phenomena related to J-chain regulation formed the core of her work for the rest of her career. After she cloned it in Baltimore's lab, she began to study regulation of gene expression, which then took her right into the realm of molecular biology: What are the detailed events regulating gene transcription and B-cell development and function?

One of the papers that she presented at the American Association of Immunologists meeting the year before she died was, I thought, one of her best pieces of work ever. She described very elegantly a very complicated system of regulating transcription. She was right there with the mainstream, not just in immunology but in transcription in general.

Hughes: Isn't that rather amazing? She wasn't a young woman at the time that she took that sabbatical in Baltimore's laboratory, yet she learned a new technology.

Allison: Yes, that's the real hallmark, I think, of her entire career. She was never afraid to learn new things and to take risks. That was before I knew her well, but I spoke with Fred Alt and the people who were in David's lab at the time, and they talked about how dynamic she was, coming in there and basically changing the whole orientation of her research.

Promoting Recombinant DNA Technology

Allison: She became convinced that recombinant DNA technology was the wave of the future--really convinced, not just giving it lip service, but coming back to Berkeley and saying, "Okay, this is the way I want this department to go." You know, the little sphere of the university that she could affect as department chairman. "This is the way we need to go," and she just worked to make it happen.

Molecular and Cellular Approaches to Immunology

Hughes: Were you and she at that stage the only representatives at Berkeley of the molecular approach to immmunology?

Allison: No, there had been a guy named Hitoshi Sakano, who was here before I was. He had trained in Susumu Tonegawa's lab, who got the Nobel Prize for immunoglobulin gene rearrangement. Sakano had been recruited earlier than I was, so I guess he was really the first one who represented the more molecular approach. He was strictly molecular. He just looked at the details of how the genes recombined. I'm not quite that molecular.

Hughes: What are you?

Allison: Well, the field has changed since then. Back then you were either cellular or molecular; now you're just an immunologist. When you need to use molecular techniques, you do, and when you need to use the cellular techniques or even whole animals, you do. The lines are really blurred. Real molecular biologists, to my mind, are people who study the details of how DNA replication occurs, just basically how the genetic materials are passed on and how they regulate the production of proteins. Whereas as an immunologist, we're concerned with understanding how the immune system works, and the molecular approach is just another tool.

[tape interruption to review Marian Koshland's curriculum vitae]

Allison: I think the most amazing thing about Bunny's career is that in the five decades that she was active, she made very important contributions in every decade, beginning with her vaccine work, continuing right up through gene transcription in the nineties.

Hughes: That was a cholera vaccine?

Allison: Yes. I don't know the details of that except that she worked as part of a team.

Hughes: That was part of the war effort in World War II?

Allison: Yes.

Hughes: Was that project what hooked her on immunology, or was she already headed in that direction before that war project came along?

Allison: I don't know. I never talked with her about that. I'm sure that was what cemented her interest in immunology--how you could get antibodies, how they were made.

Immunology as an Academic Discipline

Defining Immunology as a Discrete Field

Hughes: Immunology hasn't been considered to be a discrete field until relatively recent times. Am I correct?

Allison: Well, it depends on whom you talk to. I think Marian would feel that it should be. I guess it hasn't been fully appreciated as an independent discipline, largely I guess because it is so specialized. It's associated with a single organ system, if you will, the lymphoid tissue. So in that sense the things that you learn about regulation of the immune system are not necessarily first principles that apply universally to all of biology, like how does DNA divide or how does a cell divide? How does a chromosome replicate itself?

Hughes: And yet some of the concepts seem fundamental: sense of self or self-identity contrasted with foreignness--

Allison: And gene rearrangement, and all that's involved in producing the receptors.

Hughes: Did Dr. Koshland identify herself as an immunologist?

Allison: Yes, certainly during all the time that I knew her she did. She felt very strongly that immunology *was* a discipline that was worth representing, that here was plenty of basic information to be gained.

Probably the best example of that is what we now know about DNA repair mechanisms, and rearrangements involved in building new amino acid chains came out of immunology. But anyway, she felt immunology was definitely worthy of consideration as a discipline and devoted herself, from all the time that I've known her, to making sure that it was represented on the Berkeley campus as a discrete, identifiable entity, and not just people dispersed in different departments. After all, she could have studied regulation of transcription of the J-chain as a member of the division of biochemistry or molecular biology. And David Raulet could study the genetic origin of NK receptors as a developmental geneticist.

A Larger Vision of Immunology

Allison: Marian and those of us who were brought here by her tried to build a group that understands what is unique about immunology and what makes it different. Marian, for example, has studied J-chain transcription and these other factors that she had identified, not just as an isolated thing that happens in your experiments, but as part of a bigger mechanism that actually builds and contributes to the organization of the immune system as a functioning unit. And so I think it's that bigger vision, that placement of the things we might be studying into the whole concept of organization and regulation of the immune system that sets us a little bit apart from people who might just be studying gene transcription, for example, as a process in and of itself.

President, American Association of Immunologists, 1982-1983

Hughes: Did this world view of immunology translate into some of her positions in scientific societies? She was president, for example, of--

Allison: The American Association of Immunologists.

Hughes: Yes. Was her wider view of immunology evident during her presidency?

Allison: Yes, I think so. The organization is of and by and for people who do immunology. It's an elected position. To get the respect of your colleagues, you have to be identifiable as somebody who cares about issues that set immunology apart. That's what that organization is all about.

Hughes: Do you know much about her role as president or any of the other positions that she held?

Allison: [Reviewing Koshland's curriculum vitae] I can't say that I know anything specifically about her platform or her specific activities as the president. But the fact that she was involved in all these different committees shows her participation in the affairs of an organization whose stated purpose is to further education and research in immunology and indicates the fact that she felt that was important.

A Woman in Science

Gender Equality in Immunology

Hughes: Was there anything unusual about having a woman in those positions in the seventies and early eighties?

Allison: I've heard comments from her family about problems that she had faced as a woman in science generally. In immunology specifically, I think that she was one of the first--probably not absolutely the first but one of the first women to get a real position of prominence in that society, although she isn't the only one since then. In fact, the membership of that organization is pretty equally divided among the sexes. I'm a councillor of the society now, and it's about half women and half men now. So she was one of those that broke the ice, if you will. Immunology is one of the first disciplines where women have really been on more or less an equal footing with men.

Hughes: Why do you think that is?

Allison: I don't know. In virology and developmental biology, there's still, for whatever reason, predominantly men. But in immunology there have always been a lot of women since I've been in graduate school. All the time when I was in graduate school and as a postdoc, it was pretty even distribution.

Role Model

Hughes: Did she consciously serve as a role model to young women for what they could accomplish in science?

Allison: I think she did, but I don't think it was that she necessarily did it in an overtly political way. I think she did it more as just an example of being the very best that she could. She never had these conversations with me, but I had heard that she had had conversations with women students where she said, "Look, you just might have to work a little bit harder, whatever, but there's no reason why you can't achieve whatever you want to achieve." And she did it spectacularly well.

It's not that she necessarily chose between family and career. She managed to do both successfully, and not in an overtly political way but just as being an example of that and making it clear to people that it can be done. She served as the best sort of role model, not somebody who points at herself and says, "Hey, I'm a role model." She was someone who naturally was. People recognized that immediately.

Rumors of Opposition

Hughes: Was there a predominance of women in her lab group?

Allison: It was before my time, but I understand there was quite a lot of opposition at Berkeley to her getting a full, regular appointment. [tape interruption] When she came here, Berkeley was pretty male dominated.

Hughes: Nineteen sixty-five.

Allison: I think that it was a battle for her.

Hughes: And that opposition was based on her gender?

Allison: I think so. It's just stories that I have heard about how she was first appointed a lecturer [Associate Resarch Immunologist, 1965-1969; Lecturer, Department of Molecular Biology, 1966-1970].

Hughes: Yes, although Dan now says that Wendell Stanley--her first position here was with the Virus Lab--was very accommodating. [2]

Allison: I should defer to him because I don't know first-hand.

Hughes: It's interesting that you've heard rumors.

Allison: The traditional way of being recruited is as regular ladder-rank faculty rather than as lecturer. [Reviews Koshland curriculum vitae] She was here for five years before she actually moved into a regular tenure-track position. This first appointment is basically a research-type appointment.

Hughes: Some of that may have been her doing. She had very strong commitments to her family as well.

Allison: That could be.

Hughes: That's not to say that at some point there wasn't also some opposition.

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[2] See the oral history in progress with Daniel E. Koshland, Jr.

Allison: I don't know if I would say opposition so much as maybe the prevailing attitude was that women can't have a family and do this. So it's not sexism in an absolute sense; more it's just women get saddled with burdens of family; that's why they can't handle some of the academic burdens.

Scientific and Interactive Styles

Hughes: Please comment on Dr. Koshland's scientific style?

Allison: The thing that most typifies her is just extreme rigor, that she had very, very high standards of quality. She was interested in knowing the bigger picture but also just extremely insistent on having data that was as solid as it could possibly be--well-controlled experiments, well-conceived experiments that ideally would leave you with no alternative explanation.

Hughes: And not publish until she was absolutely sure?

Allison: And not publish. And this was very frustrating at times for certain of her trainees. They felt that maybe they should have gotten more publications. On the other hand, her work was always solid. When it came out of her lab, it immediately had the respect of people that it was going to be correct. So I think that was the primary thing, just this rigor.

She also had a style around here that frightened some of the junior faculty at times because she would test people quite often by coming down and saying, "Hey, I just read this paper"--such-and-such. "Don't you think there's another explanation?" She would just force discussion of issues like that. And then she might come down a couple of days later and take the opposite stance. I figured out what she was up to. [laughs] It was a style of probing and learning, but also testing people, and I think it was rather unnerving to people at times. She would use all the techniques of debate; you ought be able to argue both sides. Even at the risk of [her] taking a stance that was obviously wrong, she at least made you marshal your arguments. You might say, "Why is she saying that? Nobody's thought that for a year or two." Anyway, it was a style of discourse that was unnerving to the faint of heart.

Hughes: Do you have any idea where it came from?

Allison: No, not really. But I experienced it quite a bit.

Hughes: Did you find it intimidating?

Allison: Sometimes, because you had to be on your toes. You couldn't be sloppy or lazy in talking about things.

Hughes: Did she have much small talk?

Allison: Oddly enough, she did about sports, which is something that I know nothing about. [laughter]

Hughes: She had a lot of other interests.

Allison: Yes. Actually, we could talk about food and things like that. She wasn't a one-dimensional person.

Hughes: Were you ever collaborators?

Allison: In the sense of actually doing projects together, no, not really; but part of it is because I've worked largely on T-cells and she worked on B-cells. When it became necessary to know something about B-cells, I very often would send postdocs down to sit and talk with her at length about some work that we had going on and needed some help in interpreting.

Hughes: Was she available for that sort of thing?

Allison: Oh, yes. Always. Yes, she was very accommodating, very helpful. A couple of times we had phenomena related to regulation of B-cell function. We'd run it passed her, and she'd give it her best shot. Everybody was so busy; we usually set up an appointment, and people would be accommodating. You'd go talk with them, and have your hour or whatever it was, and that was it. But if something came to her days or weeks later, she'd come in and say, "Hey, about what we were talking, I just thought of something else."

Hughes: A lot of follow-up.

Allison: Yes, there was a lot of follow-up.

Hughes: Did she have the big picture and also the details?

Allison: There are people who have just the big picture and forget the details and others who have the details and not the big picture. I'd say she was in the middle. I wouldn't say that she necessarily was a great synthesizer of the whole big field of immunology, but certainly she went beyond being interested in just specific phenomenology. She tried to place it in the larger context.

Immunology and the Reorganization of Biology at Berkeley

Koshland's Vision of a Discrete Division of Immunology

Hughes: When did you come?

Allison: I think my appointment began the summer of '84. I actually physically moved here in January '85.

Hughes: So you were here for about half her chairmanship, which was '82 to '89. That was the period of the reorganization of biology.

Allison: Yes.

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Allison: She was looking down the road, wanting to recruit more people into immunology so that when the reorganization occurred immunology would be a discrete, definable entity. So there was a lot of talk and a lot of planning, and we started recruiting. I guess Nilabh Shastri was the first person who was brought in to immunology in this new era. It was a difficult period because everybody knew that the department as it existed wasn't going to exist in a few years.

Hughes: Did that make people uneasy?

Allison: It made some people uneasy. She had a clear vision of what she wanted immunology to do. I shouldn't speak for the others, all of whom are gone, but I wasn't uneasy. I shared her vision of where I wanted it to go.

Building Interactivity and Cooperativity

Hughes: What was that vision?

Allison: Well, again, it was to identify people who were interested in the immune system at whatever level--at the molecular level, genetics level, biochemistry, or whatever--who were interested in understanding the immune response and trying to get together people who appreciated that and build a really interactive group. It went beyond just recruiting a few or several people and scattering them in this giant department. It meant building a group, and you can see it now. This whole floor is immunology; all the immunologists are on this floor.

Hughes: Before they were scattered?

Allison: They were scattered in the old Life Sciences Building. I can't remember anymore what floors we were on--it was so long ago--but there were two or three people together on the second floor, I think, and then there was a group on the third floor and one on the fifth--all over the place. One of the things that happened with the reorganization was this floor [in Life Sciences Annex] was set aside to be largely immunology. So there was a real effort made from the earliest time to get people who were interested in slightly overlapping areas of immunology so that there would be a basis for communication--and people were

collegial enough to interact--and to put them all together where they could share ideas on a daily basis. That happened as a result of that foresight.

Well, actually, it worked out with other things as well, I mean, just housekeeping. When we moved into this building, there was made available to the faculty a certain amount of money--I can't remember what it was--but every faculty member got some equal share of some money to get new equipment. People's centrifuges and things were probably thirty years old. If you're moving into a new building, you don't want all this antiquated stuff around.

Some of the things that we did with her leadership were to say, "Okay, everybody, let's put this money together and collectively decide what we can do. Instead of everybody buying a lot of little things, let's see if there isn't some way we can pool our efforts and avoid duplication and do some new things." So we did that. Again, it was her idea. She was in charge of the core of this building for a long time, but the principle was established that it's governed not by any individual but by the floor as a whole.

Hughes: Core equipment?

Allison: Core equipment. As I say, she was the one that spearheaded all that. If anybody wants to get a major piece of equipment, we still usually float it to everybody, particularly because space is at a premium. What can we give up? So this whole spirit of collegiality and group cooperativity was, again, by her.

Hughes: Aside from making more of your money, did it also foster better science?

Allison: Yes, I think so. Again, you can scatter this group of seven immunologists now in the department and still have those seven people there, but our students would [not],benefit the way they do now by the close interaction. Another thing, Marian felt strongly about keeping the doors open between the labs. You'll notice you can see all the way to the other end of the hall.

Hughes: I noticed that.

Allison: Each of us faculty members have our own labs, but we shouldn't prescribe the training of our graduate students. Students flow from lab to lab, material techniques here and reagents there. It's an open-door atmosphere.

Affinity Groups

Hughes: Is it more so than other departments after the reorganization?

Allison: One of the ideas behind reorganization was to develop affinity groups. I think we've done it perhaps more successfully because immunology is more identifiable. Those of us who are interested in these issues naturally gravitate to interactions with each other. There are

people, for example, who work on yeast genetics or developmental biology that are in the same department, but they didn't necessarily end up on the same floor. In some cases they are. In part it's because of the fairly limited size of the immunology group. But I think that spirit and collegiality of the affinity group is present in the immunology group to a higher extent than it is in the rest.

Hughes: And are you saying that that [collegiality] is somewhat a characteristic of immunology per se?

Allison: Not necessarily. At UCSF, for example, there are a lot of immunologists in different departments. They're scattered all over the place. But I think here it has to do more with having been part of the concept from the start.

Hughes: What about the role of personality?

Allison: Well, all I can say is that in this group [we]'re trying to take personality into account. When you're recruiting, we insist on people being excellent scientists; that goes without saying. But given the choice of someone who will come in, do his minimum teaching, lock the doors, and let you read about his research, as opposed to someone who will contribute and be interactive, we'll choose the more interactive person every time.

Head, Graduate Affairs Office, 1994-1997, Department of Molecular & Cell Biology

Hughes: There are two offices that Dr. Koshland had in the Department of Molecular and Cell Biology, as well as being chairman of the department. One was head of the Graduate Affairs Office [1994-1997]. What was that about?

Allison: In Molecular and Cell Biology, with eighty some-odd faculty members and a total of about 250 graduate students or so, and admissions of somewhere around forty a year out of maybe 700 or so applications, that job of just looking--I'm saying this backwards. Anyway, graduate education is something that we take very seriously in this department. Marian especially took it very seriously. It's a very big job because of those numbers I just mentioned. Everything from the financing of it: how do we make sure that we've got enough to cover the stipends of all those students? It means careful husbandry of the training grants that we've got and the discretionary funds that we get from Howard Hughes [Medical Institute] and other places, and graduate student and fellows stipends, teaching assistantships, the money we get from the university--just an incredibly daunting job.

Plus then looking after the students and making sure there were procedures in place for selection of the very best students, and then for policies that ensured that they got a quality education. Grievance--ways of taking care of problems the students might have with their mentors. When the department came together, Marian was very interested in those things and very active about them. I think she might have been the very first head of that committee [Graduate Affairs Office].

It's one thing to get the faculty together and say, "Okay, you've got a big department." But the other thing is dealing with the number of students that are involved for all those faculty, and looking out after them in an integrated way. She took that very seriously and put an amazing amount of work into setting up policies and procedures that we still follow. She always took it very seriously. If a student had a problem with a professor, which happens more frequently than we like, Marian was the one that people would go to because she cared if they had a problem. It's not that she was a pushover, but she could be counted on. I know several instances where with incredible discretion she would hear a student's complaints and either counsel the student and say, "Wait a minute. Relax. Maybe you're overreacting. Or, maybe there is really a problem here, and the faculty member is wrong, and we can do something about it."

Hughes: And then she would step in.

Allison: She would step in. The mechanics were incredibly complicated, because if you're going to bring in forty-five students, you've got to know that you have funding available to take care of them for five years down the road. And if you overspend and you have to cut back--so there's that aspect. It was almost bookkeeping, in addition to the intellectual challenge of how do you set up a procedure for getting the best students, and how do you look after them? Anyway, she did an amazingly good job on that. The same systems that she developed are, just with minor tweaking, still in operation now.

Hughes: I understand she was in that position [in Graduate Affairs] almost until the day she died.

Allison: It was a huge job.

Hughes: She must have been a very efficient time manager. Yes?

Allison: I think so, yes. I don't know how she spent her days, but obviously she got a lot done.

Graduate Admissions Committee, Department of Molecular & Cell Biology, 1991-1994

Hughes: The Graduate Admissions Committee was related?

Allison: It's a subcommittee of the Graduate Affairs Office. What you get [as a member] is a very major job. Once the systems were set up, that's where most of the work falls because they've got to do this process of choosing the students. Every year it has to be done again. It can be easier as the systems are developed, but it still has to be done. Most of the other stuff just chugs along. But that's not only just a matter of a lot of work but also balancing competing interests. The neurobiology people would complain that, well, there were students who were interested in neurobiology, but Marian didn't take them; she just brought in biochemistry types. There's a lot of competing interests that have to be balanced, and coming up with a fair system. Doing that was quite a challenge, and she managed to do that quite well.

Personality

Hughes: You've given some hints about her personality, but could you address it more directly?

Allison: She was a very strong personality. She didn't beat around the bush. Typically, you knew where she stood, except in cases where she wanted to trip you up and test your mettle by making you defend your position. So I think she was a very strong person. She wouldn't tolerate nonsense and sloppy thinking. She was pretty definite about most of her attitudes. It's not that she was unwilling to change them if she saw a good reason, but she wasn't going to not take a stand. She would take one and stick with it unless there came some obvious reasons to change. I guess that's the main thing. Just a very strong Type A person.

I feel especially privileged for having known Marian. As I told you when we began this, I was in Texas doing science at a relatively obscure place, and she rightly or wrongly recognized something in my work or whatever, and gave me the opportunity to come to Berkeley. I think my career has definitely benefited considerably from that. I owe a whole lot to her and her vision, as do all the immunologists in the program here. I think the people who were recruited while she was around would talk about how important she was in providing this vision of what immunology could and should be on this campus, making it possible for all of us to do things that would have been harder if she hadn't been here, lighting the way. So that's about it, I guess.

Hughes: Very good. Thank you.

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MARIAN E. KOSHLAND (1921-1997): RETROSPECTIVES ON A LIFE IN ACADEMIC SCIENCE, FAMILY, AND COMMUNITY ACTIVITIES

Anne H. Good

REFLECTIONS ON MARIAN KOSHLAND'S RESEARCH, THE ROLE OF WOMEN IN SCIENCE, AND SCIENTIFIC APPROACH AND CHARACTER

Interviews Conducted by
Sally Smith Hughes
in 1999

INTERVIEW WITH ANNE H. GOOD

Anne H. Good

[Date of Interview: December 6, 1999]
[Berkeley, California]

Education

Hughes: Dr. Good, would you tell me your background?

Good: I graduated from Visalia Union High School in the San Joaquin Valley and then went to Wellesley [1948-1952] on a scholarship with the idea of going into medicine. And from there I went to Yale Medical School [195421957], and took an internship in medicine [University Hospitals of Cleveland, 1957-1958], and then decided I really liked basic science better than dealing with patients. So I took a pathology residency [University Hospitals of Cleveland, 1958-1959] and then decided that I really was more interested in going into medical research. So I got into immunology and earned a Ph.D. [1963] at Western Reserve University--it's now Case Western Reserve.

Then I went on a postdoctoral fellowship to John Singer's lab at UCSD [University of California at San Diego, 1963-1966]. There I was working in collaboration with Leon Wofsy. He came up to Berkeley in 64. Now, at UCSD I had met my husband [Robert H. Good], who is now a professor of physics at Cal State Hayward, and I had one child while a postdoc at UCSD.

Lecturer in Immunology, University of California, Berkeley, 1966-1998

Good: So when I came to Berkeley, rather than going to a ladder position that involves research, teaching, administration, and grant writing, I decided I'd go primarily into teaching. Leon Wofsy needed to start a graduate lab in immunology in the Department of Bacteriology and Immunology because the students were coming in without much research experience. So that was my original position, and I was teaching the grad lab. It was quite interesting because people in other fields--zoology, parasitology, a variety of other fields--needed to use immunological techniques for their research but didn't have the facilities, so they'd come to the grad lab and use it for the immunology portion of their thesis research.

By the 1980s, grad students came in better prepared, and we started an undergrad lab in immunology in 1983. And so I ended up teaching part of the undergrad lab in immunology. All of the courses in MCB [Department of Molecular & Cell Biology] are team taught, and I taught the monoclonal antibody half of the undergrad lab. First Hitoshi Sakano, then Bill Sha taught the DNA part of the undergrad lab. I also began team teaching the upper-division lecture course in immunology in 1986. So that's what I did until I retired.

Hughes: And when was that?

Good: In July of '98.

Hughes: So you arrived at Berkeley a year after the Koshlands.

Good: I arrived in '66, and Leon Wofsy came in '64. So I came a year after the Koshlands.

Marian Koshland's Early Research

Antibody Specificity

Hughes: Did you notice this female professor of immunology?

Good: My very first contact with Marian was when I was a grad student back in the sixties, and I went to the Federation meetings in, I think it was, Atlantic City that year. The American Association of Immunologists is one of the societies in the Federation of American Societies of Experimental Biology. At that time, there was a very hot debate arising between the people who thought antibodies, which are highly specific, got their specificity by having the protein folded around the antigen as a template. That was the instructive theory. And the other group thought that that wouldn't be possible, that the antibodies must differ in structure.

In this meeting, Marian in a ten-minute oral presentation presented data in which she had purified two separate antibodies from a single rabbit. They were specific for different things. She had done a very careful amino acid analysis of them and shown there were differences in amino acid composition between the antibodies of two different specificities. This would conclusively say the template theory was wrong. And of course she got a lot of really hot questions from the instructive people, and she fielded them all beautifully. She was very good looking, and it was very inspiring to a young female graduate student to see somebody who looked great getting up there totally poised and knocking down a lot of these male questioners [laughter] in the way she fielded questions. She got a standing ovation at the end. Later she published the details of the work in her paper in *PNAS* [*Proceedings of the National Academy of Sciences*]. Of course, she was right.

Hughes: Do you remember the year of the oral presentation?

Good: The paper was published in '63.[1] I would guess it was 1960 or '61. It was someplace between 1960 and 1962.

Hughes: So that was Brookhaven work.

Good: Yes. And she did it with only one other person--one collaborator listed on the paper--a great contrast to the present days when you have umpteen people on these papers.

Hughes: Would you characterize it as a biochemical approach?

Good: Oh, it definitely was, because at that time she was doing the research they didn't know the structure of antibodies. They didn't really know that there were heavy chains and light chains and how many disulfides there were.

A Workshop Revealing the Kappa Chain Sequence, 1963

Good: The next time I saw her was as a postdoc. There was an antibody workshop in 1963, and that was the workshop at which Hilschmann and Craig[2] first presented the sequence of the kappa chain that showed that there's a variable region and a constant region in the light chains of the antibody molecule. Actually they were comparing the kappa chains from two myelomas, and there was one amino acid difference in the constant region, and John Singer quipped, "Oh, that must be the allotype difference." It turned out it was [laughs].

Hughes: He just pulled that out of the air?

Good: Well, yes and no. He was joking, but it was perfectly plausible that it could be true.

Hughes: Before her first oral presentation had you known Marian's name? Had people been reading her papers?

Good: Oh, yes.

Hughes: So she was already an established figure.

Good: She was already established.

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[1] See appendix, Koshland bibliography, #14.

[2] N. Hilschmann and L. C. Craig, "Amino acid sequence studies with Bence-Jones Proteins," *PNAS* (USA) 1965, 53:1403-1409.

Koshland's Initial Appointments at Berkeley

Good: When she came to Berkeley, I believe the nepotism rules prevented her and Dan from being in the same department. So I think she was a lecturer in the Department of Molecular Biology [1966-1970].

Hughes: Right. And she was in the Virus Lab as well as associate research immunologist [1965-1969].

Good: Yes, then in the Virus Lab. And then she wanted to join the faculty. So in 1970 she joined the Department of Microbiology and Immunology [1][1970-1989], which was then called Bacteriology and Immunology, as a full professor.

Hughes: I've either read or Dan told me that she didn't want a ladder position until her last child was in high school.

Good: I think that's correct. We took two different paths. When her children were young, she spent all her time on research [rather than taking on additional professorial duties], and I think her hours were more flexible. She might even have worked half-time during those years; I'm not sure. Although half-time means you're in the lab half-time, but you're doing a whole lot at home [chuckles]. So she did minimize her work time, because the ladder positions take a lot of time. I did the opposite: I got a full-time nanny--not a live-in--who came in every day and did light housework so that when the kids were sick--somebody's going to be sick every month [chuckles]--I didn't have to cancel class. Of course, she had five children; I only had three.

Hughes: I know that she had help.

Good: Oh, she did have help, yes. I think it's essential if you want to spend any time with the kids.

More on Koshland's Research

Cholera Vaccine and Secreted and Serum Antibody Structure

Hughes: You've said a little about her research. Do you want to carry it up to the present?

Good: Her initial work, I think, was working on the cholera vaccine during the war, and that got her interested in the mechanisms of immunity, so going into immunology was logical. Then she

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[1] Dr. Good pointed out that the department merged into Molecular and Cell Biology in 1989 during the reorganization of the biological science departments at Berkeley. For more on the reorganization, see the oral history on the topic which is in preparation.

discovered relatively early that serum antibodies, the circulating antibodies, were different from secreted antibodies that secreted into the intestinal tract and the milk.

Hughes: When you say different, you mean--?

Good: Structurally different, or at least they were a different size. The most obvious difference is a difference in size because the circulating antibody is mostly monomer, and the secreted IgA [immunoglobulin A] is a dimer. And so it would be quite natural for her to go into the biochemistry of the structure of the secreted and the serum IgA. It was very natural for her to want to study IgA because that would be the antibody that is protective in the gut, which is where you initially contact the cholera bacillus. Her original approach was biochemical.

Research on the J-chain

Good: I didn't really see her on the Berkeley campus until she came down to our department in 1970 and brought her lab down to the Life Sciences Building. By then she was studying gA, and there's a lot more of it in colostrum than there is in milk. She wanted to study human IgA. So when I was pregnant with my third son, I collected colostrum for her, except that of course after his arrival he ate all of her raw material [laughter]. Her major work in the 1970s was the elucidation of the J-chain, the joining chain, which is the chain that is necessary for polymerization of both IgA, the secreted antibody, and IgM, which is the first antibody that occurs in response to infections.

Hughes: I know she was working on the J-chain when she was on sabbatical in David Baltimore's lab.

Good: Ah, that comes a bit later. She had done the biochemical structure and characterization and established that the J-chain was necessary for the polymerization of IgA and IgM. Then in the early seventies [Herbert W.] Boyer, [Stanley N.] Cohen, and [Paul] Berg began developing DNA cloning methods. She was interested because she had always been interested in antibody and its role in immunity. What turns it on? What happens when the body starts making IgG instead of IgM antibody?

To go back a little bit, the fight between the template theory people and the structural people had long since been settled, and it was well known by then that each cell could make only one kind of antibody (the clonal selection theory). I think they had also figured out how they get a variable and a constant region from gene rearrangements--or that was coming around at the time she went to Baltimore's lab. My dates are a little bit fuzzy here.

Learning Recombinant DNA Technology

Hughes: Do you remember when she went to Baltimore's lab?

Good: No. I do have my series of little black books, so I could look it up.[1]

Hughes: It would be interesting to know how quickly she leapt onto the recombinant DNA bandwagon.

Good: It was pretty quick. Initially the variable and constant regions of antibody violated the "one gene, one protein" dictum. But then it turned out that the antibody genes are not expressed until the various segments rearrange and come together, and then they're turned on. So she realized that DNA was going to be important, and she had had no experience in DNA, so she arranged for the sabbatical in David Baltimore's lab. I have to take my hat off to her: a lot of people, when there's a major new technology coming in, don't embrace it; they sort of change their focus so they can go on doing what they're doing. Not Marian. She went off to Baltimore's lab, and while there, they began cloning the J-chain gene.

Hughes: Does "they" include Baltimore? Were they actively collaborating?

Good: She was collaborating, but I don't know to what extent he gets his hands wet in the lab. In big labs, some of the leaders do. Marian always did, but many of the big leaders don't. They will assign a visiting scientist to work with a postdoc. When I was a postdoc in John Singer's lab I was mentor for a faculty member from a college in Massachusetts. I don't really know precisely with whom she worked. But just in the space of a year she picked up the DNA cloning technique, brought it back [to her lab in Berkeley], and was able to use it. She completed cloning and sequencing the mouse J-chain gene after her return.[2]

Hughes: Do you know if other people here in immunology were using DNA cloning technology? Or did she introduce it to the department?

Good: Let's see, who was here then? Leon Wofsy was not using it at the time; he was still a protein chemist. I don't know what the microbiologists were doing. The microbiologists would have been Hiroshi Nikaido, who's still around; Alex Glazer, who's still around; Terry Leighton is still around. I can't remember when David Zusman came. And Dennis Ohman, who has since left, learned DNA techniques about the same time as Marian, I believe. Certainly Mike Doudoroff didn't, and neither did [Roger] Stanier use DNA, but I think they had already left before then.

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[1] Dr. Good consulted her black books but could not come up with exact dates. She believes it was sometime between 1975 and 1977.

[2] See appendix, Koshland bibliography, #54.

Immunology at Berkeley

Hughes: Who was in immunology at that point?

Good: Leon Wofsy, who was an immunochemist, Bob Mishell, and I was there, Marian was there, and Phyllis Blair was there. We were a pretty small group. Claudia Henry, a cellular immunologist, was there, but she was an adjunct professor. Mishell was also a cellular immunologist. Phyllis Blair was primarily in tumor immunology.

Hughes: Was Wofsy chairman?

Good: He was chair for a number of years off and on. Then it was Alex Glazer, and he was chair for quite a while, and then Marian [1982-1989].

Hughes: Alex Glazer would define himself as a microbiologist?

Good: Yes. He is, I believe, in Stanley [Hall]. Zusman is in Koshland Hall now. Ben Papermaster had left by then. Mishell replaced him. That may have been all the faculty in immunology. It was a very small group.

Hughes: I can see how in the past microbiology and immunology were related. But when immunology began to take off, I wonder about the logic of that relationship.

Good: It's still there, but you see now immuno is connected to everything. It's a very good model for studying development and differentiation. It still is connected to microbiology and virology from the viewpoint of vaccines and response to infectious diseases. And of course we are discovering that antibiotics are not as perfect as we had hoped. We're going to have to pay a lot more attention to vaccines.

Hughes: Hasn't the AIDS epidemic been a stimulus to immunology?

Good: AIDS has been a stimulus, and then also the other problems. Nobody knows precisely what triggers autoimmune diseases, but there's the molecular mimicry hypothesis: in people with certain histocompatibility types, there's enough similarity between a self-protein and some protein made by a bacterium or virus that the immune response against the bacterium or virus cross-reacts with self tissues and sets off a cascade of events that leads to autoimmunity. I don't think it's ever been solidly proved anywhere, but there's a lot of circumstantial evidence. And there are a lot of demonstrated similarities between certain self-molecules and certain pathogen molecules. So there's a lot of suspicion. Right now immunology and microbiology are different arms of [the Department of] MCB [Molecular and Cell Biology]. And then there's a part in biomedical and environmental health sciences--the infectious disease unit--that also has some immunology.

Hughes: Are those people in the same building?

Good: No, they are mainly in Warren [Hall].

Hughes: So there's not much interaction?

Good: Not so much. I had a lot of interaction with them because with my medical background they needed outside members for their Ph.D. orals committees [laughter]. Actually, just before I came, there was a big split between public health and bacteriology and immunology. There was some sort of feud between the higher-ups in medical bacteriology and basic bacteriology, and they split. And those of us who came later never completely understood what was going on. The younger people who didn't know what the feud was all about would interact and talk to each other [laughter].

Hughes: You had to pay attention to the dividing line?

Good: As far as admission of students, you had to because their philosophies of instruction and how the Ph.D. went were quite different. The public health people tended to do the traditional "learn the facts, take a written exam, and then do your research." The micro-immuno and MCB model was to get the students into the research lab as soon as possible and do a proposition type of exam to be sure they could plan research and elicit the necessary factual information during the course of that--defending their propositions. So it was a very different philosophy.

Research on J-chain Transcription

Hughes: Let's go back to Dr. Koshland's research. Have you said enough about her major contributions?

Good: The differences in antibody structure, the J-chain and its role, and then she has done a lot of work on the control regions, the transcription factors involved in the control regions of turning on the J-chain. She showed that turning on transcription of the J-chain is a vital step in the activation of the B-cell. She has been working on the transcription factors involved in the control of that region. J is funny because it has to start transcribing before the B-cell can secrete. But then after it switches, it turns off again, and the B cell makes IgG, which does not polymerize, as opposed to IgM. So she was intimately concerned with studying how J-chain is turned on and how it's turned off. It's basically a problem in regulation of gene expression.

Hughes: Which was again a new area for her?

Good: Once you're in DNA you have to be in regulation.

Hughes: So it was a much larger thing than merely learning a new technology.

Good: Oh, yes. She had to learn the philosophy, and she had to keep up with other fields.

##

A Woman in Science

Determination and High Standards

Good: She had this insatiable curiosity, and she would not rest until she found out what made those B cells work. I think that driving curiosity is what makes a good researcher.

Hughes: Do you have any idea where that drive originally comes from?

Good: It probably is inborn in some people and not in others. It can be fed or squelched, I guess, but basically I think it's something inborn. She always had determination, I guess. I never talked to her about this but I heard stories about, "Oh, you can't go to medical school," or "No, women don't get Ph.D.s," and she did anyway. I'm about ten years younger than she is, and even in my time they would say, "Well, you can get the Ph.D. but you'll never get a job."

Hughes: But you didn't listen, and she didn't listen.

Good: No, you just don't listen. That's one of the things that bothers me now about the constant talking about the need for a role model. You should tell the kids, "Don't keep looking for a role model. Be one. Decide what you want to do, and be a role model." I don't know why they don't give kids that kind of a pep talk.

Hughes: And that's what Dr. Koshland was, just by living her life.

Good: She didn't worry. There were women in medicine and in research before that, but essentially you would be a sore thumb and be discouraged. But some people say, "Look, I want to do this. By gosh, I'm going to do it."

Hughes: What were her ideas about feminism per se?

Good: She was very supportive of women. She was not one to do blatant feminism. We never specifically discussed it. She was very fair. She was a very demanding taskmaster, and she helped a lot of female grad students and students, but they had to perform. I think she probably believed in professionalism and good performance, and certainly she believed that women should have an opportunity and education. I think she basically just pretended the obstacles weren't there and just plowed right on through. Rather than make an issue of feminism she just said, "Well, this is what I want to do; I'm going to do it."

Hughes: And that was her tack with the students, too? She would encourage women, but they had to perform?

Good: Yes. She was very encouraging and supportive, but they had to perform. She didn't brook any nonsense. And if you didn't perform you heard about it [chuckles].

Hughes: I understand that she was quite a direct person, that you had no doubt about where she stood.

Good: You had absolutely no doubt about where she stood [laughter].

Hughes: Could she get angry?

Good: Yes, although I've only seen her really angry once, and it was about something or other in the remodeling of her lab, and it was not done properly and she was pretty mad. She could be very stern and very imposing with a misbehaving student, but I never saw her really angry in the sense of losing control or throwing stuff or having a tantrum. That was not her way. She was direct, but she was able to handle some pretty controversial and difficult people. I know she had one grad student who was very bright, but he had a chip on his shoulder and he would have a hissy about this or that. I was grad advisor at the time and I would have to listen to his complaints [laughs].

Hughes: How did she handle it?

Good: She was polite but firm. He could rave and rant but it didn't do him any good. He would have to do his thing. She would listen to him if he had a good scientific point, but if he was just bellyaching, tough [chuckles].

Scientific Approach

Hughes: What do you have to say about her approach to science?

Good: She was very rigorous and meticulous. She would really think things through, and she would analyze a problem from all angles. She wouldn't let a paper out until all the I's had been dotted and the T's crossed. Some of the students wanted to say, "Hey, come on, let's get this work published faster," and so on, and she said, "Well, how do you exclude this? How do you exclude that?" So she never had any [publication] retractions, to my knowledge. She was very careful.

She would meet with the students individually, and they had lab meetings at which they would have to present to the group. So people would need to make progress. They couldn't slack off because they'd have to talk about what they had done to the rest of the group. When there were problems, the group would pitch in with ideas and suggestions. She saw all of the data. Papers did not come out of her lab without her having thoroughly reviewed them. So no painted mice would come out of her lab.

Hughes: Was she on top of the research her students were doing?

Good: Oh, she was absolutely on top of it. And she would get in there and get her hands dirty at the bench some of the time. Of course, she didn't always have as much time to do that as she wanted, but she would go in and occasionally do things. She would be going through the lab and looking around and seeing things. Very frequently the people doing the DNA work would run into her office with a gel, and she would stop everything and look at it.

Chairman 1982-1989

Reorganization of Biology at Berkeley[1]

Hughes: She was available on a more or less day-to-day basis?

Good: Oh, absolutely. That's what makes her work as chair more remarkable, because she could handle both big jobs. She could do both, and she wasn't chair at a nice routine time. This was during the reorganization of the biological sciences. She had a number of prima donnas to deal with.

Hughes: In the department?

Good: Within the department and outside the department, because the role of the immunologists in [the new department of] molecular and cell biology had to be defined. There were a lot of people in other segments of what came together who were very anxious to protect their turf and to have as much turf as possible. And so she had to deal with them. She also had some things to deal with in the department. I was on the animal care and use committee during the period of our chairmanship, and I remember there was a big to-do about animal care on the Berkeley campus during the early eighties which resulted in the formation of the Office of Laboratory Care [OLAC], which is a very good thing. So there were those problems.

We had to reorganize the faculty, reorganize the staff. Marian specialized in faculty; I specialized in staff. That's why she required a vice chair: she said, "There's no way I can continue my research and do all that needs to be done." We were also a very small department with not quite enough staff to do what needed to be done. When you have a very small department, any illness, employee turnover, or performance problem is a disaster because there's no backup. And the MSO [Management Service Officer] retired [laughter]. I guess he only retired a year before the reorganization, but of course they weren't going to replace him because in 1989 the new department would take over.

So Jerry Corrazza from Physiology-Anatomy really bailed us out with advice some of the time [chuckles]. And Mitch Ikuta. Mitch has been in various places, but he was with the OLAC some of the time and then with botany, I think. Anyway, Mitch and Jerry helped us out. Finally they assigned Fritz Stern to us for a while, which was great, but then he went over to the College of Letters and Science [L&S] administration, I think [laughs]. So we had all of this mess to deal with. Marian had a major thing to deal with with the overall people planning for MCB to be sure that immunology came out all right.

Hughes: Were there individuals who didn't want that for immunology?

Good: Dan [Koshland] can tell you that better than I can, because I was dealing primarily with staff. We had our problems there because we were understaffed in the business office. This was a

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[1] See the oral history volume on this topic, in progress at ROHO.

big problem. During the time between the late sixties and the eighties, grants became larger; the experiments became much more sophisticated, involving equipment, reagents, all sorts of things that didn't exist before. Also, the government became much more interested in accountability, so everything became more complex--the bookkeeping, the nature of the materials that the staff had to order and keep track of, and so forth. Did we get more staff? No, of course not--budget.

That was another problem during those years. The budget was not defined for departments; that was college-wide [L&S]. I don't know exactly how they did it, but they got a minimal amount and then you'd have to ask for anything more for emergencies. It was very hard to plan. I almost couldn't believe it when I first came and asked, "What's the budget for the course I'm teaching?" and the MSO said, "Well, there isn't one." [laughs]

Hughes: Were you both spending longer hours on departmental affairs because of the reorganization?

Good: Oh, yes. Marian was concerned with dealing with the faculty from all these other departments in the formation of MCB. I was working with the Personnel Office on reclassification of staff into the big new department, quite aside from the fact that the service room was overstaffed and the business office was understaffed. But the service people were all almost ready to retire and not willing to learn new stuff. We couldn't train them to do the business work.

Then there was for me planning the decanting of the teaching labs. I was on a number of committees, seeing the needs of the various teaching labs. We had to figure out what equipment had to be purchased in the new buildings and the surge space, what student teaching labs would go where, when they could be taught. We had to make this huge inventory; that's what is on disks that I still have.

New Faculty

Good: During that time we had several recruitments. Hitoshi Sakano arrived around 1982, so I guess the recruiting for him had started before Marian became chair. I know Marian and I were discussing the remodeling of the lab for him because it was a problem; he was in Basel and we were here. So we had to go up and look at the lab. She had this really incisive mind. She looked at it and said, "Okay, the location of the windows and the shear walls will determine where we can put in partitions." And then we took it from there. Also his grants had to go through, and we had to put them through before he came.

Hitoshi was very competent, but he came from a tradition where you didn't have all this radiation safety and so on, and it was really amusing: his English was sufficiently good that he could dictate part of his research grant to me from a telephone booth at the Gordon conference. However, when you started talking to him about administrative red tape, his English deteriorated rapidly [laughter]. So Marian had to tactfully deal with this and manage to keep both Hitoshi and radiation safety from flipping out [laughter]. Somehow she managed it. He and Marian had a very good relationship because he was a B-cell man

working on [antibody] chain rearrangements. So they had a very productive scientific collaboration.

Hughes: They did collaborate?

Good: Oh, yes. I'm not sure that they did any joint papers, but they collaborated in the sense that they would bat ideas around.

Hughes: So she did have a colleague with similar interests?

Good: Yes. But you see, he came well after she had been to Baltimore's lab.

Hughes: For many years she didn't have people in the department who were interested in precisely what she was interested in?

Good: No, although Leon Wofsy was an immunochemist. Even though they were working on different things, he had been working on antibody structure and affinity labeling, so they could talk. She didn't have as much in common with the cellular immunologists, but they could talk.

The other thing that was a big task for her--there were two other recruitments, Jim Allison, who came in 85, and Nilabh Shastri in 87. These recruitments presented a problem because when you bring in new faculty you've got to provide space for them, and you've got to provide the appropriate facilities. Needless to say, the powers that be were not anxious to spend money on remodeling at that point. Marian had to negotiate some really interesting musical labs to get room for Allison. Physiology and Anatomy had to be persuaded to release the Packer lab, and Lester Packer went somewhere else. The teaching lab had to go into Leon Wofsy's research lab. He was then retired, and he had to contract and go into Stew[art] Madin's old lab. Public Health about that time left Life Sciences [Building] and moved over to Warren [Hall]. So Marian had to coordinate all of that to get Jim Allison's space. Nilabh Shastri, I think, went into some other vacated Public Health space, but the space he had was minimal. But he didn't arrive until shortly before we moved into the Life Sciences Annex [LSA].

The fourth floor of LSA was primarily immunology, but there would be some other people like [Satyabrata] Nandi from CRL [Cancer Research Laboratory], Bob Goodenow (who has left), and Dave Drubin from the divisions of Genetics and Cell and Developmental Biology, who would be there for a while. So Marian would have to deal with all these people deciding about designing the core facilities, meaning things like the centrifuge rooms, the tissue culture facility, the photography, that kind of stuff. She was direct, but she had a lot of skill in negotiation; otherwise this wouldn't have worked. So she had a formidable task.

The Koshland-Good Relationship

Hughes: Of course she was lucky to have you taking care of one aspect of the job. Wasn't yours an unusual relationship?

Good: Well, it was an unusual relationship. We consulted back and forth quite a bit. She was very supportive: she would back me when I had to get tough with some of the performance problems. Because the money was really bad in providing facilities for the teaching lab, she was very generous. She gave us a number of pieces of equipment that were no longer useful to her research lab.

She didn't need the chair stipend, so she gave it to me because I had two sons in college at the time. And she started the Young Investigator Fund. One of the grad students had his jaw broken during a softball game, and he needed serious dental work immediately. The university loan system was so cumbersome that it would have delayed his dental work, and she said, "This is ridiculous." So she contributed some money, and some of the other faculty did, and we set up the Young Investigator Fund which the MSO could put out immediately. And we opened a bank account. So she was very generous. I know one of her postdocs from China had a bleeding ulcer. She saw to it that he was taken care of.

Hughes: An amazing woman. How did the co-chairmanship come to be?

Good: She needed, obviously, somebody to do it. I did do some research for a while, but then the area I was in was impossible to fund, and so I didn't have the problem of getting research grants funded. When it gets down to a funding level around 15 percent or below, you can't tie up ladder faculty with too much administration or they won't survive. The other thing is that I was sort of laid back and not inclined to be a prima donna, and some of the [other] possibilities [for co-chairman] [chuckles] were much more prickly. I think probably the main thing was that as a senior lecturer I would have more time for administrative duties, and I would just substitute service for some of the teaching.

Hughes: Did you experience her as a lecturer?

Good: I mainly heard her presentations at scientific meetings and at seminars, and she was superb. She would be well-organized to the point her slides were not busy. She was an extremely effective lecturer. She did go relatively fast; if you didn't know your stuff you might get lost. But of course what I was listening to was the scientific presentations at meetings and retreats. She was very careful to drill her students who were going to present so they would do a good job. She insisted that they practice.

Hughes: She didn't want anything below standard [laughs].

Good: Absolutely not. She didn't miss anything.

Head, Graduate Affairs Office, MCB, 1994-1997

Hughes: She was head of the Graduate Affairs Office from 1994 up until the day she died.

Good: I think so.

Hughes: What do you have to say about that?

Good: That was a major responsibility. The graduate admissions process was an interesting thing. They would take in between forty-five and fifty-five students who would then go through rotations in the different divisions--Biochem and Molecular Biology, Genetics, Immunology, Cell and Developmental, Biology, and Microbiology. Then the students would spend one year going through the laboratory rotations, with different faculty members. Then they would have to pick a lab for their thesis work.

In the old days there were some substandard students who were permitted to hang around and defer taking their qualifying exams until it became an embarrassment not to pass them. So MCB solved that problem by requiring them all to take the qualifyings in the spring of the second year. And if they messed up, they could take it once more.

Hughes: They were out if they didn't pass on the second try?

Good: And then they were out, which I think was a good thing because it prevented people who shouldn't be there from hanging on and on and on.

Hughes: Did Dr. Koshland have a role in that decision?

Good: The real problem is that you've got to get members from each division onto the admissions committee. There's quite a lot of politics that goes on in the selection of the grad students in the different fields. People agree pretty much on the standards.

Hughes: Are you saying that large numbers of graduate students are considered desirable?

Good: Well, let's say graduate students are very cheap labor [laughter].

The other aspect of the head graduate advisor position that I haven't told you about is that they are the P.I. (Principal Investigator) for the training grants. And there were several. There was General Medical, the big one, and then Immunology had one, and I think there was another one. I don't remember all of them. Funding of grad students was a major problem because essentially you couldn't get decent grad students unless you paid them stipends comparable to those paid by Stanford and Harvard and so forth. You could not let them work at RA [research assistant]-ships or TA [teaching assistant]-ships or they'd be scooped and never get their Ph.D. They had to spend full time on their dissertation research.

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Good: It was a great advantage to a research lab to have at least one student partially supported by a

training grant for their first three years. And then the lab head would of course have to support them for the last of their training from his or her research grant.

Anyway, the graduate advisor has a delicate path to tread, making sure that the possible interests of the admitted students would allow a fair distribution of students over the faculty in the different divisions. So there would be a certain amount of politics involved in that.

I remember doing the 1984 immunology training grant with Marian. It was no fun. You had to get the faculty statement of research interest and bibliography and all that; that's fairly easy to get. But you also had to get from them progress reports [on] the students--who graduated? What papers? That is like pulling teeth. It's not their research grant. And then of course you have to write up all about the courses and so forth, and make sure that it meets the requirements of the funding agency. The responsibility of being the P.I. and prodding all your fellow faculty [laughs] is no fun. So that was major.

Marian had other roles. She was on the board of directors at Haverford [College, 1982-1994]. She would bring back teaching materials and show them to me, and she'd ask me for some of ours, so we had some exchange there. That definitely took her time.

Work Schedule

Hughes: How did she balance all her many obligations?

Good: I don't know how she did it. She had a lot of energy. She also did some entertaining for Dan, in his professional capacity. And she had to socialize in the course of recruiting for immunology. Of course, when all this happened, her children were grown and she did have help at home. But still the amount of energy she had was enormous because she'd have to keep up with the literature, keep up with her students, keep up with the administrative stuff. It was impressive.

Hughes: What kind of hours did she keep?

Good: She would come in to the lab between ten and eleven, and she wouldn't go home until fairly late. I don't know because I had to relieve my babysitter by six or so. I think she would stay until seven or eight, and then she would go home and have dinner, and then she would work in the evening and well into the night.

Hughes: Doing what?

Good: Doing her planning and so on.

Hughes: She wouldn't come back to the lab?

Good: Well, she lived out in Lafayette, so that wouldn't be practical. I think sometimes she and Dan

stayed in [Berkeley] and ate dinner out and did stay in the lab. But more often she planned so that the work that needed to be done in the office would be taken care of while she was here, so she could finish course planning and journal reading at home. But as I say, she had a lot of other things she needed to do. She loved to garden, and she really enjoyed the grandkids and did things with them. And she liked sports and art and so on. She made time for these other activities.

Health Problems

Hughes: You mentioned off-tape that you had an anecdote.

Good: Yes. I was just amazed at her stamina and what she did in the face of problems. When she was in Boston for that sabbatical in Baltimore's lab, near the end of the time she fell down the stairs and broke her hip. It was pinned, and she was right back on the job. She came into the lab and was working regularly and walking around with a cane. I remember being her roommate at Asilomar meetings--a midwinter conference of immunologists at Asilomar [conference grounds]--and we had a room that was not way far out, but it was reasonably far out. She was still walking with a limp then (she no longer needed the cane), but she toughed it out and went to the meeting. She just didn't let health problems stop her.

I remember in the nineties one hip began hurting, and she thought it was a complication from the hip pinning. She was saying that it would hurt more and more when she walked. Well, it turned out to be an arterial blockage in one of the main arteries to the leg, and she ended up having a graft replacement. That didn't slow her down for very long; she was back at work as soon as they would let her.

This is what you're going to have to check with Dan. Somewhat after that she developed uterine cancer and had irradiation for it. She worked right on through it, and nobody ever knew about it. She came into work.

Hughes: My heavens. She must have had a response to it.

Good: Sure, she had a response to it. The focused beam stuff now is pretty good at directing the irradiation to the target, but she said she did have some skin problems.

It was several years after that that the lung cancer first showed up as a pleural effusion, a fluid in the lung. It occurred very suddenly with a collapsed lung. She came back to work relatively soon, with her little oxygen tank in tow. She had a big one at home that she would use, and then when she came to work she would put this little portable one in the car and drag it right along with her.

Hughes: Was she smoking?

Good: Yes, she did for many years until she had to quit. I think that was why she never said that it was lung cancer. That only came out after. I think she was embarrassed. But she had

smoked most of her life. She did get better for a while. She was on the oxygen for a while, and then she got off it enough that she went up to Granlibakken, which was up at, what, 7,000 feet? The immunology retreat was up there, and I was her roommate there a couple of times. She did very well, remarkably.

Hughes: So she was a physically strong person, it sounds.

Good: She could tolerate a lot of pain and she didn't let it stop her. She never missed much.

One anecdote: it was shortly after she first came back to work after the lung cancer had hit and she was on oxygen. I stopped by her office to visit her, but unfortunately about a week before I had been doing a work project on Albany Hill and my armor against poison oak was not as good as I thought it was. My poison oak rash was in the really wet, weepy stage, but I thought it was adequately concealed under my pant leg. So I went into her office and she was there going through the mail and seeing what had piled up while she was gone. Finally she started staring at me, and she stared at my leg and then she said, "And what is that?", pointing to the rash [chuckles]. I sheepishly confessed, "Well, that's poison oak." She said, "You are old enough to know better!" And then she gave me the standard caution lecture about fels naphtha [laughter].

Hughes: Fels naphtha?

Good: Yes. The old-fashioned remedy is that you put on this laundry soap, fels naphtha, before you get exposed, and then you wash thoroughly with it afterwards. It's supposed to at least minimize [the poison oak rash]. Well, anyway [chuckles], there it was. As sick as she was, she didn't miss anything. She never did.

I visited her in the hospital a few days before she died. Her death was really a shock because she had been doing better with the lungs, and then she seemed to have some sort of a cardiac arrhythmia and she was going to go in for a pacemaker. That was, in her condition, not exactly routine, but everything should have been okay. She got a staph[ylococcus] infection, I think, which was really bad news, but it was responding to antibiotics. She was well enough to see visitors, so I went in to see her. There she was sitting in her hospital bed, and spread out on the table were grant applications she was reviewing. I think they were postdoc applications for the Jane Coffin Childs fund. That's what she was doing when I walked in.

Hughes: Isn't that amazing!

Good: I find that absolutely remarkable. Then tragically something happened very suddenly thereafter, and we never got to see her again. We expected her to get the pacemaker and come home, and we expected to be seeing her in the lab again.

Hughes: Was it a cardiac problem that killed her?

Good: I really don't know. The first lung collapsed suddenly and never reinflated. If the second one collapsed suddenly that would obviously--I don't know.

Personality

Hughes: Please say something in conclusion about how she struck people as a personality?

Good: Well, she was very personable. She was always good-looking. She always looked nice and trim, was beautifully dressed, always poised, always said the right thing. So she made a favorable impression, and I think people knew from the way she talked and conducted herself that she would brook no nonsense and that she didn't miss much. Her intellect was very impressive because she could talk about sports or art or music.

Hughes: Not a narrow person.

Good: No, not a bookworm at all.

Hughes: Did she chit-chat?

Good: We did chit-chat. She was interested in people, but usually we would chit-chat about her former grad students and what they were doing now. Most of our chit-chat was related to things connected to what we were doing professionally. She didn't treat people like machines. She was really interested in them as people. She would accommodate boyfriends, girlfriends, marriages, kids. When some of her people graduated and moved on up to British Columbia, she was interested in how they were doing and what they were doing. So we had that kind of chit-chat.

She would want to know what was new around the teaching labs, and I would want to know what was new in her research. Sometimes we would talk about things in the journal. So yes, there was chit-chat but not a lot of idle chit-chat.

Hughes: Your conversation was pretty professionally oriented?

Good: It was pretty professionally oriented. Bur she would also ask me about my kids, what they were doing.

Hughes: Did she talk about her own children?

Good: Some, but not a lot because I had only met Douglas [Koshland] and Gail [Koshland Wachtel]. Actually, Gail was there visiting with her daughter [Nadine Wachtel] the day I was in the hospital. And it was a good thing because you could just see Marian brighten when that granddaughter came in. When I was alone with her, she was too apt to ask medical questions. She had a very inquiring mind and she didn't miss much. But it was really a delight to see how she brightened when the little granddaughter came in and how good she was at talking to her. So she made a very positive impression. She's not the sort of person you would ignore or forget. She is very memorable. I think it's very appropriate that they named the library for her [Marian Koshland Bioscience and Natural Resources Library].

Hughes: Do you have any more comments?

Good: I think I pretty well covered that period.

Hughes: Thank you.

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MARIAN E. KOSHLAND (1921-1997): RETROSPECTIVES ON A LIFE IN ACADEMIC SCIENCE, FAMILY, AND COMMUNITY ACTIVITIES

Catherine P. Koshland

MY MOTHER-IN-LAW MARIAN KOSHLAND: A DIALOGUE ON CAREER, RESEARCH, SCIENCE, HAVERFORD COLLEGE, FAMILY, AND PERSONAL ATTRIBUTES AND INTERESTS

Interviews Conducted by
Sally Smith Hughes
in 1999

INTERVIEW WITH CATHERINE KOSHLAND

Catherine P. Koshland: Education, Marriage, and Career

[Date of Interview: November 10, 1999]
[Berkeley, California]

Hughes: Dr. Koshland, we're talking today about another Dr. Koshland, but I think it best to start with a thumbnail sketch of yourself.

C. Koshland: Well, I met my husband-to-be [James M. Koshland] at Haverford College when we were both students [1970-1972]. At the time, I was not engaged in science or engineering. I was studying fine arts and painting, and met Jim playing lacrosse, a game we both enjoyed and which he played at Haverford. My first introduction to his mother [Marian E. Koshland] was hearing stories about her. She was presented already as a formidable person in those tales of Jim's about growing up in her household. Eventually I did get to meet her, when Dan and Bunny came out to Haverford for Jim's graduation. I think Bunny and I liked each other right away.

My own story of evolving from an artist to an engineer came through a job in Washington [Office of Coal Research, Department of the Interior, 1974-1975]. An instrumental point in that transition came when Jim was accepted at Stanford Law School, and we decided to come out to Stanford for graduate school. Jim's parents said to us, "It's our observation that couples who are both in school together do better than couples with one person working, one person in school." Then they said to me, "Would you like to go back to school, and if so, what do you want to do, art or engineering?" So they helped us financially to do that. I was able to talk to Stanford and begin taking courses as an nonmatriculated graduate student in engineering [1975-1977] and discovered that I really did want to pursue that.

Hughes: How did the engineering come up?

C. Koshland: The job in Washington was in the Office of Coal Research at the height of the energy crisis. I was fortunate to be placed with a Ph.D. chemical engineer who was half-time at the National Science Foundation and half-time at the Office of

Coal Research. When he wasn't at OCR, I was the only person in the office who wasn't in a completely clerical capacity. I was there just by virtue of the fact that I had gone to a very good school and could write and think critically.

It was a very heady time to be in Washington because it was during the energy crisis and all of the agencies that eventually became part of the DOE [Department of Energy] were jockeying for positions in various ways. There was a lot of politics. There was a lot of science and engineering going on. It was a very exciting way to be introduced to a field, which was to say: there's a problem and there are various approaches to the solution.

I had originally thought that I would pursue a more policy-oriented program. When I got to Stanford, the Energy Institute there was chaired by Professor William Reynolds, who was also chair of mechanical engineering. I ended up pursuing a much more science and engineering set of courses, rather than, say, a social science dimension. I'm fortunate that now, twenty or thirty years later, by being in Berkeley, I can pursue aspects of social science. But that transition came by virtue of that job and that work experience, and then the opportunity to go back to school, which Dan and Bunny presented to us.

I did two years as a nonmatriculated graduate student in mechanical engineering; then did my master's year [1977-1978]. That was the year our first daughter [Sarah] was born and Jim's last year at law school. And then I went on to a Ph.D. program in mechanical engineering at Stanford [1978-1985]. Of course, at that point, we were the couple that Jim's parents had advised us against being, which was one person in the work world and one person in the academic world. But that's the world we've stayed in, so in that respect it was really okay.

When I was about to finish my Ph.D., the opportunity emerged to be at Berkeley [as an Acting Assistant Professor, 1984-1985] on the faculty of the School of Public Health. In the course of being a graduate student and also being a parent, at that point of two children (Maggie was born in 1980), I had listened long and hard to many things that Bunny had said about how to manage being a scientist and academic and a parent and a spouse.

Marian E. Koshland: Career Choices

Advice to a Daughter-in-Law

C. Koshland: One of the things that Bunny had encouraged me to think about was not being afraid to do something somewhat unorthodox. One of the choices when you are a mechanical engineer is to go into a school of public health, which, in many folks'

perspective, is an unorthodox route. Yet it meant for me many opportunities to study the more applied questions that I wanted to pursue and have an active role in shaping some policy questions that were related to environmental science. And it presented an opportunity to balance children and research and teaching perhaps more easily than I might have done in other venues. And the campus was within a sixty-mile radius of Palo Alto. All those things came together. So I ended up on the faculty in 1984 and have been here ever since.

Hughes: What other advice did Dr. Koshland give you in terms of your career?

C. Koshland: She probably was the most important mentor in my life in terms of how to do this. Number one was not being afraid to take risks and go on a somewhat unorthodox path, which I think she definitely did in her own work. The line of research she chose to follow in immunology was, as she sometimes described it, a backwater. Nobody was paying a whole lot of attention. That also meant there was room for maybe some innovative and creative thinking in that area, and an opportunity to perhaps pursue questions at a slightly reduced pace, which allows you to keep track of five children, which she did over her lifetime, and to make progress in making contributions but not to be necessarily in the throes of the hottest, latest research. In other words, one of the things she counseled was, you don't always have to go with a major trend.

On the other hand, the risk taking that she did in her own work was to go off at whatever age it was when she went off to MIT and Dan went to Harvard, on her last sabbatical. She must have been in her early sixties. I can't quite remember when this would have been. In any case, the important thing is that she went off and learned the DNA cloning techniques in David Baltimore's lab and brought them back to her students and her colleagues at a time when those techniques were just beginning to be adopted. In that sense she was very much a pioneer. So she was not afraid to take intellectual risks or to explore new dimensions. At the same time, she sometimes chose problems which were sitting on the back burner because, again, that allowed her to balance other things in her life. So there's an interesting combination there of risk taking and judicious selection of problems.

Her field and my field are very, very different. To take an area which is not getting as much attention, to take advantage of an opportunity when it presents itself, for me that was a [UC Berkeley] Superfund Basic Research Program that integrates toxicology and epidemiology and the biological sciences with engineering and environmental science. To pursue a line of questioning that worked in that framework gave me an avenue, a niche; allowed me to link things at Berkeley between public health and engineering. That link wasn't as strong as it might have been. It's much stronger now, not just on my part but on the part of others as well. But Bunny advised finding a route that wasn't what anyone would necessarily predict for you, and yet knowing that it's what you want to do.

The other thing Bunny advised was to really pursue something you loved and were interested in and not necessarily take on what was the latest, hottest thing if it didn't interest you, and not to do something that looked relatively easy if it wasn't interesting. So those were not just her direct advice but also her own modeling of what she chose to do, how she chose to do it.

Different Research Styles

C. Koshland: Now, there are ways in which she and I are very different. I have tended to be more hands-off, more like my father-in-law, in the way I manage my students and my lab. I have a wide variety of students doing a wide variety of things, whereas Bunny tended to maintain a much smaller operation. She always had a corner of the lab in which she was actively doing experiments. So we differed on that. That wasn't an issue. That was just two different styles of doing science.

She was very good about acknowledging that there was more than one way to conduct research, more than one way to manage a laboratory, more than one way to pursue a line of questioning, and that she and Dan had very different styles of working. Their one experiment in working together, at Oak Ridge, was a disaster. They decided they could have arguments over the dinner table, but they were not going to work in the same laboratory and pursue the same questions. They had very different styles of working and very different ways of thinking through a problem.

Bunny did not like to write, and she found writing very challenging, and she worked very, very hard, and sometimes it was frustrating to say: I'm going to produce fewer papers than Dan, who could write at the drop of a hat. Dan is a prolific writer. Bunny was the opposite. And yet my husband told stories of her ability to extract information from his friends, if not himself [laughs] on what they were doing. Bunny was able very quickly to get to the heart of what was bothering you or made you tick.

Avoiding "Hot" Scientific Problems

Hughes: Did this style of choosing a line of science that wasn't necessarily "hot" present problems with her colleagues?

C. Koshland: I don't think so because the corollary to that is that you do what you do very, very well, and you work very hard at it. And when I say "not hot," I don't mean you pick something that looks as if it's going nowhere. You take a problem that

perhaps someone says is just too hard to tackle at the moment, and it's going to be a long time to get a payoff, but you make the investment. And because the field isn't moving quite as quickly, because either it's not being investigated or the problems are so difficult it's going to move slowly anyway, that then gives you a certain amount of flexibility and breathing room and ability to choose which path looks more interesting to you, without being pushed by the latest, hottest, craze in the area.

Now, what Bunny ended up doing at various points was coming up with things and being involved with things that became incredibly popular, so it wasn't as if that strategy meant you never were in the hunt. There were times when in fact she was very much. When you talk to Hugh McDevitt or Jim Allison, I think they can point to those areas much more easily than I can in terms of the actual contributions that she made, which were substantial.

Bacteriologist, Biology Department, Brookhaven National Laboratory, 1952-1965

Hughes: How did she tailor her life to conform to the give and take required to balance science and family?

C. Koshland: She did a couple of things which were quite strategic. Maybe I'll just say a little about some of the ways Bunny got to where she was in her profession. She had her first child when she was writing her dissertation, the second one while she was finishing it and going off to Harvard for a postdoc, and twins at the point that she was about to embark on her first job, and then one more somewhere in the beginning of her first job. And this was 1947 to 1953 or '4.

She had a job offer on Long Island. Dan was already definitely going to Brookhaven. And the person who hired her thought she was a male. When he discovered she was a woman, he didn't want her anymore. And so she was left at a point of having four kids under the age of five, and no particular future. She was really ready to give up and quit. Dan, to his credit, said: "Look, If you spend 50 percent of your time doing research and 50 percent staying at home as a mom, you will be doing what any academic does because they can only spend 50 percent of their time in research; the other 50 percent of the time is spent teaching and doing university service. So don't say you can't manage this. This can be done." One of the things that was really important was that he was extremely supportive of what she was going to do, and that she could see a way to manage this.

How did she get to a national lab? There was a major conference that was being held and proceedings that needed to be written up and organized. Bunny negotiated with Brookhaven that she would take on all the procedure around this. She didn't want a salary, but she wanted a lab and a technician. And that's the

kind of unorthodox and creative thinking that she did. And that gave her a foothold. By the time that year was up, and she completed the task at hand, she was well established, and of course they made her an offer, and of course she became a scientist. And then she was able also to negotiate working half to two-thirds time the entire time they were at Brookhaven. Again, that's another reason why she always had her own hand in things, because she was not going to be always there as a [laboratory] manager, so her work was always very much her own, and she always had a technician and staff around her.

The Decision to Come to Berkeley

C. Koshland: When they came to Berkeley in 1965, she became an adjunct member of the faculty and pretty clearly stated she was not going to compete for a regular faculty position until Douglas, who's her youngest, had graduated from high school. And when he did graduate from high school, she then competed for and attained a faculty position and very quickly became a full professor [1970]. And so that's the position she occupied for the next twenty years.

Hughes: Did she attain these positions strictly on the basis of her own credentials or was her initial appointment a concession because UC was hiring Dan? Her credentials were solid?

C. Koshland: Yes. If you look at her publication record, you look at where she published, you look at the content, she established herself as an immunologist, as an independent scientist. I think had she chosen to compete for an academic position prior to coming to Berkeley, she could have done that. And it was a choice that really related to having five children and wanting to be involved in their lives.

When Jim's parents came to Berkeley, there was a shift in what they did. They had been very active in the Bellport community, which is the town they lived in on Long Island. Bunny had been active in the League of Women Voters. Dan was president of the school board. They were very ensconced in that community. It was a very painful and difficult decision to move to Berkeley. It was a six-to-one vote against coming. Bunny in the end said to Dan, "I'd rather you be beholden to me than I be beholden to you, so we're moving to Berkeley," which was really what Dan wanted to do but which no one else did.

I think if anybody made concessions in their professional life, it was Bunny. I think she certainly sacrificed both for Dan and to some degree in order to have children because she wanted to be engaged as a parent. She came into full-time professional life at age fifty, when many people are already thinking about cutting back. But it meant she was less available as a grandparent. If there was anyone who didn't get attention or time to some degree it was her

grandchildren. She has nine. And I think that was one of those choices that I'm not sure was necessarily an easy one. But I can understand from a professional perspective wanting, now that you can be fully engaged and be involved in the community, to be on the board of the National Science Foundation, to be on the board of the Haverford Board of Managers, to be president of the National Immunological Society. Those are things that she very much wanted to do and did extremely well and was very involved in.

Teacher

Hughes: How was she as a teacher/mentor of graduate students and postdocs?

C. Koshland: She and I would talk about our students and issues around students, and I was aware of the ups and downs that occurred, so we'd compare notes once I was on the faculty about these things. She took that role very seriously. She had a long-term interest in her students, even after they had left her lab. She was interested in them and kept in touch and provided advice and counsel at various stages in their subsequent careers.

I think she was very involved in their intellectual development and, from what I can gather, was a very effective teacher. A few of my students took her immunology course and found her to be a stimulating and demanding teacher. She had very high expectations for folks. She would have very high standards and hope that you would come up to them.

It was an interesting combination because she also tended to downplay her own strengths and downplay the strengths of those close to her, at least in a direct sense, so getting direct praise from Bunny was unusual. You heard more often indirectly that she approved of something that you did. And she was more apt to tell you more directly what she didn't approve of than what she did approve of--which wasn't always easy for her kids.

I think she always perceived that as being sort of bluntly honest about what was going on, and when you talk to people about her, her honesty and integrity are things that very much come out. But at times that also meant she was less forthcoming with praise. It might have made life a little bit easier at various times for various people if there had been a little bit more of that softness around the edges.

Associations with Haverford College

Member, Board of Trustees, 1982-1994

Hughes: Please talk about her service on the Haverford Board of Trustees.

C. Koshland: Both my husband and I and Jim's brother Doug are Haverford graduates. Doug's wife, Mary Porter, is a Bryn Mawr graduate. We were all there at the same time. Dan and Bunny had both been "visitors." Haverford has a special visitors' slot, the Phillips Visitors, where you come for three days and interact with the undergraduates and give some lectures. It's one of the college honors. Dan and Bunny had both done this and had been very impressed with Haverford. Their sons were looking for schools and applied to Haverford just because of their [parents'] experience there.

When the college approached Bunny to join their board of managers, it was at a point when they were beginning to expand their pool of people whom they would turn to as trustees. Haverford has been managed by a Quaker corporation since its founding in 1839. The pool of Quakers was shrinking. In any case, she was certainly one of the first people that they asked to be on this somewhat larger board. In fact, she may even have been approached before that and may have been part of the board that helped make a transition into an expanded board and more non-Quakers as a part of it, and to look to a structure that would allow for a wider array of talents and greater geographical representation. Haverford has always chosen to have at least three or four if not five or six board members who are academics in other institutions. And to have someone from Berkeley, from a big, major research university, was something that they valued. So the combination of being a parent and being a scientist was an important element in asking her to be on the board.

Hughes: Not that she was a woman?

C. Koshland: Oh, that may have been a factor. When did she join the board?

Hughes: She was on the board from 1982 to 1994.

C. Koshland: Haverford made the decision to go co-ed in 1978. I think they admitted their first upper-class women in '78. I and about three or four or five other women are anomalies who got degrees in 1972. We were called the refuseniks: we refused to leave. And one other from a group of about six is also on the board with me now. They also wanted to diversify the board, and so the fact that Bunny was a woman and a parent-scientist didn't hurt either.

It was a wonderful coup for Haverford to get her. It was also a wonderful experience for her because I think it allowed her to explore some of her interests in undergraduate education that were a little harder to do here at Berkeley. I don't know that one's colleagues often value your interest in undergraduate education. Graduate education is fine, and eventually Bunny did all sorts of things in graduate education in MCB [molecular and cell biology].

But being in a place where there was a focus on the undergraduate, on the eighteen- to twenty-two-year old, on the kinds of experiences that small liberal arts colleges can give, and it was a way of giving back, in a sense. She herself was educated in a small liberal arts college [Vassar, 1938-1942] where that intimate relationship with a science professor was absolutely instrumental in sending her on the path that she went on. So I think that was a way of giving back to that community. It was also because both Douglas and Jim and I had had exceptional experiences at Haverford. She was grateful as a parent for the kind of education that her children had received there. So I think there was a two-way street: there was a lot of benefit for Bunny, and there was a lot of benefit for Haverford.

I think her acumen, her ability to see through problems and say, "Here's what's going on," and then to come up with creative solutions-- Bunny never was someone who would just analyze a situation; she was always interested in the next step. She said: "Well, what are you going to do about it? How are you going to address this problem?"

I think she brought to the college an understanding of what it meant to have very good science. She was helped by the fact that there were three distinguished biologists on the Haverford faculty at the time who were interested in how they were going to make a transition in the college between what they had developed, some of the faculty who were there, into the next generation of faculty. That's a problem that plagues any academic institution, but particularly for small colleges, particularly when you have a cohort, a faculty that are all the same age, how you renew that faculty and do it in a timely way that allows you to maintain strength is difficult. It's one thing when you have a biology faculty on a campus like Berkeley, where you may have a hundred faculty; another thing when you have six or seven biologists, six or seven chemists, in a very different program.

Bunny was extremely interested in that particular problem: What do you do about deadwood? What do you do about faculty who are losing their interest? What do you do about faculty who have a real strength in one area and not in another; how do you balance that? Again, when you have small faculties, all those problems get amplified.

Chair, Educational Affairs Committee

C. Koshland: She chaired the Educational Affairs Committee, which I now chair, and worked very closely with the provosts. The provost at Haverford is the last stop, in a sense, before the president in terms of dealing with faculty recruitment, hiring, retention, promotions. She worked closely in terms of evaluating cases and offering counsel and advice that I think were critical to maintaining the quality of faculty at Haverford.

Bunny was broadly interested in faculty in general and pushed very hard to help develop resources for faculty research and support for junior faculty leave. Faculty at Haverford teach four or five courses a year. She pushed as a board member for finding resources to support research by assistant professors as they developed professionally. She was instrumental in helping them shape their programs in which they could compete, for example, for a Howard Hughes grant, which they got.

There are some very impressive things that that faculty was able to do, and I think Bunny was a sounding board and counselor and advisor in much of that. Her experience at Berkeley she brought directly to bear on things there. And so the combinations of standards, how you put these things together, how you present yourself-- There were faculty at Haverford who were capable of doing this, had done some of this but for whom it would have been more difficult--

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C. Koshland: --had she not been there. That focus, which was initially on the biological sciences, expanded, and she was instrumental in helping plant the seeds for what is now a very exciting development there, the integrated science facility. It in fact is being named for her, the Marian E. Koshland Integrated Science Center. A major reason it's being named for her is because of her contributions to fostering an understanding on the board of the value of inquiry-based learning at the undergraduate level, of the value of supporting faculty research and scholarship--not just in the sciences but across the board at Haverford; that the intellectual life of a university community involves the engagement of both faculty and students in inquiry, in scholarship, and asking questions and finding answers; and that it's not just a question of faculty transmitting content to students, but also side-by-side exploration of a question, and not only side by side with your biology majors, but walking across the hall and talking to your physics colleague about the problem, and then having the students collaborate and go forward was something else she was also supportive of.

So Bunny was instrumental in working with the faculty but also instrumental in educating the board about the importance of making this kind of commitment to the sciences, of moving Haverford into the next century at the

leading edge of undergraduate liberal arts colleges in science; and, as I say, in having that then go forward in terms of the other dimensions.

She and Dan helped establish at Haverford resources which enable faculty research. They're not limited to the sciences. They recognize the value of providing support to folks in history and English and art and other areas, as well as in science.

Hughes: To do what sorts of things?

C. Koshland: The faculty would write a proposal for funds to support sabbaticals or occasionally a semester off to explore an idea. And then an expectation that that would then inform, invigorate, change the dimension of your teaching as you revise courses based on your new discoveries, the discoveries of your students, that that would then continue to enhance and enrich the whole education process.

Hughes: Was Dr. Koshland considered the science voice on the board?

C. Koshland: Very much so. I'm trying to think if there's any other practicing scientist. There may have been one or two others when she was there. But she was very much a key voice.

Hughes: It's interesting that you in so many ways stepped into her shoes.

C. Koshland: [laughs] She worked very hard on that in many ways. I think she wanted that to happen, and she certainly took a lot of time to talk to me and work with me.

Hughes: Nineteen ninety-four she went off the board.

C. Koshland: And I went on the board in 1994. She was very good for me in suggesting which committees to be on. She knew where there were interesting issues and where my expertise might be most available. She was very good about that, too, on the Berkeley campus in what committees to be involved in and what not to be involved in. I've done things somewhat differently than she or Dan might necessarily have chosen to do, but their advice was nevertheless useful. [laughs]

Family and Social Life

Family Rules Perpetuated

Hughes: Was their advice sometimes intimidating?

C. Koshland: It was very intimidating, absolutely. I think in some ways it was easier being a daughter-in-law than a daughter--one step removed.

Hughes: Yes, it gave you a little independence.

C. Koshland: A little independence. That's exactly right. Also, Bunny and Dan were very hands-off. They did not want to impose, for example, an expectation that we would show up every Sunday for dinner, which in Dan's growing up was clearly an expectation in the family. We were given a lot of breathing room to be ourselves, and to interact and be together with them when we wanted to be, but also to not feel that there was an obligation that was going to mean we didn't have a certain amount of independence. So that was very good.

Hughes: Do you think that was a deliberate strategy on their part?

C. Koshland: I think it was a combination. I think it worked both ways. It was deliberate in the sense that they did not want to impose on us the way they at times may have felt they had been imposed upon. I think on the other hand, they also wanted their freedom, and they wanted to be able to pursue their interests. So I think it worked both ways.

Choices Regarding Social Life

Hughes: In talking with me, Dan was very explicit about how, when their children were young, they absolutely did not accept social engagements in the week.

C. Koshland: They didn't. And they had rules which have been perpetuated I think in virtually all of our families. For example, you are home for dinner. We have done that with our children since they were little. It was something that I remember Jimmy saying early on. Sometimes dinner was late because Bunny was out in the garden with her flashlight, tending the garden [laughs], and then they had dinner. Bunny always did her own cooking, and this is something, again, that we continue. My kids sat at the kitchen island or sat in the family room, which is right adjacent to our kitchen, and did their homework while I made dinner. Everybody would be in the kitchen, and homework would be being done, and conversation would be going on, and then you'd have dinner, and then you might disappear into rooms to study.

Bunny and Dan certainly had an active social life and circle of friends in Bellport, one that I think was very hard for Bunny in particular to leave. I think she had a wonderful group of friends in Bellport, who were women of all different types, who weren't in the academic world, some pursuing professions, some not.

I think one of the hardest things about moving from Bellport to Berkeley was the change in social life. They made a very conscious decision when they moved to Lafayette that their center of engagement was going to be the university, and they never really engaged in Lafayette. They did not become particularly active in that community. Having taken on the university, that became their community, although they chose not to live in Berkeley. But there was a very conscious shift in their thinking.

Hughes: Did Dr. Koshland ever again develop a circle of women friends?

C. Koshland: No.

Hughes: Her friends became her colleagues and her family?

C. Koshland: Yes. She had some graduate students with whom she was close, I think. But I can't think of any women with whom she was as close as people like Phyllis Strite and some of the other folks who were in Bellport and with whom she maintained a long-term contact even after they left for Berkeley.

Hughes: So she in a sense did make a sacrifice.

C. Koshland: Yes, definitely.

Life with a Young Family

Hughes: Talk more about the organization of a life with as many strands as hers had.

C. Koshland: She was fortunate in two things. One is they had the economic resources to have help. When the kids were little--there were five children under the age of ten--there were one-and-a-half people working. There was someone working full time, and then someone working at least half time, so there was a lot of support in terms of getting things like the laundry done and the house cleaned. It meant there was always child care. There was always someone in the home. If Bunny and Dan were not there, there was someone there.

Hughes: But Bunny was always the cook.

C. Koshland: Oh, yes, she always did. Luna was their principal nanny and second mom and everything else. Bunny may have done the shopping because she was picky about things like that. And Bellport also is a very uncomplicated community. It still has one stoplight. There was a whole network of folks around. As my husband would say, there was always someone around to keep track of all the kids and be available if an adult was needed. So she did have help. She often said this was

something that made life much easier for her, and she was grateful for the fact that they were able to do that.

Interactions with Dan

Hughes: My impression of Dan, whom I'm very fond of, is that he is a force to be reckoned with.

C. Koshland: [laughs]

Hughes: How did she deal with him? He speaks about how much the two of them interacted.

C. Koshland: Oh, they did, and not always peacefully. [laughs] There were times when there were lots of fireworks, although they were intellectual fireworks in the sense of honest disagreements or exploring different sides of an issue and debating and arguing and then coming forward to some resolution. They shared a very strong commitment to education, at all levels. Bunny was as passionate about Head Start and programs at early childhood and the impacts that those could have as she was in considering the value of postdoctoral education. She was interested in the entire gamut of ways in which people learn and are educated.

She and Dan didn't always see eye to eye. There is a famous story about the biology reorganization.[1] This was when Dan was head of the biology council, and they were getting ready to consolidate departments at Berkeley and redesign the whole way the biological sciences are organized on this campus. And he likes to tell a story that two of the most vocal opponents to the first version of the plan were Bunny and me. And if anybody thought there was a voting bloc on the campus among the three Koshlands, there was not! Bunny never hesitated to challenge him. She was important in keeping him grounded and helping him hone and shape his own arguments, because she would challenge things. He was not averse to challenging her and arguing.

[1] There is an oral history in progress on the reorganization of biology at UC Berkeley in the 1980s and 1990s which includes an interview with Daniel E. Koshland, Jr.

Personal Attributes and Interests

Efficiency

C. Koshland: Bunny was very efficient in how she chose to allocate her time. I'll give you a couple of examples. One of the pieces of advice she gave not only me but others was to be selective in the kinds of things you agree to do. So if you're asked to be on the NIH [National Institutes of Health] study section in your area, you drop everything else and you say yes because professionally it is not only prestigious and not only do you have influence, but you learn everything that's going on in your field, and so it provides you with a perfect foil for doing all the reading you ought to do anyway. If you can get two things for the price of one, do it.

Bunny was very judicious in her selection of what she chose to do. She didn't spread herself around into five thousand different things. She was very targeted in her choices and in how she chose to spend her time. That was true throughout her life. One of the organizations that was important in Bellport was the League of Woman Voters, and that was an opportunity to serve the community, to be outside the world of science, and do something different. She chose to do that when her children were growing up and it got her involved in things in Bellport.

I think she made choices at Berkeley about what she chose to be involved in, both professionally and otherwise. Serving on the board of the National Science Foundation was clearly something that had enormous benefit, was interesting, and in which she was able to have a real influence on programs that benefitted women. She had some unique insights into some of the challenges that were out there.

Not an Avowed Feminist

C. Koshland: She was not someone who ever would have identified herself as a feminist. She didn't like things like that.

Hughes: You mean the labeling?

C. Koshland: The labeling, and I think she also didn't have a lot of sympathy with whining about being downtrodden and oppressed. That was just not her side of things. She was very much the kind of person who just forged ahead, did what she wanted to do, and figured out how to make things work, in spite of obstacles and hurdles. She just creatively went around them.

Philanthropy

Hughes: Where does philanthropy fit in?

C. Koshland: I think early on it probably fit in in small ways. She didn't make a big deal about that. I think she chose later in life to serve in various capacities. She ended up on the board of the Lawrence Hall of Science; she ended up on the board of the Exploratorium. Those were things she was beginning to really enjoy doing. One loss from her early demise is that she was really beginning to enjoy the opportunity to give resources to make certain things happen, but also to be in a position to have a voice in shaping how those things might happen.

Bunny never wanted to give money without giving of her time and intellect as well. While she had some of her own resources that she was able to develop, a lot of the resources came from Dan's family and the Levi[-Strauss Company] enterprise. I think she certainly didn't mind that, but I think she shared that as a partnership with Dan. I don't think that was something she really did herself in a direct way. Now, did she have an influence and a voice in where some of his resources went? Absolutely. And I think she was instrumental, for example, in shaping their gift to Haverford. I think she was instrumental in shaping gifts elsewhere. But I think quietly so.

Strength and Vulnerability

C. Koshland: There was a part of her that was formidable, that was very strong and came across as this person who knew what she was about. I think those of us who were close to her often saw a very different side, which was someone who was much more vulnerable and much more often in need of recognition and praise, in a way that I think her colleagues in the outside world did not see. I think she wanted to be reassured at times that she had other dimensions to her besides the objective scientist and that her interest in the arts and aesthetic things was acknowledged.

Home Decoration, Design, and Gardening

C. Koshland: At times she was unsure of herself, even though she had an unbelievable sense of design and spatial dimensions. I think she could as easily have been in sculpture or landscape architecture as she could have been a scientist. Had pathways gone differently, she could have pursued some of those things with equal success. She

cared a lot about her physical surroundings. She enjoyed having beautiful things. She didn't need a lot. She was the opposite of a pack rat. She was always getting rid of stuff. She never accumulated that much stuff. But what she did acquire or did choose to have around was beautiful, and she was not averse to spending money for something that was particularly pleasing. But she didn't have to have five of them. The Yankee thriftiness was definitely there, but it didn't inhibit--

I think probably the most dramatic and extravagant expression in some way was her garden, which was really spectacular and to which she devoted a lot of time and energy and which was just an absolute pleasure to look at and a work of art. Her sense of design came out in their home and the little house they had for a while in Berkeley. She enjoyed working with architects and doing a remodel or planning a redesign of a room. But she didn't have to do it every year, and she didn't have to have something new all the time. She loved arranging flowers. Like Bunny, her arrangements were highly controlled and very formal. Throwing a bunch of flowers in a big vase was just not her style. Form was as important as, say, color or other things.

Hughes: You describe a very multidimensional person. But was her presentation to the world usually as the objective scientist?

C. Koshland: I think so. Very professional, very competent. Bunny would walk up two flights of stairs with a graduate student and get to the bottom of whatever was bothering that student, when you'd had them in your office for an hour before and still couldn't figure out what was going on. So she had a way of just going right to the heart of the matter and dealing with it.

There was a side of her that was more vulnerable and less sure, and she was brutally honest in her assessment of herself as to what she did well or didn't do well, as she was brutally honest with everyone else as well. That vulnerability, that need for reassurance, I don't necessarily think her colleagues ever really saw. I suspect perhaps her Bellport friends saw that and that there was a level where she could be entirely herself in ways that I think were more difficult at the university. The university is a community, but it's also unlike a community where one person may be the teacher and another person may be the lawyer. Here you're all faculty, so at some level you're all the same; at some level you're all competing. You make very close friends, but they're also your work colleagues. You have to have a professional relationship that I think makes intimacy just a little bit more difficult. It takes a lot of trust to have that level of intimacy within a work community--different than what happens, say, in a community such as Bellport was in the fifties and sixties.

I think there's also a difference when you create relationships when your children are young, and there's a bond that you have with people that bring their children up at the same time as you do that is not easily duplicated subsequently. When you don't have that platform with which to create relationships, it's

different. I certainly feel that myself. I suspect for Bunny that you can't go back. Once your children are older, you don't have that same kind of ability to connect with folks around the issues you might have connected with early on.

By the time they moved to Berkeley, their youngest was in seventh grade; their oldest was already at Pomona. When you have teenagers, you have a different set of issues, a different set of relationships, even with your children. Your children's friends' parents are not necessarily your friends in the way that that happens when your children are two and three and four.

Bunny in her understanding of how to manage complex lives and two-career relationships said if we're going to do this, I can't be split in more than a few places. So I'm still going to have to have my kids as a focus, but I'm going to then take the energy that I might have put into the high school or the community of Lafayette and I'm going to put it into the community of [UC] Berkeley, and am going to put my volunteer time into my professional life, not into something like the League of Women Voters. So those were changes and conscious choices she made when she got here.

The Role of Religion

Hughes: Was religion a factor?

C. Koshland: Well, there's a wonderful story. Bunny grew up in the Congregational Church, and probably when she was about to be confirmed, she asked a series of questions of her minister about God and faith and other things, and he really wasn't able to answer them to her satisfaction. She concluded that she was not someone for whom the church was going to be central, although it had been an important part of her life growing up. But I think she concluded that religion wasn't going to be an issue. I think Dan had arrived at the same conclusion about his faith. Believe me, their religious traditions permeated what they did. I think they formed, in Bunny's case, much of the basis of her ethic. She had very much the Protestant work ethic. And I think the "Protestant" was as important as the "work" in some dimension.

It is also true that much of Judaism in terms of its value of the word and the life of the mind as being a way in which to reach God was not incompatible with the way in which she and Dan thought about things. Because they came from two different faiths, they chose to not impose either on their children. I think for their children--I speak mostly for my husband--there was a level where they missed having some sort of focus.

Jimmy and I certainly have had the same issue. Had we lived on the East Coast, I think we would have been a part of a Quaker meeting and a Quaker community. Yet the meeting in Palo Alto is not quite where we are. Jimmy's been very active in Jewish philanthropy and Jewish community efforts but not at all part of the spiritual side. And that's been true for Dan. Dan has pursued Jewish philanthropy. I think Bunny didn't always approve of that. I think she was not entirely comfortable with that, much as she was embraced and found a family with Jimmy's dad's family--a family in which she felt much more a part than she ever felt in her own family. Her sense of where she was in her own family was very much different from her parents and her brother. She found in this big extended Jewish family a real place, a place to be an intellectual woman that was appreciated, a place in which having a career was encouraged and accepted.

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C. Koshland: Her mother, Margrethe Schmidt, had been a teacher, and it's not as if education wasn't valued. But I think the expansive kind of thinking, the multidisciplinary thinking that Bunny had as a part of her life--even though in some ways I would define her as a very disciplined scientist--typified a much broader thinking kind of person.

One of the other interesting things about Bunny and her science was that she was not someone who saw her field just for the sake of it. I think she could move from that to bigger problems and bigger questions. Part of serving on the National Science Foundation board was understanding the role of science in American life and the value of science in the context of a total education. So that kind of expansive thinking was what she found in Dan and what she found in the overall family, which I think her own family, by virtue of lack of education and lack of opportunity, didn't necessarily have, although it clearly gave her a grounding from which she could move.

But if you read her little autobiography in the *Journal of Immunology*[1], there are some wonderful stories in there, and she attributes her success to luck. She had the good fortune of having a circle of friends in high school who expanded her horizons and her world. They happened to be a group of Jewish boys, so that may have been part of the reason that being in a large Jewish family was sort of comfortable. She certainly had other people--teachers and individuals--who saw her as someone with talents and gifts and encouraged her in ways that her family wasn't necessarily able to do. They didn't discourage her, but they weren't able to encourage her.

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[1] Marian Elliott Koshland, "Sheer Luck Made Me An Immunologist," *Annual Review of Immunology* 1996, 14:ix-xv.

Ignoring Feminist Issues

Hughes: Do you remember having discussions with her about the stereotypical ways that women scientists are thought to go about their work? The stereotype usually emphasizes networking and cooperativity and tending to have smaller, more intimate groups and being less aggressive in terms of publication. Are these myths?

C. Koshland: I don't think she was a terrific networker, although she certainly maintained relationships and liaisons. But I don't think she sought out other women scientists any more than she sought out men. She sought out interesting people with whom to interact and collaborate; she was very much that way. And I don't think she sought out women students any more than men students or vice versa. She took students who were interested in what she was doing and who she thought would be creative and with whom she could work.

Hughes: So the science was the important thing?

C. Koshland: The science was the important link throughout all of this. So working with people who were interested in the problems that she was interested in was really what was the driving force. I will say this: I think her style of a perhaps more intimate laboratory, a smaller group is maybe typical of many women scientists. I would simply say that was Bunny's style. I'm not sure that she would have chosen to do things any differently.

I have a much bigger group, and I tend to be much less focused on one thing. I tend to have many more things going on. I think in that sense I'm much more like my father-in-law than my mother-in-law. Her publication [record] related to the fact that she didn't particularly like writing and it had nothing to do with being less aggressive; it was simply a combination of her own standards and expectations for how well she should write an article. She perhaps held herself to an incredibly high standard with respect to professional writing.

She didn't let the myths get in her way, for example, the myth that you couldn't have children or the myth that you had to delay having children. I have heard some outrageous comments on this campus by leading women scientists that young women scientists should delay having children, that you can wait till you're forty. You may be able to do that, but that's not necessarily the right path for everyone, nor is it necessarily the easiest path. I've seen too many of my colleagues struggle to have children at forty when it would have been possibly easier for them at thirty, from a biological perspective.

One of the things that Bunny really set as an example: all of her daughters and daughters-in-law--with one exception--who have chosen to have children chose to have them earlier. In other words, we were all lucky to find people that

we wanted to marry and be able to have that. But I think she was very encouraging in saying that having children when you're young and having your career when you're older is not a bad strategy, and we don't have to emulate men. We don't necessarily have to make our contributions at age thirty; we can make them at age fifty.

She very much typifies a very different pathway and one that I followed in my own life, although I've done more of the simultaneous having young children and having a career than she did. But I certainly emulated her in having full-time help and being strategic about outside activities and things like that. I think she was very influential in shaping my choices about what I was going to do when.

Bunny was not a slave to fashion in the sense of catering to the image that was out there in 1950, nor in catering to whatever image for feminism that existed in 1970 or 1990. In that sense she did very much her own thing. Part of that was supported by the fact that there were resources that allowed her to have certain independence by virtue of being able to have help.

She had a husband who was supportive of her career from day one. I think it helps that Dan was supported in pursuing his interests and not necessarily having to follow the family business. His own mother had said, "Be a scientist if that's what you want to be. You don't have to go into Levi's." So in that sense he had already made a break. I think that provided him in some ways with the opportunity to turn to his spouse and say, "You don't have to do what everybody thinks you should do. You can pursue this. You *can* do this. It is possible." In that way Bunny was lucky. As she said herself, she was lucky she found someone who would be supportive of her in that way--which is not to say that she didn't make some very traditional sacrifices. She certainly took the burden of bringing up five children, very much so. She was the primary parent. There's no question that her career took a slower path.

As I said before, she was strategic about that. Instead of crying bitterly about that, she figured out how to go after it and how to do it in a way that in the end, when she went full time on the faculty at age fifty, she had enormous energy and enormous interests. She wasn't burned out. She was not ready to retire. She was ready to take the world by storm.

Grandparenting

C. Koshland: I think it was hard on all of us as young parents to confront the fact that we weren't going to have a traditional sort of grandparent. That was something we all had to deal with and say, Oh, okay, the kids aren't going to spend the weekend at grandma's. We aren't going to get the kind of relief that many grandparents

provide to their two-career children's families because Bunny *was* actively pursuing her own interests and her own career at that point.

The flip side is she had enormous confidence in all of her children to do well and to function, and therefore she felt she wasn't needed. I'm not so sure that we all didn't say, "No, wait. We do need some things, and our children need some things." I think her oldest grandchildren--Hannah, Sarah, Maggie, Jessie, maybe Eliza--have a real sense of her. I think the little ones have much less of a sense of her, and they never got beyond the point of her being simply grandma. Perhaps Sophie and Jake, who are now twelve and thirteen, were somewhat intimidated by her and didn't get to get to the other side.

All the kids went through a period as they were beginning to shape themselves where it wasn't easy dealing with her because of her ability to ask these incisive questions that at thirteen you're not able to answer or articulate. And then, as they emerged into young women at fifteen, seventeen, eighteen, nineteen, they were able to begin to interact with her on a very different level. I think Hannah and Sarah in particular had a little bit more of a chance, and Maggie to a lesser degree and Jessie even to a lesser degree to get to know her as adults and relate to her in a different way. It would be interesting to see what they have to say about their grandmother! I think Sarah in particular, by virtue of being at Berkeley in that last year and a half or so of Bunny's life, had a very different relationship. Sarah made a really special effort to be with her grandmother through some pretty tough times. I think others of them would have had they been here, but she was the one child who was actually here. Subsequently, Hannah and she and Maggie all gave Dan a lot of support after Bunny died. He was very lucky he had three granddaughters on campus for that first year after he lost her. They kept him going. Now he's charging along![1] [laughs]

Hughes: Thank you.

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[1] On August 27, 2000, Daniel Koshland married for a second time.

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MARIAN E. KOSHLAND (1921-1997)
RETROSPECTIVES ON A LIFE IN ACADEMIC SCIENCE, FAMILY, AND COMMUNITY ACTIVITIES

Daniel E. Koshland, Jr.

THOUGHTS ON A LIFE WITH MARIAN E. KOSHLAND

Interviews Conducted by
Sally Smith Hughes
in 1999

INTERVIEW WITH DANIEL E. KOSHLAND, JR.

MARIAN ELLIOTT KOSHLAND, PH.D.

Upbringing and Education

[Date of Interview: May 26, 1999]
[Berkeley, California]

Hughes: Please tell me about your wife's upbringing.

D. Koshland: Marrying her was by far the most important thing I did in my life. It was a spectacular marriage. You're much too young when you get married to really realize all the things that go into it. But she was a most unusual person. I may be prejudiced, but there's pretty good objective evidence that she was.

Family

D. Koshland: Bunny grew up in a devoted family. The parents were devoted to each other but each was limited in different ways. Her father, Walter Elliott was a decent, honest man. He was a hardware salesman. He had grown up in the South in a family which he believed had been distinguished. I never could figure out how much. And he had southern prejudices. He would talk about his southern upbringing as though it was some glorious past.

His prejudices were important in Bunny's career. For example, when she was in high school, he said to her once, "Can't you bring anybody home whose name doesn't end in -sky or -vich?" He wanted good Anglo-Saxon names. And she did have some friends who were that, but she tended in high school to just like kids and didn't care about their origin. Bunny never heard any overt anti-Semitism from her father, but when I came along, it became important because I was going to marry her. Her father never said anything to me, but I found out later that he had big reservations. He didn't hate Jews, but he thought the marriage just wouldn't work. Whatever it was, she just overrode all of

those obstacles. If you read that biography[1] of hers she just brought her Jewish friends to the house and did what she thought was right despite her father's complaints. To be fair, he was always polite and they never knew he had prejudices, but it was typical of her that she just serenely ignored them.

Bunny's mother Magrethe Schmidt Elliott was Danish and spoke Danish until the time she came to the United States. But she had learned English and came over to the States as an English-Danish teacher. She met Bunny's father, and they got married. She was, I would say, more intelligent and maybe more cultured than her husband but also knew very little about the world.

Bunny grew up in a family that was really poor. For example, she told me that at one time, when it was a nickel apiece to go to a movie, they had a family consultation whether they could afford (four of them) to go to a movie for twenty cents, or whether they really needed that money to buy bread. That's how poor they were. The construction business was not a good one during the Depression.

Education

D. Koshland: Bunny had a very good high school record, and her mother sent her to some dancing lessons with a very avant-garde woman who was highly intelligent and highly unconventional. Bunny did so well in high school that she wanted to go to college. It was quite clear her parents were not going to be able to help her, so she'd have to support herself. Bunny thought she'd better go to a public school. Public colleges in the East are not as good as they are on the West Coast, and this dance teacher, who took a great interest in Bunny, suggested to her that private schools provide much better scholarships. The tuition costs more, but if you get a scholarship, it covers tuition and room and board. She'd have an easier time than going to a public university, which has scholarships for tuition but not for room and board.

So Bunny went to Vassar. Her parents thought it was ridiculous when she applied. She did anyway and got scholarships for four years and lived in the co-op dorm, the dorm in which girls did everything themselves. So that cut down on expenses. She had a job as a secretary in the art department during all four years. She sewed all her own clothes. She was a very well organized and determined young lady who got excellent grades despite all the extra work.

When Bunny graduated, she wanted to go to medical school, and she applied to schools based on whether she could afford to get there. She discovered that if she went to the University of Chicago, she could sit up all night in a coach seat for some very small amount of money, and she therefore could afford to go to Chicago where she had gotten a

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[1]See appendix: Marian Elliott Koshland, "Sheer Luck Made Me An Immunologist," Annual Reviews of Immunology 196, 14:ix-xv.

scholarship. She decided she had to have a job to survive. So she got a job in a research lab, with a very nice professor, William Burrows. Her professor thought she was very good at research and encouraged her to go on. But he joked saying she would never go back to medical school. She said, "No, no," she was determined to finish medical school. He bet her that she'd never finish. He said she had the research bug. And he was right, she stayed in research. She met me sometime in that first year--I've forgotten exactly when. I was doing research, so she got more and more interested in research and decided that she probably would prefer to go to graduate school rather than finish medical school.

War Work

D. Koshland: She finished the first year of medical school, and then I went to Oak Ridge [1942-1946]. She needed a job, and so she went to the Colorado Airborne Diseases Project, a war project. [interruption]

Hughes: Do you know how that happened?

D. Koshland: Oh, yes. Because they were recruiting at the University of Chicago for people to do this project, some Chicago professors were in charge of the Colorado project. She was recommended, and so she decided that she better do that to get some money. I had gone to Oak Ridge. We weren't engaged at the time, but we were very interested in each other. We wrote letters maybe every other day. There was a big stack of letters. So we were clearly very interested in each other.

During that year Bunny visited me in Oak Ridge a couple of times, and we saw each other in Chicago a lot. And then we got engaged and married the following year 1945. And it was a really great marriage. She was a highly intelligent woman. She became a member of the National Academy, a member of the National Science Foundation Board, and so on with many honors. But she was also a great wife and a great mother. She was really a pioneer in the era when it was tough for women to get jobs. A lot of places discriminated but the determination of her childhood came through. She never complained, just persevered and succeeded.

Early Professional Career

Gender Discrimination

D. Koshland: When Bunny graduated from the University of Chicago [1949], they told her she'd had this spectacular record, but she should never count on working there because they didn't hire any women. Can you imagine? Now nobody would even admit to those sentences even if they did it. But at that time they were proud of it.

Hughes: Was that the only time she ran up against discrimination?

D. Koshland: Oh, no. For example, after we were married and had children, we went to Brookhaven National Lab. I went into the biology department, and we didn't want to be in the same department so we wouldn't have criticisms of nepotism. The head of the medical department, a Dr. Rogers, I think, said he wouldn't hire a woman. So that was eliminated. Fortunately, the head of the biology department, Dr. Curtis, not only didn't get perturbed by that but also didn't even worry about the fact that we were husband and wife, so he hired her. She did very well.

Immunologist, Brookhaven National Laboratory, 1952-1963

Hughes: What was that job?

D. Koshland: She was a staff member in immunology. She had her own lab, and she also helped publish the annual Brookhaven volume on biology.

Associate Research Virologist, Virus Laboratory, University of California, Berkeley, 1965-1969

D. Koshland: In 1964 I was invited to come to Berkeley as a professor, and by then I was pretty well known, and Bunny had published really very good work. My condition was that if she didn't have a job, I wasn't going to come. And so Berkeley offered both of us jobs. Her job was not a teaching job at the time because she said she couldn't do three things--teaching, research, and raising children; she could do two out of three but not three out of three. She would be interested in switching to a teaching job when her kids got old enough to go to college, and that's what happened; she later became a professor of immunology, but at the beginning she chose research and motherhood.

Hughes: Did her first appointment require a special arrangement?

D. Koshland: Not really. Wendell Stanley gave her a position in the molecular biology department. He gave her a lab and let her do research and gave her money. Then, when she got a start, she applied for her own money, and then paid him back the money he had sort of loaned her in the beginning, although he never said she had to pay it back. I was concerned because I thought at the beginning he would put his name on all her papers. He was a Nobel laureate, so that would raise a question of who had the ideas, but he never did. He was extremely nice to her. He liked her, and she liked him.

Professor of Immunology, Department of Microbiology and Immunology, 1970-1997[1]

D. Koshland: It turned out in 1970 that the immunology department had an opening, and they offered her a job on the faculty, and her last child was in high school and was about to go to college, so that's what she did. Bunny worked there a few years as a tenure-track person, and then they nominated her to get tenure, and she got tenure, and she was a full member of the faculty.

Chairman, 1982-1989

D. Koshland: Then she became chairman of the department, and it was really a good thing for U.C. The department had dwindled. Professor Weiss went to Israel, and one retired, and one formed a company. So the department was very depleted. Bunny was really crucial in building the department back up. She was instrumental in landing Jim Allison and others like Alex Glazer who wrote that up in the account read at her memorial service.[2] She had a really good eye for picking people. She was really a very dynamic influence in increasing the number of faculty in the department. She became chairman of the department for two terms, and they wanted her to do a third term but she said no.

Hughes: Yes, 1982 to 1989.

D. Koshland: Right, so that was seven years. That's longer than the usual chairman's term. That was a very happy period. She liked that department, was very pleased with it. She kidded me when I was busy with the reorganization of biology at Berkeley, that the LSB [Life Sciences Building] was a terrible building, and she said she'd never worked in an attractive building in her life, and remember all the things she'd done for me. I said, "You're going to have an attractive building before you retire." And she ended up in a very nice building, Life Sciences Addition, which she loved.

But the important thing about Bunny was that she was a very passionate woman—passionate about life, passionate about her family, passionate about her science, passionate about teaching, passionate about citizenship, even passionate about her husband. It made her a wonderful yet sometimes difficult person to live with. For example, one of her passions was Christmas and sometimes when I was up at 3 a.m. wrapping packages, I would suggest maybe it wouldn't hurt the kids if all the packages weren't wrapped perfectly. But she would have none of it so we labored on. She took the same care with her students, her civil duties, and her gardening.

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[1] As a result of the reorganization of biology at Berkeley in the 1980s, the department in 1989 became the Division of Immunology, Department of Molecular and Cell Biology.

[2] Marian Elliott Koshland, 1921-1997, Memorial Service, December 1, 1997, University Art Museum, University of California, Berkeley.

Family Life

D. Koshland: I was so spoiled because when I'd go home at night, I didn't care about having any other social life. I was just perfectly happy to be at home with her. I remember at the beginning of our marriage people would be sorry for me because I had a working wife. The tradition at that time was, the wives stayed home and took care of everything. So because Bunny worked part-time it ended up that I had occasionally to do chores for the family. She did almost everything regarding the family, but I had occasionally, for example, to pick a kid up at the dentist. [interruption] But the conventional people would imply that that was something the wife should do, but it meant in our life that we both worked at night because she didn't need the entertainment that a wife who'd "labored over a hot stove all day" felt was required at night.

Evening Routine

D. Koshland: We had a routine every night. We'd get home about quarter to six, and generally Bunny and I would have a drink before dinner and would start to hear what was happening at school.

Hughes: So the children were present?

D. Koshland: They were all there. Bunny would be cooking and the kids would start talking and telling what had happened at school that day. But we always all had dinner together. There was a rule that everybody in the community knew about: the Koshlands didn't go out in the middle of the week. Monday to Friday with both parents working, we'd always have dinner with the kids.

At the dining table Bunny was unbelievably relentless about going around the table and quizzing the children on their school day. The tales were usually random during the cocktail hour but dinner was really the time to tell all. She'd start with the first child: "How did it go today?" The child said, "Well, it was wonderful." She said, "What do you mean by 'wonderful'?" They still remember that. They mentioned that in the memorial ceremony. All remembered the grilling they got as we went around the table. A lot of people commented to me afterwards that they were going to start doing it with their children. Douglas, who was the youngest, complained bitterly because as the others left home he was the only one left at the dinner table, so he got the full weight of both parents for the whole dinner. She was really a wonderful mother.

Hughes: Did the children learn to tell in full about their day? Or was it always a struggle?

D. Koshland: They got so they learned how to do it. They knew that their account couldn't be superficial.

We frequently started dinner late because Bunny would come home and she'd cook really excellent meals for all of us. Remember, there were seven people at dinner every night. We'd sometimes be in houses where working women complained that they were worried about a big dinner party for five people. Bunny was cooking for seven people every night of the week, and they were really good meals. The kids just got used to that.

After dinner we parents would split up, and she'd read to a couple of them and I'd read to the others, based on age levels. We'd put them all down around nine o'clock, and then we usually both worked from nine to twelve. We'd read articles or write up notebooks. Not every night; sometimes we went out. It meant that instead of coming home to a wife who said, "I've been working at a hot stove all day; I want to go out," I had the luxury of being able to work at night because my wife wanted to work, too.

Sharing Scientific Interests

Hughes: Did you talk in detail about each other's science?

D. Koshland: Not really. It was really wonderful: she was in immunology, which has a lot of biochemistry in it, so I understood what she was doing, but I never became enough of an expert to do research in the field, and she understood my biochemistry but she never became that much of an expert at what I was doing. But we understood what each other was doing, and we frequently made comments. For example, I was doing molecular biology, and I was using so-and-so's procedure. She could say, "Oh, I used to use that procedure, but I discovered this new one that was published by so-and-so a few years ago." That was a great help to me. And I did the same for her scientific interests, but we were never expert in the other person's field, and that was fun.

Membership in the National Academy of Sciences, 1981-1997

D. Koshland: And then, of course, we knew a lot of the same people--science is really a small town. She did a lot of things on National Academy committees, and I was on National Academy committees, so we overlapped quite a bit in discussing things like that.

Hughes: Who was elected to the Academy first?

D. Koshland: I was elected first [1966]. I don't know how my ego would have been affected if Bunny had been elected first. Of course, she was always pleased that I got elected, and I was pleased she got elected. It was enough of a male world that I'm sure it would have affected me if she got elected first. But the way it was, we both got enough awards so that we got greater pleasure out of the other person getting the award than we did from our own.

Hughes: Her curriculum vitae doesn't give the date she was elected, but I'm surmising that there were not very many women in the National Academy at the time. [interruption to retrieve publication on Members of the National Academy of Sciences][1]

D. Koshland: I don't remember the date, but there were very few women in the Academy. I was driving one of my daughter's friends home, a very cute little kid. She looked up at me and said, "Dr. Koshland, how does it feel to be married to a woman who's smarter than you are?" I laughed and said, "You know, it's one of the cleverest things I did. I can retire and depend on her anytime I want."

A Major Contribution to Immunology

Evidence for the Clonal Selection Theory

Hughes: Why did she choose immunology?

D. Koshland: That's described in her biography. She went to college sort of being interested in science, but not that devoted to any one area. Then she had a very excellent teacher in microbiology who got her going in microbiology and immunology. Microbiology departments were usually associated with immunology in those days. When she went to medical school, she got a job in the bacteriology department, though her graduate work was in immunology. So it was a gradual process. She went from general science to bacteriology to immunology.

Hughes: I read that one of her contributions was determining differences in amino acid composition of antibodies.

D. Koshland: That was a very big contribution. In my opinion, it probably was a Nobel Prize-winning discovery. She discovered the difference that decided between two theories of how antibodies are formed. One is the instructional and the other is the selection theory. She did a crucial experiment that really decided that it was the selection theory and not the instruction theory.

Hughes: Macfarlane Burnett's clonal selection theory?

D. Koshland: Correct, versus [Linus] Pauling's instruction theory. Bunny did the crucial experiment that could test the difference.

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[1]Bunny was elected to the National Academy of Sciences in 1981.

Facing Opposition

D. Koshland: She would present these results at meetings. She was a young, attractive, blond girl from Brookhaven, an institution that was not as well known as Harvard, Yale, and so on. There was a former Pauling student, David Pressman, who would occasionally get up after her speeches and say, "Well, I can't reproduce your work." I think if Bunny had been at a major institution, they would have just fluffed him off. But because she was a woman and sort of young and didn't have a big reputation, it took a while for her results to get accepted. I was just infuriated by this. I tried to coach her to just look at him very sweetly and say, 'Well, I don't know what's wrong, but I'd be glad to have you come to my lab and I could show you how to do it.'" But she didn't; she was just not going to be belligerent, and she didn't want to have a big fight, so she'd just let it go. But she won eventually; people gradually thought she did very good work. There were a number of people right from the beginning who thought she was right. But she didn't get the recognition she would have if she had been a man from a bigger institution.

Hughes: This was in the early sixties?

D. Koshland: Yes.

Hughes: People were still in debate about which theory was right.

D. Koshland: Yes.

Hughes: She was one of the people to swing opinion in favor of the clonal selection theory?

D. Koshland: Yes. Her experiment was really critical, and then people came along with other experiments, and it shifted for good.

Demonstrating That Different Antibodies Have Different Amino Acid Sequences

Hughes: Can you outline the critical experiments?

D. Koshland: Oh, the critical experiments are very easy to outline. She used an amino acid analyzer which was a very flighty and fragile instrument at that time. It was invented by [William H.] Stein and [Stanford] Moore, who got the Nobel Prize [1972] for it. Moore in particular was very fond of Bunny because she ran the instrument so well. [interruption]

A lot of people were sequencing multiple myeloma. In multiple myeloma, you make an excessive amount of one antibody, instead of making a moderate amount. It's sort of like a cancer; it just grows out of proportion. There were a lot of big labs all competing with each other in trying to get sequences of myelomas. But there was a real

question regarding that work because myeloma is a disease, and so you could not tell whether those antibodies are really illustrative of natural antibodies.

So Bunny devised a system involving two different natural antibodies. She was able, using the amino acid analyzer, to show for the first time that the two antibodies had different amino acid compositions. That really completely excluded the instruction theory. The instruction theory was that all antibodies were exactly the same in sequence, but were molecules that fold around the antigen, which was the noxious element you want to protect against. So they all would have the same amino acid composition but just would be folded in different ways. What she showed is they were formed differently, and the reason they folded differently was because they had a different sequence. Once she determined that, it was unequivocal evidence for the selection theory.

Hughes: Was she the very first to show that?

D. Koshland: She was the first to show that, absolutely.

Hughes: What had Burnett shown up to that point?

D. Koshland: Burnett and [Niels] Jerne postulated theories. I don't think Burnett did any more than that. Jerne published a more detailed theory, and later [Cesar] Milstein came up with the monoclonal antibody, which I think was the first application of the selection theory, but was years after Bunny's demonstration. But that was appreciably later [1975].

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Hughes: Do you remember Dr. Koshland being excited by the Kohler-Milstein discovery?

D. Koshland: She was very excited by that, yes. It was more that it was an exciting discovery in immunology than that it was a practical application of the selection theory. By the time of Milstein's discovery the selection theory was believed by all.

Hughes: I read that she spent time in David Baltimore's lab, and that it was there that she learned recombinant DNA techniques.

D. Koshland: Yes, that's where she learned eukaryotic DNA techniques. She had a general knowledge of DNA techniques before, but with bacterial DNA not eukaryotic DNA. The whole idea that antibodies determine the three-dimensional structure was pretty much accepted in biochemistry, but it was not accepted in immunology, and so she had to fight some of the big honchos in immunology to convince them.

Hughes: How could immunologists not think biochemically?

D. Koshland: It was sort of ridiculous, in my opinion, but antibodies were bigger than enzymes and some (not all) immunologists thought they'd be different. The idea was that two antibodies had to get together--a dimerization phenomenon--and that's the way you influenced the formation of an antibody. The best antibodies were formed against big molecules, like the whole bacterial surface, and a small peptide generally was not a good

antibody. The antigen had to be part of a protein to be very antigenic. The argument was that two antibodies had to come together to be active; it was called an association-dissociation model. Biochemists didn't like it as much as her idea, the idea that the three-dimensional structure was determined by the folding pattern. She then took two small peptides and that showed that the amino acid composition of antibodies against them was different, and that clearly was revolutionary. But the immunologists kept saying that the big molecule was needed to really get a very effective antigen.

Hughes: Did she take that more biochemical approach because she had access to an amino acid analyzer?

D. Koshland: Well, everybody had access to amino acid analyzers, because they were commercially available. I had postulated the induced fit theory, so she heard a lot about that because she was married to me.

Hughes: That theory seems to fit.

D. Koshland: Of course, that fit perfectly because, protein was a big molecule, and the antigen bound here [demonstrating] and then the antigenic part turned on something called the Fc region, which was the complement fixation region at the other end of the molecule, and that clearly suggested an induced conformational change, which she believed, too. The nay sayers who were then reduced to a Henry Metzger at NIH didn't want to believe that. It was in his opinion not caused by the induced conformational change; it was the association of the molecules.

A Critical Experiment

D. Koshland: Bunny did what I consider to be an utterly brilliant experiment. She took a section of the active site of papain, which is a protein, and dinitrophenolated it. Dinitrophenol is known to be a good antigen-producing molecule. She dinitrophenolated it and she took the small peptide out. She found it was a much better antigen when it was part of the big protein than when it was just a little peptide. The reason that Metzger et al. gave for that was this association-dissociation reaction was made possible by the big molecule.

And then she made the protein to the dinitrophenol papain. Papain is an enzyme of about 25,000 molecular weight, whereas the peptide is 1200, let's say. And then she chopped the big protein down, down, down, down until she got back the small 1200 [molecular weight] peptide, where binding affinity was what decreased. But she could saturate the protein with the small peptide at high concentration and it was just as good as an antigen. So that was really good proof that what she was saying was correct and that the other people were wrong. This was work after her early work on the selection theory on the different sequences.

Two Sabbatical Leaves in Boston

Learning DNA Technologies in David Baltimore's Lab

D. Koshland: Bunny was doing this rather biochemically and I wanted to go on a sabbatical, but Bunny didn't want to take a year off and just be a housewife. By then our kids had grown up, and so we tried to find a place where she could go to a scientific lab, too. So I went to the Harvard chemistry department, and she went to MIT, to David Baltimore's lab. She became really expert in DNA handling of mammalian systems; that was very advanced at the time. And I was doing DNA work with bacterial systems, which are a lot easier than mammals. Bunny became better at the DNA techniques in mammalian cells, than I was and I would get good advice from her.

Hughes: Why did she want to work with mammalian cells?

D. Koshland: Bacteria don't have immune systems. She was always telling me that fish, I think, are the lowest species that has an immune system. But she was working with rabbits and mice and humans, and eukaryotes and mammals.

Hughes: Was Baltimore one of the leaders in the molecular biology of mammalian cells?

D. Koshland: Yes, he was. He was just getting interested in antibodies. His main work before had been viruses, a very important virus, namely, polio virus, which of course was the basis for the Salk vaccine and a lot of vaccines, which was immunology. So he was pleased to have her in his lab. He taught her how to do molecular biology, and she taught him a lot of immunology, so he really liked having her. They got to be very good friends. Seven years later, I had a second sabbatical, and I went back to Harvard, and Bunny went back to MIT with David.

Hughes: Into Baltimore's lab?

D. Koshland: In Baltimore's lab. It was very hard to get into Baltimore's lab. It was very popular.

Hughes: What did she do the second time?

D. Koshland: She did the same thing. The second time, she broke her hip--fell down a staircase in an apartment we had rented. But she got her hip fixed, and she went to the lab on crutches and continued her experiments. The young students in Baltimore's labs were really impressed.

The first time, she knew very little DNA cloning.

Hughes: When was the first sabbatical?

D. Koshland: I've forgotten when it was. [gets up to find his bibliography]

Hughes: Is that when you wrote the book on chemotaxis?

D. Koshland: I think the book on chemotaxis I wrote on my first sabbatical, and my second sabbatical was in the [Harvard] chemistry department. [skimming his bibliography to check publication date] Okay, 1980 was the chemotaxis. So I probably had my first sabbatical at least seven or eight years before that.

But Bunny really got to be very good at working with mammalian cells. She used them a lot.

Hughes: Recombinant DNA?

D. Koshland: Recombinant DNA, all of that, yes. Working with human chromosomes is really a great deal more complicated than working with bacteria. My work started to go more in that direction. She was very helpful to me.

Hughes: Why is it more complicated?

D. Koshland: Because the gene is much bigger. And then there's splicing involved in mammalian genes--introns and exons. That's a much more complicated genetic system than it is in bacteria.

So anyway, Bunny was really one of the pioneers. You can ask Tij [Robert Tjian] about her. She did a lot in the [cell signals] transcription area, and she shared a lot of equipment with Astar Winoto in the department. They didn't publish together, but Winoto exchanged ideas with her a lot. She really liked him a lot, and they had adjacent labs, so their students would flow back and forth. She really liked that.

Contrasting Scientific Styles

Hughes: Please talk about her scientific style and compare it with yours.

D. Koshland: We kidded a lot about the differences in our scientific styles. I tended to be less cautious than she was. I would jump to conclusions with less data.

In World War II, I became a group leader at Oak Ridge, even though I only had a bachelor's degree. I had fourteen people working for me, seven of whom were Ph.D.s. Bunny was a pretty independent woman, and so she enlisted for a job in Oak Ridge, where they had the hiring center. She put her name down as Marian Elliott because she didn't want to trade on my reputation. They hired her, and they assigned her to a plutonium project run by a guy named Daniel Koshland.

Hughes: [chuckling]

D. Koshland: And remember, it was not easy to get jobs. She was not at all sure when she found out that she had been assigned to my group if she said no and left, she'd ever get another job.

So she accepted it, and she had to work for me. That was a big strain. If you got a really good result, my theory was, you apply that result and go onto the next thing. If that blows up, you say, Well, maybe I made a mistake and go back and repeat. But the idea of running a duplicate result I always felt was silly because most of the time the result turns out to be right. So I say, think of a new experiment that confirms but goes on instead of just repeating what you did. Whereas the tradition in science is you always duplicate a result before you go on to the next thing. And so she really felt she should do that. I told her, "No, I'm the boss. Go on to the next experiment." So we had these terrible fights. I was kidded that she never would have stayed married if she had to continue to work for me. [laughter]

Hughes: Did she follow that rigorous approach throughout her career?

D. Koshland: Yes, I was upset with her and felt she didn't get as much credit as she deserved because frequently when she had a result and I thought it was 99 percent sure, she'd think it was only 75 percent sure, and she wanted to run one more experiment to really prove she was right. And then, in the modern, competitive world, somebody else would discover the same thing from a different direction, and she'd get scooped. And so I'd get impatient with her and tell her she just couldn't spend that amount of time on duplicating results. She had a small lab. She didn't ever have a big lab the way a lot of these competitors did, and so she really had to publish more rapidly. I would tend to publish a theory if I had just one result and my theory was based on a lot of detail recurring--and even that was risky but in fact my theories all turned out to be right. little questionable. So that was a different scientific style from her more careful approach.

Hughes: Did you ever get caught out?

D. Koshland: No, most of my theories were correct--induced fit and things like that. Well, I wouldn't say I published based on one result; that was probably an exaggeration. But if I had a theory, it was usually based on a fair amount of knowledge of the field. In addition, I had one or two supporting experiments, but they were perhaps not enough experiments for somebody who was cautious before you postulate some big new theory.

Hughes: Was that just her nature, or had she been mentored by a cautious person?

D. Koshland: No, I think that was her nature, but her Ph.D. advisor was very cautious, and probably a little bit due to being a woman and being less secure. She was a good scientist, and at the end she knew she was a good scientist.

Hughes: I would think that if she had had trouble with people disputing her results early in her career--

D. Koshland: That association you understandably jump to, but I don't think so. She knew then she was right and this other guy was wrong. And there she was looking at very small differences in amino acid sequence, but she was confident she had done the analysis so well, she knew they were real differences. But that was a big, big theory. Remember, the world was really watching, and she had a very exciting result. She did more experiments than I

would have done, but even I would have been worried about those. I mean, she really stuck her neck out there.

A Woman in Science

Hughes: Do you think that if she hadn't been a woman and/or hadn't been at Brookhaven, that she would have gotten more attention?

D. Koshland: Yes. I think she certainly would have gotten more than she did, although at the end of her career everybody really respected her a lot. She was on lots of National Academy and NSF committees. But she would have certainly gotten the recognition earlier.

I was always infuriated because periodically she wasn't invited to big international scientific meetings, whereas people I thought were much less productive were invited. She was never that angry. She would get hurt because she'd see a program, and it was clear they were picking people that were less productive than she had been.

Hughes: Because they were men?

D. Koshland: Yes and part of it was that because she had a family, she didn't go to as many meetings as some of the men. People go to meetings, and are around when you plan the next meeting, the next convention. So the fact that she was home with the kids or had to go to a graduation meant she wasn't there to be in on the assignments.

Hughes: She was invisible.

D. Koshland: Yes, sitting around a room, they didn't think of her automatically. So I think it was a combination of reasons. Certainly some discrimination against women was probably the biggest single factor.

Hughes: What effect did her experience have on you?

D. Koshland: I would say everything that she did was positive for me, but I became angry if she didn't get her fair share of credit. I was chairman of the department of biochemistry for years. She became chairman of the Department of Immunology and Bacteriology.

Hughes: Did your terms overlap?

D. Koshland: Yes, we were chairmen at the same time [MEK:1982-1989; DEK:1973-1978[1]]. Once when we were having dinner, she announced to me that she had just gotten $26,000 from Sandy Elberg, the dean of the graduate school at the time. I said, "I am furious about this because Sandy Elberg just turned me down for $6,000, based on the fact that he had no

[1]DEK was also chairman of the Chancellor's Advisory Council on Biology from 1982-1993.

money and couldn't do it." I wanted the $6000 for the department to have an Asilomar type of retreat. Clearly, that wasn't absolutely essential. And Bunny had asked for $26,000 because some professor needed to be bailed out in order to do very important research. I've forgotten what it was, but it was really needed.

So Elberg made an absolutely correct decision. But I can tell you, he never forgot that because I kept bringing it up. An attractive, blond, chairman of the department is going to get yes from Sandy Elberg while he's telling a poor old male professor in another department that the dean doesn't have any more money.

Hughes: That's all very interesting, but what I was really meaning by my question is: you were married to a woman who had had certain roadblocks in her career because she was a woman, and I'm speculating that maybe that opened your mind to the difficulties that women in science have.

D. Koshland: That is certainly true. I was always very supportive of women's rights. From the beginning, I was naturally that way anyway, but Bunny's experiences made me more so.

Bunny was great eyes and ears for me in the scientific community. When I was editor of *Science*, she religiously refused to meddle in any decisions about *Science* magazine. For example, she didn't read my editorials before I published them. On the other hand, she was in the midst of the scientific world. When *Science* had a special issue on immunology, she would suggest to me who might be a good author for it or what subjects to cover. We had ad hoc editors who dealt with a special issue. But she didn't let me solicit an article from her.

Hughes: I heard that she was somewhat jealous about the ease with which you wrote--

D. Koshland: Oh, yes, that's true.

Hughes: --that writing a scientific paper was much more laborious for her than for you.

D. Koshland: What she finally wrote was very good. She loved to be chairman of the department in terms of recruiting people, and she had wonderful rapport with the students, but she hated all the letter writing and committee reports. She was just great as a graduate advisor. The students thought she was their mother or even better than their mother, a sort of independent person who cared about them. She did care about them. She'd talk to them for fifteen minutes and learn all about their personal histories. I was always diffident to ask my students any personal questions. She had a great rapport with students.

I was helpful in terms of understanding women's problems. I was always active in getting barriers broken down for women. But Bunny really did much more than me with that. She'd go to meetings at the National Academy of Sciences and she'd interact with people, so she'd hear the latest gossip. And that was really very useful for me at *Science* magazine, even though she would not interfere with the magazine itself. I had management crises, and I would consult her about the situation. She wouldn't appear publicly in it, but she was certainly very helpful to me.

She was a really good chairman because she was imaginative. A lot of people think a chairman just bullies his way through it; you just say, "I'm going to have this." But if you're clever, you think of a way to solve the problem. Professor X needs this little room and Professor Y wants the room, too. You find if you can give Professor X a room down the hall which is bigger than the other room but a little out of his way, he will accept it. So she would come up with very imaginative solutions. But then she hated being chairman because a chairman has to write letters all the time. Her letters would come out excellent because she labored over them. I would write an editorial in an evening, and it just drove her nuts that I could write that quickly.

A 70th Birthday Gift

Hughes: [chuckling] I want to read something to you.

D. Koshland: I'm going to report you and say the trouble with Ms. Sally Hughes is she reads things about you.

Hughes: [chuckling] I'm reading from a mock issue of *Science* that your wife designed for you.[1]

D. Koshland: This was on my seventieth birthday. That was a wonderful birthday present. Bunny arranged for the children and colleagues of mine to write short articles and put them all together in the format of a *Science* magazine. They delivered this to me. I knew nothing about it. She got all the children, she got people here at Berkeley, she got people at *Science* all to contribute articles.

Hughes: Well, for the purpose of the tape, I'll say that it's an issue of *Science* in honor of Dan's seventieth birthday. It's modeled after *Science*, but tongue in cheek. All the articles are humorous. But I want to read an excerpt from one written by your daughter-in-law, Catherine Koshland, and edited by James M. Koshland, who identifies himself as "an attorney for Unaware and Filthyrich, and Catherine Koshland is a professor in "The Building Next Door, Blue and Gold University."

D. Koshland: [chuckling]

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Hughes: It relates to our discussion about scientific styles. [reading]:

> In a retrospective study to identify the antecedents of current behaviors, we learned that in the early days of his marriage, Professor Koshland was given the opportunity to work with

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[1] A copy of the cover of the mock issue, featuring Daniel Koshland, is found in the appendix to this oral history.

his wife in the same lab. However, the behavior of each in the lab was the antithesis of the other. Professor M. Koshland was meticulous and precise, adding just the right 'spice;' Professor D. Koshland: throwing this or that in, and drawing sweeping conclusions. Needless to say, the chemistry of the lab wasn't right, and Dr. Dan Koshland concluded that it was time to retreat from the lab or 'kitchen,' a behavior that persists to this day. [chuckling]

D. Koshland: Bunny liked to work in the lab, enjoyed doing it. I really liked theory, and I was really delighted to have my students carry out the lab experiments.

Hughes: She liked the experimental part?

D. Koshland: She liked to work with her hands.

More on Family Life

Gardener

D. Koshland: She liked gardening for the same reason. In addition to having a career and being a great mother, she not only liked gardens but really had great theoretical understanding; she knew when to grow plants and how to grow them.

Hughes: Was that intuitive?

D. Koshland: No, she just learned everything quickly. So she learned about plants and picked up quickly what would grow best. I didn't have trouble convincing her that California was a great place for a gardener. She would always have our garden in Lafayette in color. When the gardenias were dying or whatever it was, she'd rip them out and put in petunias or whatever it was--I don't even know what the names of these flowers are. But she knew that when they started to wilt a little bit--and they still looked perfectly good to me--they were on their way out and she had to get the petunias in. Then they would blossom in ten days or whatever it was, when the gardenias really collapsed, you see? So she always would be turning over the garden so that it was always colorful.

Paid Help

Hughes: Did you have help in the home?

D. Koshland: Oh, yes.

Hughes: Without help, I can't imagine how she'd have time for gardening.

D. Koshland: No, we always had help. When our children were growing up, we had a daytime maid in the house. That meant that we about broke even if you want to say, in terms of--because as a scientist she got paid more than a maid, but it meant that she came home and the house was clean and the laundry was done. The kids, of course, had gone off to school, so the person in the house could be on their own and do sweeping and things like that.

Both of my daughters-in-law were very fond of my wife--there were never any mother-in-law problems. They looked up to her a lot and really had a great rapport with her. One of them, Jimmy's wife, Catherine Koshland, who's now a professor here at Cal, imitated her right from the beginning. Mary Porter, Douglas's wife, sent the kids to daycare for a while. When you send kids to daycare, they take care of them during the day, but then when you come home, you have to do all the cleaning; you have to sweep the house. Bunny said to her, "There's a better way. If you get somebody who can do the cleaning up and watch the kids during the day, it doesn't cost much more than daycare." So Mary switched and just was delighted.

Hughes: Did that person live in?

D. Koshland: No. We never had live-in; we always had people who came in for the day. We liked the privacy of being alone at night.

Social Networks in Science

Hughes: One of the images that is associated with women in science is that they tend not to have a professional network in the same way that men do. Did your wife take advantage to some extent of the incredible network in science that you had created?

D. Koshland: I don't really know. That's hard to nail down. She was not as widely known as I am, but she was quite widely known and we certainly helped each other in that regard. I think that there's a symbiotic effect, which certainly worked for both of us in the sense of getting elected to the National Academy. Getting elected to the National Academy is mainly how good you are yourself in your own scientific discipline. You first have to be proposed by your own group, like the immunologists vote on the various immunologists proposed for membership. Then when you get beyond that stage, you have to be voted on by a bigger group, like all the biologists, and then you get to the next stage and are voted on by an even bigger group including chemists and astronomers. I would say there was no sense that I helped her in getting elected by the immunologists. They all knew her well. But once she got up to another level, the name Koshland probably was familiar to chemists and physicists that would not normally have known her and she helped me with biologists and medical people that probably didn't know me.

Later, if I as editor of *Science* magazine needed to call up an immunologist or needed something in the cancer field, then the fact that I was Marian Koshland's husband

certainly helped. So I think we helped each other in name recognition. As far as doing my research, I would say that the network she had didn't help me very much, and I don't think mine helped her at all.

Team Sports in the Life of a Woman Scientist

D. Koshland: On the other hand, you put your finger on something very important. If you read that autobiography she wrote of herself, she claims that one of the things that women suffer from is not doing sports. She said sports are really a good network for learning how to work with people but also for learning how to be competitive.

She really felt the sports experience of learning at times how to subjugate yourself and pass the ball off to somebody else, but at other times be aggressive and take the ball to the end of the field yourself, was a kind of training that women needed to have if they were going to compete with the world of men. She ends her article very cutely by saying, "I didn't learn to play soccer, but my granddaughters do. Let's see what happens."

Science and Motherhood

Hughes: When the twins came along and the number of children in the family was suddenly doubled, she considered quitting science entirely. Your advice was not only to continue to work part time, but also to choose projects that were a little far out. Why did you give her that advice?

D. Koshland: That was my instinct about research. I always liked sort of wild ideas. She really had very clever ideas. In her career, she had one really outstanding idea after another. So I didn't really need to point that out to her, but I did think it was worth emphasizing because all of a sudden she realized that with this big family she was not going to be able to work full time for a number of years. Whereas before she thought well, she'd soon be finished taking care of little kids—the children would be going to school and she could now work full time and have a regular lab.

Bunny was saying when the twins arrived that she wanted to quit, that she wanted to become a full-time mother. I knew that she would just be bored as hell. We had then two older girls, and the oldest of them was just starting kindergarten. It was only going to be a limited period until everyone was in school, and then she would really regret not having anything to do in science. So I felt she should do it just part-time until the kids were old enough. But then I said, If you're part-time, then you've really just got to emphasize the originality, quality instead of quantity. Bunny wrote that concept into that autobiographical article. What she didn't say is that everybody can't be original. It's the kind of advice which is good for people like her, but only a limited number of people can take that advice.

Hughes: That's interesting. I suspected that a routine approach would be safer; you could follow other people's lead and you wouldn't be sticking your neck out.

D. Koshland: That's true, and it's perfectly good to be safe with a routine career. But Bunny was ambitious enough that she wanted to compete with the top people. To do that, I said, you've just got to be very original.

Hughes: I remember Stan Cohen saying that he tried to choose projects that were in underpopulated fields where the competition wasn't as stiff.[1]

D. Koshland: That's one variation. But there are a number of ways you can do that. If you're really original, you either pick a populated field but then go one step beyond, or you take an unpopulated field and discover something that everyone must learn.

Personal Qualities

Hughes: Tell me now about her as a personality. What was she like to meet on the street and what was she like to meet as a scientist?

D. Koshland: Bunny was a wonderful personality. She was lots of fun. She was not the stereotype of an ambitious woman. If you met her at a party you'd say she was a typical homebody. If you met her in the lab you'd say she was a typical scientist. She was very good at a party. She was unusual in the sense that she really was (a) interested in people and (b) remembered everything. I'm not bad at meeting people. I get along pretty well and easily, and a lot of people offhand might say, "Well, he's more gregarious and open to people than she is." But in fact, I'd go home and I'd sort of vaguely remember what somebody had said to me. Weeks later she'd say: "Well, the person that you introduced me to has a kid in the third grade in Connecticut," or something like that. By then, I had forgotten how many kids he had and most details of our conversation. So she really absorbed and remembered everything she heard.

I'd have a visitor to the department, and I'd invite him home for dinner. I'd say to Bunny, "Don't worry about it. He's a very nice guy, and we'll just have what we normally would have for dinner." She would say, "Oh, no, I can't because we're going to have ham and the last time he was here we had ham." I said, "Bunny, it was ten years ago. How can you remember what he had?" But she would! And she was right. So she said, "I'm not going to serve him ham again." I said, "He never would remember that he had ham here." So I went out and bought veal and he said at dinner, "The last time I was here we had ham and it was so good I still remember it."

Hughes: That is phenomenal.

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[1] Oral history in progress with Stanley N. Cohen.

D. Koshland: She was also a great typist. Every once in a while, in a crisis, she'd type something for me. If I had to get something off the next day I'd ask her. She was better than any secretary I had. She composed her own papers on the typewriter. She would sit there and just type them out. But she was very good at almost everything she did. She was very good at conversation. She was a very warm, easy person to get along with, but she was also tough.

Bunny was famous at faculty meetings and in the Academy for saying the equivalent of, "The emperor has no clothes." Everybody would be sitting around at these big scientific committee meetings, considering so-and-so for a job. People would be saying, "Oh, yes, Joe would be great; he just got this big award." Bunny would be the person to say, "Well, he's good at all those things, but he really isn't very good at giving a speech, and this is the kind of position where you have to give a lot of speeches." And then everybody would say, "Well, yes, Bunny is really right." One of those eulogies of her mentioned that she would tend to be very blunt.

And the grandchildren mentioned her high standards at the memorial service. They were very upset that she died because they said, "We don't have any standards to live up to anymore." And they have very good parents. (My kids are really very unusual parents; I'm very proud of them.) But the grandchildren knew Grandma loved them, but they had to live up to certain standards. If you didn't have your bib on straight or you didn't have good table manners, you knew Grandma would give you trouble. They knew that getting her approval was not automatic.

Hughes: Was that true of her own children as well?

D. Koshland: Oh, sure. We were both pretty old-fashioned parents in many ways. But I think we had very good rapport with our children. They were all really very good children, so we had very little trouble. But they knew that they had to toe the mark.

Childrearing

Hughes: Did the two of you agree about how to raise the children?

D. Koshland: Oh, the two of us instinctively agreed. I was a little bit more permissive about, say, whether they had to go up to bed immediately at nine o'clock. But I was a lot stricter about things like walking across streets safely. Bunny would be stricter about not eating before dinner, for example. They'd have to save their appetite for the meal. So they would periodically do things like coming to one of us to ask us about the thing they knew we were more permissive about. But we both had pretty good instincts about when we were being treated like that, so I'd think about what Bunny would say and Bunny would say what she thought I would say. The children would get very angry and say, "Why do you two always agree?"

But as far as anything serious about their lives was concerned--Bunny and I agreed almost completely. And it's a good thing because we were both very strong people, and we would have had one big awful fight if we hadn't agreed. For example, my second daughter Phyllis, who was really a rebel but a very cute child, came home with a pomegranate from the grocery store. It wasn't a big thing, but it was more than she had money for at the time.

I said, "Well, how did you pay for it?" She mumbled and moved off. I deduced that my daughter had snitched it, and she confessed. The punishment I devised was, she had to go back and tell the grocer and pay the money and say she was very sorry and she'd never do it again. She was willing to pay the money, but she didn't want to have to go back. I said, "You walk right back and you tell him. You say this is what happened, and that you took it, and you shouldn't have done it and you're sorry." Then I went out because I had to do something. Bunny came home, and I don't know what happened, but in the process my daughter confessed. But she didn't tell Bunny about my punishment, and Bunny gave her the exact same punishment. [chuckling] So we sort of instinctively brought up our kids the same way.

But we had various crises. I remember one famous episode. Our rules were that we agreed on a time when we thought they ought to be home, but we didn't care what the hour was, as long as we knew where they were at any time. So if they went out to a school play and then they all decided to go someplace and have pizzas, as long as they went to the phone and told us, "We're having pizzas and I won't be home till one o'clock instead of eleven thirty," we said that was fine. That was okay, and everybody in Bellport knew that when they went out with the Koshlands, they would have to go to the phone and report where they were. But one night Phyllis was very late and no phone call. (Later we found out she'd gone sailing and got becalmed.) She got home at 4 a.m. and found both parents sitting on the front steps waiting for her. She said that was the biggest shock of the evening.

We went to breakfast any old time because the kids were on double sessions. That meant some of them had to go very early in the morning, and some late. So we devised a strategy that the dinnertime was the time the whole family got together, whereas breakfast people could all eat on their own schedule. Everybody knew they had to be home for dinner. And they were. They claimed it was a terrible thing, but they really all liked it.

We had five just wonderful children. And they've been wonderful to me. When Bunny died, as you can guess, I was really in a depression, and my children just appeared. We knew she had cancer. She didn't want anybody to know, but we of course told the kids, and they all knew about it. So they would just arrive here. Ellen, my oldest daughter, just came and stayed at the house when Bunny was dying and I really needed her. And then the other kids came after. I'm sure they all conspired. All of them had jobs and were busy as hell. I felt guilty about it, but they would just announce, "Dad, I'm coming for the weekend." And I'd say, "You know, it's really inconvenient." "Well, too bad. I'm coming." And they really knew that I would make space for them, no matter what. And although it seemed inconvenient, it was good for me to be doing something with them.

At any rate, they were just good kids all their lives. I don't remember any real problems. Our first daughter, Ellen, was an ideal child from her start in the nursery. For the first year or so, Bunny and I acted superior as hell. We just thought all these people who were having trouble with their kids just weren't as smart parents as we were. Then our second daughter Phyllis came along. She was a rebel. She screamed in the nursery before she was even released from the hospital. She was a hellion. We really had a hard time getting her to sleep at night. She'd wake up all the time. Anyway, it was clearly genetic. The nurse told me, "Well, she's going to be a handful. But usually these types are very bright." And she was. But she was a big rebeller. So we sympathized with the other people who didn't have docile children.

For example, we complained about her spelling, and she wrote this letter, which I still have, when she first started camp. We had told her she had to write from camp. She couldn't spend two weeks at camp and not let us know what had happened. So we got this letter from her, "Dear Parents, I'm doing what you told me to do." She had misspelled many more words than normal. It was clearly just to get us furious! Anyway, that's the kind of rebelling she did. The other kids--well, you can read from that mock issue of *Science* the kind of irreverence they treated their parents with.

The family had a big tradition of kidding. That I didn't start. That was started with my parents. My father was a big kidder, and Bunny always said that one of the big things she had to learn in the family was how to take kidding. If you were the kind of person that got annoyed or weren't a very good sport, then they'd kid you about this, so you really had to learn how to take it. That was very good for the kids later on.

Hughes: So your mother and father had this same sort of relationship?

D. Koshland: Yes. Except my mother never went to college. It's something she always felt deprived about, because she was a very educated woman. She read a lot, and then she later enrolled in correspondence courses. She got married at eighteen. In that age, women did that, and they didn't go to college as was true of most of her peers.

Our attitude towards our kids was very similar to my parents' with me, and that is they really always gave me enormous security. I was expected to go out and do well, but it was never said, well, I had to be a big success. If I was conscientious and a good citizen, I'd be loved and anything more was up to me.

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D. Koshland: If I cheated, then I would get hell. But on the other hand, I wouldn't be thrown out of the house. If I was prejudiced and said nasty things about people because they were a different color, I knew the family would disapprove of that. Because we grew up in a household where we always had enough money, it was expected that we would be charitable and go out of our way to help underprivileged people. So we had to do those kind of things. On the other hand, it was never that we had to be first in the class or we had to make a lot of money in our careers. I think we conveyed the same thing to our children.

Cooks in the Family

D. Koshland: As I said, my wife was really so good at everything, it was hard for anybody to compete with that. On the other hand, the children learned that they were supposed to do things. My sisters didn't ever learn to cook very well, yet all my children, even the boys, learned to cook well. I never did. I was the disaster as far as that was concerned.

Hughes: Your sisters didn't learn because there was a cook in the house?

D. Koshland: Yes. They always had a cook, and they weren't that interested. They both now can run a household and they can cook, but they just never became good cooks, whereas Bunny was a superb cook.

Hughes: Did she deliberately teach her children how to cook?

D. Koshland: I don't think deliberately--they all just learned. We never had a cook. At the very end, when she was really quite ill and frail, she'd cook for me right up to the end. I tried to get her to have a cook, and she just wouldn't do it. Fortunately, she loved Chez Panisse, so I'd take her there. It was really sort of a struggle because it was hard for her to walk up the stairs there. She didn't want to have a cook; Bunny and I enjoyed the privacy, just being alone.

Social Life

D. Koshland: Both of us being so mutually sufficient affected our life in many ways. As long as I could have an evening with her, that was it. I always enjoyed talking to her more than I enjoyed talking to anybody else. We would come home from work, have dinner with the kids, and work, or just sit around if we were tired, and talk with each other. We had lots of friends, but they all really knew that we didn't like to go out a lot. That was just accepted.

Bellport was really a small world. It was sort of like having a big family. There were a whole bunch of couples who got along very well. We'd go to their houses, and our children would go there, too. I was president of the school board and was very much involved in the community. So was Bunny; Bunny was a force in the League of Women Voters; she led a big study with the zoning of the area which led directly to zoning law legislation for the area.

And then we moved to Berkeley. I was forty-five and she was forty-three. We decided we just weren't going to do the community bit. It would be enough to be interacting with people at the university, so that's all we did. We did make lots of friends among our colleagues, but we never made much effort in the community. We knew and liked all the people in the houses around us in Lafayette. But we made no effort to have them to dinner and go to their houses.

It was really busy. It's conventional for the wife of the chairman of the department to entertain his departmental affairs and for me to be there at all her department events. We just agreed we weren't going to do that. Everybody understood. Because of the number of professional things Bunny and I had to go to, if we each went to the other person's events we never would have had time alone together at all. So I went to my things and she went to her things, and everybody sort of understood. If it was something very important, occasionally we would both go together.

Hughes: What do you most like to remember about her?

D. Koshland: Oh, she was just everything I wanted. We started out with this big physical and mental attraction when we were first met. We were attracted intellectually right away, and not just in science. She would challenge me intellectually, and make me confront reality. I tended to be kind of a rose-colored romanticist. I'd always come back from a first meeting saying so-and-so was great, and she would restrain me saying, "Why don't you get to know him a little better?" Which usually turned out to be excellent advice. I just enjoyed talking to her at every level. She was a very good dancer, so we used to go out and dance together a lot.

Her one failing that I remember is that she wasn't as good in sports, partly because she had terrible astigmatism, so she never learned hand-eye coordination. She was really very strong for a woman and for somebody her size--she was not very big. She could play tennis, but she was not really good at it, and it bothered her. She wanted to be good at everything.

Remembering an Early Incident

D. Koshland: I distinctly remember one thing. When I was taking her out, we went to a movie called "The Ox Bow Incident." It was a movie in which a bunch of people lynch a couple of kids that they think are horse thieves. In the West, that was standard. You got hanged for stealing a horse because that was stealing somebody's livelihood and so forth. There was a scene in which this mob of people comes in to a small town, and the two little sheriffs are clearly outnumbered. The sheriff stands there and says to the mob, "I'm going to shoot you if you come up these stairs." But they started advancing, and of course he doesn't shoot, and the mob takes the guys out and hang them.

I said, "The sheriff should have shot them." And Bunny said, "No, you can't shoot somebody if they haven't committed a crime yet. You could shoot them as they were committing the crime, but not if they were just threatening." I don't know what happened. This was just an intellectual argument. We were arguing on the front steps of her boarding house, where I was leaving her. Finally about two o'clock in the morning somebody said, "Will you guys please shut up?" We didn't have any idea how long we were talking. That was typical of the kind of discussions we had before we even got married, so by the time we got married, we knew each other very well.

Bunny was just a very warm person. She really cared about things. She remembered everything. I used to kid her all the time. She'd serve a dish and she said, "Well, was that better than last time?" And last time was about ten years ago, and I was supposed to remember! In the beginning, when I was inexperienced, I'd say, "God, I don't remember the last time." And that was worse than anything else! So I learned later to pretend to remember, "Oh, yes," I said, "you've done a little better this time." [both chuckle] But anyway, it was just a kick. I just enjoyed everything she did. She was a great mother for those kids, a great wife, a great gardener, a really talented architect, a real expert in art.

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MARIAN E. KOSHLAND (1921-1997): RETROSPECTIVES ON A LIFE IN ACADEMIC SCIENCE, FAMILY, AND COMMUNITY ACTIVITIES

Douglas E. Koshland

A YOUNGEST SON'S VIEW OF FAMILY LIFE, PARENTS, SCIENCE AS A CAREER, HAVERFORD COLLEGE, AND HIS MOTHER'S STRENGTH OF CHARACTER

Interviews Conducted by
Sally Smith Hughes
in 2000

INTERVIEW WITH DOUGLAS KOSHLAND

Douglas E. Koshland

[Date of Interview: July 5, 2000]
[Lafayette, California]

Education

Hughes: Start please by giving a thumbnail sketch of yourself.

D. Koshland: I am the fifth child, the baby in the family. I grew up for twelve years in Long Island and then we moved to California; went to middle school and high school; went to Haverford College [B.A., chemistry, 1976], and then went to MIT for graduate school in biology [Ph.D., 1982]. Did a series of postdoctoral fellowships and ended up as a staff researcher studying chromosome dynamics and structure at the Carnegie Institution of Washington in Baltimore, Maryland.

Hughes: And that's where you are now?

D. Koshland: That's where I am now.

Hughes: Oh, I thought you were at Johns Hopkins.

D. Koshland: No. The Carnegie Institute in Washington is a very interesting nonprofit research institution that was started by Andrew Carnegie and has departments around the country. There's the embryology department on the Hopkins campus, which I am a part of. There's plant biology at Stanford, astrophysics at Cal Tech. But despite the fact that our buildings are on the [Johns Hopkins] campus and we get to take advantage of the campus facilities, we are completely independent, though I do have adjunct positions in the biology department and the medical school.

Douglas Chooses Science Despite Parental Advice

Hughes: What, if any, influence on your choice of chemistry did your father have?

D. Koshland: [laughs] He had very little.

Let's go to the first story of my mom, which is she had five kids, and she didn't want any of us to be scientists. She thought science was boring. She'd rather have people do something different. My oldest sister is a writer, my next sister is a sculptor, my brother is a lawyer, and my next sister started out in physical therapy. When I showed an interest in science, my mom joked that she got tired and by the time she got to me she no longer had the energy to direct me elsewhere. So I became a scientist by her failing to be a good mother. [laughter]

Hughes: Why chemistry?

D. Koshland: Why I became a chemist? Haverford is one of the best small colleges in science in the country. At that time there was a huge fraction of people that was going on to medical school. Most of them were biology majors, and some chemistry, but I was just perverse and decided I didn't want to be a doctor. I also took a beginning biology course which I didn't like at all. The chemistry department I liked a lot, so I just did chemistry.

Life in the Koshland Family

Mother's Sense of Fairness

Hughes: Say something about family life.

D. Koshland: It was a really good family life, and I attribute that to a combination of things. One was, my father's side of the family is a big Jewish family with wonderful attributes of humor and belief in the importance of large family.

I think the other important side featured my mother. People were attracted to her because of her tremendous sense of fairness. That really manifested, luckily for me, most in the way our family dynamics ran. My siblings and I just have a tremendous relationship, and it's in large part because we never felt one was more loved than the other. Essentially there was no jealousy.

There's a story my brother [Jim] may have mentioned that's an example of that. My brother and I would get into fights--two boys, two years apart--but we did actually fight very little. My mom would invariably come into the room, and me being the younger one and Jimmy being the older one, she'd tell my brother, "Jimmy, quit being a bully. Douglas, quit being a crybaby," and she'd walk out of the room. It infuriated both of us,

because I was sure he was being the bully; he was sure that I was being the crybaby. But in some ways we got mad at her rather than at each other. We asked her years later about this and she said, well, she knew that one or the other was at fault, but she wasn't in the room, so it was just better to get mad at both of us.

Family Dinners

D. Koshland: Because my mom worked, was a professional and a mother, and my parents were both very busy, the key thing was at dinner. We had to eat dinner together, which made me mad because I couldn't go over to my friend's house for dinner. But that was the time she got to see us, so we'd get home every night between six and seven-thirty and then we'd go on with our busy lives. But if one of us had gotten in trouble, we knew that the dinner table was when it was going to come up.

What the others knew was once she started on me, if I had done something bad, then she would have stored up all these things against everybody else, and she would just march around the dinner table leveling everybody and getting it out of her system. [laughs] But the result of that was just a tremendous bond with my brother and sisters because we just felt all equally loved. And I think that had a real impact so that even today we get along--we have a really warm relationship. And that started out in Bellport.

Bellport, Long Island

Childhood Activities

D. Koshland: Bellport, Long Island was a spectacular, idyllic place. It was sixty miles out on the south shore of Long Island. It's an old whaling town that had been deserted and then rediscovered by academics who were working for Brookhaven [National Laboratory]. We lived about 200 yards from the seashore, which had a yacht club that had some of the best small sailing in the world. It was a three-mile ferry ride over to Fire Island, and a part of Fire Island which was about twenty miles in either direction from a bridge where cars could go.

This little town had something like a couple of hundred people who belonged to this yacht club and something like forty miles of beach all to ourselves. You could get on the ferry every day as a kid. Although my mom worked, very nicely the next door neighbor's mother or father would be on the ferry taking their kids over and they would keep an eye on us, so we would go over and body surf and swim every day. [interruption]

Hughes: Did you do any sailing?

D. Koshland: Yes, we all sailed. I left [for California] just at the time I got into it. My middle sister, Phlyp [Phyllis], sailed with some world-class sailors in that area. There were yacht clubs and racing and it was just an amazing place. We'd eat crab and we fished and waterskied. And in the wintertime it froze over and you could iceboat and skate and it was one of the great existences.

My parents moved from [New York], New York. They took the job in Brookhaven because my father being Jewish actually didn't get a lot of job offers. In fact, one offer at UCLA was taken back because they found out he was Jewish. So he went to Brookhaven with the idea that he was just going to stay for a few years. Then they found out, as I was just describing, that Bellport was a pretty idyllic town, a great place to raise a family. We went into New York City every, say, eight months and saw a play or a musical. It was great.

The Job Offer from Berkeley

D. Koshland: My father being born and raised in California always wanted to go back, and he finally got an offer from Berkeley. And so we had a family meeting, voting whether we wanted to move or not, and the vote was six to one to stay. We didn't realize at the time, of course, but the five kids' votes really were paper votes, but my mom's vote was important. So actually my dad turned down the job at Berkeley.

Then about a week later, as I know the story, my mom told my dad that she had changed her mind and she wanted him to go to Berkeley. And the story goes that then she said she'd rather have him making it up to her for the rest of his life than her making it up to him. She'd always wanted a sports car. He was always afraid about her getting a sports car. Out of that deal she got a 1965 Mustang which is a very cool car. We liked that car a lot.

Hughes: You weren't thrilled about moving here?

D. Koshland: No, we were not thrilled at all. We had a lot of visits out to my grandfather [Daniel E. Koshland, Sr.] and we liked California. But Bellport was a very nice place and so it was a bit of a family trial, although it wasn't as bad as it could have been because a lot of people in Long Island sent their kids off to private schools in New England--Choat, and places like that. My parents weren't into that private school thing at all, so we were going to lose some of our friends as they went off.

Nannies

D. Koshland: When we lived in Long Island, my mom had gotten help--two wonderful people, Luna and Pecolia--who helped us out a lot. My mom was not a very hug-plus person, and that's sort

of expected of mothers. She was not extroverted either physically or emotionally, so she hired two women who were particularly that way. At one point she said she knew she wasn't and it was good for us to have that. If she couldn't give it to us, then she'd hire some women who could do it. I think that not very many women would look at themselves and say, "I have this deficit as a mother," particularly a hugging kind of thing.

Hughes: It was typical of her to look at herself as clearly as she looked at others?

D. Koshland: It was, actually. There were times when she'd say things and we'd later on think, "Oh, no, that isn't what I would expect her to say." It was so characteristic of her to be very clear about other people and herself that when she was a little bit out of kilter, it was a stunning thing. One time I remember she said she thought she'd be a good actress, and we all thought, "No way she could be an actress." It was so much in contrast to the way she normally was.

Helping in Mother's Laboratory

D. Koshland: When she was in Bellport, she worked for a three-quarter day. She used to take me to the lab with her. Now, in retrospect, being a scientist, I'm amazed that she let me carry relatively valuable things down to the cold room. In dialysis, you put your sample in sort of a plastic bag and you put it in a big vat of solution and then you stir it with a little magnetic stirring rod. So we got to carry the solution down there. That was something she did with lots of kids, and she probably did it in her science labs. She gave you confidence by the fact that she was trusting you. She was very trusting in terms of, "You can do this. Go out, you can handle this."

Hughes: Did she take all the children at one stage or another into the lab?

D. Koshland: I don't know how much. I don't remember my brother and sisters going; I suspect she must have. I don't know whether it was because I was the youngest and the others were busy and she had to do something with me so she dragged me along. She also realized I liked it. She had a really old-fashioned calculator and I loved punching numbers in the calculator and adding and subtracting for a while. It was a lot of fun.

Community Activities

Hughes: What about community activities? I understand that both your parents were very active in Bellport community affairs.

D. Koshland: Right.

Hughes: Can you tell me about that phase? You were pretty young.

D. Koshland: Well, I can remember a little bit. My father was president of the school board, which to me as a scientist is hard to imagine, and my mom, equally hard to imagine, was on the League of Women Voters. With somebody else, she redid the whole zoning document for the entire town and surrounding areas. I remember her meetings and having all these people over doing very important stuff for that. Bellport is a small town of about 3,000 people, and I think it was very easy to get sucked into community activities.

One thing which was a change when we moved to California was that Lafayette is a country town but it's not a community. You may know your next-door neighbor, but you have no sense of doing something for the town of Lafayette. My parents switched their public service from their community--which was also probably a reflection that their kids were growing up--to the scientific community where they now did their public service.

Marian Koshland's Science

D. Koshland: When we moved to California, I was twelve and going to school, and I think starting at that point she made a much more full-time commitment to her career.

Hughes: Yet I have the impression that some of her breakthrough work was done at Brookhaven, the work on antibody specificity.

D. Koshland: Yes, well, [laughs]--this is an embarrassing moment for me, particularly because I'm supposed to be the scientist in the family. But it goes back to what I was saying before: she didn't want us to be scientists. The real truth is that I really don't know that much about her scientific accomplishments. We didn't talk science at all in our family. Almost never. I cannot remember my parents sitting down to talk about science. Sometimes they'd talk about the politics of what was going on at work, but we'd never talk science.

They were very interested, both of them, in logic and rational thinking. It manifested itself most strongly with my mom in terms of sports. She loved sports [laughs], so a typical dinner conversation was about sports. She was a gourmet cook, spectacular food, but we ate in about ten minutes. The food was just wolfed down, as large families tend to do, but we sat at the table for usually an hour and fifteen minutes or hour and twenty minutes with conversation going on, and quite often they were about sports, because for months we'd rationalize every play. She was a diehard Dodgers fan. It didn't matter what it was; we went over and over the logic of the game, which my three sisters had to sit through because they unfortunately had a mother that was on the side of the boys in this way. It was an exercise in logic useful in all aspects of your life which came out in these conversations.

I know a little bit about her early contributions to the fact that antibodies are not a homogenous species. And my father has talked about the fact that people didn't believe her because they didn't believe you could be that careful and really see a difference among

antibodies with the methodology she was using. I'm sure that was in part true, and also true that because she was a woman, people took her less seriously.

University of California, Berkeley

A Molecular Approach to the Immune System

D. Koshland: I got much more familiar with what she was doing in the later part of her career when she got here and started getting more into the molecular biology of how the immune system works, because I have trained as a molecular biologist. She was interested in that and we had good conversations about how you regulate antibody expression and how people respond to certain types of challenges to the immune system. At that point in her career, she decided to step it up because the kids were all growing up and they didn't really need her any more to get around.

Initial Positions

Hughes: [reviewing curriculum vitae] She was appointed associate research immunologist in '65, which is when they came, and then professor in 1970. It could be that she didn't immediately plunge into a full-time career at Berkeley.

D. Koshland: I might have a little different read on that. I don't want to be accused of slander here—I'll protect the names--but it's my impression that she was initially hired in--I don't remember which department.

Hughes: Well, her first appointment was in the Virus Lab [1965-1969], according to this curriculum vitae.

D. Koshland: But it was associated with a department.

Hughes: I think it was biochemistry.

D. Koshland: I know it had a group of people which included Howard Schachman. I don't remember which the group was, but in any case, it was my understanding that she kept coming up in that division's faculty meetings as possibly being promoted to a real tenure track position. People on the outside were saying, "Oh, she's a very good scientist; it'll happen." And then she kept getting rejecting and put off. I think later she found out that some of the people she thought were pushing her were sort of stabbing her in the back. And this other department, the immunology and bacteriology department [Department of Microbiology and Immunology], realized that she was a really great deal--that these other people were being stupid. And they in some sense stole her away; just said, "Hey, we'll give you a full

tenured position if you come over here and join our department," and so she switched and got real space and real respect at that point [1970].

I never understood exactly whether that was a lingering of anti-women sentiment, or whether that was an individual situation, or whether it was because she originally got a non-tenure-track position and they felt: we've got her already; we don't have to commit further. But I think that lag represented five years of trying to work her way into a department that in the end didn't want her, and she switched.

Hughes: Another possible factor is that it was Dan who was appointed, and there may have been some resentment that a place had to be found for his wife.

D. Koshland: Yes, I think that's true. Absolutely.

Administrative Ability

D. Koshland: I remember either Barbara Meyer or Sidney Kustu who were on the faculty at Berkeley, one of them saying she loved going to my mom's meetings, when she became chairman of the department, because they were run quickly and expeditiously. There wasn't a lot of bull that slowed things down, and you could just tell this was a person who knew how to run things. That was evident from the first departmental meeting.

Hughes: Where did your mother get her administrative ability?

D. Koshland: Well, she was just an incredibly efficient person. I think that had to be by nature. She was a super mom before super moms existed, having five kids and a full career and juggling all that stuff. And that was just the consequence of knowing she didn't have a lot of time to waste. Also, good judgement made her cut through quickly stuff that she figured was going to be a waste of time. My mom did a lot of wonderful things. She was on a whole series of boards for the National Institutes of Health. She also helped the National Academy [of Sciences] produce a booklet on being a good mentor.

Hughes: For anybody--not just for women?

D. Koshland: For anybody.

Attitudes Towards Women

D. Koshland: My mom had an interesting relationship with other women, particularly women who were championing feminist causes. Here was a woman who had made a career at a time before it was popular to do so and had suffered a fair amount of prejudice in her career in terms of her name being left off of committees, papers being held up--subtle things like that. All

those things would seem to make her compassionate with the feminist movement. And I think she did have a certain amount of compassion, but there are two things.

First of all, I think she felt that a lot of the hurdles she had overcome were not the same hurdles that women were going to be facing today and so it wasn't appropriate that she could give advice to them. She was not the type of person who was going to give advice unless she thought it was reasonable. I remember one time she told me, "Somebody asked me once was it bad being a woman. I said, 'Yes, there are some problems being a woman, but I'd much rather be a woman than a male who's five feet one.'"

Humility and High Standards

D. Koshland: She never built anybody up. Nobody was deified. You didn't put people on pedestals. And that's always made me comfortable if I meet some famous scientist. Some people get scared and apprehensive and I never felt that way because I had always been brought up where, yes, this is a good scientist, but we don't know, maybe he's a lousy poker player. [laughs] He's got some problems and we know that. I find myself doing it with my own children because I'm her child. Like coming back from a baseball game and my son will hit a triple or something and I'll say, "Well, that was great, but in the second inning, you forgot to cover first base," which would be typical of what she would have said. So we didn't get a swelled head.

Hughes: She was very demanding of herself and others?

D. Koshland: Yes, she had extremely high standards. It was in all aspects--her social behavior, science, and everything. If you were going to do something, you were going to do it right.

Hughes: And that wasn't hard to live with?

D. Koshland: No, I don't think so because she demanded it of herself, so she wasn't being hypocritical.

Enthusiasm for Life

D. Koshland: I don't want to give a misimpression. She had tremendous joie de vivre. She'd go to, let's say, some famous gallery and she'd come back: "This was just great. This was really fantastic. You've got to go see this; you've got to go to that. It's unbelievable. It's great, you know." So this self criticism and evaluating of oneself wasn't overt. It wasn't negative and nasty all the time, but the humility and high standards were always in the background.

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D. Koshland: Her approach was a little bit cold. Not cold, but a little more rational, less emotional, and I think my sisters have sometimes wanted more of the more emotional side.

Service on Outside Boards

Haverford College and the Jane Coffin Childs Memorial Fund

Hughes: Because you went to Haverford, do you have any special insight into her service on the board?

D. Koshland: Not really. It's a funny thing because neither of my parents went to Haverford, and yet she became absolutely devoted to this college.

A series of very prestigious postdoctoral fellowships are given out by the Jane Coffin Childs [Memorial Fund for Medical Research]. There are about fifteen or sixteen given across the entire nation.

Advocate of the Small College

D. Koshland: My mom was on the board for the Jane Coffin Childs Fund [1976-1983] and was then asked to give a lecture to the students, an after dinner talk. And she did a very thorough job--typical of her. She decided the theme was how small institutions can make a difference, because the National Institutes of Health, for example, give out, say, 1,000 postdoctoral fellowships, so why do you need a Jane Coffin Childs giving out fifteen? She drew the analogy to a small college: you have Berkeleys and you have MITs and you have Harvards; why do you need Haverford or Swarthmore or Williams?

She had done her homework and found out that those small colleges train successful scientists way out of proportion to their size. At Berkeley or Harvard, you can work with a Dan Koshland, a Marian Koshland, or a famous Harvard professor. So why would scientists be coming out of those small schools? I don't remember all the conclusions she came to, but it was a lot about how small places can be incredibly nurturing and generate interest in a way that you can't have in a large institution. And I think that part of her fondness for Haverford was an understanding of how many good scientists had come from small departments.

Another attraction to Haverford is the honor code. Haverford is an old Quaker school and we have an honor code. Professors aren't in the room where tests are taken. You just sign a statement saying you have or have not cheated. You can take a test, pick it up, and go off to the library and take it. It's a community where it actually works. And

part of the reason it works is because when students get caught cheating, the students are harder on the student cheaters than I think the faculty would be. You're out; you're gone from the college; you have no choice. That honor code, I think, was very attractive to her. Honesty was incredibly important to her. It was the foundation to everything--trust and all that kind of stuff. That college epitomized her view of what you should have as a moral social fabric.

Marian and Dan Koshland

Hughes: Please comment on how your parents interacted.

D. Koshland: [laughs] I think they were a very remarkable couple. Fifty-two years of marriage--something like that. The first thing I can say about that, which I think is a little unusual, is that they were the most important people in each other's lives.

Well, I'll have to be careful. I might be caught making a sexist statement. My mother will roll out of her grave and crush me or something. [laughter] For most women, when they have children (probably men, also) the children become the more important focus of their emotional self. But not my parents. We used to have very interesting conversations around our family dinner table. One was: you are put into a situation where you have to choose your husband or your kids. Who would you choose? And I distinctly remember my mom saying, "I'd pick my husband."

As I said, none of the kids felt unloved. We had a wonderful life, but my parents were clearly numero uno in each other's life, and they were very supportive of each other. Despite what I said earlier about her being very good at being self critical and critical of others, the one place I thought that broke down was with my father. She was the only person who challenged him and he paid attention to, but she would challenge him less often than she might other people. He was the one guy who sort of broke the rule; he was slightly--

Hughes: Favored?

D. Koshland: Favored--idolized.

They loved an intellectual battle. That's what partly attracted them to each other, besides this tremendous emotional attraction to each other. And so they would argue a lot. We never felt uncomfortable. These weren't arguments where you felt like you had to run away because you thought your parents were going to slug each other.

There's a famous story they tell about going to see a movie called *The Oxbow Incident*, which is a movie about a mob that gets out of control and hangs somebody. And the question the movie poses is should the whole mob be thrown into prison, or was it the one person who pulled the rope and killed the person? I don't remember who chose which side, but my parents got so mad at each other that they walked down opposite sides of the

street on the way home from the movie theater after the discussion. They didn't usually get that mad, but they had very strong arguments.

My mom was definitely more liberal than my dad because of being self-critical. A good aspect of that was that she was compassionate. She understood why people may not succeed. My father is an incredibly gifted person and sometimes is a little bit, "Well, everybody can do it because I can do it." She said, "Well, not everybody is you, and these are problems." She had a real sense of compassion for people--not everybody was blessed equally in terms of their environment and life.

My parents would argue a fair amount about intellectual issues. They invariably agreed on a lot of the important moral and social issues. I think she pushed him a lot, which was good. She was the one who challenged him when his ego got in the way.

There's a book by Doris Kearns Goodwin about the Roosevelts.[1] A big difference is that Eleanor and Franklin Roosevelt had really huge emotional problems in their relationship. That doesn't carry over to my parents, but their intellectual challenge and respect for each other was very similar. Franklin really listened to Eleanor. It was the same thing with my dad; he really listened to my mom. He would argue with us kids, but eventually my mom would pipe up and say something, and all of a sudden he'd listen to what we were trying to say.

A Determined and Disciplined Woman

Hughes: Even though your mother decided that she was going to spend a good part of her life with her family, she also had an identity that was very separate from being mother and wife.

D. Koshland: Right.

Hughes: Which nowadays is not unusual but in those days was quite unusual.

D. Koshland: Right.

Hughes: Where did that strength of character or determination come from? Did she bring it into the marriage?

D. Koshland: Yes, she did. I think a little of that was environmental in the sense that, as far as I understand it, a lot of women were forced into the workforce as a consequence of the war. Most women decided after the war that they weren't going to go back to being just housewives. However, in my mom's case I don't think that had much to do with it. My parents said to me one time, which was the truth: it's very good to have more than one child because you realize that they are born with [different] physical and emotional traits.

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[1] *No Ordinary Time: Franklin and Eleanor Roosevelt*, New York: Simon & Schuster, 1993.

For example, our first child didn't have any diaper rash. Then you have the second one and you realize that 95 percent of it is they're born that way. You find out the second one has diaper rash like crazy, and all the people I thought were bad parents, weren't really bad parents; they just had one like my second child where no matter how quickly you changed the diaper there was going to be a rash. Same is true for personality. Some kids are born stubborn. I think my mom was born with determination.

Her Father

D. Koshland: My mom didn't get along with her father, partly because they were quite similar. She didn't get along with him because he was a Southern Baptist and he had a lot of trappings of prejudice and other things she found quite upsetting to her values. But he was born with tenacity. She told a story of her father taking a belt to her. She was eleven. I don't think he was abusive; it was the thing he did every once in a while as a punishment. She turned to him and said, "Hey, you can keep doing that, but I'm not going to change." For a young child to say that-- She was born with a certain attitude of, "I'm going to do this. That deterrent's not going to work." Despite his trappings, he was a remarkable man, a world expert on hardware and very determined in his own right.

Her Mother

Hughes: What about her mother?

D. Koshland: Her mother was a school teacher. She was Danish, and she came over to this country, and when she married, she became a housewife.

I think my mom was extremely close to her mother. That was sort of a tragedy in the sense that there weren't very many people my mother could open herself up to. My father was certainly one and that's why their relationship was so special. There was her mother, and unfortunately her mother died when she was in her fifties. My mother was in her late twenties, early thirties. My mom didn't talk very much about her, to be honest. A real strength in both my parents is, they don't reminisce, don't spend a lot of time in the past, which makes them very active people. The sad part is that we children didn't get a lot of the stories. We don't know some of the history. I don't think she talked about her mother very much because that was an emotionally hard thing for her to do. I can imagine my [maternal] grandmother being someone who would be stubborn and fight for a cause. She had very high standards and I think that partly came from her mother.

Marriage of a Gentile and Jew

Initial Opposition

D. Koshland: For example, my mom decided to marry my father. That was a problem because that was a Protestant marrying a Jew. My [paternal] grandfather said he wasn't going to go to the wedding. My grandmother turned to him and said, "Well, if you don't go to the wedding, you can find someplace else to live." He came to the wedding. The ironic thing about it is that my father ended up liking my mom's father better than she did. I mean, they had a better rapport. My father can forgive him for all the things in an earlier life that she couldn't.

A Reason for Marrying Outsiders

Hughes: Did the Koshland side have anything to say about marrying a gentile?

D. Koshland: Oh, my father has a joke about that. I think they were actually fairly open to it. The joke [laughs] is that my father's father married his first cousin. Most people know genetically it's not the best thing to do. They actually were very avant garde. They consulted the genetics department at Berkeley which had a very famous geneticist at the time, a founding father of genetics. They were probably going to get married anyway, but they wanted advice on whether they should have kids or not. The geneticist said, "I think you can go ahead and have kids, but your children probably should marry outside of the San Francisco Jewish community because it is a little bit inbred and you do have relatives in the community. You're beginning to challenge the genetic system and you can end up with some things like the kings and queens of England. And so my grandparents encouraged their three children to marry Jews who were not part of the San Francisco community. So my dad brought home a gentile. That was carrying it a little bit further than my grandparents had anticipated. [laughs]

Adaption to a Large Jewish Family

D. Koshland: My mom was in many ways more Jewish than they were. I mean, she really adopted the values of Judaism. She told me there were two big differences between Christianity and Judaism. Jews believed an eye for an eye, and Jesus said, "Turn the other cheek." When you cut to the chase, that is more or less it. In Judaism you are really responsible for your actions. There's no forgiving you, and there's no second life where somebody says, "Oh, it's okay if you sinned." She strongly felt you were responsible for your actions. She always used to say to us, "You have to be accountable for your actions, whatever you do."

My mom strongly identified with the large Jewish family, because she didn't have a big family and she didn't get along particularly well with her father or her brother. She loved her mother very clearly. She enjoyed being taken into this big warm family where there's a lot of humor and joking. I think they always seemed to accept her, although the fact that she was professional and didn't stay at home with the family was always a bit of an adjustment for them

The Role of Religion

Hughes: Did you have a religious upbringing?

D. Koshland: No, that was the one area where I think my parents were kind of irrational. [laughs] Rarely were my parents irrational. They felt very strongly that it would be insulting to either the Protestant or the Jewish halves of the family if we were brought up as one or the other. That's partly because they were scientists. They were not particularly into any kind of formal religious training. I think my mom had a lot of problems with her formal Protestant training because she felt it had been used as a sort of weapon for bigotry.

But they felt that we might want to have religious training, so they sent us to a Unitarian church for a while. They didn't go. [laughter] It was a complete failure because of course if your parents don't believe it, there's no way, as a kid, you're going to become interested in a cause like that.

One thing she thought the Christians did better than Jews was Christmas. It was a much better holiday than Hanukkah or anything else, not necessarily the religious part of Christmas, but just the fact that it was a family holiday where you showed appreciation to other members of your family with some sort of gift, plus the emotional togetherness. So we usually did some kind of Christmas celebration, and then occasionally we would go to services for Yom Kippur or Rosh Hashanah. But that was about it. I spent my college years taking a lot of history of religion courses so I could understand literature, because I had no religious training at all as a kid.

A Woman of Energy and Enthusiasm

Hughes: Any more anecdotes?

D. Koshland: When we lived in Bellport, we'd come into New York--for Christmas shopping, actually. She often would take us to New York. It was partly to shop and partly to see whatever we wanted. I remember being dragged in this whirlwind with my four siblings trying to keep up with her as she zoomed through New York City. And that view epitomized her. She was a woman with a tremendous amount of energy and a leader and a go-getter. Doing

something fun like Christmas shopping, it was, "Let's go! Let's do it." And that's the way she was, I think, in all aspects of her life.

Hughes: Well, thank you very much.

D. Koshland: I enjoyed it.

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MARIAN E. KOSHLAND (1921-1997): RETROSPECTIVES ON A LIFE IN ACADEMIC SCIENCE, FAMILY, AND COMMUNITY ACTIVITIES

James M. Koshland

A SON'S VIEW OF KOSHLAND FAMILY LIFE, PARENTAL ROLES AND RESPONSIBILITIES, HAVERFORD COLLEGE, AND MARIAN KOSHLAND'S CHARACTER

Interviews Conducted by
Sally Smith Hughes
in 2000

INTERVIEW WITH JAMES M. KOSHLAND

James M. Koshland: Education and Career

[Date of Interview: January 10, 2000
[Palo Alto, California]

Hughes: We should start with a thumbnail sketch of yourself, just so people know who is speaking.

J. Koshland: Well, I am the third child of my mother and father, by three minutes. I have two older sisters, and then I have a twin sister who was born three minutes after me, and then a younger brother. I was born in 1951 in Cambridge, Massachusetts, and our parents then moved to Bellport, Long Island, where both of my parents were scientists at Brookhaven National Laboratory. We lived there for fourteen years.

We moved to California, to Lafayette, and both my parents became scientists at Berkeley. I then graduated from Lafayette High School, in '69. Went to Haverford College for four years [1969-1973]. Majored in history, not science. Then worked in Washington for two years and went to Stanford Law School for three years [1975-1978] and since then have been a lawyer in Silicon Valley [at Gray Cary Ware] for the last twenty years. So that's my background.

Hughes: So you were about thirteen, fourteen, when you left New York.

J. Koshland: I was fourteen, yes.

Bellport, New York

Hughes: What are your recollections of family life in Bellport?

J. Koshland: Well, we lived in a very small town, three thousand people. In ways, it was very idyllic. We lived right near the water. [There were] houses with white picket fences and green shutters. It was a very classic kind of eastern small town. Walked to all the schools. My parents had a lot of friends. We were on the water, with boating and sailing and those

types of things, which I was involved in. My father was teaching in New York [City], so he spent a lot of time in New York, and we visited New York.

Hughes: Were most of their friends connected with Brookhaven [National Laboratory]?

J. Koshland: Not totally. The laboratory was a big employer, so there were a fair number of people outside the laboratory. But there were a lot of people who were not involved [with academics]--certainly more than they have now, where most of their friends are connected with [the University of California,] Berkeley.

Hughes: How active were your parents in the community?

J. Koshland: My father was head of the school board; my mother was very active in the League of Women Voters and some other things. So, yes, they were very active in the community at that time.

Hughes: They had five children. How did your mother organize her career and home life?

J. Koshland: At Brookhaven she was a research scientist. She was very active. She was a highly disciplined, organized person. She would stay up late at night working. One of the things we joke about is that I can count on one hand the number of breakfasts my mother made for me. That was just not her thing. She worked late, and we knew we didn't disturb her in the morning. She was at Brookhaven every day, as far as I remember, but she certainly took off time to do things with all of us, whether it was to take us to a doctor's appointment or have a meeting with the teacher, whatever. She was the primary parent and clearly made time for that.

Luna Carroll

Hughes: What role did Luna play? There could be two mother figures here.

J. Koshland: I think there was no doubt about it. I think my mother would have acknowledged that. My mother was not the warmest person. That was not her style. She was a wonderfully caring, disciplined, intelligent woman, but she didn't have a lot of warmth in her. And I think that Luna was a wonderful housekeeper--warm, loving, loved to give big hugs type of person--and I think my mother saw that, and they developed a relationship and a real friendship over the years.

My mother was as exacting about the household as she was about everything else, and she really trained Luna, and Luna learned a lot, and they had a great relationship. I think my mother really saw Luna as being a substitute for her on certain of the things that she didn't do well. They enjoyed each other and it was great, and I think she saw her as a wonderful person for her children.

Hughes: And perhaps recognized what Luna could give that perhaps she couldn't give?

J. Koshland: I think so, absolutely. I'm sure my mother was aware of those issues.

Hughes: Did Luna live with you?

J. Koshland: No, she never lived with us. My mother was pretty strong about that. She felt that it was important that we have our family time and Luna have her family time, so Luna lived in a nearby town. But my parents are very caring and Luna was a part of the family but did live separately. Luna's husband physically abused her, and my parents refused to allow him to step foot in their house. He liked my parents and liked us, and it had an impact on him, and I think that helped to reduce those incidents. So they cared about her and knew she was important, but they also felt it was important that they keep separate lives.

Dinnertime Ritual

J. Koshland: Mother came home every night and made dinner for all of us, and dinnertime was a time that we all had to be home. We all had to eat dinner together. As we got older, it was something we disliked as teenagers, but I think long term was very beneficial. We look fondly upon it [now]. My mother was a very good cook.

Hughes: I understand that there was a ritual of going around the table and asking the children how the day had gone.

J. Koshland: A lot of people say that. I don't remember that. One thing I remember: good food. Everybody ate tremendously fast because we all loved to eat and talk at the same time, so it was fairly chaotic. And it was clearly highly charged. Everybody was expected to express their opinion. You couldn't just sit there and be glum which, as a teenage boy I tended to want to do, and that was not allowed. You had to express your opinion. If you didn't, you were asked to. So clearly, we would go around and ask people about their day, but also in any conversation you were asked your opinion. It wasn't like somebody was allowed to sit there idly. And, to be honest, most of the time we all wanted to. It was very participatory. It usually took a half an hour to an hour. We didn't want to spend the time to do it, but most of the time I think we all enjoyed it, and it was considered a fun exercise. There was a lot of humor, a lot of yelling and screaming, so it was not a torturous event at all.

Hughes: Were manners an issue?

J. Koshland: My father loved to misbehave, and my mother would chastise him, and so there would be running jokes. My mother was clearly the disciplinarian and cared much more about strict manners. My father would flout that a little bit. So that was part of the joking and kidding that went on.

Hughes: Were they usually highfalutin conversations at dinner? What were the topics?

J. Koshland: It's hard to remember when I was younger. Certainly, as I got older and was in high school, there were a lot of arguments about Vietnam, a lot of arguments about political issues, sports issues. My parents are pretty scrutinizing people. They ask a lot of questions and don't allow easy answers, so they were pretty intellectual conversations. They would ask a lot of "why?" You couldn't just say something. "Why did you say that?" "What went on to do that?" So there was a lot of inquiry and a lot of discussion, not in a harsh way, but in a way that you had to back up your opinions.

I think my father also particularly liked taking adverse positions, certainly on Vietnam and other things. My sisters rebelled and didn't like his contrarian views. He would like to argue with people. My mother would tend to feel that sometimes she had to make sure my father wasn't getting too uppity, and she put him in his place. So there was some arguments on that aspect. But yes, they tended to be very intellectual and on a wide variety of topics.

Parenting Style

Hughes: How deeply were your parents engaged in your education?

J. Koshland: I don't remember [their] being that engaged. Clearly, they knew how I was doing, what I was doing, but whether they provided me day-to-day help, I don't remember that much. But some of my siblings and my parents said that's my selective memory. I complain to my children now that I'm involved in a lot more of their homework than I remember my parents being. The feeling we got was you had to do your best no matter what. I think if you did your best and didn't do well, that would be fine. But if you didn't do well and didn't do your best, that was a problem. So my impression of them was that they really knew what was going on. They cared. But the day-to-day stuff, management, they weren't as deeply involved because they had five kids and they wanted us to do it on our own.

I was a pretty motivated kid. The story Mother loved to tell about me was that she was going to tell my third grade teacher that the teacher was putting too much pressure on me, and the teacher was going to tell my mother that my mother was putting too much pressure on me. So they both exchanged views and started laughing because they realized it had not been due to either one of them. They came to the conclusion that I was the one who was putting the pressure on myself. I was pretty driven, and so maybe my parents knew that and didn't put the pressure on me as much.

Hughes: Were all five children pretty much doing what was expected of them and didn't require much parental direction?

J. Koshland: Most of them did. My second sister, Phyllis, was very gifted, but they didn't think that she always worked as hard as she could, so there was some conflict there. But basically all of us were pretty motivated and worked pretty hard. I don't think they had issues with that.

We'd leave some homework assignment till the last minute and complain about how it was unfair. They would quietly ask, "Well, when did you get the assignment? You got it two weeks ago. You mean you're now doing it in the last couple of days? Why should we have sympathy for you?" You didn't get away with much with either my mother or father.

Marian J. Koshland: Primary Parent and Scientist

Hughes: Do you think that your parents worked out that your mother was to take the major responsibility with the children? Or is that something that just evolved?

J. Koshland: I think it was a combination of both. I think it was a two-edged sword. I think my mother was more interested. She was very interested in development and always said that her kids were a lifetime experiment, that it never stopped and it was always there, so I think she clearly had a lot of interest in parenting.

She also believed that my father's career came first and that she would work part time till all of us left high school. So I think there was a design. But I think responsibility shifted at different times, depending on how hard they were working. So I think it evolved over time, but there was a conscious decision on how to do it.

Hughes: The story your father told me was that your mother when you, when the twins came along, was set to abandon science, at least for the time being. His story is that he said, "No, science is something you're good at and interested in," and then suggested a way of doing science as well as raising children. That strikes me as a little unusual for a man of his generation. Do you have any insight into why he would take that attitude?

J. Koshland: I haven't discussed that with him; I can hypothesize. I think most women have what we call the super mom syndrome. They're supposed to be a super mom, a super wife, and a super participant in whatever field they're in, and that's a lot of burdens. At some point in their career, every woman I've known say[s], "Hey, I want to give one of these up. It's just impossible."

I think my father saw that my mother drew a lot of enjoyment out of science and that she would have a hard time adjusting to not having some work. I think, one, he was very proud of her and wanted her to do science; two, he thought sitting at home would drive her crazy; and, three, they were really soul mates about science. I think they discussed science a lot, and it was important that she keep up with it. The overall is I

think he generally believed that she would have a hard time not working, and I think he was right.

I was young, and I don't remember specific conversations, but we did spend a year in Ithaca, in which my mother didn't work, when I was six years old. My impression is that she was somewhat bored, and I think she was happy to go back to having her responsibilities at work. From that experience they learned that it really was much better for her to work, although it put a lot of pressure on her to do all these things, and I think that was hard.

Hughes: Why the move to Ithaca?

J. Koshland: It was just a one-year sabbatical when my father spent a year at Cornell. I don't know the reasons why she didn't work.

Hughes: On later sabbaticals, at least the two that I've heard about in Cambridge, specific arrangements were made so that she, too, could have a position--the two times that she went to David Baltimore's lab.

J. Koshland: Oh, no question. And on one of them, she taught herself genetic engineering. I think a lot of people were very impressed that she was able to kind of reinvent herself as a scientist through that sabbatical. It was a significant event.

Hughes: And not at a young age.

J. Koshland: Correct.

Moving to Berkeley, 1965

Difficult Adjustments

Hughes: Do you remember having any feelings about the move to Berkeley?

J. Koshland: There were a lot of famous stories about it, but one of them that we all remember and tease my father about is that we had a dinner table discussion about it, and then I'm not sure why my father allowed this, but he did. We had a family vote on it, and the family vote came out six to one against the move. It was clear that we still were moving, so it was clear we were not a democracy at that point.

I think it was very hard for all of us, especially for myself and my twin sister Gail and my brother Douglas. We had lived in Bellport all our lives, so it was a place you were being uprooted from. We had had a housekeeper, called Luna our whole life there, whom

we all loved and really very much wanted her to move with us, and that was considered. She actually came out for a while, but she had a husband and it just didn't work out. That was the hardest initial emotional drawback to moving. I was too young to know the implications. I went from the East Coast to the West Coast. There were dramatic differences. It was hard on me. I think it was very hard on my sister, Phyllis, who moved halfway through high school. It was a very different environment and very difficult.

On the other hand, I think it was a very good learning experience. Going to college was easy after that. It just felt like it was much less an uprooting than it was for me to move when [I was] fourteen years old. At the time I thought it was hard but not that hard. I thought, Hey, this is fun. We're going to California. I've got to say goodbye to my friends. I'll start a new life. I didn't feel like it was the worst thing. [At the time,] I didn't see the impact on my mother. I think it was very hard on my mother.

Hughes: In what ways?

J. Koshland: Well, she had a lot of close friends in Bellport, and to give up those people was very hard on her.

New Priorities

J. Koshland: My understanding from my father is that they made a decision that when they moved to California they would focus on science and not be as active in the community and not make as many friends, which I think, again, was somewhat hard on my mother. She enjoyed the science stuff, but I think later on, when my father was editor of *Science* magazine in Washington D.C., she spent a lot of time by herself, which my mother both loved and hated. I think that made her more isolated in her life.

Hughes: So she didn't develop in California the circle of personal friends that she had had in Bellport?

J. Koshland: No, she did not.

Hughes: And their community activities declined?

J. Koshland: Right, her community was University of California, Berkeley, and she was active in a lot of things at Berkeley. Both my parents when we moved to California became much more active nationally. My father and mother were appointed to the National Academy of Sciences. My mother was on the National Science [council and committees] for many years. She had some major roles in Washington, for a long number of years. That's where she spent a lot of her time, so she just didn't have time for community involvement in the Lafayette area.

Hughes: Did the daily routine change after the family moved to Berkeley?

J. Koshland: No, I think it was very similar. As I said, my mother traveled a little more, as did both of my parents. We of course were older and more independent, so that allowed for that.

Hughes: Was there an adult who took over when they went away?

J. Koshland: We'd have a housekeeper, usually. By the time we moved here, my sister was sixteen and we twins were fourteen and my brother was twelve--we basically took care of ourselves.

It was rare that my parents both went away together. For a long time--and I don't know when they switched--they refused to ride a plane together. They felt that if there was some accident, with five children, it was an unfair burden to impose on anybody, so they would not travel together.

Gardener and Cook

J. Koshland: The other thing that I remember that was significant when we moved to California was that my mother was always a big gardener, and as we became more independent, she would garden more. We all complained that she would come home from Berkeley and garden, and then we wouldn't get dinner until eight or nine o'clock at night. We thought it was terrible that she would delay our eating habits that way, so that was probably the only significant change in the house. She was a fabulous cook. To have hamburgers and potato chips was not something my mother tolerated very much at all. She always felt that it had to be a good meal, and so she would spend half an hour, an hour cooking a meal. That was just part of her. Light gardening for her was very cathartic. She liked it.

Hughes: The gardening I know she did on her own, but the cooking?

J. Koshland: Yes.

Hughes: You weren't standing around and chopping the onions or whatever?

J. Koshland: Never. We've all had spouses, and my sisters are all very good cooks, [but] my mother controlled the kitchen. She had done it for so many years by herself that it was not something that she'd delegate. She eventually learned to delegate, but she really liked doing things her way, and the kitchen was her domain.

Hughes: What about teaching her children to cook?

J. Koshland: I don't think she wanted to impose on any of us. I think my sisters showed more interest, and she would be involved to the extent that somebody showed interest. Certainly all

three of my sisters are good cooks and I think learned from her, but I also think she let you decide how much you wanted to be involved and how much you wanted to learn.

Hughes: And that wasn't a decision that you made?

J. Koshland: That was not a decision I made. [laughter] I like to eat good food. I learned enough that I can cook a basic meal. I'm not totally incompetent, although my children might say I am. But no, I just didn't show an interest, and that was okay with her and with me.

Hughes: And Douglas as well?

J. Koshland: I think he showed a little bit more interest than I did. He was obviously the last one, by himself with my parents, so he had more involvement with them. We all had jobs, and we traded off setting the table and doing the dishes. When there were seven of us eating every night, doing the dishes was a heavy-duty task, and so we all were involved in that. For me, doing those tasks was enough.

Siblings and Parents

Hughes: You described the dining table scene as rather chaotic. Was there a lot of give and take and roughhousing among the five children?

J. Koshland: Yes. Well, first of all, I'd say that it's part luck, part genetics: the five of us got along pretty well. We always have. Clearly, there were times of conflict. Certainly, my brother and I wrestled a lot. My brother would complain--still complains to this day--that I beat him up too much, which is probably partially true, but we really did get along. It was not an issue. We're now spread out all over the world, with two sisters in Australia, a sister lives part-time in Paris, my brother on the East Coast, and me on the West Coast. We don't communicate much because I think we just feel this bond. We don't need to. I'm not sure that's always good. I think communication is better.

My older sister and my twin sister had times of strife. My older sister rebelled and had some conflicts with my parents that isolated her a little from the rest of us, but generally we got along very well and enjoyed each other. We also were bonded against my parents in a good way in that my parents were, as I indicated, very strong--strong intellectually, strong in discipline, strong people, who agreed on everything. On discipline, on anything, they were pretty uniformly in agreement, and I think they just thought a lot alike. They also believed it was important that they support each other, but I also think they thought a heck of a lot alike. We all felt, hey, we have these strong parents; we as kids would unite. We really felt that it was our parents and the kids, and there were two separate units. We just all enjoyed each other and liked each other from day one.

Hughes: When we talked about his science, Dan made no bones about the fact that he really enjoys a controversy, and I got the feeling that even perhaps took an extreme position just to provoke a response. It sounds to me as though that is happening on the family level as well.

J. Koshland: There's no question. There's no question he likes being contrary.

##

J. Koshland: [If the family was] spending a long time in a car, we would have license plate races where we'd [see how many] different state licenses could you see. He would love to take one position or the other, saying, "Okay, are we going to see twenty licenses? I'll take either over or under. We won't see it or will see it." Then he would be on one side or the other. If we were supposed to get less, he'd drive around, try to avoid it, speed up, do whatever he could so he couldn't see a license. If he wanted to get more, he'd go off the road and go to some big parking lot to try to find some. So he would even take contrary positions there. So there is no question that being difficult is part of his personality.

Religion

Hughes: Did your parents have an understanding about how religion would be handled in terms of the children?

J. Koshland: There was never really much discussion about that. Religion was just not a very important part of our household. They did at one point have us go to the Unitarian church. I think they felt it was important to give us exposure to that. It was not very successful. My parents didn't go. As kids, none of us liked it. It didn't stick very much.

When we moved to California, we actually got more exposure to the Jewish traditions because my father's family lived there. They are more religious. They do go to temple once in a while. So we did things like go to Yom Kippur services, which we never did in the East Coast. Typically, my mother knew a lot more about it than my father did, even though it was his faith.

My parents were scientists. We always say science was their religion; that's what they really believed in, and it became very apparent. There wasn't a put-down of religious things at all. They had a very scientific approach to everything, and really believed in that. That's what came across, and so there was just wasn't a lot of talk about religious aspects of life.

Hughes: Was there any pressure from the larger family to raise you in the Jewish tradition?

J. Koshland: There was no pressure like that at all. We moved when I was fourteen, so I was past the bar mitzvah age.

Science and the Family

Hughes: What about pressures in terms of career, particularly science? Douglas ended up in science. Is he the only scientist in the family?

J. Koshland: Gail really is now. She did physical therapy, but now she does kinesiology, which is the study of muscle movement, so it's really a science. No, no pressure at all. That was very clear. In fact, there was some pressure against going into science because I think they felt that they were fairly well-known scientists and you would have that burden of being their children and did we really want that?

One sister, Phyllis, went to Harvard and was involved in science and actually dropped out for a while. And one of the issues was that she got a lot of "Oh, you're Dan Koshland's child" or "You're Bunny Koshland's child," and she didn't like that at all. But Douglas really loved it from day one. It was clear he really liked it. That's what he wanted to do. And they didn't say, "Oh, no, you can't do it." To their credit, they really wanted us to do things we loved.

Hughes: What do you have to say about your mother as a woman in science?

J. Koshland: Not being a scientist and not knowing much about science, I can't say anything about that. It was fun to observe as a kid and reflect on how differently my parents approached their science. My mother always liked a small lab with a small number of students, and she was very hands on. You could tell by going to her lab [that] she was very hands on and liked it. My father liked a big lab and a lot of students, and he didn't get his hands dirty at all and kind of directed people.

People ask me why I work so hard, and part of the reason I always tell people is it's what I expect because [it's what] I saw my parents doing, and so [I] do it. They just loved what they were doing and worked hard at it. My father, I think even more than my mother, if given a choice, he would be in the lab. He loves it. I think my mother really loved it. As a young adult, it was a time when there were more working mothers and people said, "How do you stand on working mothers?" And I said, "Well, I think I turned out pretty well, so I'm very in favor of it."

I was always very proud of my mother, the fact that she worked. I thought she handled it well, and I was proud that she was a working mother and a fairly well-

renowned scientist. As a kid, I never felt that I was burdened because my mother wasn't home making me cookies when I got home.

Hughes: Did they both make an effort to include the children in their science?

J. Koshland: Once in a while, they discussed science. I don't think they felt like imposing that on us. My brother, who is in science--they've enjoyed talking with him. I think what they really wanted to [im]pose on us was a rigor and an intellectual approach in the scientific way. As I said, you couldn't be superficial about issues. You really had to think it through. They would get angry if you didn't approach things in a way that they felt was thorough. So there was much more of that than talking about science. I don't think they did that very much.

Hughes: Your parents would talk about science when you weren't around?

J. Koshland: Yes, I think they talked about science a fair amount when we weren't around. I don't remember them talking about science at the dinner table very much. Or at other times.

Service to Haverford College

Board of Managers, 1982-1994

Hughes: Your mother served on the board of managers of Haverford College [1982-1994], your alma mater. Did she talk to you about that, since there was obviously a connection there?

J. Koshland: Obviously, we talked a lot about it. It was a great thing for me in that I was the first one [in the family] who went to Haverford, really loved it. My brother went there and liked it also. My mother's connection there I thought was fabulous. It gave us connection. I thought she was great for the school. It was really a good place for her. I always felt a little bit guilty that she never had that connection with Vassar, which was where she went. I think there were some reasons for that. Vassar didn't develop science. Haverford had tremendous science and had some young people on the faculty that she really connected with. I was always very happy that she contributed so much to Haverford.

Hughes: How did she come to be appointed?

J. Koshland: I don't remember. Robert Stevens was the president she really got along with. He was this rotund Englishman who had a good sense of humor and was very intellectual. He wasn't as much a glad-hander as you see in presidents now. He and my mother had a very special relationship. Unfortunately, subsequent presidents could never measure up because she had such a special relationship with Stevens.

Hughes: Wasn't she engaged on the board in the scientific revitalization of Haverford?

J. Koshland: Haverford has always been very good in science. As a small school, because most big-time science is done in big-time colleges, it had issues as science has really changed over the years. She was good at anticipating some of the issues and working them through, but I don't think it ever needed to be revitalized. Recently, they were creating a new [science] building and she was active in that until she got off [the board]. There were some older faculty who had to get pushed aside, and I think she was helpful with that. Unlike my father who had to restructure [biology] in Berkeley, she didn't have a particular event at Haverford that was as big. But there were a lot of events during the course of her tenure that had a lot of impact, and she was very involved.

Hughes: Was she looked to as the board member who was responsible for science?

J. Koshland: Yes, to a certain extent. I think she was more looked at as someone who was very interested in faculty development. She was an academic but also a parent and also was [a] contributor. I think she had a wide view, but her real interests were in the academic areas. She wasn't interested as much in raising money or with the facilities. Her main interest in Haverford was that it was unique, a small school that had very good science, and she wanted to keep that. So in that sense, yes, she was very much the sciences advocate on the board.

Catherine P. Koshland as Board Member

Hughes: And now your wife [Catherine P. Koshland] follows in her footsteps. Is that an accurate statement? Was there a philosophy passed down from your mother?

J. Koshland: Well, I think they're similar people; they're both pretty tough, demanding people. My wife, being an academician and very interested in academics, has wanted to take my mother's role, so I think she's perceived as that. My wife is an engineer and not as much in the biological sciences [as my mother was], so I think she's interested but not as driving as my mother was on that aspect. But I think in a lot of ways she has very similar interests and is a similar personality to my mother. I think they're seen as kind of two peas in the same pod.

MEK: Appearance and Personality

Hughes: Please describe your mother, both physically and in terms of personality.

J. Koshland: She was about five-five, had blond hair, always was slight and cared about her weight. She always had short hair, wore glasses. She was a Puritan; she grew up in New England, had a very strong, straight approach. She loved a sense of humor, liked to laugh, was not somebody who cracked a lot of jokes. But my father loved to play jokes and pranks, and I think she loved that aspect of him and felt that was an important characteristic.

My mother cared about good clothes. When she went shopping, she spent time and looked for high-quality things and had a good sense of clothes. She was not flamboyant in any way, but liked quality.

Hughes: So she was definitely not a dowdy woman scientist.

J. Koshland: No, not by any stretch of the imagination. She cared very much about what people [thought] about her. My mother, as I said, was very strong, caring, very moral, very ethical. Lying, cheating, honesty were very key issues for her. She wanted to make an impact on people's values.

She was insecure in certain ways. She had a tough childhood and did not feel very close to her family, other than her mother, who died when I was very young. She came into a big Jewish family that liked each other a lot, and she basically said, "I don't want anything very much to do with my family." At one point she told my grandfather Koshland Daniel E. Koshland, Sr.[1] that she felt a lot closer to him than to her father. My grandfather thought that was terrible, just because he just didn't think that was the right way you should feel about family.

So I think in some ways she felt isolated and insecure because of her family background and tried to overcome that. But on the other hand, she believed in her way of thinking and her way of doing things. People who knew her well liked her intellect and her direct, honest approach. They could really know how much she cared about people.

Relations with the Larger Koshland Family

Hughes: Do you think she felt accepted by the Koshland family?

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[1]See the ROHO oral history with Daniel E. Koshland, Sr., *The Principle of Sharing*. Interviewed in 1968 by Harriet Nathan.

J. Koshland: Yes and no. I think she felt very accepted by my grandfather, and my mother and father had a very special relationship. They were really soul mates for a long time and really cared about science, and that clearly came through. A lot of people who liked my father saw that and enjoyed that.

There were certain other people with whom my mother had a lot of conflict about raising kids and jealousies. Later on I think they reached peace about that. But I think it was hard on my mother. When you have a tight family, as an outsider--we always laughed: the outlaws and the inlaws--there's always a sense of that. You can never overcome that. With certain people she felt like an outsider at times--but I also think her caring came through, and she became more accepted. So I think she became accepted, but it was not perfect.

Conversational Style

Hughes: Did she have any small talk?

J. Koshland: She was not good at that. It was not her strength. I think she liked good talks. Some of my high school friends always enjoyed talking to my mother and were amazed because my mother knew a lot about sports, which was pretty unusual, and she cared about them. I think she knew it was a way for her to interact with her sons, especially me, [for whom] sports was almost everything in life. But I also think she liked them. My friends were amazed at how much she knew and it wasn't just superficial. On the other hand, I had friends who'd see my mother and they'd say, "No, no, I just can't talk to your mother right now. She's just too intense. She'll ask me all these questions." And that's just the way my mother was. When she got in a conversation, she asked you questions; she engaged you. You just didn't have a superficial conversation with Mother. It just wasn't what she was about.

Family Stories

Hughes: Are there anecdotes that you would like to record?

J. Koshland: There are so many. I'll tell one story that's famous in our family. The year we were in Ithaca was a tremendous snow year. In fact, we missed three weeks of school. My brother and I were playing outside. There were certain designated areas in the snow where or where not we could go. We happened to be outside of them. We were on a farm outside of town [which had a pond], and we saw this animal struggling in the water. At first we thought it was a bird, and then we both looked and realized it was one of our dogs. We had this tremendous feeling that we had to go tell somebody about this. My mother

was home. [But] we were afraid that she was going to find out that we were not where we were supposed to be.

It was April Fool's Day--which shows you what a snow year it was. So we told my mother about this, and then my mother, in her typical stern way, said, "If this is a joke or if you're not telling me the truth, I'm going to kill you." She went down, and it turned out it was our dog, Duchess. Mother did a stupid thing. She admits it. She went into the icy cold water with boots on, without pants tucked in. She went in and almost flipped and her feet got real frozen because the water obviously went right into the boots. But she got the dog out. And then the problem was, she had to walk up the hill to the house, and walking with these frozen feet was very difficult. But fortunately, the dog was saved and my mother really showed a lot of courage. It's typical of my mother. But the famous part was obviously that she said, "If you're joking, I'm going to kill you." On the other hand, she was going to do what was right.

The interesting thing about both my parents is that we weren't spanked. It was, "You're going to have my wrath, and I'm not going to be very happy with you." Both of them never had to say many words. They just had to say, "We're not very happy with you," and you felt it. There was no question.

The last story I'll tell about my mother is about her temper. My mother could get mad, very mad. My parents felt that it was very important that they treat all of us kids alike. You shouldn't show favoritism. I think this was something my parents talked a lot about. When she would blow up and get mad at one of us, all of us started hiding because we knew that she had to be totally democratic. So she'd get mad at one of us, and then she'd start going down the line. We had signals. Oh, God, when is she coming? We knew when Mom got mad at one of us, it was going to come around to the others at some point in time. And that was her way of dealing, [and] was very representative of my mother's sternness, and yet her approach to things was in a very democratic and caring way.

Hughes: Anything else?

J. Koshland: My mother was a trailblazer who didn't care about recognition. She just wanted excellence in everything.

Hughes: Well, thank you very much.

J. Koshland: Thank you.

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MARIAN E. KOSHLAND (1921-1997): RETROSPECTIVES ON A LIFE IN ACADEMIC SCIENCE, FAMILY, AND COMMUNITY ACTIVITIES

Hugh O. McDevitt

VIEWS OF A FRIEND AND COLLEAGUE ON MARIAN KOSHLAND'S RESEARCH IN IMMUNOLOGY AND LIFE AS A SCIENTIST

Interviews Conducted by
Sally Smith Hughes
in 1999

INTERVIEW WITH HUGH MCDEVITT

Hugh McDevitt

[Date of Interview: November 23, 1999]
[Stanford, California]

Education

Hughes: Let's start with a thumbnail sketch of yourself.

McDevitt: Well, I was an undergraduate at Stanford and left at the end of my junior year; graduated with the class of '52 [B.A. in biology]. Went to medical school at Harvard [M.D. 1955] and did the usual internship at the Peter Bent Brigham [1955-1956], residency at Bellevue [1956-1957], two years in the army [1957-1959], senior residency at the Brigham [1961-1962]. Mixed in there were two years of postdoctoral fellowship with Albert Coons at Harvard, which was educational but not terribly productive, and then two years [1962-1964] at the National Institutes for Medical Research at Mill Hill in London, where I worked with John Humphrey and Birgitta Askonas.

Career in Immunology

McDevitt: The experiments that I did there grew out of some experiments of John Humphrey's, and essentially got me started on a set of experiments which led to the discovery a number of years later that the ability to make an immune response to synthetic polypeptides, a type of artificial protein, was linked to the major transplantation antigen complex.[1] It's called the major histocompatibility complex. That discovery was made in 1967 and was published in '68.[2] That led to a great many discoveries: the discovery of what are called class II major histocompatibility, or MHC, molecules; the isolation of these proteins using

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[1] See: Hugh O. McDevitt, "Discovering the role of the major histocompatibility complex in the immune response," *Annual Review of Immunology* 2000, 18:1-17.

[2] See McDevitt's bibliography in the appendix to this volume.

antisera that we made in a variety of inbred and congenic mouse strains; and then finally the cloning of the genes, and the characterization of all the different alleles.

A great many other labs had made the congenic mouse strains that permitted us to map this genetic trait. Then using those strains, my lab and many others made antisera, which permitted us to isolate the proteins which now are called class II MHC molecules. Like the class I MHC molecules, they bind peptides and present them to T-cells. But the way that happened was that first the immune response trait was discovered, then it was linked to MHC, then it was mapped using these congenic strains, then the protein was isolated, then the genes were isolated, and then in the early eighties it became possible to purify these molecules.

The real breakthrough was by Emil Unanue, showing that what these molecules did was bind peptides and that any one MHC molecule could bind lots of different peptides and present them to T-cells. So it's sort of the first obligate level of recognition in the immune system.

Hughes: The latter was the work of the eighties?

McDevitt: The work of the eighties. There were some indications shortly after our work that the T-cell saw not just foreign antigen but foreign antigen plus self-transplantation antigen. We and a number of others had shown that one particular form of these class II molecules could determine the ability to respond to a lot of different proteins. The thing that clinched it was that Unanue showed that these molecules had a peptide binding site, that foreign proteins were taken up in cells of the immune system, broken down, and the peptides were loaded into the MHC molecules, and that's what the T-cell saw. At the same time, Mark Davis and others had begun to clone the genes for the T-cell receptor. So by the late eighties, the general picture of how the immune system worked really first fell into place. There's a great deal more progress that has gone on, lots of new discoveries, and the system gets more complicated, but those are sort of the broad outlines.

My work was concentrated on first analyzing this genetic control, linking it, mapping it, using the antisera to identify the gene products, working first with proteins and then cloning the genes, then getting all the sequence polymorphism, then showing that those are associated with a lot of autoimmune diseases. More recently what we've been concentrating on is getting down to the molecular basis of exactly how different forms of your class II molecules make you susceptible to type I diabetes or rheumatoid arthritis. Actually, this is quite different from the sort of work that Bunny did because she worked mainly on antibodies and proteins related to the structure and assembly of antibodies.

Marian Koshland: First Encounter at Harvard

Hughes: I read that in the early years, she took a chemical approach.

McDevitt: Oh, yes, and I can tell you a story about that. When I had first come back from the army and was a postdoctoral fellow in Albert Coons's lab at Harvard from 1959 to 1961, Bunny Koshland--I didn't know who she was until the notice appeared for her seminar. I think in both the years I was at Harvard, before I went off to Mill Hill, she gave seminars. I heard her talk two or three times in that period. When I returned to Harvard in the mid-1960s, she was at Brookhaven. She came up to Brandeis and gave a seminar. So I heard that antibody specificity talk several times.

Hughes: Were you impressed by it at the time?

McDevitt: Yes, I was impressed by it. Here was somebody who was addressing a major problem that we were all trying to figure our way through, and she was doing a very careful, very thorough, and I thought convincing job. In fact, in the seminars I went to, nobody got up and said they didn't believe it. But there may have been some--I don't know who the people were who said they didn't believe it, but there may have been some guys who tried to reproduce it and couldn't.

And you can imagine thatif you don't have chemically very different haptenes, if you use similar haptenes, and you realize that the number of amino acid residues that actually are in the antibody combining site is maybe twenty or thirty out of 1500, and if you didn't have very, very good amino acid analysis and very good quantitation, it would be easy to come up with the answer that all antibodies had the same amino acid composition.

Hughes: Her scientific approach could be defined as rigorous?

McDevitt: Yes.

Hughes: What else do you have to say about how she approached her science?

McDevitt: Well, that was an impressive set of experiments because it was approaching a tough problem. It was applying a new technique to the problem and a new approach; namely, let's make antibodies to haptenes of similar size and shape but very different charge and see if we can pick up differences in their amino acid composition.

Hughes: Nobody else had done that?

McDevitt: Nobody else had done that. In addition to that, it was the quality of the immunization, the purification of the antibody, and the quantitation of the amino acid composition that was impressive. And it fit with the idea that there were lots of cells that made lots of different antibodies. But it didn't prove it; just like our experiments on fate of antigen didn't prove it; [C. B.] Anfinsen's and [Edgar] Haber's experiments didn't. The amino acid sequences proved it.

Immunology Circa 1960

McDevitt: But you have to know the state of immunology in 1960: people knew there were antibody molecules, but they were still arguing whether there was a single gene that encoded a protein that was the antibody molecule. There was a theory of antibody formation called the instructive theory, originally put forth by [Linus] Pauling and [Stuart] Mudd and [Felix] Haurowitz. The idea was that cells of the immune system knew how to make an antibody molecule, but how did it acquire its specificity? It had been shown by lots of people that you could induce antibodies to almost any chemical configuration of any protein we wanted, as long as it was foreign. You could take, say, the benzene ring and put a nitro group on the ortho-, meta-, or para-position, or on all three, and produce antisera that would discriminate between all those different forms of a benzene ring--as long as you attached the benzene ring to what's called a carrier protein. That was work of [Karl] Landsteiner from the thirties, so it goes way back.

People wanted to know how antibodies acquire their specificity. There were two main ideas. One was that there was a single chain, and it was folded around the antigen, and that would predict that in antibody-forming cells there would have to be some detectable number of antigen molecules. The other idea was one that had been brought forth in the mid-fifties and later fifties by [Niels Kaj] Jerne and by Dave Talmadge, who was at the University of Chicago when Bunny was there [1943-1949], and by a number of others, who argued that there were lots of different cells in the immune system, antibody producing cells. At this time, nobody knew anything about T-cells. The textbooks said that the thymus was a vestigial organ like the appendix and didn't have any real function.

Any animal has the ability to respond to almost any chemical configuration you want to shove into it. And so the main idea was really this instructive theory: one or a few antibody genes coded for proteins that folded differently because they folded around the antigen molecule. Pauling's idea--because he discovered disulfide bonds between the -SH groups of cysteine molecules--was that they got stabilized by these intrachain disulfide bonds. The other idea had been put forward by Talmadge and by Jerne and [Joshua] Lederberg and Gus [Gustav J. V.] Nossal in various different forms. And also to a certain extent by [Frank Macfarlane] Burnett. The idea was that there were lots of different antibody-forming cells, and they made antibodies with different structures, and the antigen then somehow had the ability to selectively stimulate antibodies that were complementary to the antigen.

Hughes: What was their evidence?

McDevitt: The evidence was all very indirect. For example, Jerne had done a bunch of experiments immunizing I think either rabbits or guinea pigs with diphtheria toxin and showing that the antibody that was made in the secondary response, after a second injection of toxoid, was different. It had a stronger affinity for the diphtheria toxin than the first response. So it was likely you had selected out a different cell.

Hughes: You induced--

McDevitt: You had induced a different cell, and you were getting a better and better fit. There were indirect reasons like that. There were arguments based on the specificity of antibody and the specificity of enzymes and the ability of antibodies to combine with viruses so that there must be lots of different antibodies. One possibility was that there were lots of different cells, and you selectively stimulated them to make their particular antibody. It goes all the way back to [Paul] Ehrlich's side-chain theory that cells have side chains on them that can interact with ligands in the cellular environment.

There were two big ways to distinguish these ideas. One of them would be to develop a very sensitive way to look for antigen in an antibody-producing cell, and actually Nossal and [Gordon L.] Ada did that experiment. (By that time we knew that antibodies were produced in plasma cells. A lady in Sweden named Astrid Fagraeus had shown that.)

The other way would have been to show that antibodies to different determinants had different chemical compositions, different amino acid compositions, different sequences really. And that's what Bunny did. At the same time, Nossal and Ada (and actually our own experiments with John Humphrey and Brigitte Askonas at Mill Hill) agreed that there was no antigen in an antibody-producing cell.

Hughes: So did that push you in the other direction?

McDevitt: Oh, absolutely. Anfinsen and Haber had done an experiment in which they purified antibody to a dinitrophenyl ring, and took that antibody and completely denatured it by breaking up the disulfide bonds in mercaptoethanol and eight-molar urea, which breaks up all the hydrogen bonds, so the antibody molecule was completely unfolded.

Then they simply dialyzed out all of the urea and all of the sulphydryl reagent, and they got antibody activity re-formed. They purified antibodies with, say, dinitrophenyl and then completely denatured them, then renatured them. They could recover antibody-combining activity for dinitrophenyl. If you did it with picryl chloride--picryl is a trinitrophenyl group, or some other determinant, you could do the same thing. This said that the ability to make the antibody-combining site in the three-dimensional structure of the antibody was encoded in amino acid sequences of the purified antibodies. But that came after Bunny's experiments.

More on Marian Koshland's Early Research

Antibody Specificity

McDevitt: The experiments that I first heard of Bunny doing were these really very, very technical, very difficult experiments, in which she made antibody to I think a positively charged haptene. A haptene is a chemical determinant stuck on a protein carrier. If you inject a small chemical like dinitrophenyl or picryl chloride or any others, you don't make any

antibody to it. But if you couple it to a protein carrier, particularly if the protein is foreign-- So if you couple it, say, to bovine serum albumin or crab hemocyanin or something like that, then you get antibody to the carrier molecule and to the haptene, a chemical determinant. So Bunny took antisera that she made in rabbits to a positively charged haptene--in other words, some small chemical configuration with a positive charge--and then another antiserum to a negatively charged haptene, and she purified these two antibody populations.

The way you purify the antibody is to couple the haptene to a different protein, and then put this inside of a matrix and trickle the antisera through it. The antibody binds to the haptene, but because the carrier is different, none of the anti-carrier antibodies bind, so you then get mainly the pure anti-haptene antibodies. We now know there are lots of different heavy chains and light chains in it, but it's all against a positively charged haptene, or, from the other experiment, a negatively charged haptene.

Hughes: Why did you call Dr. Koshland's experiments "difficult?"

McDevitt: Well, we're getting to it. Because first she had to make the antisera, which in itself is not trivial. You've got to take whatever your haptene is, your small chemical compound; chemically couple it, usually through reactive chemicals like diazo groups, to the protein; then immunize the rabbit with it, then take the rabbit serum and purify out of the rabbit serum the antibody specific just for the haptene. By putting the haptene on a different carrier, and putting that in a column matrix, then only anti-haptene antibody binds. By sharply lowering the pH, you can then elute semi-pure anti-haptene antibodies.

You then get a population of molecules which contains lots of different molecules. But the vast majority of those molecules have the ability to bind to the positively charged haptene. With another haptene, you can do the same thing: make antibody to a negatively charged haptene. Then the really tough part comes. How are you going to find out how the antibodies are different?

Hughes: Is this Brookhaven [National Laboratory] work?

McDevitt: This is Brookhaven work. This is 1960.

Hughes: So she didn't know about light and heavy chains?

McDevitt: Absolutely not. No, the structure of the antibody wasn't known at all.

Hughes: All those steps that you were describing sound very biochemical to me.

McDevitt: They are.

Hughes: How did she know how to do them?

McDevitt: Well, the techniques of isolating haptenes of various chemical configurations came straight from organic chemistry. The ability to couple them to protein carriers was developed by Landsteiner and many people in the twenties and thirties.

Hughes: The techniques were in the literature?

McDevitt: Yes, sure. Those techniques were sitting there, available. The idea of making an antigen column and eluting the antibodies off of the positively or negatively charged haptene column was not a breakthrough. But the trick is when you've got these sets of antibodies. If you took those and ran them out in an electric field, called electrophoresis, you wouldn't see any difference. You might see a little bit of difference in the migration, but it would be awfully hard.

If you used any of the other physical or chemical methods of characterizing antibodies at that time, you couldn't have told the difference unless you were able to do an amino acid sequence. Those techniques were just being developed by [Fred] Sanger, who sometime in the early sixties published the first sequence of insulin. He got the Nobel Prize for it. It was a great technical breakthrough.

Hughes: So how did Dr. Koshland do it?

McDevitt: She said, "But we do have amino acid analyzers." In other words, you put a protein in an ampule with relatively concentrated hydrochloric acid and then evacuate it so there's no oxygen, and then you heat it to I think 110 degrees for twenty-four hours. The nature of the peptide chemical bond is that the acid will cleave at the peptide bond, so you get a whole bunch of individual amino acids. You can then take those and run them through an amino acid analyzer, which quantitates the amount of all twenty amino acids that protein is composed of.

We now know, for instance, that the heavy chain is about 50,000 molecular weight. It has something like 525 amino acids in it. The light chain is 20,000 molecular weight; it has about 215 amino acids in it. You have two heavy chains and two light chains in an antibody molecule, so you have a molecule of about 160,000 molecule weight. The two heavy chains and the two light chains in an antibody are identical. So the total is about 1500 amino acids.

So Bunny had to run that through the analyzer and have such good techniques in the analysis that she knew she had loaded on exactly, say, one milligram of antibody protein, or two milligrams of antibody protein. You must quantitatively cleave all the peptide bonds and then quantitatively recover the amino acids. And so she could say, "This antibody, the one to a positively charged one, has, say, thirty-seven molar equivalents of glutamic acid, which is a negatively charged amino acid, and it has twenty-six equivalents of aspartatic acid, which is another negatively charged amino acid. Whereas, the antibody to a negative haptene has less of those negatively charged amino acids but, compared to the first one, more of the positively charged amino acids.

In other words, in the antibody-combining site for the positively charged haptene, there are a couple of negative amino acids, and for the negatively charged haptene there are a couple of positive amino acids. Bunny had quite good quantitative, statistically significant data saying that antibodies of different specificity have different amino acid compositions.

Disagreement

Hughes: She could say that because she was an exacting scientist?

McDevitt: Very careful. It was a tour de force. And as she says in this article[1], some people said they couldn't reproduce her result, and they criticized it, and a lot of people didn't believe it.

Hughes: She said that in the context of the problems she thought she encountered as a woman.

McDevitt: She didn't think she encountered them; she did.

Hughes: She did. If the people who disagreed with her results were not as exacting, then of course they couldn't reproduce her results.

McDevitt: That's true. In those days, immunology was much less of an exact science than it is now. Now, when you're saying something about A, B, or C, people want to know if you cloned the gene or if you've knocked it out; and if you have, have you done this, this and this experiment to test the activity in the knockout or in the transgenic that overexpressesit? So there's not often a lot of argument because most experiments are pretty straightforward.

But in those days, you'd go to a meeting and someone would say they took this antigen (x) and immunized rabbits to produce antisera. and someone, often Elvin Kabot or Merrill Chase would get up and say, "Your antigen wasn't pure" or "Your antibody population was impure or it was contaminated." There were frequent arguments over purity, quality of the preparation of the reagent or the antibody, or interpretation of the data.

Hughes: How could Dr. Koshland counter such disagreements since one of the essences of science is its reproducibility?

McDevitt: Right. One thing is that she could show that it was reproducible in her hands. Another one is that she had positively charged haptenes and negatively charged haptenes. The other kind of haptene that you can have is a molecule that is hydrophobic; in other words, it has chemical characteristics that make it not like water. It would dissolve readily in organic solvents like acetone or methanol or something like that. She tested positively charged, negatively charged, and hydrophobic haptenes and got different amino acids for each one. I can't remember all that she did, but she had controls that pretty much said the only thing that stood out in all of these different analyses was the correlation of the amino acid composition with the nature of the haptene. And if you're going to say it's an impurity, then you've got to say there's a systematic error that goes with positive haptenes, and a different error for negative haptenes. And it's terribly unlikely.

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[1] Marian Elliott Koshland, "Sheer Luck Made Me an Immunologist," *Annual Review of Immunology* 1996, 14:ix-xv.

Advances in Immunology

Hughes: So she eventually prevailed?

McDevitt: Oh [laughs], that was a funny time for me because I went to Mill Hill, spent two years, and "Hurrah," I said, "there's no antigen in an antibody-producing cell." And just as I was getting those first results, Nossal and Ada--Gordon Ada, who had come to Mill Hill as a visiting scientist at the same time, said, "We get exactly the same results, and it's in press." So I knew we were right. I said, "Well, as long as I'm right, it's okay." And Gordon says, "You're kidding yourself." I said, "What do you mean?" He said, "You know damn well what you really want is to be first." I said, "Well, you're right."

Then almost before you knew it Haber and Anfinsen published their studies that if you denatured an antibody to a haptene and then let it renature by itself, you got back antibody activity. The one technical cavil to that was that maybe when you denatured it and dialyzed it, you didn't get rid of all the haptene; maybe some haptene stayed there and made it re-form.

Hughes: How did you counter that?

McDevitt: Well, they couldn't really. At some low level you can't. But then the next thing that came along was that people realized that myeloma proteins from multiple myeloma tumors were really just antibody molecules. Then [Gerald Maurice] Edelman showed that they were made up of a heavy chain and a light chain. Then the real thunderbolt came only in 1965, at the first antibody workshop, which was held somewhere in the Southwest.

Edelman at the Rockefellar with Norbert Hilschmann and [F. W.] Putnam--each had sequenced a myeloma light chain. What they saw was that they had different sequences in the first hundred amino acids, and then exactly the same sequence in the last hundred, which said that two genes had to get together and contribute to that sequence, and that different antibodies had different amino acid sequences. That put Bunny's, Nossal and Ada's, ours, and Anfinsen and Haber's work--all more or less indirect--to rest. Each antibody has a unique amino acid sequence.

More on Antibody Specificity

Hughes: Dr. Koshland says, in writing about one of the influences on her life, that as early as Vassar [College], the postdoc or whoever was brought in to teach the lab section, wanted them to be creative.[1] She says that it was at that point that she became interested in the specificity of antibodies. If you're interested in antibodies at all, how could you not be interested in specificity? What else would you be interested in?

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[1] "Sheer luck made me an immunologist."

McDevitt: Right, and she refers to Landsteiner. We all read Landsteiner in those days.

Hughes: Why?

McDevitt: Well, because he published a book called *The Specificity of Serological Reactions*. He could immunize a rabbit with dinitrophenyl and with some other haptene coupled to a carrier, and the antibodies would distinguish each haptene. He has tables and pages and chapters on the specificity of antibodies to ortho-nitrophenyl, meta-nitrophenyl, para-nitrophenyl, in other words, the three possible positions. And the antisera would distinguish between those.

Those proteins weren't there before. You had to immunize the animal with the haptene coupled to a protein. But then what Landsteiner showed was you could induce antibodies to almost anything. And there was the main conundrum: what is the actual basis of this specificity? He was showing the serological basis. In other words, he could take sera from rabbits that had been immunized and show that they would react selectively if they were immunized with orthonitrophenol, and so on.

But the actual basis of antibody specificity had to be chemical. People didn't know until the late thirties that antibody molecules were gamma globulins, and it wasn't until they developed methods for determining molecular weights of proteins that they knew that the molecular weight of the main kind of antibody was 160,000, which is a big protein.

Hughes: If people were not taking a chemical approach, what were they doing?

McDevitt: Well, there was the way Nossal and Ada and we did it. In other words, we were saying if the instructive theory is right, there should be antigen in an antibody-producing cell.

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McDevitt: So if the antigen was some kind of a template, there had to be enough antigen in the cell for antibodies to fold and get out at a very fast rate. What we showed was, there wasn't any antigen in an antibody-producing cell. What Anfinsen and Haber showed was that the denatured antibody would renature and recover its antibody specificity. And what Bunny showed was that the amino acid composition of specific antibodies, specific for different haptenes, was different. But that's nothing like a sequence.

Hughes: Dan [Koshland] I guess would define himself as a protein chemist. Do you think he and Dr. Koshland learned from each other?

McDevitt: I think there was a certain amount of cross-talk, but Dan was not an immunologist. You have to understand the absolute primitive level of our understanding of antibody structure at that time. Now, by 1965 it was a lot better, and by 1970 it was very much better. But in 1958, when she had to have been starting these experiments, nobody knew much of anything. And there were indications of the sort that I told you about from Jerne and Talmadge and Burnet and Lederberg that there were lots of cells making different antibodies.

What Bunny did was an experiment that asked the question: If that's true, antibodies should have different compositions. And that's what she found. It certainly convinced most people, even if there were some who didn't believe it.

Koshland's Use of Automated Amino Acid Analysis

Hughes: The story you seem to be telling is of a technology-driven science, would you not say? The refinement of the vision of what's actually going on is to a large extent dependent on more refined techniques.

McDevitt: That's always true.

Hughes: But she didn't let technological problems stop her.

McDevitt: Well, she went out and got a new technique. Automated amino acid analysis was just coming on line in the late fifties.

Hughes: But I mean before that, when she was using the haptenes.

McDevitt: She applied them in a different way. But the ability to make haptene protein conjugates, to purify the antibody, that was around.

Hughes: Yes, but it hadn't been applied in this particular problem, had it?

McDevitt: Well, it probably had in the sense that you could study the purified antibodies. But, as I said, if you look at them in terms of their electrophoretic mobility or their molecular weight or any of the cruder measurements, you didn't see any difference. What she did was take a relatively new technique, highly quantitative amino acid analysis, compositional analysis, and apply it very carefully.

Hughes: You'd have to have a lot of confidence in your technical abilities, wouldn't you?

McDevitt: Somebody said to me once that scientists were the ultimate romantics because they thought they could start with some rabbit sera, a few reagents, or a mouse and a syringe and a needle and find something out. And they do!

Hughes: [laughs]

McDevitt: Bunny was building on what Landsteiner had done; she was building on the guys who were working to develop methods for amino acid analysis. And amino acid analysis until you automated it was incredibly hard work. But they developed a system where every amino acid came off a column with a different mobility, and the amount of the amino acid was reflected in the height of the peak at each point in the curve as you eluted the amino acids from this column. So she took a relatively new technique, applied it very rigorously, and got a clear answer.

But we were all swamped by the appearance of sequencing, the demonstration of different sequences and different antibodies--and Edelman's demonstration that there were heavy chains and light chains, and Rodney Porter's demonstration that if you digested with papain or pepsin you got the Fab and Fc fragments. So the story really began to fall in place by the early seventies.

Hughes: Do you know enough about her history to make an observation about Dr. Koshland's tenacity?

McDevitt: I saw the final result. [laughs] How she got that way, I don't know. What I saw was a lady who was a clear speaker. She had a nice loud, deep voice, unlike a lot of women. She came to Brandeis and presented her story and answered questions very well. She may have run into a lot of argument and criticism from her colleagues; I didn't see that. I mean, I didn't know her; she didn't know me. In fact, if she'd met me she would have said, "Oh, nice." I was just a beginning postdoc. Remember, I was an M.D. postdoc.

Reencountering Koshland in California

McDevitt: I re-encountered her when I came out here [Stanford University] as a faculty member in '66.

Hughes: And she had come the year before.

McDevitt: Yes.

More on McDevitt's Research

McDevitt: What happened was that I went to Mill Hill and made the initial discovery that there were genetic differences in inbred strains of mice in their ability to make a response to these synthetic polypeptides. These experiments were started by John Humphrey because he wanted to use these synthetic antigens, very heavily labeled with radioactivity, to test whether there was antigen in an antibody-producing cell.

So when I saw the immune response differences in rabbits, which is where John had started, I asked him if I could get a lot more rabbits and do an experiment to see if we could breed them and show that the low responders bred through and the high responders did also. So he got me two whole rooms over at the Lister Institute. And Michael Sela gave us a large sample of this peptide. It's a big molecule, about 100,000 molecular weight. But it was made up of only four amino acids.

By the time I left Mill Hill, we could show that there was a genetic difference. It didn't work very well in rabbits, so I switched to inbred mice. And you could see big

differences in inbred mice. And then I went to Harvard. I was there for two years, and those were what I call the wilderness years because I kept trying to link immune response to this peptide to any gene I could link it to.

Then when I came out here, for largely a different reason, we discovered it was linked to the MHC, the major histocompatibility complex. Not many people paid attention to that, but a few people did. I had a grant for $25,000, which I was running the lab with--one or two technicians and a couple of postdocs. And I said I needed more money, so I asked for another $15,000--in those days that was a huge sum--and got that, and then I was invited to join the NIH study section in immunology and met Bunny.

The Bay Area Immunology Club

McDevitt: Then she and I, at her suggestion, started the Bay Area Immunology Club.

Hughes: Please tell me about that.

McDevitt: Well, it ran for several years. We met at restaurants, mostly in the City [San Francisco]. We would invite somebody from UC Berkeley or Stanford or UCSF to talk. It was a way of getting all the immunologists in the Bay Area together to interact. It sort of collapsed of its own weight, because if something like that doesn't have somebody really after it all the time, it doesn't last.

Bunny and I got to know each other then. She kept reading my work. The work went through a period in the middle or late seventies where my lab did a lot of cell transfer studies that were hard to understand for the non-aficionado of the system. She used to say those experiments were very difficult to understand and give me a hard time about those papers. I wasn't quite clear what she was working on. Somewhere in there she moved into the regulation of immunoglobulin synthesis, and then she started looking at J-chain synthesis.

Recombinant DNA Research

Koshland's Work on the J-Chain

Hughes: She went to David Baltimore's lab in the late seventies, and she cloned the gene--

McDevitt: Cloned the gene for J-chain?

Hughes: Yes.

McDevitt: Well, she started out with an interest in antibody specificity. Then the basis of that became very clear, with all the sequencing studies, and then the structure became very clear with the amino acid sequencing of the heavy and light chains, the characterization of the two different types of fragments you get with papain digestion, and then people went after the immunoglobulin genes. That broke open somewhere in the mid-seventies. People cloned the genes. They were able to get the genes out, even before you had recombinant DNA. You could move them around. People were able to take messenger RNA from a multiple myeloma tumor and begin to identify the RNA messages for heavy and light chains.

Hughes: Without recombinant DNA?

McDevitt: Yes. The first cloning experiment was done in '74 [1973], and by '77 and '78 [Leroy] Hood's lab, [Philip] Leder's lab, Edelman's lab--a whole bunch of labs--were sequencing proteins as fast as you can imagine.

The Bay Area as a Center of Recombinant DNA Technology

Hughes: Did it make any difference that you were here at the center of recombinant DNA technology? Did that give you any advantage?

McDevitt: Well, it did a little because I knew about it, because you needed some way to start. And so what we did was isolate major transplantation antigens. First monoclonal antibodies came along; [Cesar] Milstein developed that technology. Then we used those to isolate one particular type of these class II MHC molecules and get a beginning sequence. And then I collaborated with an old classmate, Sherman Weissman, who was at Yale, who was very good at getting sequences by the wandering spot method. He got what he thought was a pretty good sequence for about twelve amino acids, and we were able to use that to pull out the first class II gene, which we then published, and then we went on from there.

So sure, it helped. Being at Stanford, where all that stuff was going on all the time made the effort to clone the genes much less daunting.

More on Marian Koshland's Work on the J-Chain

Hughes: Dan said that Berkeley wasn't directly involved in the recombinant DNA revolution at its start.[1] I'm wondering what that meant for his wife's research.

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[1] See the oral history in preparation with Daniel E. Koshland, Jr.

McDevitt: I didn't follow her research that closely, so I don't know when she began to work on the J-chain.[1] The J-chain was described by people who were studying IgA [immunoglubulin A] and IgM. They first found it in IgA, then they found it in IgM, and then they knew that it was required for making the pentimer of IgM and the dimer of IgA, and she got very interested in that. And then she began to study the gene and its regulation.

Hughes: I think she went to Baltimore's lab in 1978.

McDevitt: That would have been it. And of course Baltimore discovered reverse transcriptase. He got the Nobel Prize in 1975.

Hughes: They were making complementary DNA?

McDevitt: I'd have to go back and read those papers. But the way it was first done was that [Stanley N.] Cohen and [Herbert W.] Boyer[2] figured out how they could use DNA polymerase and restriction enzymes to take a piece of one gene and another gene and join them together and move them into a bacterium and get replicates. The trick in that was using a bacterial plasmid. The famous plasmid was pSC101.

Other New Technology: Reverse Transcriptase and Complementary DNA

McDevitt: The other thing that made an enormous difference was that Baltimore had discovered reverse transcriptase. That meant that you could take a preparation of RNA from a given cell type, for example a myeloma tumor that's making a ton of immunoglobulin, isolate the RNA message from it, and copy it back into DNA. There were a whole army of labs that were racing to do this first: Leder's lab, Hood's lab, a whole bunch of labs.

You could take reverse transcriptase and make a cDNA [complementary DNA] copy. Even then it was hard work to sequence until Sanger and then [Wallace] Gilbert developed fairly reproducible methods for sequencing DNA. It's always been hard to sequence RNA; it still is hard, compared to DNA. But that breakthrough permitted you to go and get a DNA coding sequence, instead of going to genomic clones which are harder to isolate. There are all sorts of things that came along then. The whole area busted wide open, and no one has yet written a really good history of it that makes it all clear. As soon as you could begin to copy messenger RNA and look at the cDNA, the complementary DNA, you saw that the complementary DNA was the actual coding sequence of, say, hemoglobin or immunoglobulin all the way to the end. But if you looked at the genomic sequence, it was broken up into a lot of little pieces.

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[1] Dr. Koshland's first paper on the J-chain was published in 1971. See her bibliography, #26, in the appendix to this volume.

[2] See the oral histories in progress in the Bancroft Library series with Cohen and Boyer.

That exon-intron structure turns out to be true of eukaryotic genes, not of bacterial genes, not of viral genes. That was another great discovery, and that was part of what complicated getting hold of genes for antibodies, until Susumu Tonegawa was able to show that for a heavy chain or a light chain there was a variable region gene and a constant region gene. There was a leader sequence and a variable region gene, which was most of the variable region, but not all of it. It also had a D-segment and a J-segment, and then there was the constant region. In the cDNA, the messenger RNA for V-D-J-C are all together as one continuous string. All that intervening genomic DNA may have a lot to do with regulation of expression. All that came out bing, bing between '75 and '80. And I don't know quite what Bunny was doing during those years.

Hughes: She was in mid-career when she learned recombinant DNA.

McDevitt: By '78? She was in her fifties.

Hughes: Yes, late fifties. I would like to know what motivated her. Dan was on sabbatical at Harvard. Where was Baltimore?

McDevitt: He was at MIT. I suspect that Bunny and Dan probably sat down and discussed, "We can go on sabbatical. Where do you want to go?" And they found places where they both found something interesting to do.

During that period, I was going through a lot of personal problems with a difficult divorce. I wound up with all the kids living with me. So there was a period from 1977 till well into the mid-eighties when I was kind of a single working housewife. [laughter] I didn't pay too much attention to anything. And I didn't play close attention to what Bunny was doing. But I would run into her at meetings. She and I interacted a lot when we were both on a study section [Allergy and Immunology, National Institutes of Health] from '68 to '72. I think she came on in '69, and she was on till 1972. Then we had this Bay Area Immunology Club, and we interacted a lot.

She kept asking me about the gene(s) we had discovered. They regulated the immune response, so we called them immune response genes, and the short for that was Ir genes. She kept asking what they were and how they worked, and I kept showing her these complicated experiments where we transferred cells from responders into non-responders in all sorts of combinations. Her kindest comment was that these experiments were "complex." I was just so distracted that I was doing well to keep track of what was going on here at Stanford. Then I looked up one day, and she was hard at work on the J-chain.

More on the J-Chain

Hughes: What's the significance of the J-chain?

McDevitt: Well, J-chain is a molecule that you have to have if you're going to make certain kinds of multimers. A B-cell starts out making an IgM molecule, and first it makes a molymer. In other words, it makes one subunit which has two IgM heavy chains and two light chains, either a kappa or a lambda. That's what's put on the surface. There's a membrane form and a secreted form, and that is a pentamer of five IgM monomers, linked together by J-chains.

The germ line tends to have antibody sequences that react with bacterial antigens, such as phosphorylcholine, which is on the surface of pneumococci. If a bacterium comes along and cross links surface IgM monomers, it triggers the cell to make the IgM pentamer. If at the same time a T-cell is in the area that makes a cytokine that turns the B-cell on, the B-cell not only makes more IgM, but it can switch to IgA or IgG synthesis. It takes the whole light chain and the variable region of the heavy chain and moves it from the heavy chain gene for IgM to the gamma heavy chain gene for IgG. Then the cell becomes a plasma cell and puts out large quantities of antibody.

Cells that secrete IgM, secrete a pentamer, which used to be called macroglobulin. It has five IgM antibody units. Those are held together by J-chains, which form disulfide links with a cysteine at the carboxyl terminal end of the IgM heavy chain. So you can't make IgM pentamers without a J-chain.

And in IgA-producing cells, which are mainly in the gut, skin, and mucous membranes, and which produce secretory immunoglobulin, IgA is secreted as a dimer of two IgA molecules held together by a J-chain. the J-chain is coupled to a piece called secretory piece, which is made in the intestinal epithelial cells. Secretory piece has a mechanism that leads it to be secreted into the intestinal lumen, along with attached IgA.

Hughes: Was Dr. Koshland keeping in touch with your research?

McDevitt: Well, yes, because she read all those cell transfer papers, and they were complicated, because we didn't know how the system worked. What kept driving us crazy was that you could have a gene that behaved as a single gene--and by then we had a gene product that looked like it was the Ir gene product--and it determined the immune response to many different peptides. There were actually two genes: one called I-A and one called I-E. In the mouse there were these two class II MHC molecules. These mice are inbred, so they were identical on both chromosomes. And some strains of mice only had I-A. They only had one class II MHC molecule.

There are several inbred mouse strains with different I-A alleles which responded completely differently to a variety of synthetic polypeptides and to some complex proteins. That "nonspecificity" drove the whole field crazy. How could one gene control the ability to respond or not--there were huge quantitative differences--to many different peptides and proteins? We know now it's because this MHC molecule has a peptide-binding groove--and any class II protein can combine with thousands of different peptides, but not the whole universe of peptides.

Until the MHC class II sequences came along, and until Unanue did his experiment that showed these molecules, all by themselves, could bind many different peptides, and

that what the T-cells saw was a class I or class II molecule with a bound peptide, all of the phenomenology in the immune response field was complicated, and people were always cursing and saying, "Impossible to understand." But Bunny did follow it and keep track of it. She had me come up and lecture in their course. She read and she taught and she kept track of a lot of things.

Koshland as Teacher

Hughes: What kind of a teacher was she?

McDevitt: Well, from hearing her give seminars, I would think she'd be an outstanding teacher, very demanding, very rigorous. In setting her course up--I know for a fact--the students were required to read and think, which you can do with undergraduates.

Hughes: She didn't want rote learners.

McDevitt: No. They didn't have medical students at Berkeley. They had undergraduates. And I was always impressed when I went up there to lecture that she had given them some papers of mine to read, and she had given them an introduction, so when I presented what was for that year the latest hot results, I got lots of questions and lots more feedback than you get from medical students. Medical students, because they're so overburdened, often just want to hear the basic facts. They want to have it in a syllabus and be able to go home and memorize it for the exam. That's not true of all medical students, and it's the fault of an overloaded curriculum.

Hughes: Is there anything more to say about your interaction with Dr. Koshland in the study section?

McDevitt: Oh, sure [laughs].

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McDevitt: We used to argue quite strongly, but in a friendly way. (I have found in the course of consulting for a variety of large and small companies--pharmaceutical companies and biotech companies--that they regard many academic scientists as monsters. Academic scientists would come in and listen to the data, and then question the scientists in the company very hard, and argue with each other very aggressively. Many people have a hard time understanding that scientists go at the data and argue it very strongly, but there's nothing personal in it. After that, you go out and have a drink or go to lunch.

Impressions of Koshland

Hughes: And that was your relationship with Dr. Koshland?

McDevitt: Oh, she was a very good arguer [laughs]--no question. She was very quick and very critical and very rigorous.

Hughes: Do you think she liked an argument?

McDevitt: Oh, she liked it, yes. It was fun. She liked to argue, I liked to argue, so we argued a lot. But it was always about a particular grant application--is it good, or it's not good. And, you know, when you're reading a grant, sometimes you read into it that this guy is on the right track, so let's give him money because he'll find out what he hasn't said explicitly. And then somebody else will say, "I've read through it, and he doesn't say the really important thing." Then you argue and go at it back and forth. It's not so much that you were arguing about whether the reagents were pure or not, the way a lot of us did in, say, the early sixties. But it was more, "Does this guy really see the point?"

There are some applications in which the scientist lays it out beautifully, it's perfectly clear, and you say it's a great project, and everybody agrees. And there are others where the guy is obviously floundering and nobody disagrees there. But there are lots that fall in between, and that level determines where the cutoff is of who gets funded and who doesn't. So you would argue a lot about that.

Hughes: Was there anything characteristic about the stands that Dr. Koshland took?

McDevitt: I think that she would tend to be tougher than most of us in wanting to have the guy state explicitly exactly what he was about and make clear that he understood it. It wasn't that she was nitpicking or looking for dotting the i's and crossing the t's; but she wanted to be sure that this guy understood. She was a very tough critic, tougher than most people.

Hughes: What about her role in the American Association of Immunologists? She was president in 1982 and 1983.

McDevitt: And I was president in 1980. She was on the council at the same time. She was very good.

Hughes: As president?

McDevitt: I wasn't on the council when she was. Once you're president you rotate off. But in committees, she had sharp, clear, well- thought-out criticisms, questions, that sort of thing.

Hughes: Had she prepared in advance? Had she thought about everything?

McDevitt: Oh, always. At the study section and in council meetings and things like that, yes. In those days I was still going to clinic once a week and seeing a fair number of patients. She read more broadly in basic immunology than I did. What I was always impressed by

was that she kept track of a lot of work, and it was not necessarily of her specific field. She often helped me keep track of new areas.

Hughes: You've spoken of family responsibilities, and she had five children.

McDevitt: Well, she was fortunate that she had a husband who worked and that they had enough money so they could afford helpers and babysitters. But yes, I can't imagine a woman having five kids and being able to work even half time, unless you had a pretty well-organized home, with people to clean the house and do the laundry.

Hughes: Which she did. But even so!

McDevitt: Well, I agree. It's hard to imagine.

Hughes: Amazing that she came prepared for any task.

McDevitt: She was very well read, very much up to date on everything, asked sharp questions. When she had a lot of grants to review, she had read every word very carefully. She always came well prepared. She was sharp. The main thing is she was more rigorous, more critical, and more demanding than most of her colleagues.

Hughes: Do you have any impression of her as consciously or unconsciously being a role model for women in science?

McDevitt: Well, she was certainly a role model. She couldn't avoid being a role model. Although she often had lots of women working with her in her lab as graduate students or postdocs, she was very demanding of them. She asked one student to leave whom I later took on; Bunny was absolutely right; it was a mistake.

Hughes: Some female scientists make a point of selecting female postdocs because they think they need a special environment.

McDevitt: I don't think she made a point of it, but I don't really know. I certainly met lots of male postdocs from her lab. She was not what I'd call a blazing feminist. She was more of the school, "If you're going to be a woman in science, you have to be as good as men, and you have to show it." You have to be prepared to talk and publish and stand up for your rights, just like anybody else. She wasn't the sort of person that said, "Well, she's a woman, so let's give her the money."

Koshland's Scientific Approach

Hughes: Jim Allison and Alexander Glazer wrote a memorial piece[1] which I think is an adaption of what they said at her memorial service. Did you go?

McDevitt: Yes.

Hughes: There were a couple of things that they said that I hope you will comment on. This is a quote: "If there is any single feature that marks Koshland's work, it was this ability to reduce complex phenomena to experimentally addressable components." Is that something that you noted?

McDevitt: Yes. I think she did that, for example, with the antibody composition experiment. She also did it with J-chains. What she was working on most recently was the sequences in the promoter region of the J-chain gene that were responsible for initiating synthesis of J-chain, and also looking at the sequences in the immunoglobulin genes and the J-chain genes that were responsible for turning on J-chain synthesis.

It's not my field. My memory is that she was working at some point on finding out how the timing of turn-on of J-chain occurred relative to immunoglobulin, and what the transcription factors were: the proteins that were made in the nucleus that bound to the DNA, that turned on the J-chain gene and turned on J-chain synthesis.

I've heard her talk about other people doing analysis of promoter sequences. (In a given sequence in the five-prime part of the gene, the upstream part of a gene, there are a whole series of sequences that clearly have identity as binding this particular transcription factor or that particular factor, such as, NF kappa B or jun or AF-1 or many, many other transcription factors that are responsible for initiating the synthesis of the messenger RNA. She was very rigorous about evaluating her own and other people's evidence as to what was really going on in the initiation of RNA synthesis on a given gene.

Hughes: Could she have gotten all that knowledge in her sabbatical year in Baltimore's lab?

McDevitt: No. There are always new techniques. First, you find the J-chain gene, and then you find out that other people are analyzing promoters for immunoglobulin. So then you sequence the five-prime region of the J-chain, and then you see there's there's an X box and a Y box and an interferon gamma consensus box, and so on. Then you do the experiments to prove that those transcription factors bind to those sequences. Those are all new techniques. They're not impossible to learn, but you've got to learn them.

Hughes: It's expected of any reputable immunologist to master all the new techniques?

McDevitt: No. I don't do those kinds of experiment. I had to master a different kind of experiment. But I can say with confidence that, in my own experience and in many other people's

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[1] See appendix.

experience, the methods you used, say, in 1980 changed by 1985. And they're going to continue to change. But one person doesn't do everything.

Presidency of the American Association of Immunology, 1982-1983

Hughes: Is there anything to say about the year that Dr. Koshland was president of the AAI? Eighty-two to '83, so there would have been one president between you and her, right?

McDevitt: I finished in '80, so somebody did '81, somebody did '82, and then she finished in '83. She was the sort of person who was very careful about doing all the important things. In other words, she probably would have been a much better administrator than most of us, and much more thorough in supervising the program committee and the membership committee and all that sort of thing. But I can't remember anything specific.

Hughes: Well, maybe we could end with a comment on what she was like as a personality.

McDevitt: Oh, she was terrific. Here's a person who was smart, sparkling, who when she came into a room or met a group of people--she wasn't loud or anything like that--she would immediately look around the room, get into an interesting conversation, ask sharp questions, and she was just fun to be with, fun to argue with, fun to discuss with. We would discuss somebody else's recent findings, whether we believed it or not, the sort of thing that comes out all the time. There was always some recent paper in *Science* that you thought you could see a hole in and you didn't believe it. She was just a lot of fun, had a lot of sparkle, and a lot of intellectual vigor. And it came across. I'm sure that it came across to students. I'm sure it came across to the people in her lab. It certainly came across when she gave a talk. A really remarkable woman. And to then have raised five kids? Really amazing, really amazing.

Hughes: Well, is that a good place to stop?

McDevitt: Yes.

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MARIAN E. KOSHLAND (1921-1997): RETROSPECTIVES ON A LIFE IN ACADEMIC SCIENCE, FAMILY, AND COMMUNITY ACTIVITIES

Gail Koshland Wachtel

A DAUGHTER'S REFLECTIONS ON SHARED EXPERIENCES IN SCIENCE AND MOTHERHOOD

with the additional comments of Nadine Wachtel

Interviews Conducted by
Sally Smith Hughes
in 2000

INTERVIEW WITH GAIL KOSHLAND WACHTEL

Gail Koshland Wachtel

[Date of Interview: August 5, 2000]
[Lafayette, California]

Education

Hughes: Gail, let's start with your background.

G. Wachtel: Well, I consider myself the middle child of the family because I have two older sisters [Ellen and Phyllis], a twin brother [Jim], and a younger brother [Douglas]. I was the third girl to come along, along with a boy. So that's my family. I went to high school here in California, moved to California with my family just before high school, and then went on to get a degree in physical therapy. I didn't think about becoming a professor like my parents, but I succumbed eventually and went back and got a Ph.D. in kinesiology. I now teach at the University of Arizona, in the medical school, and do research.

Parental Career Influence and Advice

Hughes: Tell me a little more about becoming a professor.

G. Wachtel: My parents were scientists and did research. As a child, I remember visiting their labs. When we moved to California, they became part of academia. I was more aware then that they were working at nights and weekends. This is a child's impression: I thought, Oh, I don't want to work that hard. I don't want to be married to my job. I'm not going to become a professor. My impression was you had to do that. My parents did not tell us that we needed to work so much; rather, we were much encouraged to choose something you really liked to do because it made you like your life better. I don't think it was just my parents; the whole generation believed that your job was your self-esteem. So therefore I gravitated back to getting another degree because academia is something I really like to do.

Hughes: Was there any family pressure to move into science or any particular field?

G. Wachtel: No, none at all. My oldest sister is a poet and organizer for social change, my next sister is a sculptress, my brother is a lawyer, and the last two of us have gone to science. But I didn't start there. I kind of moved to it slowly. I didn't feel my parents pressured. I think it was more the pressure of: Choose something you're interested in and be good at it. Both of them, but particularly my mother, were so much Protestant ethic. She was very much: Give it all you got. Work hard at whatever you're going to do, for your own satisfaction as well as your commitment.

Family Life in Bellport, Long Island

A Working Mother

Hughes: Were you old enough to remember Bellport?

G. Wachtel: Oh, yes, I lived there up until thirteen years of age.

Hughes: What was family life like?

G. Wachtel: I'm going to make an aside. As you know, I have a ten-year-old daughter, [Nadine Wachtel], and I remember thinking when I had her how much time was I going to take off work. My impressions were that my mother didn't take much time off. But then, when I talked to her, actually with each kid she probably took a year off. They moved from Boston to New York when my youngest brother was born and she was between jobs. But I don't know whether she chose to take a year off or that it just happened that way. But I laugh at my own impression that she worked to the bone and didn't take time off for kids. That was absolutely wrong.

Family Routine and Activities

G. Wachtel: I never felt that I was gypped by my mother working. In fact, I liked coming home and not having my mother there! But there were clearly things that were very important, and I'm sure all my brothers and sisters said this. Dinnertime was a really important occasion, and we all shared ideas. It was a coming to talk and to share things. It is hard to continue this tradition as a single parent of one child, but these are traditions I try to carry on.

My mother went to work later in the morning, so we got up and got ready for school, and Luna helped us get ready because my mother liked to sleep. But my mother was there. She was available.

Hughes: So breakfast wasn't a mandatory family occasion.

G. Wachtel: Oh, no, definitely not. I remember on the weekends, particularly Sundays, we were not to wake my mother up till noon. So she got to sleep in on Sundays. We went out and played, but we were to be quiet. We did a lot on the weekends because we were by the Great Sand Bay. We would do things as a family: going over to Fire Island and having family outings.

Hughes: Did the family sail?

G. Wachtel: Yes.

Hughes: Did you have your own sailboats?

G. Wachtel: Well, we had a bigger boat that we would take, and then there was a smaller Beetle Cat--that's what they called it then; I don't know what they call it now. It held two or three people. My sisters and brothers would sail, and there was a boat-racing community. You could walk most places. Our house was a meeting place for a lot of friends in the neighborhood. You could walk down to the yacht club and be a part of it.

Hughes: Was it largely an academic community?

G. Wachtel: It was a mixture. There were people from Brookhaven National Laboratory that my parents knew, but there were a lot of other people who didn't work there. One of my mother's best friends was Phyllis Streit, who was an artist who lived in Bellport. My father is much better at telling the history, how the whole group got together. My mother really liked that they were different, not just scientists. They had different interests.

Mother's Interest in Gardening and Cooking

Hughes: My impression is that your mother did not live a narrow academic life.

G. Wachtel: Well, she loved gardening. She loved cooking. We argued about whether health food could be gourmet or not. She said it couldn't be, but I said, "Oh, yes, it could be." But anyway, she loved gourmet. She never ate at a McDonald's or a Jack in the Box in her whole life--never. So she had these interests besides science.

Mother's Work with the League of Women's Voters

Hughes: She was politically active in Bellport, I understand.

G. Wachtel: I remember her being part of the League of Women Voters.

Hughes: Dan [Daniel E. Koshland, Jr.] said that she worked on zoning.

G. Wachtel: Oh, yes. I was young, so I have only a vague impression. One of the impressions now, which I wasn't aware of as a kid, is that my mother had a lot of energy and didn't need a lot of sleep. It's amazing all that she did. I'd say, "Mom, how did you manage with five kids?" And she'd say, "I don't know. I just did it."

Hughes: She had five children within seven years, or something like that?

G. Wachtel: Well, there are two years between Ellen and Phlyp, now called Phyllis. And then twins. She did say that when she had twins, with two other children, aged two and four, that was really a stretching point.

D. Koshland: [Enters livingroom] Hi. [tape interruption]

Hughes: Please tell us about your wife's work on zoning with the League of Women Voters in Bellport.

D. Koshland: She was a pretty good politician, and also she had a very good plan. She really thought it out very well, and so when she presented it to the supervisors, they really listened. Bellport had no industrial area. Remember, this was a long time ago. This was before everybody discovered it was a good idea to have an industrial area. Long Island was really a bedroom community, which was what the community wanted. They didn't want any industry. The average person moved out there to get away from New York City.

What they didn't realize was that if you don't have any industry to assess for taxes, the school system has real problems because then you could collect only household taxes. So Bunny had to convince them that it was really worth having industrial parks and locating vacant land where you could get industry, and then even giving industry tax benefits to get them to move out there. Her plan made so much sense, the supervisors adopted it.

Hughes: Where did she get that idea?

D. Koshland: Well, it really wasn't so original; other people had done it. But you have to put it together, and then you have to do your homework and locate some marshland or other place that people don't want and then find out if industry would really

move there. And then you had to have transportation. You either had to be near a railroad or near the ocean, near the bay.

Hughes: What was her motive? To keep the residential areas residential but provide a tax base?

D. Koshland: Have a tax base, yes. You pick out the land that really wouldn't be very good for homes or hadn't had homes yet and just set that aside for industry. As a result, you don't have industry intermingled with housing. People really liked that.

G. Wachtel: The child's impressions: I knew she was part of League of Women Voters, and I was very proud, but I didn't know anything about her activities.

Hughes: No, of course you wouldn't. How did she fit that in? Would the League of Women Voters be an evening commitment? Did she work at Brookhaven nine to five?

G. Wachtel: She was part time. That's what I didn't realize. She worked ten to five. She was very regular; she always came home at five in Bellport. Didn't work weekends. Occasionally I guess she'd pull out papers and a slide rule or whatever.

Hughes: Her weekend was family time?

G. Wachtel: Yes.

Homework

Hughes: Do you remember how the evenings went? You had dinner as a family and then what happened?

G. Wachtel: I don't remember her literally saying, "Time to do homework." There was a clear rule: You did your homework. She always said you bring up the first child, and then that child helps to bring up the rest of the children. So doing homework was a clear rule that was probably enforced more by my older sisters. We clearly always did our homework.

For instance, in fourth grade, I had to write a story, and I couldn't figure out what I was going to do. You could easily go to my mother and ask for help. She said to me, "Well, what's something you really like?" I said, "Well, my dolls." "What doll do you particularly like?" "Raggedy Ann." And so then she said, "Well, what kinds of things would you really like to say about her?" I

thought for a while. And somehow she helped me identify that I really loved the idea that she had this little candy heart inside of her. She really helped me write the story, which my teacher always loved.

That was pretty typical of my mother being good at drawing you out and helping you be practical and put something together. So I think the general routine was not so much this is when you sit down to do homework, and you sit down with your mother. No. You get your homework done, and if you have a problem, you come to her.

Dinnertime

Hughes: What was her pattern after she finished dinner?

G. Wachtel: I don't really remember. The pattern [for the children] was, you come home and help to fix dinner. Different kids helped, and so certain kids set the table or cleared off during dinner, and other kids washed the dishes afterwards.

Hughes: Did you have a schedule?

G. Wachtel: Yes. The younger kids did the clearing off, and as kids got older, they moved into the cleaning stage. But it was also a time to talk a little more informally. Not everybody there at the dinner table.

Hughes: I've heard of your siblings saying that you didn't just gobble your meal and leave.

G. Wachtel: The Koshlands eat very fast. Then they talk a lot--conversation around the table.

Typically, my mother would come home, and she'd garden a little and then cook. It was hard when my brothers were wrestling because they were hungry; they couldn't eat before dinner. Dinner was usually at eight o'clock. So by the time you ate and cooked and cleaned up, it was nine, ten. You might sit around and watch the news. That's when I remember my parents would do some work.

Domestic Help

Luna Carroll

Hughes: Several people have mentioned Luna, but nobody has said much about her, and so would you, please?

G. Wachtel: Sure. Luna came to work for us in Bellport when I was three months old. Actually, her daughter was supposed to come and didn't show up, and my mother wasn't very happy about that. She had four small kids and needed help, and so Luna went to work. As the family history goes, she didn't know how to make a hospital corner on bedsheets, and my mother taught her. Luna worked for my family until we moved from New York. We wanted her to come with us to California, but that didn't work out.

Luna and my mother were very different. Luna was this very warm, huggable person, and my mother was more reserved. If there was jealousy, I never saw it. I remember my mother talking about how Luna would deal with things. If we misbehaved during the day, she didn't want to put Luna in the position of a tattle-tale; on the other hand, Luna had to let my mother know. So the rule that they worked out was that Luna wouldn't tell anything, but if my mother asked a question, Luna would answer. She wasn't a tattle-tale, but she could answer honestly.

Hughes: Was that a restraint on you children?

G. Wachtel: Oh, I didn't know that rule till much later. But I think we were aware that our every little misdeed wasn't told. I think my mother did encourage Luna to hand out discipline during the day, not wait till she came home from work. Luna was such a softie that she didn't do much. Anyway, her role was to clean but also very much to take care of us. I think they did a lot of shared parenting.

Hughes: Did they talk about parenting?

G. Wachtel: I think about some things. How deeply, I don't know. For instance, my sister was scatterbrained and forgetful, and we all would come to her rescue, including Luna. If she'd forget something, we'd take it to school for her. I know my mother really wanted my sister to face some of the consequences and not be rescued all the time, so I know things like that were discussed.

Hughes: Was there much change in pattern when you moved here? Your mother worked part-time when you first moved to Berkeley?

G. Wachtel: I don't know. I know she had to work her way up through the university. It was a big change in commitment. Teaching and writing lectures and doing all that was a big difference from her job at Brookhaven. My sisters and my brother and I were in high school, and Douglas, the youngest, was in middle school. I think my mother was pretty conscious of the fact that we were old enough, that she could make that change. But it's always a shock how much a change it is, like getting new people in the household to help out and establishing that relationship. We actually went through several people until we found Willie May Barret.

Hughes: Neither Luna nor Willie May actually lived in your home?

G. Wachtel: Yes.

Hughes: Why was that?

G. Wachtel: I don't know exactly. I'm sure my mother got very close to Luna and Willie May in sharing day-to-day things about the household, and yet you're an employer with an employee. So there's this fine balance between they're really a part of the family and yet they're not, and how you keep that clear and okay with everybody sometimes takes a little bit of work and good finessing. So not having somebody live in helps to keep some separation. Luna had her own family, so in a sense they weren't available for moving in anyway.

California: Social Patterns Change

Hughes: Dan said that they made a conscious decision when they moved to Berkeley that they would become more involved in the academic community rather than in the larger society.[1]

G. Wachtel: Yes, I think that's one of the big changes. There was this group of adults with kids in Bellport that my parents really had a connection with, and I think my mother had some real women friends. When they came here, it was through the university that they made friends, and that's hard. I didn't know a lot about this part of my mother's life, but I think she had some very important women friends and colleagues, but still they were university people. They weren't here in Lafayette. I don't think there was any friend or couples here in Lafayette. So that was a very different society from the one in Bellport.

Hughes: So their social life centered on the university?

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[1] See the oral history in progress with Daniel E. Koshland, Jr.

G. Wachtel: Right.

Hughes: Did she keep in touch with the women friends that she had known in Bellport?

G. Wachtel: Yes, although my mother was a terrible letter writer. She never wrote letters. Occasionally, very occasionally. bShe was not a big small talker. She would talk on the phone, but she didn't gab, gab, gab.

Hughes: She would call for a purpose?

G. Wachtel: Right.

Marian Koshland

Physical Appearance and Attire

Hughes: Please describe her to me, first physically and then as a personality.

G. Wachtel: Nadine, would you like to answer? [tape interruption]

Hughes: What did your grandmother look like?

N. Wachtel: Well, she was very fair-skinned. She had blond hair and little eyes, not dark but pale-ish. And that's pretty much it.

G. Wachtel: Medium height, slight. She cared that she always was slim. She always worked at that. I remember her square jaw, maybe because of jealousy because I inherited my father's no jaw. She had this strong, square jaw, which sort of symbolized her strength and set-ness. You could tell when my mother came alive because her eyes just sparkled.

N. Wachtel: Her eyes, when they looked at you, you could know that she was excited, even without a smile. She had a lot of intense energy. It would be focused on you. Sometimes you wanted it, and sometimes you didn't.

Hughes: How did she dress?

N. Wachtel: She usually wore jeans. She liked sandals, not high- heeled but just regular. She usually wore a cotton T-shirt. She always wore a necklace or a watch.

Hughes: Earrings?

N. Wachtel: Sometimes.

G. Wachtel: You knew her mainly on the weekends. That was her at-home, gardening, around-the-house attire. But then the other side was suits. She wore dresses, but my impression is suits, tailored suits.

Hughes: Would she go to the lab in a suit?

G. Wachtel: She got a little more relaxed and wore a dress or skirt and shirt.

Hughes: Appearance was important to her?

G. Wachtel: Right.

Hughes: How did she reconcile loving to cook, without, I gather, much emphasis on healthfulness, and keeping a slim figure?

G. Wachtel: [pause] See, I think she would say she ate healthfully. She probably used a little less fat, as the French diet is pretty fatty. She ate health consciously--not lots of fancy sauces and not French fries. She watched how much she ate.

Hughes: Was dinner always special?

G. Wachtel: Yes, and on your birthday you got your favorite meal, whatever you requested.

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N. Wachtel: She made chicken taste better than it usually did. It was really, really good.

Hughes: Did you have a favorite dish?

N. Wachtel: The fish was the best. She made it nice and delicate but not so sloppy that it would fall apart.

Personality

G. Wachtel: The way I usually describe my mother is a female Humphrey Bogart, with a tough exterior but an inside vulnerable softness.

Hughes: Why the tough exterior?

G. Wachtel: I remember being struck that both my parents were very competitive people. I knew they wanted to be the best of the best. They were up there, the top 1

percent, and wanted to be the best of the best. My mother talked about this, that certain things happened early in childhood, such as her brother being sick, she skipped several grades, and not feeling she was very pretty. She developed a toughness to deal with problems like this. In that sense, she was very practical: what do you need to do to get where you want to go.

Upbringing

Hughes: How did her family and upbringing shape who she became?

G. Wachtel: She wouldn't have said this, but I put this together. I remember discussing what makes somebody a genius, or why some people get further ahead in terms of career or whatever, and that it wasn't necessarily smarts, who got the highest IQ test score, whatever. There was one argument about obstacles that make you want to jump to get through those obstacles, and the tougher obstacle makes you work harder so that it shows what inner resources people have.

I think in some sense that was true of my mother. She talked about her father as a conservative, Southern, short man. She was stubborn herself and rebelled against him. She had a go-get-'em personality, but he set up a negative attitude, and so that just made her go even more. They didn't have money; she had to go to college on a scholarship. He didn't support her, and she was on her own.

Her father was conservative. I know he was a man of his time, when women didn't go to college. But he pushed her to meet high standards and challenged her intellectually. I gather that her mother was sweet, a warmer personality, but fairly reserved. That was the Scandinavian temperament. So that's maybe the Humphrey Bogart aspect of my mother.

Religion

Hughes: Did religion play any role in your family?

G. Wachtel: Well, my mother was brought up a Protestant. She knew the Bible better than my father. But I didn't hear much from them in terms of spirituality--live your life according to God's rules, that kind of stuff. I don't know how much that was because my parents came from different religions. I know they argued during the engagement years about how that was going to work out. But they were also scientists. I don't know where they fit religion in. But my mother

was definitely Protestant ethic; she was definitely New England, brought up with the idea that you work hard, and that's part of the purpose of life. And high morals. She was very high in terms of honesty, in terms of principles.

Hughes: And her values were drummed into the children?

G. Wachtel: Yes, yes, yes.

Hughes: What about Jewish traditions?

G. Wachtel: What I tell people is I wasn't brought up in any religion; I was brought up in Jewish family tradition. I once as a kid said to my parents, "It's not fair. I have two Jewish mothers as parents." My father had the worrywart attributes; my mother had the Jewish mother attributes, you know, "I want you to achieve very high standards."

I remember at thirteen in Bellport I had sort of a religious crisis, where one day I woke up and I really had to know is there a god or not. I remember going and talking to my father and getting very teary-eyed. He said, "Well, you can read the Bible. We can talk to you about it." Somehow, miraculously, some time later, my two brothers and I started going to Unitarian Sunday school. Now, I look back on it and I think, I know they orchestrated that. It probably came out of trying to address that question of mine.

Adjusting to the Koshland Family Style

Hughes: The impression I get of the Koshland clan is that it is anything but reserved and introspective. Your mother presumably was the one who had to make the adjustment, particularly when she moved to California.

G. Wachtel: Yes. I remember hearing about when she first came to meet my father's family. In those days, you took a train across the country, and then you had to take a ferry across San Francisco Bay. The way it turned out, my parents had to do this at seven in the morning. My mother was not at her best in the morning. Literally, physically, her eyes would be puffy. She was one of those people who's slow to wake up, a grumbly bear in the morning.

She had to get up and be ready and dressed by seven. Then as the ferry approached the San Francisco dock, she saw all these people, and she wondered who all these people were out at seven in the morning. Well, they're the whole Koshland family there to greet her. [laughter] Oh, my God, that would be hard! Of course, my mother was the type, you give her a challenge like that, and that made her live up to the challenge.

My parents did have very different backgrounds. I think my parents also had some very strong common ground that they shared, so those differences were strengths rather than difficulties. But my mother did have to adjust--the kind of crazy big family stuff versus her quieter, more reserved family. I think partly why they lived first on the East Coast was to have some distance [from my father's family]. My mother wasn't somebody who could be told how to live her life in terms of: You should go to a social occasion because of guilt; you should do this with Aunt Toosey and this with Uncle Watsie, just because they're aunts and uncles. She wouldn't go along with that. So I think that period on the East Coast gave them some space to develop and still be connected to the family. So moving back here was easier.

Hughes: Because they had a family style, a pattern already.

G. Wachtel: Yes.

Hughes: Did she like the exuberance of the Koshland family?

G. Wachtel: Yes, I think so. That's why the Humphrey Bogart analogy works for me. It isn't that she didn't like that; it was hard for her to bring it out. And the Koshland family helped her bring it out. She adapted to the family style. She enjoyed the arts and doing artistic things, which she didn't have the opportunity to do before, so it was easy for her to move into a family that did that.

Nadine's Impressions of Her Grandmother

Hughes: Do you have any stories about your grandmother, Nadine? Do you think of any particular time you spent with her?

N. Wachtel: I always remember when we went hiking, she could remember her Girl Scout song, and I was just really amazed. And so it was really a nice day for that.

Hughes: Did she sing it for you?

N. Wachtel: Yes. And she's not a big singer. On that trust me. She was not a big singer.

Hughes: You had a pretty special relationship with your grandmother, don't you think?

N. Wachtel: Yes. I think it was a nice relationship. She was caring. Sometimes you know how you get yelled at a little bit, and you get a little bit upset? But she wouldn't exactly yell. So it was very nice. I remember one time it was Christmas time, and I was under the table, where you usually put your presents that you weren't wrapping yet, and I found a present that was for me. I was just looking at it,

and she said, "Oh, you can't open that." She didn't really yell at me. It was a really special relationship. I really loved her.

Hughes: Do you usually come here for holidays?

N. Wachtel: Yes. Unless I'm at my Daddy's.

G. Wachtel: I remember watching her with the grandchildren, which may go back to how she was with us as children. She was a wonderful teacher without being obvious that she was teaching. So she'd be out gardening, and Nadine would go out there to talk with her, and she'd say, "Well, how 'bout you pick up that trowel and dig that plant out? That plant is going to be moved here, so that means you have to catch all the roots. So how big do you think you have to dig the hole?" She'd be teaching, and you would realize you'd be doing it together. That's something she was good at, and she liked the children to participate.

Hughes: Was it typical that she would be doing something and the children would be brought in, rather than designing an activity that was just for the children?

G. Wachtel: I'm sure she organized things. We all had birthday parties. That's five kids in a year. That's a lot of birthday parties. And I think, How could she do all that? That's again where I don't know how she did it. But she did. So there were organized activities.

Christmas with the Family

G. Wachtel: Christmas was very important. I said this at her memorial. She didn't say, "I love you," but if she got a present for you, she would go all out. She would try and figure out what you wanted, and you hadn't said you wanted it. It didn't stop there. When she figured out what you wanted, she had to give the best. So it didn't matter if she went to ten stores. She wasn't going to ten stores to get the cheapest, the best buy, it was to get the perfect thing for you. That was how she showed that she loved us.

Hughes: Did she usually figure it out right?

G. Wachtel: Many, many times.

N. Wachtel: Yes.

G. Wachtel: Christmas was very important as an occasion to share in the family. Giving money or a gift certificate was an absolutely bad present. You never gave a present like that. But there were all kinds of little traditions that became family

group activity. I don't know how this developed--my father might know how it started--but we wrapped presents with a clue to what was in the present.

N. Wachtel: You would put a clue on the wrapping. For instance, if you got jeans, you made, drew, or cut out of ribbon the little jean design on the pocket on the back of your butt.

G. Wachtel: As we kids got older, this was a major production, and more presents, so the back room would be covered with wrapping paper which we had to clean up. We'd be up till two or four Christmas Eve night to wrap these presents. It was kind of silly, how much production it was, but it was part of showing your love.

Hughes: What do you want to add about Christmas?

N. Wachtel: One Christmas we had bought a big piece of felt, and she had never used it, and so she said, "Hey, how 'bout we make a little thing to go around the Christmas tree?" So me and her made a little Christmas tree skirt--it would bring out the spirit sort of. And so we put little reindeer, and now it's our Christmas tree skirt at my house.

G. Wachtel: It covers the bottom of the Christmas tree. It was a project.

N. Wachtel: It was a project that we did together. It took a very long time, though, because we had to put sequins around the edges.

G. Wachtel: I remember it being pretty funny. Typical of my mother, she had very strong ideas. It had mixtures of colors, like blues and purples, and my mother said: "Christmas is red and green. Don't put that blue sequin there." I said, "Well, Mom, you said Nadine could do what she wanted to do, that she could help."

N. Wachtel: So she gave me my own individual Christmas tree and said I could decorate it. She started decorating it, so it was sort of "our" Christmas tree. We still have the Christmas tree. It's in the back room.

Parenting Style and Family Code Signals

Hughes: Talk about parenting style and how your parents--

G. Wachtel: Did things?

Hughes: --did things.

G. Wachtel: Well, you couldn't divide and conquer. That's for sure! I'm sure all kids try, but my parents pretty much agreed on how to handle us.

You asked me earlier, was there pressure to be scientists? No. But both my mother and father were so much scientists, that's the way they approached the world. So in parenting style they were analytical. Things got discussed and analyzed and figured out and torn apart and put back together. That went on behind the scenes, as well as in front of us.

Hughes: But they maintained a united front.

G. Wachtel: Yes, they pretty much agreed on what was important, what wasn't, and that was pretty well conveyed to us.

Hughes: What about discipline?

G. Wachtel: Make the punishment fit the crime. I remember hating that as a kid. You didn't get spanked; I don't remember even being yelled at much. But you got a consequence that had to do with your infraction. If you rode your bike unsafely, you got your bike taken away for a while. We respected our parents because there were rules and they were strictly held to. There was a real sense of the rules. I would [later] say to my mother: "Now, how did you put those rules in place?"

Hughes: What was her answer?

G. Wachtel: She said, "I don't know. You bring up the first child, and then they bring up the rest." But she really didn't have an idea on how she did it.

Other things I remember: code signals. There was a family hold back code, "FHB." This was at parties when my mother wasn't sure if she had enough food. Since there were a lot of us, the code meant don't go hog wild on the food. You had to take one small helping and then see how much extra food there was.

My brothers and I had two codes: "Saudi Arabia is blowing up," or "Bombs over Tokyo." And that meant my mother was on the rampage. Something had pushed her over the edge. Once she started yelling at one kid, she would go down the line. So that meant scram! Go hide! Run away so you wouldn't get it. Now as an adult I look back and think, there must have been days she was under so much pressure.

My mother felt very strongly, you don't have favorites. I don't think either of my parents had favorites. They encouraged the individual in each of us.

D. Koshland: [enters livingroom, joking as usual] This doesn't sound to me like a serious discussion. I want to tell you, Gail's only partly reliable, and Nadine is totally untrustworthy, so don't believe a thing either of them says. [exits]

Hughes: [laughing] Well, that comment is now on tape.

G. Wachtel: My mother loved little babies. Then, when the kids started wanting their own way at age two, she might not agree with how we were disciplining [her grandchildren].

Hughes: But nonetheless, everybody came?

G. Wachtel: Yes. She did really enjoy kids.

A Woman in Science

Not an Overt Feminist

Hughes: Do you have any observations about your mother as a woman in science?

G. Wachtel: I remember her talking about being the token female on a committee or advisory board or whatever. That was her typical way to downplay her role.

N. Wachtel: Can I tell about women in science?

G. Wachtel: Go ahead.

N. Wachtel: When I would go to her lab, I would think, Oh, that's just neat stuff. I would be amazed at her lab because it would have very many stuff [scientific apparatus], like shakers. There would be a very cold room and medium-temperature rooms, and I thought that she was pretty amazing because she had a lot of stuff. And she reared a family even doing science.

Hughes: She had several people in her lab. Did you meet them?

N. Wachtel: I met them.

G. Wachtel: My mother always had a small lab and was always a very good mentor. She was much more hands on than my dad. She was always part of doing the experimentation. She liked to tease and poke at ideas, so I think there was a lot of interaction with different people in her lab.

Her attitude wasn't: "I'm a female scientist." Rather, "I'm a scientist." When I was in graduate school, I had a female mentor. There happened to be five, six other female graduate students, and we'd talk. I realized the other female graduate students had a little bit chip-on-the-shoulder, fighting that they were females in a male world. Coming from my mother, I just walked in, expecting equal treatment.

For my mother, it wasn't female-male; it was, "I want to be the best scientist." On the other hand, she'd certainly say that things were tough as a woman and people didn't listen to her. But, being a practical person, she'd say, Okay, they don't listen; so how am I going to get them to listen? People would invite her to speak about women in science, and she'd often say, "Well, I don't have anything to say. I just did it." So she didn't see herself as a women's libber in that sense.

N. Wachtel: I was just thinking of her as having neat stuff [in her lab], and now I'm thinking of her as a very powerful and amazing woman who did a lot of stuff that other women just think, Oh, I won't do that. She would go and do it, even though she wouldn't want to.

G. Wachtel: She was amazing, her willpower and discipline. It's all part of that Protestant ethic, and that carried forth in science as well at home.

Balancing Professional and Family Responsibilities

Hughes: What gave when there was a family problem? I can imagine with a family of seven, there was probably always something happening.

G. Wachtel: I know my brother had a slight medical problem, and she came immediately home from work. That's not surprising. Life was somewhat compartmentalized. Home life was home life, and work was work, and you didn't mix the two. We could call her at work, but you didn't do that too often.

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G. Wachtel: As I said earlier, she had amazing energy.

Hughes: Did that continue up until the time that she got really sick?

G. Wachtel: Oh, yes.

N. Wachtel: When she was at the hospital very sick, she took walks with her walker around the hospital. Sometimes she would look at football on television. We have a

tradition at Thanksgiving: we always play football. Sometimes she would come play for maybe five minutes.

G. Wachtel: She certainly was a sport advocate. In the household there was always lots of sports on TVs, and she'd be a part of the discussion with my brothers and father and whoever else was around.

Hughes: Did that have any influence on what you chose to do professionally?

G. Wachtel: That didn't per se, but I like sports. We all like sports. Again, it's the fun of competition. Something that came to her from her father was that if you like guys and you want to talk to them, you've got to have things to talk about, and one of them is sports. He'd take her to Dodger baseball games when they were the Brooklyn Dodgers. For a while, the women in my family were the Dodger fans and the boys were fans of the San Francisco Giants. As I got older, I used to trade back and forth.

My mother wouldn't have admitted this, and I'm not sure my sisters and brothers would all agree with me, I think she always did have a lot of conflict. There were times when she said to my father, "Oh, I'm not doing a good job parenting; I'm not doing a good job in science. I should quit and just move on." And he'd say, "Reexamine that."

D. Koshland: [re-entering livingroom, eating ice cream bar] I want you to see that I share everything with my children. Would you like to share this ice cream with me?

N. Wachtel: I don't trust you.

D. Koshland: You don't want it?

N. Wachtel: I don't trust you.

D. Koshland: See? When I suggest that she share dessert with me, does she share it? No.

Hughes: This younger generation--what are we going to do?

G. Wachtel: Go get your own ice cream bar if you want one.

N. Wachtel: Grandpa?

D. Koshland: I find they taste much better if you're standing right on the edge of the pool. That makes them taste really good. [He and Nadine exit.]

G. Wachtel: One of my mother's vulnerable spots was a suggestion that her parenting had some weak spots, and I think it was partly due to feeling, did she do enough? Which I think is one of the dilemmas of a working mother. You love both,

family and work. Everything she did, she thought she should do the best she could.

Sense of Humor

Hughes: Please say something about her sense of humor.

G. Wachtel: She was intense. However, she loved humor. She was not really a joke teller, but she could be the straight person to help my father be funnier. She clearly loved doing that. You have to be humorous yourself to do that. She wouldn't be silly, but I remember once, in the days of the twist, she got up and said, "Oh, watch me do the twist." And she did this very tight little twist. I don't know if she intended to be funny, but it was very funny. [pauses to think of other examples] The main thing I remember is that she would play off my father a lot. Her other form of humor was verbal banter.

Hughes: Do you want to say anything about your parent's relationship?

G. Wachtel: That's another book! Obviously, they had a very strong relationship and a very deep love for each other. My mother didn't lose that even with five kids. Now there were years when they had major disagreements. During the Vietnam War we were all of college age. She was very intense, and there were big political arguments with my father. So things weren't easy. They were pretty private, so it's hard to know intimate things. [tape interruption]

Hughes: Do you have anything more to add?

G. Wachtel: My mother was a doctor of science and also a parent, and she made some very clear choices. She said, "Well, I can't put in enough hours to be competitive in science and also be a parent. But what I can do is be very creative and really try new things." That was a way to do things in science without having to do ten million talks around the country. She encouraged us kids to be creative, and I think each of us are relatively creative.

Hughes: Thank you.

Eight interviews in volume transcribed by: Amelia Archer, Mim Eisenberg, and Gary Varney
Final Typed by Kathy Zvanovec-Higbee

TAPE GUIDE--Marian Koshland

* this interview session is extracted from a longer series of interviews with Daniel E. Koshland.

APPENDICES

MARIAN ELLIOTT KOSHLAND

PERSONAL DATA

Birthplace: New Haven, Connecticut Birthdate: October 25, 1921

Married: Husband - Daniel E. Koshland, Jr.
Professor of Biochemistry
University of California, Berkeley

Children - 5

EDUCATION

B.A.	1942	Vassar College, Poughkeepsie, New York	Bacteriology
M.S.	1943	University of Chicago, Chicago, Illinois	Bacteriology
Ph.D.	1949	University of Chicago, Chicago, Illinois	Immunology
	1949 -1951	Postdoctorate Fellow, Department of Bacteriology Harvard Medical School, Boston, Massachusetts	

POSITIONS

University of California, Berkeley, California

1989- Professor, Division of Immunology, Department of Molecular and Cell Biology
1982-1989 Chairman, Department of Microbiology and Immunology
1970-1989 Professor, Department of Microbiology and Immunology
1966-1970 Lecturer. Department of Molecular Biology
1965-1969 Asociate Research Immunologist, Virus Laboratory

Brookhaven National Laboratory

1963-1965 Bacteriologist, Biology Department
1952-1962 Associate Bacteriologist, Biology Department

Manhattan District Atomic Bomb Project

1945-1946 Research Assistant, Oak Ridge, Tenn.

GOVERNMENT SERVICE

1991-1994 National Advisory Allergy and Infectious Diseases Council
1976-1982 National Science Board, National Science Foundation
Budget and Executive Committees, 1977-1981
1975-1976 Interdisciplinary Cluster on Immunology and Microbiology
President's Biomedical Research Panel
1972-1975 Advisory Committee to the Director
National Institutes of Health
1969-1972 Allergy and Immunology Study Section

SERVICE TO PROFESSIONAL SOCIETIES

American Association of Immunologists
- 1982-1983 President
- 1977-1983 Council
- 1972- At various times, membership on Nominating, Membership, and Award Committees

Midwinter Conference of Immunologists
- 1992-1994 Council

National Academy of Sciences
- 1994- Committee on Science, Engineering and Public Policy
- 1989-1995 Commission on Life Sciences
- 1985-1988 Council
- 1982-1983 Committee on Election Procedures

American Academy of Arts and Sciences
- 1994- Executive Council

American Cancer Society, California Division
- 1985-1988 Fellowship Screening Committee

Jane Coffin Childs Memorial Fund for Medical Research
- 1976-1983 Postdoctorate Fellowships Screening Committee

SERVICE TO UNIVERSITIES

Haverford College
- 1982-1994 Board of Trustees, Educational Affairs Committee

University of California, Berkeley
- 1994- Head, Graduate Affairs Office, Department of Molecular and Cell Biology (MCB)
- 1991-1994 Graduate Admissions Committee, MCB
- 1990- Lawrence Hall of Science, Advisory Committee
- 1989- Various ad hoc and search ommittees

EDITORIAL SERVICE

Annual Review of Cell Biology
- 1987-1992 Editorial Board

Journal of Immunology
- 1973-1978 Editorial Board

Biochemistry
- 1973-1978 Associate Editor

Immunochemisltry
- 1964-1980 Associate Editorial Board, Regional Editor

HONORS AND AWARDS

Junior Phi Beta Kappa
Vassar Fellow
Excellence in Science Award, Federation of American Societies for Experimental Biology
National Institutes of Health Merit Award
Honorary Degree, Haverford College
Distinguished Service Award, University of Chicago

Election to the National Academy of Sciences
Election to the American Academy of Arts and Sciences

The Katherine D. McCormick Lecture, Stanford University
Eighth Mildred Trotter Lecturer, Washington University School of Medicine
Distinguished Scientist Lecture Series, Bard College
Dan H. Campbell Memorial Lecture, Midwinter Conference of Immunologists
R.E. Dyer Lecture, National Institutes of Health
Bertram Marx Lecture, University of Alabama

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3. Burrows, W., Elliott, M. E., and Havens, I. (1947) Studies on immunity to Asiatic cholera. IV. The excretion of coproantibody in experimental enteric cholera in the guinea pig. J. Infect. Diseases **81**, 261-281.

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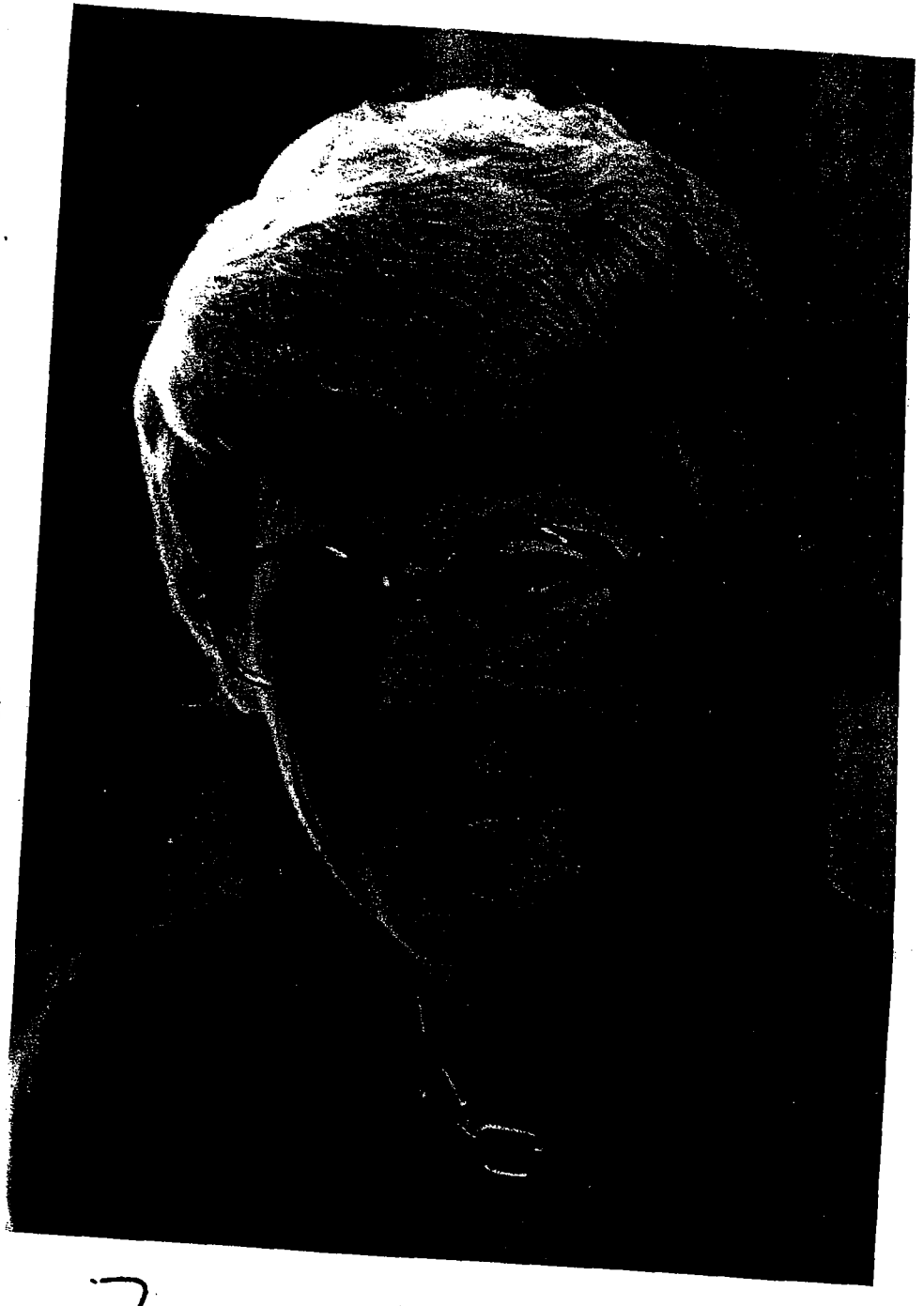

Marian E. Koshland

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SHEER LUCK MADE ME AN IMMUNOLOGIST

Marian Elliott Koshland

Division of Immunology, Department of Molecular and Cell Biology, University of California, Berkeley, California 94720

A Head Start

The luck began when I was four years old and my younger brother contracted typhoid fever from a carrier in the local dairy. It was not a lucky event for him, of course; he became desperately ill and was not expected to live, so my parents spent months in the hospital by his side.

It turned out to be lucky for me, however, because I was farmed out to the next-door neighbors who had two preteen daughters. For some reason, the girls decided to undertake my education. They knew nothing of the techniques, neither the do's nor the don'ts; they just taught me to read simple words and sentences and to count. Proud of their handiwork, they took me to school and had me show off my accomplishments. This was very heady stuff for a four year old who was feeling, rightly or not, somewhat left out by her parents.

My brother finally recovered from typhoid fever, but his immune system was so depleted that he promptly succumbed to every known childhood disease. As a consequence, I was quarantined at home for a year, and my martinet father took over my education. When I finally was allowed to go to school, it was clear that the tutoring by the girls and my father had put me ahead of my contemporaries, and this assurance served to support my conviction that "studies" was the one thing I was good at.

Upward Bound

My second piece of good luck was provided by three Jewish boys who were my main friends during high school, particularly during the last two years. I emphasize the word "friends" because in those days religious differences meant that I could never for a moment be considered a "girl friend." We were simply a gang who did things together, and since they came from a more cultured

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and ambitious tradition than mine, I was challenged to go along and keep up with them both outside and inside school. Outside of school during the 1930s depression, we went to WPA theater productions such as *One Third of a Nation* and *Murder in the Cathedral*; we sat at the top of the Metropolitan Opera House to hear Kirsten Flagstad and Lauritz Melchior sing *Tristan and Isolde*; we visited the downtown fish market at dawn and browsed through Chinatown. Inside school, when my friends chose to take a physics course, I went along and, much as I disliked it, learned to wire a light box and measure gas pressure. When they got A's in American history, I had to do as well. And when they picked up a three-foot constrictor black snake in biology class, I followed suit. And then I had to eat the canned rattlesnake meat that was the prize for the only girl who dared to touch the reptile. Thus, I was not only intellectually stimulated during my teen years, but I also escaped the usual pressures to be a "girlie."

Start of a Research Career

I was a sophomore chemistry major at Vassar, bored with memorizing organic reactions, when I was saved by a third stroke of luck. The College hired a PhD fresh from Yale to teach bacteriology and, on the side, a little immunology. The young professor, Catherine Dean, was such a good and enthusiastic teacher that within her first year she collected twelve majors from my class alone—a number that surpassed the usual total of science majors at the College. It was her introduction to the new science of immunology that provided me with a research career for a lifetime. Only a year before, Tiselius and Kabat had finally identified serum antibodies as members of the γ-globulin fraction, and one of the critical questions that arose from these studies was whether the various serum antibody activities—agglutination, precipitation, neutralization, complement-fixation—were carried out by different immunoglobulin molecules or were different manifestations of the same molecule, the so-called Unitarian Hypothesis.

I was much more intrigued by the ability of immunoglobulin molecules to recognize and eliminate an almost infinite number of foreign invaders. I read Karl Landsteiner's *The Specificity of Serological Reactions,* (Charles C. Thomas 1936), which had been published just four years before, and I was determined to try my hand at resolving the basis for the diversity and specificity of antibody reactions. How fortunate I was at the age of nineteen to be able to select a research area that, despite a few dips here and there, has grown and developed over the years and is even more fascinating and important today than it was when I began.

Graduate Reinforcement

My choice of graduate school was based on hard cash: Having spent my life in the East, I wanted to go as far West as my limited amount of money would

allow. The University of Chicago won out as the best institution with the lowest traveling costs, $16 for an overnight coach ticket from New York. Little did I realize that the University had become a hub of wartime research, not only for the development of the atom bomb and various explosives, but also for the control of infectious diseases among the troops. But being broke and having to work my way through graduate school turned out to be the source of great luck because it gave me the opportunity to work on two such projects. Each experience emphasized the need to understand the fundamental mechanisms of immunity, and both reinforced my desire to participate in such research.

The aim of the first project was to develop a vaccine that protected troops in the Far East against cholera. Since the infection is confined to the lumen of the gut, parenterally administered vaccines were of little efficacy. Our studies used an experimental disease in guinea pigs to show that oral administration of a nonlethal dose of vibrios prevented the disease, and the protection was associated with the secretion into the gut of antibody, politely called copro- rather than fecal antibody. These findings provided an important precedent for the subsequent development of the oral polio vaccine and left me with a lifelong interest in antibody secretion into the body cavities.

In the second project, the Commission on Airborne Diseases was charged with reducing the incidence of respiratory disease during Army basic training. Streptococcal infections were particularly prevalent among recruits from rural areas; they had little previous exposure to the causative organism and often developed serious autoimmune complications, such as rheumatic fever and glomerulonephritis. In this case, the problem was solved not by immunological approaches but by controlling transmission of the pathogens. Epidemiological studies showed that the incidence of disease could be significantly reduced by measures that reduced the number of airborne pathogens in the barracks sleeping quarters—measures such as spraying a thin layer of oil droplets on blankets and clothing to prevent the release of bacteria during bedmaking.

Career Crisis

Perhaps the greatest luck of all was the support and advice my husband gave at the end of our postdoctoral training at Harvard. Up to that time, I had proceeded along the usual route, marriage, graduate school, two children, postdoctoral studies, and had blithely assumed the usual outcome, a full-time academic career. An unexpected complication arose, however; namely, the arrival of twins, which meant that our family suddenly consisted of four children under the age of five. Reality struck as I had to face how I could do both—be a good mother and a good immunologist. When I told my husband of my decision to become a full-time mother, he would have none of it and instead suggested part-

time work as a solution. He overrode my objections with two arguments; first, that most tenure-track scientists did research only part-time because of teaching and administrative duties, and second, that a half-time research adjunct could remain competitive by being creative and undertaking high-risk projects that a tenure-track scientist could less afford to do.

I followed my husband's advice for twenty years until our youngest son finished high school. It meant that I had a great time doing "crazy" research and did not suffer severe guilt pangs about the children.

A Paradigm for Science Education

Many of the chance events that shaped my career as an immunologist are now the basis of educational programs aimed at generating an interest in learning. Thus, preschool tutoring, such as I had, has been employed by Head Start to remedy the backgrounds of children from disadvantaged homes. The program has an impressive record of turning out children who perform better on entering school and thus like the learning process. Maintaining that performance and liking of learning during the teen years, however, requires programs that can successfully counteract the anti-intellectualism inherent in some social traditions and teenage peer groups. My father was a good example of the traditional problem. His ambition was for me to be a "lady," which translated into taking courses that aimed to produce a good hostess and a good housekeeper rather than a good physicist or a good mathematician. He didn't approve of my Jewish friends, but fortunately he could not override the stimulation and security of their friendship. Programs such as Upward Bound are providing support to high schoolers equivalent to that provided by my Jewish friends. Their aim is to combat not only social pressures but also peer pressures that place the highest values on being a cheerleader, or starring on the football team, or wearing expensive clothes. Organizations such as the National Science Foundation are trying to create rewards for scientific achievement that can compete with the publicity given high school sporting events or prom queens. The best science students in each state and his/her high school teacher meet in Washington and have their pictures taken with "big shots" like the President's Science Adviser or the Director of National Science Foundation.

"Hands-on" experience is a powerful career attractant. A research project in my senior year at Vassar and the subsequent wartime projects completely destroyed any notion I had of alternative careers, even medicine. The research bug had bitten! And my bite has been duplicated hundreds, thousands of times. The Oberlin Report clearly documented that it is an undergraduate research project conducted in close contact with a professor that is responsible for the high turnout of scientists by the small liberal arts colleges and for the high rate of success among those scientists. As a result, "hands-on" laboratory work

has been adopted as a lure by many public and private organizations to interest minorities and women in scientific careers. The MARC program of the National Institutes of Health and the undergraduate programs sponsored by the Howard Hughes Medical Institute are good examples of how effective the hands-on approach can be.

Where Is Creativity?

One of the elements, perhaps the most important one, that shaped my career as an immunologist has, however, fallen through the cracks of most science education programs. That is an emphasis on creativity. At the beginning of my senior year at Vassar, Catherine Dean led the bacteriology majors to a room in the basement and said, "Here is a centrifuge, a pH meter, an incubator, media on the shelf, and various bacterial strains in the icebox. Dream up your research project!" We did, because senior research was a requirement for graduation. In the graduate program at the University of Chicago, students (that included me) were expected to develop a thesis proposal and then sell the project to the appropriate professor in the Department. In the postdoctorate scene at Harvard, appointments could be made to do independent work provided that the proposed work passed faculty review. I took full advantage of the opportunity. The emphasis on originality in these programs reflected the notion that students could be trained to think creatively and that such training was critical to their success in their subsequent careers. This notion was amply supported in my own case because the training enabled me to follow my husband's advice and to remain competitive by concentrating on more "far out" research ideas.

Many of the devices used in the past to foster creativity, e.g., graduate students designing their own thesis projects, have been discarded. They became impractical as the biological sciences grew more sophisticated, more team-oriented, and more competitive. Unfortunately, the emphasis on training in creative thinking has disappeared along with the devices. The de-emphasis has occurred as young scientists have a harder and harder time creating research niches to establish themselves, as study sections award their limited amounts of money to safe rather than innovative proposals, as the number of scientists being trained is reduced without any criteria for selecting those with the most original minds. The de-emphasis has also occurred at a time when creative thinking is needed for young scientists to tackle socio-scientific problems such as the dealing with the question of a second job for a spouse or significant other, the division of time and effort between research and parenting, etc. In my opinion, the immunological community would do well to focus on such issues and devise techniques for encouraging creative thinking that are applicable to the current state of the science world and its teaching.

Bad Luck

At this point in my recital, you may ask whether there were any unlucky events that affected my career. Of course, there were. To give a few examples: A professor at Chicago gave me a foretaste of things to come as I was going off to Harvard for my postdoctorate. He said that I had been an excellent graduate student, but because I was a woman, I should not entertain any hopes of being hired by the faculty of the Department of Bacteriology and Parasitology. At another institution, the head of the department proclaimed he would never hire the wife of anyone, not even the janitor, so my husband and I had to look elsewhere for the second job. Early in my career, one or two immunologists would get up after each of my research presentations and say they couldn't reproduce the data. Although their criticisms were never documented, the constant voicing of doubts had the effect of delaying recognition of my work.

To be fair, there were also many helping hands along the way. The faculty at Chicago sponsored me for membership in the American Association of Immunologists before such well-deserving male peers as David Talmage, Maurice Hilleman, and Riley Hauswright. Dr. Howard Mueller, head of the Harvard Department of Bacteriology, went into the shop and personally made 24 guinea pig metabolism cages that I could not afford to purchase on my fellowship. Dr. Wendell Stanley, head of the Virus Laboratory at Berkeley, bought some $25,000 worth of equipment for my research without any guarantee that I would get a grant and pay him back. I suppose the unkindness and kindness come out about even.

As I look back, I think my one significant piece of bad luck may have been not playing a team sport. Recent analyses have indicated that women scientists tend to be loners who do not belong to a network of collegial associates. Typically they run small laboratory groups and are relatively unaggressive about promoting their research accomplishments. These characteristics contrast sharply with those of the average male colleague, and the question is, why? A number of explanations have been offered: differences in mentoring, in standards for masculine and feminine conduct, etc. One of the most intriguing is the difference in athletic experiences. Most men have participated in some form of team sport throughout their education, whereas most women have not. Moreover, men continue to participate in their adult years; the male graduate students, postdoctorates, and able faculty from our immunology floor still get together for a basketball game using a net rigged up outside the laboratory building. In these team sports, the players get the opportunity to practice competitiveness; they learn how to develop winning strategies and, at the same time, how to work cooperatively and form successful liaisons—very valuable lessons for any

subsequent competitive endeavors. The idea of a contribution from team sports came much too late to affect the education of my three daughters. However, all seven of my granddaughters, ranging in age from five to eighteen, have been or are currently playing on soccer teams! We'll see what happens.

Obituary - Marian Elliott Koshland

Noted immunologist and educator Marian Elliott Koshland, professor of molecular and cell biology, died Oct. 28 of lung cancer at Alta Bates Medical Center in Berkeley. She was 76.

An international leader in immunological research, Koshland was a member of the National Academy of Sciences and past president of the American Association of Immunologists, as well as a past member of the National Science Board of the National Science Foundation. She also served on various national science committees and on the editorial boards of several research publications.

Koshland published some 200 articles in the scientific literature, among them the major finding that antibodies differ in their amino acid composition. This was a decisive argument for the now-accepted selection theory of antibody diversity and against the instruction theory.

Her most recent work investigated how cellular hormones called cytokines regulate gene expression in cells of the immune system.

Born in New Haven, Conn., in 1921, she earned a PhD in immunology in 1949 from the University of Chicago. During World War II she was a member of a research team that produced a vaccine for cholera and later was a researcher on the Manhattan Project based in Oak Ridge, Tenn. After a two-year post-doctoral stint at Harvard Medical School, she worked as a bacteriologist at Brookhaven National Laboratory for 13 years. She came to campus in 1965 as a researcher and lecturer and joined the faculty in 1970.

During her more than 30 years at Berkeley, she served as chair of the Department of Microbiology and Immunology from 1982 to 1989. At the time of her death she was head of the Graduate Affairs Division of the Department of Molecular and Cell Biology. She continued active research in her laboratory until recent weeks despite her illness.

Among her research achievements was discovery of the J chain, a key chain in antibody structure that allows antibodies to be exported from the cell and to circulate in the bloodstream to provide the immune response.

She received an Excellence in Science Award from the Federation of American Societies of Experimental Biology in 1989 and a National Institutes of Health Merit Award.

Koshland is survived by her husband of 52 years, Daniel E. Koshland Jr. of Lafayette, professor emeritus of molecular and cell biology and past editor of the journal Science; five children, Ellen, Phyllis, James, Gail and Douglas; and nine grandchildren.

A campus memorial service will be held Monday, Dec. 1, from 5 to 7 p.m., in the Berkeley Art Museum. Donations in Koshland's memory may be sent to the Graduate Fund, Department of Molecular and Cell Biology, 597 Life Sciences Annex, MC 3200. Checks should be made out to the UC Regents.

Berkeleyan, November 12, 1997

PROFESSIONAL ORGANIZATIONS:

Member, American Association for Advancement of Science
Councilor, American Association of Immunologists
Member, National Academy of Sciences
Founding Member, Academy of Cancer Immunology
Roll of Honor, International Union Against Cancer
Member, American Society of Gene Therapy
Fellow, American Academy of Microbiology

PROFESSIONAL SERVICE AND COMMITTEES:

NATIONAL INSTITUTES OF HEALTH:

1987-1990	Chair, Experimental Immunology Study Section, National Cancer Institute
1987	ad hoc member, National Institute of Child Health and Human Development Board of Scientific Counselors, Review of Cell Biology and Metabolism Branch
1993	ad hoc member, Review of Experimental Immunology Branch, National Cancer Insitute
1995	ad hoc member, Review of Laboratory of Immune Cell Biology, National Cancer Institute
1995	ad hoc member, Review of Biological Resources Branch and Laboratories of Immunogenetics and Immunopathology, National Institute of Allergy and Infectious Diseases.
1996	ad-hoc member, Immunobiology Study Section
1996	Site Visit Committe Member, Laboratory of Molecular Immunoregulation, National Cancer Institute
1996	Site Visit Committe Member, Laboratory of Experimental Immunobiology, National Cancer Institute
1997	Site Visit Committe Member, Laboratory of Tumor Immunobiology and Biology, National Cancer Institute
1997	Site Visit Committe Member, Laboratory of Biochemical Physiology and the Laboratory of Immune Cell Biology, National Cancer Institute
1997	Participant, AIDS Vaccine Research Committee workshop
1997	Member, Search Committee for Laboratory Chief, Experimental Immunology Branch, National Cancer Institute
1997	Member, Biologics Subcommittee of the Developmental Therapeutics Program Review Group, National Cancer Institute
1998	Member, PRG Expert Panel on Gene Therapy, National Cancer Institute
1996-present	Member, Board of Scientific Counselors, National Cancer Institute
1998-present	Member, Immunology Board of the U.S.-Japan Cooperative Medical Science Program, National Institute of Allergy and Infectious Diseases

OTHER:

1985-1989	Member, ASC Fellowship Committee, International Union against Cancer
1989-1994	Chair, ASC Fellowship Committee, International Union against Cancer
1986-1988	Board, Midwinter Conference of Immunologists
1994-1996	Member, Public Affairs Committee, American Association of Immunologists
1995-1996	Member, Program Committee, American Association of Immunologists
1997	Member, International Benchmarking of U.S. Research Fields Immunology Panel, National Academy of Sciences
1996-present	Consultant, NeXstar Pharmaceuticals, Inc.
1996-present	Member, Scientific Advisory Board, Tularik, Inc.
1996-present	Member, Scientific Advisory Board, IDEC Pharmaceuticals Corp.
1996-present	Councilor, American Association of Immunologists
1996-present	Member, Board of Scientific Counselors, National Cancer Institute

PROFESSIONAL SERVICE (CONTINUED):

1998-present	Member, Committee on AIDS, Infectious Disease and Immunology, American Society of Gene Therapy
1998-present	Member, Breast Oncology Program, UCSF Cancer Center

UNIVERSITY COMMITTEES:

1995	Member, Advisory Committee, Multipurpose Arthritis and Musculosckeletal Disease Center, UCSF
1996	Member, Task Force on Animal Housing Space: appointed by Chancellor's Advisory Committee on Biology
1997-1998	Member, Chancellor's Planning Committee on Animal Space
1997-1998	Chair, Northwest Precinct Planning Committee, appointed by Interim Vice Chancellor for Capital Projects
1998-1999	Member, Vice Chancellor for University Relations Search Committee
1998-1999	Member, Biomedical Facilities Study Committee
1995-present	Chair, Committee on Animal Housing Space Assignment (CARSA); appointed by Vice Chancellor for Research
1997-present	Member, Immunology Search Committee, Department of Molecular and Cell Biology

NATIONAL AND INTERNATIONAL COURSES TAUGHT:

1984	Faculty for advanced courses in Evolution and Regulation of the Immune System, American Association of Immunologists, St. Louis.
1985	Faculty for advanced courses in Evolution and Regulation of the Immune System, American Association of Immunologists, Monterey.
1987	Convenor and Faculty for Indo-US Course on "The Molecular and Cellular Biology of the T Lymphocyte," All India Institute of Medical Sciences, New Delhi.
1992, 1996	Faculty for advanced course in Immunology, Federation of European Biological Societies, Greece.
1995, '97 & '98	Faculty for advanced course in Immunology, American Association of Immunologists, Berkeley.

MEETINGS ORGANIZED:

1988	Chair, Midwinter Conference of Immunologists
1990	Co-Chair, Gordon Conference on Immunobiology and Immunochemistry
1991	Chair, Gordon Conference on Immunobiology and Immunochemistry
1994	Co-Chair, Keystone Symposium on T cell Activation, Keystone, Colorado

EDITORIAL APPOINTMENTS:

1985-1987	Reviewing Editor, Science.
1987-1993	Associate Editor, Journal of Immunology
1993-1996	Section Editor, Journal of Immunology
1993-1997	Editorial Board, Journal of Experimental Medicine
1988-	Transmitting Editor, International Immunology
1989-	Editorial Board, Developmental Immunology
1997-	Transmitting Editor, Proc. Nat. Acad. Sci USA
1997-	Associate Editor, Immunity

CURRICULUM VITAE
Anne H. Good

Present Address

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Personal Data

Birthplace: Everett, Washington Birthdate: 6/25/31

Education

B.A., Wellesley College, Wellesley, MA, 1952 (High honors)
M.D., Yale University School of Medicine, New Haven, CT, 1957
Ph.D., Western Reserve University, Cleveland, OH, 1963

Positions Held

1998 - present:	Senior Lecturer Emerita, Department of Molecular and Cell Biology
1979-1998:	Senior Lecturer in Immunology, Department of Microbiology and Immunology and Department of Molecular and Cell Biology, University of California at Berkeley
1966-1979:	Lecturer in Immunology, Department of Microbiology and Immunology, University of California at Berkeley
1965-1966:	American Cancer Society Postdoctoral Fellow, University of California at San Diego (laboratory of Dr. S. J. Singer)
1963-1965:	Postgraduate Research Fellow in Biology, University of California at San Diego (U.S.P.H.S. Special Fellow, laboratory of Dr. S. J. Singer)
1961-1963:	Fellow in Pathology, Western Reserve University
1959-1961:	Fellow and Resident in Pathology, University Hospitals of Cleveland and Western Reserve University
1958-1959:	Assistant Resident in Pathology, University Hospitals of Cleveland
1957-1958:	Intern, Medical Service, University Hospitals of Cleveland, Cleveland OH
1954-1955:	Assistant in Instruction in Anatomy, Yale University School of Medicine

Honors and Awards

Phi Beta Kappa (1951)
Alpha Omega Alpha (1956)
Sigma Xi (1961)

Professional Societies (Member)

American Association for the Advancement of Sciences
American Association of Immunologists

Major Research Interest

Immunochemistry

Regional Oral History Office
Room 486 The Bancroft Library

University of California
Berkeley, California 94720

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Anne Haines Good

Date of birth 6/25/31 Birthplace Everett, WA

Father's full name Henry Rockwell Haines

Occupation Salesman Birthplace Parris Island, S.C.

Mother's full name Cecile LeConte Haines

Occupation housewife Birthplace Berkeley, CA

Your spouse Robert Howard Good

Occupation Physics Professor Birthplace Ann Arbor, MI

Your children Howard Randall Good (1965); Joseph Henry Good (1967); Samuel Thomas Good (1971)

Where did you grow up? Southern California

Present community Albany, CA

Education B.A. Wellesley College, 1952; M.D. Yale University School of Medicine 1957; Ph.D. Western Reserve University (1963)

Occupation(s) Lecturer, Sr. lecturer in Immunology University of California, Berkeley, 1966-1998; postdoctoral fellow 1963-66.

Areas of expertise Immunology

Other interests or activities wilderness activities (hiking, backpacking, snowcamping); wilderness medicine

Organizations in which you are active American Association of Immunologists; Sierra Club; volunteer, Oakland Red Cross

SIGNATURE Anne H Good DATE: 11/23/00

CATHERINE P. KOSHLAND
Wood-Calvert Professor in Engineering
University of California, Berkeley

Catherine P. Koshland is the Wood-Calvert Professor in Engineering at the University of California, Berkeley, and Professor in Energy and Resources and in Public Health (Environmental Health Sciences). Professor Koshland graduated with a B.A. in Fine Arts from Haverford College, studied painting at the New York School of Drawing, Painting and Sculpture, and received her M.S. in Mechanical Engineering in 1978 and her Ph.D. in 1985 from Stanford University. She joined the U. C. Berkeley faculty in 1984. She teaches engineering, energy and environmental health, emphasizing mechanistic approaches as well as a systems perspective.

Professor Koshland's research is conducted at multiple scales, from mechanistic analyses of combustion products in flow reactors to control strategies in urban airsheds to improved management of the global industrial production system. Her combustion research has focused on pollutant formation particularly involving chlorinated hydrocarbons, droplet and spray combustion, and the development of advanced diagnostic tools for non-intrusive monitoring of combustion species including chlorinated hydrocarbons and metals. In addition, she has worked at the intersection of energy, air pollution and environmental (human) health. Her recent work is in the area of green manufacturing and industrial ecology, addressing the conception and assessment of improved technologies in energy and manufacturing that consider environmental needs and the barriers to implementation of new technologies or policies. Prof. Koshland is Associate Director of the UC Berkeley Superfund Basic Research Program, and Director of the Health Effects of Modern Technologies, the Berkeley component of the UC Toxic Substances Research and Teaching Program. At Berkeley, she is an elected member of the Divisional Council of the Academic Senate, former Chair of the Committee on Undergraduate Scholarships and Honors, and served on the Commission on Undergraduate Education. She is a director and Secretary of The Combustion Institute. She is Vice Chair of the Board of Managers of Haverford College, and chaired its Educational Affairs Committee from 1996-2000. She is married to James M. Koshland, and has three children, Sarah (Cal '99), Maggie (Cal '02) and Jacob (age 13).

EDUCATION

1968-70 Smith College, Northampton, MA.
1970-72 Haverford College,Haverford, PA, B.A. Fine Arts, 1972.
1977-78 Stanford University, M.S. Mechanical Engineering, 1978.
1978-85 Stanford University, Ph.D. Mechanical Engineering, 1985.
Thesis: Combustion of Monodisperse Hydrocarbon Fuel Droplet Clouds

Studied art with Charles Stegeman and Chris Cairns, Haverford , PA (1972-73); studied at the New York Studio School of Painting, Drawing and Sculpture (1973-74); non-matriculated graduate student, Stanford University (1975-77)

PROFESSIONAL EXPERIENCE

1974-75 Management Assistant, GS-9, Energy Research and Development Administration, and Office of Coal Research, Dept. of Interior, Washington DC
1977-78 Technical Editor, Stanford Energy Report, Stanford Energy Institute, Stanford University
1980-84 Graduate Student Researcher, High Temperature Gasdynamics Laboratory, Stanford University,
1984-85 Acting Assistant Professor, University of California, Berkeley
1985-92 Assistant Professor of Environmental Health Sciences, University of California, Berkeley
1987- present Faculty Member, Center for Occupational and Environmental Health
1987-95 Affiliated Faculty Member, Energy and Resources Group
1992-96 Associate Professor of Environmental Health Sciences, University of California, Berkeley
1995-96 Associate Professor, Energy and Resources Group, University of California
1996-present Professor, Environmental Health Sciences, and Energy and Resources, University of California
1992-present Associate Director, UC Berkeley, Superfund Basic Research Program

1996-present	Director, University of California Toxic Substances Research and Teaching Program, Berkeley Lead Campus Program: Health Effects of Modern Technologies
1996-present	Member, College of Engineering Faculty, University of California, Berkeley

HONORS

1979-80 Energy Fellowship (DOE), Stanford University

1978	Member, Stanford University Science and Technology Delegation to The People's Republic of China, 1978.
1985	Sigma Xi
1995-	Wood-Calvert Professor in Engineering, 1995-present
1999	Nineteenth Annual Steven Manly Memorial Lecturer. University of California, Santa Barbara
1999	Best Paper Award, Indoor Air Journal 1996-98, International Academy of Indoor Air Sciences (with J. Ten Brinke, S. Selvin, A.T. Hodgson, W. J. Fisk, M. J. Mendel and J.M. Daisey)

PROFESSIONAL MEMBERSHIPS

The Combustion Institute
The American Chemical Society
The Air and Waste Management Association
American Conference of Governmental Industrial Hygienists
American Public Health Association

Daniel E. Koshland, Jr.

Born in New York City on March 30, 1920, Daniel Koshland received his B.S. from the University of California in 1941. He received his Ph.D. from the University of Chicago in 1949 and was a Postdoctoral Fellow at Harvard University from 1949-51.

Professor Koshland was a Group Leader on the Manhattan Project (1942-46), a Senior Biochemist at Brookhaven National Lab (1951-65) and an Affiliate at Rockefeller University (1958-65). In 1965 he became a Professor at the University of California, Berkeley and was a Guggenheim Fellow (1971-72). He was Editor of *Science* Magazine from 1985-95.

Among his numerous honors are the T. Duckett Jones Award of the Helen Hay Whitney Foundation (1977), the Distinguished Lectureship Award of the Society of General Physiologists (1978), the Pauling Award (1979) and the Edgar Fahs Smith Award (1979) of the American Chemical Society, the Waterford Prize of the Scripps Institute (1984), the Rosenstiel Award of Brandeis University (1984), the City College of NY Bicentennial Distinguished Scientist Award (1987), the Chauncey Leake Award of the University of California at San Francisco (1988), the Merck Award of the American Society of Biochemistry & Molecular Biology (1990), The National Medal of Science (1990), the University of California Alumnus of the Year (1991), the Lasker Special Achievement Award (1999), the Berkeley Medal of the University of California (2000) and the Seaborg Medal of the American Chemical Society (2000).

Professor Koshland was Chairman of the Biological Division of the American Chemical Society (1968), President of the American Society of Biological Chemists (1973), U.S. Representative of the International Union of Biochemistry (1973-74), Chairman of the Department of Biochemistry at the University of California, Berkeley (1973-78) and Chairman of the Editorial Board of the *Proceedings of the National Academy of Sciences* (1980-1984).

He is a member of the National Academy of Sciences, the American Academy of Arts and Sciences and the American Philosophical Society. He has been a member of the Council of the American Academy of Arts and Sciences (1975-79) and the Visiting Committee for Biology, Harvard Board of Overseers (1975-84).

Editorial boards on which he has served include *Biochimica et Biophysica Acta, Journal of Molecular Pharmacology, Journal of Biological Chemistry, Accounts of Chemical Research, Biochemistry, Journal of Molecular Catalysis, Journal of Molecular Biology, Bioorganic Chemistry, Annual Reviews* and *Science83*.

Professor Koshland is the recipient of numerous lectureships and honorary memberships that include the Japanese Biochemical Society, the Royal Swedish Academy of Sciences, and the American Medical Writers' Association. He has received honorary degrees from the Weizmann Institute of Science, the Carnegie Mellon University, the University of Chicago, the University of Massachusetts at Amherst and Brandeis University.

Curriculum vitae

Name: Daniel E. Koshland, Jr.

Born: March 30, 1920, New York, New York

Education:
University of California 1937-1941 B.S.
University of Chicago 1946-1949 Ph.D.
Harvard University, Postdoctoral Fellow 1949-1951

Professional Experience:
Professor, University of California, Berkeley 1965-
Editor, *Science* Magazine 1985-95
Senior Biochemist, Brookhaven National Lab 1951-1965
Affiliate, Rockefeller University 1958-1965
Group Leader, Manhattan Project 1942-1946
Chemist, Shell Chemical Company 1941-1942

Honors:
Albert Lasker Award for Special Achievement in Medical Science, 1998
Clark Kerr Award, University of California, 1994
Alexander M. Cruickshank Lecturer Award, 1994
American Academy of Microbiology Distinction of Fellow Award, 1994
Gilbert N. Lewis Medal, University of California, 1993
Givaurdeu Rouve Award, Sarasota Florida, 1993
University of California Alumnus of the Year, 1991
The National Medal of Science, 1990
Merck Award, Amer. Society of Biochemistry & Molecular Biology, 1990
Chauncey Leake Award, University of California, San Francisco, 1988
City College of NY Bicentennial Distinguished Scientist Award, 1987
Rosenstiel Award, Brandeis University, 1984
Waterford Prize, Scripps Institute, 1984
Edgar Fahs Smith Award, American Chemical Society, 1979
Pauling Award, American Chemical Society, 1979
Distinguished Lectureship Award, Society of General Physiologists, 1978
T. Duckett Jones Award of Helen Hay Whitney Foundation, 1977
President, American Society of Biological Chemists, 1973
The Berkeley Citation, University of California, Berkeley, 1970
National Academy of Sciences
American Academy of Arts and Sciences
American Philosophical Society
Honorary Member, Japanese Biochemical Society
Honorary Member, Alpha Omega Alpha, Medical Honor Society
Honorary Member, Royal Swedish Academy of Sciences
Honorary Fellow, American Medical Writers' Association, 1990
Faculty Research Lecturer, University of California, Berkeley, 1980
Visiting Fellow, All Souls College, Oxford, 1972
Guggenheim Fellow, 1971-72
Chairman, Biological Division, American Chemical Society, 1968

Honorary Degrees:
Honorary Ph. D., Weizmann Institute of Science, 1984
Honorary Sc. D., Carnegie Mellon University, 1985
Honorary LL. D., Simon Fraser University, 1986
Honorary D.H.L., Mt. Sinai School of Medicine, C.U.N.Y., 1991
Honorary LL.D., University of Chicago, 1992
Honorary ~~degree~~ Ph. D., University of Massachusetts at Amherst, 1992

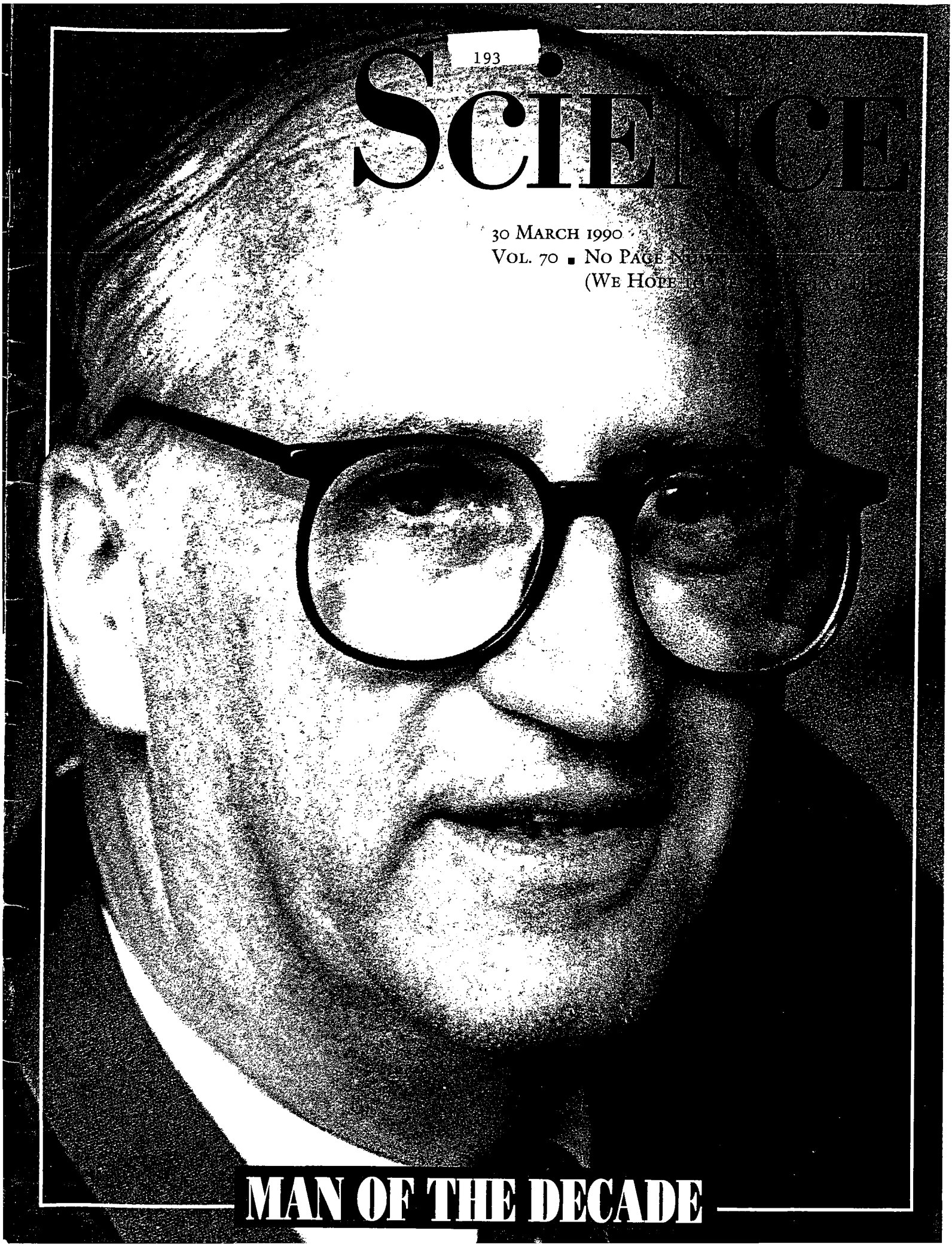
Honorary Name Lectureships:
Walker Ames Lecturer, University of Washington, 1964
Eli Lilly Lecturer, Eli Lilly Company, 1968
Phillips Lecturer, Haverford College, 1968
Alpha Chi Sigma Lecturer, Washington State University, 1969
Carter Wallace Lecturer, Princeton University, 1969
Harvey Society, 1969
Rennebohm Lecturer, University of Wisconsin, 1970
National Sigma Xi Lecturer, 1970
Christian Herter Lecturer, New York University, 1971
Schaeffer Memorial Lecturer, Washington University, 1971
Battelle Lecturer, University of Washington, Seattle, 1971
Welch Foundation Lecturer, 1971
Leo Marion Lecturer, National Research Institute, Canada, 1972
Weizmann Memorial Lecturer, Weizmann Institute, 1971
Phi Beta Kappa Lecturer, 1974 and 1976
Johns Hopkins University Lecturer, 1974
Nieuwland Lecturer, University of Notre Dame, 1975
Sutherland Memorial Lecturer, University of Miami, 1976
Weissburger Lecturer, Rochester University, 1977
John Edsall Lecturer, Harvard University, 1979
NIH Distinguished Lecturer, 1982
Carnegie Mellon Distinguished Lecturer, 1982
Pauling Lecturer, Stanford University, 1982
Rudin Lectures, Columbia University, 1985
William H. Stein Lecturer, Rockefeller University, 1985
Robt B. Woodward Visiting Professor in Chemistry, Harvard, 1986-87
Philip R. Jonsson Visiting Professor, University of Texas, 1990
R.C. Fuller Lecturer on Science and Public Policy, U.Mass.at Amherst, 1992
Linnaeus Lecture, University of Uppsala, 1992

Service on Editorial Boards:
Biochimica et Biophysica Acta, 1968-72
Journal of Molecular Pharmacology, 1970-74
Journal of Biological Chemistry, 1961-72
Accounts of Chemical Research, 1970-82
Biochemistry, 1972-77
Journal of Molecular Catalysis, 1977-
Journal of Molecular Biology, 1972-78
Science, 1969-75
Bioorganic Chemistry, 1971-
PNAS, Chairman, Editorial Board, 1980-84
Annual Reviews, 1982-
Science83, 1983-86

Educational and Governmental Agencies:
Chairman, Academy Forum, National Academy of Sciences
Council of American Academy of Arts and Sciences, 1975-79
Visiting Committee for Biology, Harvard Board of Overseers, 1975-84
Chairman, Public Policy Committee, Amer.Soc.of Biol. Chemists, 1973-75
U. S. Representative, International Union of Biochemistry, 1973-74
Chairman, Department of Biochemistry, U.C. Berkeley, 1973-78
Chairman, Gordon Conference on Proteins, 1968
Chairman, Chancellor's Advisory Council on Biology, University of California, Berkeley, 1982-1993

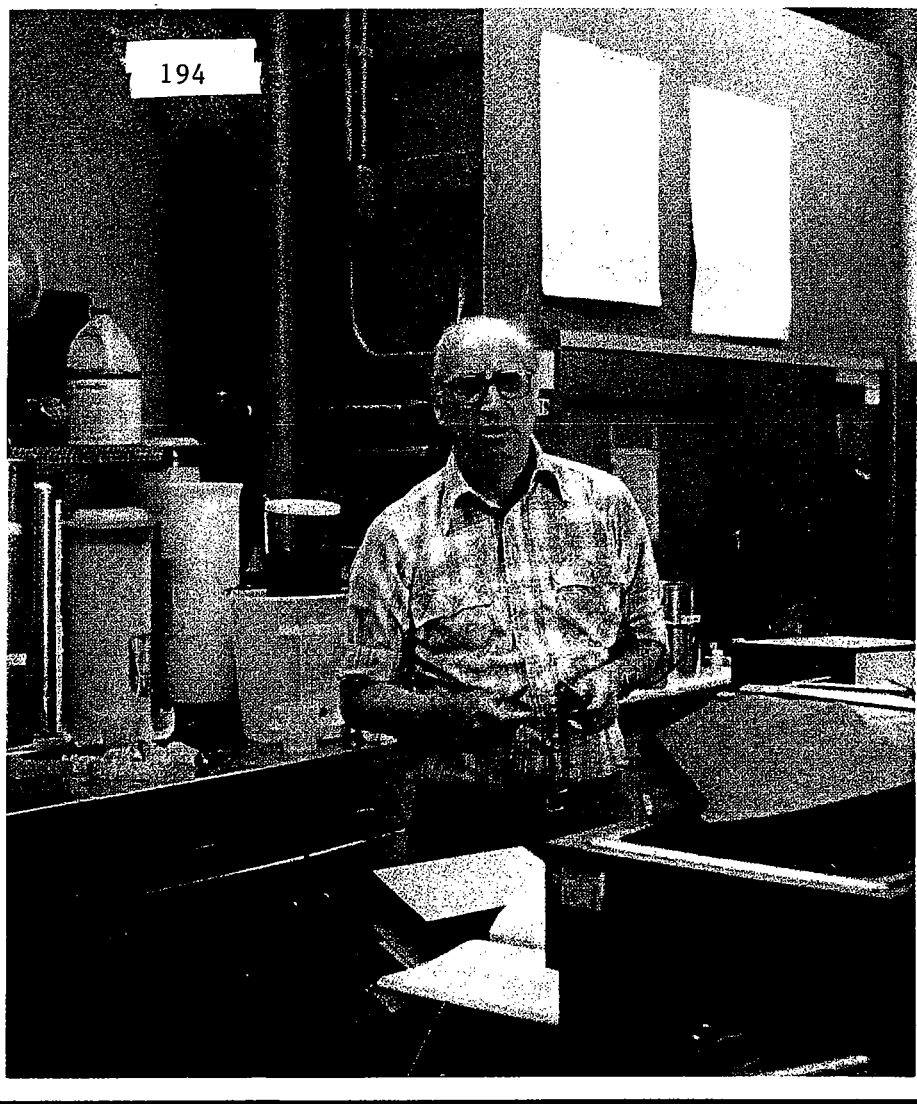
SCIENCE

30 MARCH 1990
VOL. 70 ■ NO PAGE
(WE HOPE



MAN OF THE DECADE

FASHION MODEL



MASTER TECHNICIAN

SUPER GRANDFATHER

STAR ATHLETE

AMERICAN
ASSOCIATION FOR THE
ADVANCEMENT OF
KOSHLANDS

SCIENCE

ISSN 0000-0070
30 MARCH 1990
VOLUME 70
NUMBER 1

■ **SCIENCE is published weekly on Friday, except for weeks when the editor is on vacation or incommunicado because of earthquake damage.** Second-class postage seems most appropriate, provided USPS takes checks.

■ The American Association for the Advancement of Koshlands was founded in 1920 and incorporated in 1945. Its objectives are contained in a privileged document.

Dear Dan E.

Readers, the editorial board at Science *has decided to add yet another feature to expand and improve how* Science *as a magazine can serve its subscribers. In accordance with our policy to broaden coverage of the disciplines and interests of all areas of science, we have decided to begin an advice column where issues of the 'other lives' of scientists can be addressed.*

Dear Dan E:
I am a biochemist with five children, eight grandchildren, and the ninth grandchild on the way. I was wondering if there was any special advice that you could give about how to cope with the huge responsibilities of being the family patriarch—**MISSION IMPOSSIBLE.**

Dear MISSION IMPOSSIBLE:
When travelling in Europe, always get a copy of the *Herald Tribune* and read it in the tub. Be sure to sign the back of all credit cards, so as not to embarrass any family members. When driving, try to straddle the white line so you will have the advantage of choosing any lane at any time. Be sure to have your camera flash plugged into the wall for all occasions. Always give advice on etiquette to grandchildren. Always be willing to go to the airport for relatives. Never turn down a ripe persimmon. Have at least four sweaters with holes in the elbow. These are just a few answers to a very complicated question. However, always remember: knowing you are a strong patriarch is as good as being one.

Dear Dan E:
I have a colleague who is bright, dignified, a wonderful father and career man. BUT he loves to watch football. As this isn't bad enough, he becomes very emotional and constantly calls the coaches stupid. How can I discourage this undignified behavior?—**THE UNADDICTED, NONFANATIC SPORTS-HATER.**

Dear UNADDICTED, NONFANATIC SPORTS-HATER:
Of course, the coaches are stupid and your colleague is right.

Dear Dan E:
My husband and I want to wish my father a very special birthday for this 70th. But we can't think of just the right thing to say or do. Instead, here we sit tight-lipped, contemplating how we can express our deep feelings. I guess all we really want to say is—we love him and think he's the greatest. What should we do—**TORMENTED IN TUCSON**

Dear TORMENTED IN TUCSON:
You've said it; he's read it; that's that!

This Week in SCIENCE

Forked tongue biophysics

IOLINGUISTS from Colgate University have documented what for many years had been suspected: the human tongue, like the human brain, is a schizoid organ. On page 49, Montana, Rice, and Craig report that the left side of the tongue (under the guidance of the right side of the brain) shows a strong chemotactic response to peanut butter whereas the right side of the tongue (whose responses are controlled by the left brain hemisphere) is most stimulated by caviar. In taste tests, taste buds vibrated rhythmically when exposed to test materials and moved measurably in the direction of the stimulants. When taste buds on one side were responding to the preferred stimulant, the vibrations could be suppressed by exposure to the preferred stimulant of the opposite hemilingula. In a related report, Ames points out that if the two stimulants are mixed, there is a high risk of offending the palate (page 76).

Professional evolution

ATURE versus nurture and phylogeny recapitulates ontogeny, two couplets fundamental to biology, have been brought together in a stunning discovery by researchers from Darwin University (page 1869): the evolution of professional development, like the evolution of biologic form, proceeds from the primitive to the complex. For example, a full 83% of cardiologists report pumping iron as teens. 87% of lawyers had chosen apple torts over cherry danish at bakeries as kids. And 92% of magazine editors wrote limericks as young adults. One example is illustrated in the unsophisticated limerick—There once was a Martian whose plight, Was to experience immeasurable delight, In travelling to Earth, Where he did give birth, To new beverages like Pepsi and Sprite—which was followed two decades later by an editorial written by the same person (*Science* Magazine, 1985, 6 September) in which the themes of interplanetary travel, brain functioning and soda development were more fully expounded. In a related report, Gould speculates however that if the "tape of life" were run again, limerick writers might just as likely evolve into football stars.

Loma Prieta rehash

NUSUAL seismic patterns, never before recorded in association with the occurrence of an earthquake, were noted during the Loma Prieta earthquake last October 17 and were also noted during the numerous aftershock events in the succeeding months. Computer analyses show that, although the overall seismic activity decreased as predicted with increasing distance from the epicenter, there was a complete deflection of seismicity under the Barker biochemistry building on the University of California's Berkeley campus (page 911). Slip, Strike, and Richter report that the shape of the so-called "seismic warp" mimicked the shape of the rectangular building and that the deflection was complete for distances of 100 yards on each side of the building. Seismologists are now concluding that the deflection was caused by a "magnetic personality" present in the biochemistry building during the mainshock event; the magnetic force was apparently strong enough to exert its influence even during aftershock events when the source was absent from the building.

Translational error

EW archeologic evidence indicates that major errors were made in both transcription and translation of the Old Testament book of the Hebrew prophet Daniel. Daniel had been a favorite of a line of Babylonian kings because of his facility in interpreting dreams and deciphering handwritings on the wall. The classical "central dogma" supported by Talmudic interpretation held that Daniel was thrown into the lions' den for refusing to obey a decree against prayer. This is now refuted by new evidence gathered by Steinsaltz and Shadrach (page 606 B.C.E.). Daniel was in fact thrown into a linotyper's den; there he learned to set type, and he put into print for later distribution information that he transcribed off the wall where it had originally been written. Daniel eventually became the editor of the Babylonian Science Monitor. In a related report, botanists Mishrach and Abednego question whether the dandelion, named to commemorate the original story, will now have to be renamed dandelinotyper (page 606 B.S.).

Designer genes

EARS of genetic studies have revealed that love-of-science genes (L-S) do exist, are blue, and are inherited in strict Mendelian fashion. L-S was identified in a study by Denim *et al.* of a large Jewish family in the San Francisco Bay Area. The gene is dominant but shows incomplete penetrance. In the family study, the F1 generation consisted of a phenotypically L-S pair. The F2 generation consisted of 5 members, of which one was phenotypically L-S, as expected; this individual was described as one "who really dug science." Due to outbreeding, Mendelian genetics could not predict the number of individuals in the F3 generation that would express L-S. However, of the eight available members of the F3 generation, two were phenotypically L-S and produced an eliza assay and a hannah palindrome. In a related report, The Human Genome Center has announced that a long-awaited choice has been made concerning "whose genome" will be sequenced. The short list, reported in these pages previously, included a Dr. Watson from Cold Spring Harbor, a Mr. Holmes, and a Professor Koshland from U.C. Berkeley. Koshland has been chosen as the "perfect specimen." A spokesperson from the Genome office says that Koshland will be contributing her blood samples in the coming weeks. (Ah, Dan, old loyalties remain the strongest but have a happy 70th birthday just the same!)

■ RUTH LEVY GUYER

Tradition

Genetic Engineering by Koshland.
You can count on it.

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SCIENCE

30 MARCH 1990
VOLUME 70
NUMBER 1

American Association for the Advancement of Koshlands
Science serves Koshland as a forum for the presentation and discussion of half-baked and inflammatory ideas that would scarcely merit a quick scan at any serious scientific journal. No attempt is made to balance coverage unless underrepresented areas attract the attention of cronies and relatives. Everything is signed so the editor can avoid responsibility—but in a effort to make things more exciting not all signatures are valid.

ADVERTISING REPRESENTATIVES
Same as above.

Editorial and business correspondence to: Department of Molecular and Cell Biology, University of California, Berkeley, CA 94720. No phone calls please.

The Ultimate Irony

Effective January 1, 1985, Daniel E. Koshland, Jr., was named the new editor of *Science* Magazine. To the outside world this appointment seemed logical since Dr. Koshland is a renowned biochemist from the University of California at Berkeley. To his children, however, this appointment was the ultimate irony. For years we've listened to our father rant and rave against journalists, complaining constantly about their inaccuracies and their inability to think things through to their logical conclusion.

Most often these tirades occurred at breakfast. Our father was not particular as to his audience. So from early ages when we would seek a bowl of cereal until later when one of us would stumble to the table in search of a cup of coffee and a quiet perusal of the newspapers, one could expect a bombardment from a disgustingly cheerful, alert parent eager to inveigh against the follies of James Reston, Anthony Lewis and the whole New York Times in general, not to mention the criminal justice system, the coverage of the Kennedys and the kneejerk sympathy for the Soviet Union or Nicaragua. In general the whole press was incompetent and could be done much better by guess who. But guess who was too busy being a scientist. . . .

Then came the appointment to the ranks of the long-maligned. Many questions arose immediately to the minds of the long-suffering. Would our father be able to subvert from within and raise the standards of journalism? Could he bring intellectual rigor to the world of sloppy thinking? Could scientific reasoning be applied by example to reports on economics, sociology and psychological criminal testimony? Or would being one of the crowd make him more sympathetic to the difficulties of journalism? Would time constraints, editorial cutting and the dictatorship of page space for advertising affect him? Would *Science* change Daddy or Daddy change journalism through *Science*?

The answer to these questions is that tigers do not change their stripes, much. Journalism has not undergone massive changes and the editorship has not mellowed our father's opinion of most journalists. He has, however, brought his personal style of intellectual integrity laced with humor to *Science*. And his editorials from our loving viewpoint are well written, funny and very provocative. So while our love for him remains constant and our pride in him increases, we would wish for only one small item—peace at breakfast. Not likely, however.

—THE KOSHLAND CHILDREN

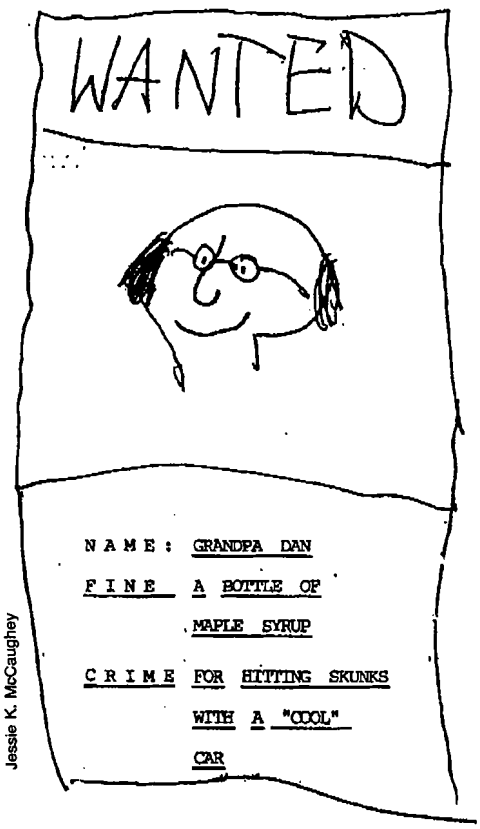
Letters

Voice of the People

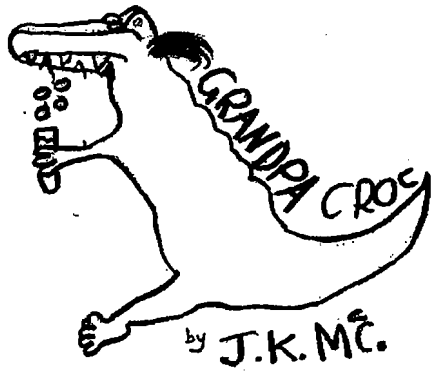
I am concerned about the fact that there are no articles in *Science* that are written for children. When the mail comes I usually look it over to see if there is anything interesting. When *Science* arrives I do not look at it because the articles in it are too complicated. I do not understand the ideas so I just put it with all the other mail that I don't read.

I propose that you put in a childrens' section in the journal. You could include articles about science that would be interesting for children. For example, you could include lessons from the College of Beautiful Manners. After the articles you could have games, puzzles, etc. having to do with the ideas in the articles. For the lesson on how to eat potato chips quietly, you could include the recipe for dipping potato chips in milk. If there was a kids' section I would probably be interested. I am sure many other children feel this way. Please consider my suggestion. Thank you!

SARAH KOSHLAND, MSSS*
College of Beautiful Manners
*(*Master of Science in Slurping Soup)*



Jessie K. McCaughey



I am writing to enquire about the recent closure of the C.O.B.M. (College of Beautiful Manners). Has the Dean really thought about the opportunities lost to these young people so lacking in knowledge about how to perform in the outside world? Is he really going to deprive them of their chance to do well in modern society?

HANNAH MCCAUGHEY

Why do we go to bed at night? Why do we have an alphabet? Why do my leggo towers fall down? Why can't I eat chocolate every day? Why do you wear special pants to go down the slide at the school near your house? Why does my football helmet go down to my shoulders and yours stick up above your ears? Why do you always want to call the Statue of Liberty play when we play football together? Please answer all my questions.

JACOB KOSHLAND
The grand inquisitor

I want to be a scientist some day. Reading your magazine helps my science a little, but I want to learn more. If you put in a small section for kids that would be great. I would read your magazine a lot more, and I'm sure I'll be waiting for the next *Science* Magazine.

MAGGIE KOSHLAND
Age 9
Atherton, CA

Grampa is silly. Love.

SOPHIA

You're the best Grandpa in the hole wide word. Happy Birthday. Love.

ELIZA

Look Who's Talking

While waiting in my womb, I thought I would write this letter to you. As you know, I haven't read any of your editorials. But having a mother who is the perfect child and a father who greets challenges willingly, I thought you might be interested in just a few of my suggestions for future editorials, such as:

"Knowing When to Throw Out Tattered Diapers"

"The Infant's Guide to Better Financial Management"

"The Real Meaning of Birthdays—From an Embryonic Perspective"

CLEMENTINE (?) WACHTEL
Amniotic Fluid Department
University of Perfect Grandchildren
Tucson, AZ 85718

A modest proposal

In the 1990's the sciences will be seeing an increase in faculty retirements and the formation of many new positions in academics and industry. In order to fill these positions it will be important to ensure graduate students finish their thesis work in a timely fashion. The question has arisen as to how to get students to get their theses written and move on. Here at the University of Californington, we have been working on this problem for several years. At first we tried helping the students by cutting their funding, bench space, and then food and water after 5½ years, but for some people this just isn't enough incentive. Now we hang a 15 lb. CPK model over their desks by a 10 lb. rope—it almost always works. The rope is the lock and key to this method. It really induces the students to fit their ideas into a thesis quickly. As a result, we find that students don't hesitate to move up in the world of science, providing the community with more scientists and us with more bench space.

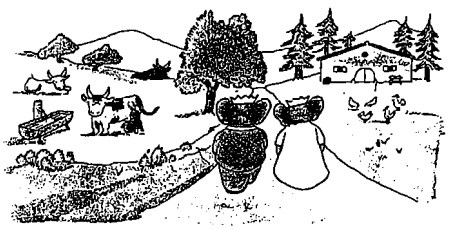
DR. ASPAR TATE
Department of Biochemistry
Division of Biology, Genetics,
Molecular Biology, Evolution, Immunology,
Agronomy, Astronomy, Numerology,
and Sociology,
University of Californington,
Maltose, CA 20416

Origins of Type

In a recent study of typographic evolution, we have discovered the ancestral lineage to the current type face used on the cover of *Science* magazine. As evident from the accompanying figure, the typeface used in the popular children's book, *Babar and his Children*, shows striking homology to that of modern-day *Science* magazine. Since the

JEAN DE BRUNHOFF

BABAR AND HIS CHILDREN



former book was first published in its present form in 1934, we believe that the appearance of the dot over the I on the cover of this journal in 1986 provides an excellent example of punctuated evolution.

While it is theoretically possible that the two dots evolved independently, two other pieces of evidence confirm the direct lineage between the two publications:

1) In the children's books Babar often travels in a bright yellow balloon—perhaps this is the same ". . .balloon rising above earth bound reality from which to look forevermore for distant intellectual horizons" that Daniel Koshland describes in his editorial on the new *Science* format.

2) Anyone who has seen the editor of this journal in his swim trunks will note his resemblance to the protagonist of the children's books shown in the figure.

Now that the ancestral roots of the typeface of *Science* magazine have been uncovered, our future studies will examine the relationship between the subdued, bland, and uncommercial nature of the present-day covers and their influence on the covers of other "serious" magazines, such as *Time*, *Newsweek*, *People*, and *The National Enquirer*.

DR I. V. SEENITALL
Department of Typobiology,
College of Letters,
Cornell University

Corrections

It has come to my attention that a correction is required to an article published some twenty-four years ago in the pages of *Science*. Due to an inadvertent typographical error in my article "The Genetic Code Solved: Three's Company, Four's a Crowd" one of the codons listed in table four was assigned to the wrong amino acid. The codon TTG was incorrectly listed as coding for the amino acid leucine, when in fact it codes for glutamic acid. Although I have reported this in a footnote to my recent review article in the journal *Sequences!*, I feel that this fact might be of interest to a wider audience.

I realize that this information will result in certain changes in published protein sequences, and I hope it does not greatly inconvenience the biological community. However, I am also excited by the new knowledge that will result from this historical oversight. As an example, I would like to point out that the recently discovered "Leucine Zipper" motif found in some DNA binding proteins, is in reality a "Glutamate Zipper."

I would also like to call your reader's attention to the publication of a new journal, *"The Journal of Corrected Sequences"* which I will be editing.

PROFESSOR T. T. GEA

FOR RELEASE MARCH 30, 1990

The American Association for the Advancement of Literature (AAAL) is hereby officially reprimanding *Science* Magazine for the conspicuous lack of literary allusions in its editorials. We of the AAAL wonder whether such omissions are calculated slaps at liberal attempts to bring the two cultures closer or are the result of ignorance on the part of the editor.

There have been—we admit to having seen—a line or two of Mark Twain's, W. S. Gilbert's, Robert Burns', Lewis Carroll's and maybe a few words from other such 'cult' figures.

But there is a noticeable absence of any quotes from a writer with the most superb ear for true dialogue, John O'Hara; or any social commentary from the supreme satirist of our time, John Updike; or any suspense in the columns using the skills of Elmore Leonard.

Furthermore, the editor alludes to no female authors. Does he think, for example, that the novels of Jane Austen, the poems of Elizabeth Barrett Browning, the short fiction of Eudora Welty have been published only to satisfy affirmative action?

I do not believe O Best Beloved Editor that you can plead ignorance. You may have forgotten your past, but the record shows that you attended an elite eastern prep school where you must have spent your time doing more than just learning how to play squash.

Before that we know you ran a successful lending library out of your childhood penthouse suite. Or, did you merely collect the pennies, nickels and dimes while your sisters read the books in their windowless rooms on the floor below.

One other matter, on which to comment: You seem to be soft on plagiarism; but I can understand and forgive that—as does our mutual good friend, Herman Wouk, whose masterpieces you claim you wrote.

For now, I remain your secret ideal woman (does your wife know about me?), who sends you congratulations on your brilliant, beautiful life so far! My love always,

/s/ Marjorie Morningstar
by F. K. Geballe

Shock, Horror! Koshland At Presidential Facility, Pennsylvania Ave.

FOPPISH PLAYSCIENCE editor and west coast aristocrat Daniel E. Koshland Jr. was recently anointed Presidential Scientology Advisor over the well respected polymath Dr. Noitall. The appointment is all the more surprising because Koshland must soon appear before a Congressional committee on fraud in science. On the cover of a new journal, "Structural Biology at U.C. Berkeley 1989–90," he has published the structure of a novel enzyme composed entirely of D amino acids. Unable to reproduce these astounding results, colleague and hence deadly enemy, Bob "Benedict Arnold" Stroud of U.C.S.F. squealed to the committee. However, Koshland is dismissive. "After all," he observed, "Many people on the other side of San Francisco Bay are of a different chirality." More serious, perhaps, are rumours that his personnel slaves have long banned him from actually doing any science in his laboratory gaol at U.C. Berkeley. "Letting Dan play with a bunsen burner is like giving Guy Fawkes gun powder" said one pale villein, still chained to the lab bench at 3 in the morning.

Koshland's outlandishly liberal views have been well documented in his little red editorials. During a press conference, held in a Disunited Jet over Hays, Kansas, he was asked if his new anointment might not require him to be marginally responsible on occasion. "No more so than anybody else in town," he answered. Since Hays is exactly midway between Washington, D.C. and Berkeley, it's not clear to which town he referred. This was taken as a good portent for his future political career—he has finally learned that questions are not to be naively answered in a scientific manner, but to be dodged at all costs.

Koshland took the opportunity given by his anointment to expound his views on health, the environment, drugs and education. He proposed that the FDA permit small quantities of plutonium to be added to breakfast cereals as a means of delivering the radiation he feels is necessary to maintain a healthy US population. 'DNA repair mechanisms need just as much exercise as muscles," he noted, "and they go flabby just as quickly—witness the widespread cancers of people not normally exposed."

As with another apauling scientologist of the same generation, Koshland's concern for health has led him to dietary excesses, such as avoiding organic foods because people are always dying of natural causes. Indeed, Koshland ascribes his own good health and longevity to the consumption of vast quantities of junk food and hooch which provide the necessary preservatives for a long, if unproductive, life. The side effects of the diet are apparently minor, "a little pserosis, some impairment of efficient signal transduction and long term memory, but nothing that interferes with my train of thought as much as reality." When challenged, he noted that the necessary control requires his death to determine if he indeed dies prematurely and added that he does not intend to complete the study for at least a decade, thereby guaranteeing continued NIH funding for his gastronomic pleasures.

Long a champion of environmental causes, he will encourage the reintroduction of species now extinct in their natural habitats. Noting recent successes with the snow leopard, Koshland hopes to expand the approach. "Unfortunately, people are all too willing to support a good cause if the species is a harmless, cute, furry feline and other species make the great sacrifice of being eaten. It is high time that other, less cute species be given the right to roam free and we humans make that sacrifice. In this regard I shall unerringly seek the reintroduction of smallpox." When it was suggested that this might cause a widespread decimation of humans he replied, "All the better. There are far too many humans on the planet and they cause much widespread devastation—witness the destruction of the rain forests. If we didn't introduce smallpox, we would have to dredge the Atlantic Ocean to drain the Pacific to make room for them all. This is impossible, since we could no longer justify the invasion of Panama on the grounds that we know what's best for Central Americans. They would no longer be central, you see, but on the eastern seaboard of a vast continent." When asked where smallpox would be liberated first, he noted that it must be neither California nor Washington DC because it might upset his longevity experiment. He is also proposing the eradication of the baleen whales, on the grounds that these species are utterly useless and cause unnecessary pain and suffering to the many planktonic species upon which they feed. "Thus, for the benefit of the majority, we must sacrifice a small minority," he said.

As before, Koshland cogently argued that the War on Drugs must inevitably fail. He feels strongly that drugs should be legalized in a society that prefers to feel happy rather than be sane. He notes the use of L.S.D. will enable Americans to better appreciate the President's thousand points of light, now that smog has all but eliminated the starry skies in America's major metropolises. He argued, "The crisis of our nation's farms is easily rectified if they produce poppies, cocaine and marijuana. The mass export of drugs to Germany, Japan and other potential consumers would eliminate, once and forever, the Medellin cartel, and redress the balance of payments deficit whilst helping American industry by undermining foreign competitiveness."

His proposals for education reform include the elimination of the liberal arts degree on the grounds that the decline of American preeminence in science, industry and the arts is largely caused by too many people knowing too little about too many subjects and who are consequently too uninformed about any problem to solve it. As examples par excellence, he points to the bureaucrats and politicians in Washington DC who are "so ignorant of so many subjects important to this great nation, that they quite are incapable of choosing competent advisors."

■ ANTONY DEAN

Psychology Prize Announced

The American Psychological Association will announce next week that *Science* editor Daniel E. Koshland Jr. is this year's recipient of the B. F. Skinner Award for outstanding contributions to the health of the field of psychology.

The award cites Koshland's acumen in limiting the number of psychology papers appearing in the pages of *Science*, accurately anticipating a downward trend in the number

federal law prohibiting compulsory retirement, action had to be taken.

"Such practices have to be stopped," the source said. "This university is a performance-based institution. You either come to work and do your job, or you're out."

■ MARCIA BARINAGA

NIH Moves Against Conflicts

In a surprise move, the National Institutes of Health announced last week that it was suspending all grants and contracts with the University of California at Berkeley. NIH officials declined to explain the move, saying only that it was necessary because the university had failed to comply with certain paperwork requirements spelled out in the NIH Guide to Grants and Contracts.

"There is nothing more we can say at this point," said an NIH spokesman. "Certain forms typically required in triplicate appear to be missing from the university's file, and until they are provided, we can not legally give them money."

But *Science* has learned that the suspension is linked to possible conflicts of interest on the part of certain senior Berkeley faculty members. The NIH had proposed new guidelines for conflict of interest in the 15 September issue of its Guide to Grants and Contracts, but they were torpedoed by Health and Human Services Secretary Louis Sullivan. It is an ill-disguised secret that NIH officials believed that unfavorable journal coverage of its guidelines had led to their demise.

"The scientific establishment was terrified that we were going to force them to remove their snouts from the federal trough," said an NIH official on grounds that she remain anonymous. "They really cranked up their propaganda machine to defeat us on this. But we're not done. Not by a long shot."

Officials at NIH have repeatedly said that although no formal conflict of interest guideline exist, they still have a right to expect the highest standard of conduct from researchers and research institutions receiving federal money. Many observers see the latest move as an attempt to make an object lesson of a university that has routinely flouted even minimal standards of ethical behaviour. It is well known that many in HHS bureaucracy feel that a university that receives federal funds shouldn't permit its faculty to occupy positions of editorial responsibility.

"Why should we give them money just so they can criticize us at every turn?" asked one disgruntled NIH official.

Although NIH appears to have acted unilaterally in its decision to cut off Berkeley scientists, Berkeley faculty members have never been terribly popular with the HHS Brahmin downtown.

"They're always carping at us," complained one senior official. "First they don't want any money for the genome project, then we're not giving them enough. There's no satisfying these people."

There has also been irritation at what Bush Administration officials perceive as a non pro-family attitude among Berkeley faculty. "Of course abortion is a relevant issue for the NIH director," complained one official. "Many scientists voted for George Bush, and his opinion on abortion has always been clear. That's why it's so vitally important that the NIH director be willing to join the fight against illegal drugs," the official explained.

NIH has set no definite timetable for when the funding for the university might be restored. "We should get to it as soon as we resolve the Baltimore case. Perhaps even sooner," said an NIH spokesman.

■ JOSEPH PALCA

Fight of the Decade: Dingell v. Koshland

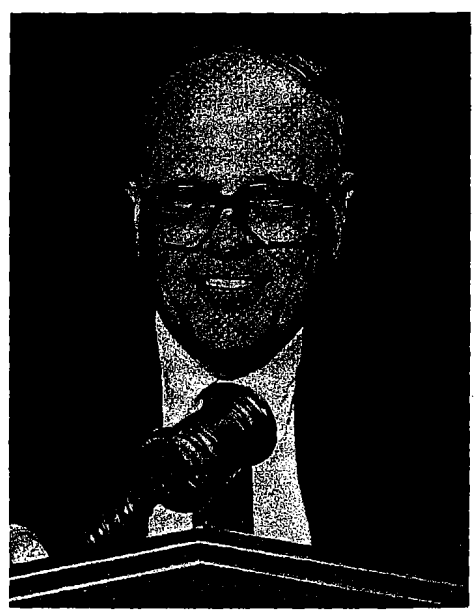
Congress opens investigation of prominent scientist. Tipoff of wrongdoing from suspiciously long CV

BEFORE THE DUST has had a chance to settle on the David Baltimore misconduct case, John Dingell (D–MI) has moved on to new quarry. The powerful chairman of the House subcommittee on oversight and investigation has opened a full-scale inquiry into the publications of another famous scientist and member of the National Academy of Sciences, Daniel E. Koshland, Jr., of the University of California at Berkeley.

As with the Baltimore case, at the heart of the Koshland investigation is the issue of an author's responsibility for the experiments of his collaborators. The Koshland case first came to Dingell's attention when Dingell was anonymously sent a copy of Koshland's *Curriculum vitae*. "I don't see how any scientist can publish 341 papers and possibly know what all of them are about," Dingell told *Science*.

Further investigation led Dingell to focus on a paper, entitled "Purified muscle proteins and the walking rate of ants," which was published in the *Proceedings of the National Academy of Sciences* in 1959 by Koshland, in collaboration with H. M. Levy and N. Sharon (v. 45, p. 785). Dingell says he was tipped off that something may be amiss with the paper, because it was the only one of the 341 that had the name of an animal in the title. "I had a feeling that this guy might have been in over his head with this one," he said.

The famous 1959 paper, which launched Koshland's career and led to his appointment as a professor at U.C. Berkeley in 1965, was published while Koshland was a senior biochemist at the Brookhaven National Laboratory. Sources close to the Dingell committee suspect that no research on



On the attack. *Crusader Dingell*

of psychology papers published anywhere.

"We thought we were still in an expanding period for the field of psychology," said an APA spokesman. "Koshland convinced us that most of the work in this field is properly being subsumed into more serious branches of science."

There had been a heated internal debate over the choice of Koshland as this year's awardee, as many felt that *Science's* decision not to publish many apparently worthwhile psychology papers had actually contributed to the decline of the field. But a clear majority of the executive board (10 to 8) decided that it was unfair to blame the messenger just because they didn't like the message.

"It hurts to be told—even indirectly—that your discipline is hardly worth a bucket of warm spit, but an accurate appraisal is undoubtedly the best in the long run," said a psychologist who asked not to be named.

The Award consists of a stack of brightly coloured plastic tokens which can be exchanged for food rewards in most psychology department offices around the country.

■ JOSEPH PALCA

Time Runs Out for Berkeley Prof

The first professor to have his tenure revoked under a new system at the University of California at Berkeley is rumored to be a biochemistry professor who has for years led a mysterious double life.

The plan for removing tenure from "grossly incompetent faculty" was initiated by a frustrated group of young faculty, who say they are exasperated because old professors refuse to retire.

"They think they can coast along on their past laurels, and no one will notice that they're slipping," said a Berkeley professor who leaked the story to *Science* on the condition that his name not be revealed.

"We have had our eye on this one biochemistry professor for years," said the source. "One week out of every month, he mysteriously vanishes. Does he think no one notices?" The source refused to release the name of the professor, but said the professor's family has been trying to buy him leniency by contributing large amounts of money to the university.

The professor in question turns 70 this March, and the university was willing to look the other way until mandatory retirement forced him out. But with the new

Dramatic Victory for Animal Rights

Animal rights activists last week comandeered a construction crane on the University of California, Berkeley campus, and used it to begin dismantling the biochemistry building.

"We will take this campus apart piece by piece, if we have to," the group wrote in a letter addressed to UC Berkeley chancellor Ira M. Heyman, "until the last captive research animal is freed."

Their cause has been actively supported by Berkeley mayor Lonnie Hancock, who has provided round-the-clock guards around the base of the crane. Hancock has also arranged for daily delivery to the crane-sitters of vegetarian meals donated by the famed Berkeley restaurant, Chez Panisse.

"The whole community has rallied behind this worthy cause," Hancock told *Science*. She pledged continued city support for the crane-sitters, until they finish their goal of dismantling every building on campus that houses biology and psychology research.

For several days after the crane takeover, a group of displaced scientists kept a vigil on the sidewalk beneath the crane. They took turns at a makeshift podium, speaking to the growing crowd of animal-rights supporters with the aid of slides projected against a plywood fence.

"We've given up on trying to reason with the protesters," said Daniel E. Koshland, Jr., who organized the impromptu seminar series. "I have never seen such a scientifically illiterate group. When I tried to explain how my research on chemotaxis in bacteria is likely to cure Alzheimer's disease and other neurological conditions, their eyes just glazed over."

After that failure, Koshland led his band of scientists in an attempt to retake the crane. The effort was quickly snuffed when Hancock called in riot police. Koshland and two other researchers were arrested in the melee, and the others were chased from the scene by an angry mob, shouting "free the fruitflies," and "what does C-kinase have to do with me?"

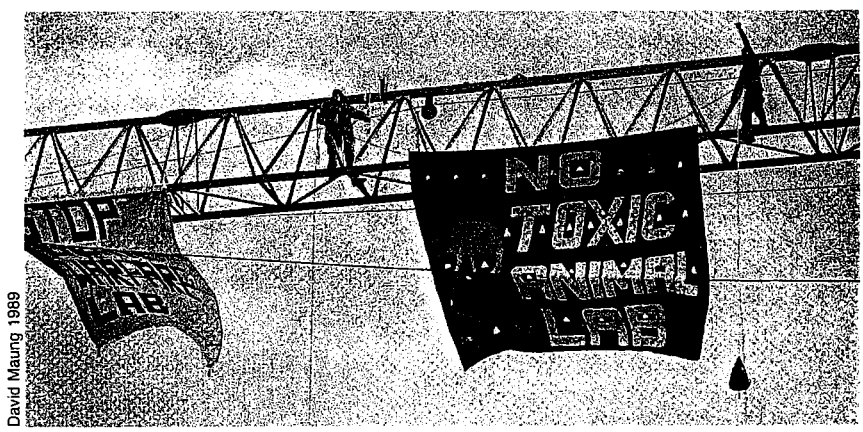
Opposition to animal research, and biology in general, is growing on the Berkeley campus as well. Last month the faculty senate passed a proposal to discontinue biology as a major. "I certainly can't understand what they do, and I have a sneaking feeling they don't know what they're doing either," said a history professor who requested anonymity. "The study of biology represents a barbaric period in our intellectual history, and I only hope this is really the beginning of the end."

Some of the anti-biology sentiment surfacing appears to arise from years of inequality in the university system. "They get hundreds of thousands of dollars to equip their labs as state of the art animal torture chambers," said an English professor, "while the rest of us struggle to make ends meet."

A nuclear physics professor among the animal rights protesters agreed that biologists have had it too easy until now. "The anti-nuke groups forced us to go to Idaho to do our experiments. I don't see why the biologists shouldn't have to suffer a bit too."

But mayor Hancock makes it clear the non-biologists should not be so smug. Hancock says she hopes the "brave action" of the crane-sitters will encourage other grass roots movements to join in. "The university has been an elitist affront to the fine people of Berkeley for too long," she said. "It's our Berlin wall, and it's coming down."

■ MARCIA BARINAGA



David Maung 1989

Fighting Back. *Animal saviors intend to make a crane to freedom*

ants was ever conducted at Brookhaven. "But if they did work on ants at our national lab," said Dingell, "we want to know about it." The committee believes that Koshland himself only conducted the protein biochemistry experiments, leaving Levy and Sharon on their own to perform the crucial experiments on the walking rate of ants. When asked how the experiments were conducted, Koshland replied, "as best as I can remember, Levy and Sharon put the ants on a treadmill, watched how fast they walked, then threw them in a Waring blender and gave the soup to me."

An anonymous source told *Science* that "Koshland doesn't know an ant from a termite, and has never, for that matter, worked on anything more complicated than a single cell. The closest he's ever come to an ant was with a can of *Raid*." Reached at his office on the Berkeley campus, Koshland commented, "don't be silly, of course I know that ants have four legs."

The Dingell committee subpoenaed Koshland's Brookhaven laboratory notebooks, but Koshland claims "my wife must have thrown them out when she was cleaning out the garage."

The data in question led to the conclusion that ants—and by analogy, humans—that walk faster have higher hydrolytic activity of their myosin and thus more advanced enzymes. To a biochemist, this conclusion is profound, and is believed to have kept Koshland moving at such a fast pace right up to his 70th birthday. The results of the paper have been widely quoted in the popular fitness and health-food press for the past 30 years as supporting the notion that people who move faster are more highly evolved. It is believed that a retraction of this paper by Koshland could have devastating effects on the fitness industry across the country.

"I am personally distraught by this allegation about my long-time friend and colleague," Ira M. Heyman, chancellor of U.C. Berkeley, told *Science*, "But I am concerned that the chairman of my Chancellor's Advisory Council on Biology has such a potentially embarrassing skeleton in his closet." In addition to the Dingell investigation, Heyman announced that he will be setting up a blue ribbon commission of Berkeley entomologists to investigate the paper.

■ MARCIA BARINAGA

with reporting by C. S. Goodman, in Berkeley

New Breakthrough in Scaeno-demiotics

FOR 25 YEARS microcellular dramaturgs have confidently predicted sure testing methods for the performability of a playscript.

The problem that has given rise to this quest is well known. Authors may rise hot from the page, suffused with the conviction of success, directors may add dream-like imagery, actors may seduce themselves and their fellow-actors with the cadence of the lines, only to find themselves playing on the opening night to a feedback of coughs, shuffles, and departing feet.

This waste of human resources and threat to productivity have invited widespread research on a testing system which will determine in advance whether a play will be a hit and to detect and eradicate potential lemons. Scientific administrators have been particularly keen to support such a line of investigation because of its obvious commercial implications.

Early efforts in the field received a now notorious setback from the too enthusiastic application of the Geballe-Friedman test in the early 1970s. This team, based on the University of Hillsborough, flushed by their success in decoding the aesthetic principles of interdolphin communication, turned their techniques to the communication of human pleasure or libiotics, as it was popularly known at the time. Because of uncertainties about the side effects, the first tests were conducted in the Third World.

The work used for the experiment was *Only Whiskey Sours*, by promising young New York librettist Daniel E. Koshland, and the location for the trial program was Iran.

Two hundred and fifty Iranian males between the ages of 25 and 35 were chosen as a test group as preliminary to a wider distribution among the population. In the initial runs the results seemed to predict an overwhelming success for the book, and the researchers proceeded immediately to a more widespread distribution.

It soon became clear, however, that initial responses were excessively influenced by two factors:

1) All the preliminary groups were administered a dose of lithium at the same time as receiving the book.

2) The teaching of English in schools had so suffered since the deposition of the Shah (a personal friend of Koshland's) that the test group had mostly not got beyond the front cover and had taken the book to be a tract against the consumption of alcohol.

Wider distribution brought it to the attention of more literate, if not more amiable, mullahs, who quickly discerned in its pages incitement to materialism, sensuality, and a disrespect for the teachings of the Prophet. This led to anti-American riots, public burning of the book, and death threats to the author, who retreated to obscurity in a remote and earthquake-prone part of California.

Despite this setback, researchers Wach and Tell of the University of Lafayette have renewed efforts once again, using the poetry of the said D. E. Koshland. They isolated the enzyme in his work which transforms mere doggerel into lyrics of high artistic accomplishment and mass appeal.

That enzyme once isolated, it has been relatively easy to establish tests to determine whether it is present in the librettos of all those who aspire to write for theater. The researchers expect to be able to produce it in marketable quantities by 1992 and are now setting themselves to the more complex task of engineering the gene which produces this enzyme in the hope that the lasting legacy of the life and work of D. E. Koshland will be that all future generations of poets and librettists will be able to be implanted with his vitality, wit, and imagination.

Two problems alone cloud this optimistic scenario. The American Federation of Screen Writers has taken out a writ in the Supreme Court calling for an indefinite moratorium in the testing and use of these substances. And the Societe d'Auteurs de Paris has called upon President Mitterand to direct the immunological research program of the country to find a defense mechanism against products of the proposed new gene. (In this effort they have conscripted noted American immunologist M. E. Koshland.) Both of these reactions only testify to the likely success of the program. The Americans clearly fear widespread redundancy among their membership once the new drug is developed, and the French clearly see it as the final victory of American sensibility over French culture. (M. E. Koshland's motives are thought to be purely personal. She feels that if all this goes through, she will *never* see her husband at home.)

When the Ayatollah Khomenei heard news of the breakthrough, he died.

■ SEAMUS MAC EACHAIDH
DEPT. OF SCAENIOTICS,
UNIVERSITY COLLEGE, GALWAY

A History of the Illustrious Career of DANIEL E. KOSHLAND

F. H. WESTHEIMER, JACQUES, MONOD, AND MEL SIMON

In order that Science Magazine maintain its reputation as a balanced magazine, one that presents all sides of an issue, the magazine commissioned a history of Daniel E. Koshland as seen through the unbiased eyes of his mentors and competitors.

THE EARLY YEARS BY FRANK WESTHEIMER:

Koshland's career began in my laboratory at the University of Chicago. Despite his pretense of being a sophisticated and well cultured young man, Koshland came to my laboratory with a lot to learn. Therefore, I was quite pleased when he was able to conceive and execute a simple but important experiment, the fermentation of glucose-1-C^{14}. The experiment involved making glucose-1-C^{14} and then fermenting it anaerobically by Fleischmann's baker's yeast. This experiment was a landmark in Koshland's career. First, it was one of the few experiments which he has ever performed with his own hands. Second, Koshland reported that the fermentation yields were 75 to 90%. Not only was the experiment a success, but I am told by all who have ever lived with him that it is the only successful cooking experiment that he has ever done. Finally, in the publication, the crucial data is presented in a table reproduced below:

*p*H	Other conditions	Percent of radioactivity (based on glucose fermented) found in		
		CO_2	CH_3	CH_2OH
6.2	Live yeast	6.5 ± 2.4	92 ± 4.0	4.8 ± 2.4
5.7	Live yeast	1.0 ± 1.8	76 ± 4.0	7.6 ± 2.8
5.7	Dried yeast powder	3.7 ± 1.8	92 ± 7.8	0.2 ± 0.4

The most remarkable feature of this table is the presence of standard errors next to the values. It was the first and only time to my recollection that Koshland ever has admitted directly or indirectly that he is capable of error.

ENZYME KINETICS BY JACQUES MONOD (reconstructed from his memoirs):

Koshland and I were lifelong competitors over the mechanism of action of enzymes. To his credit he started the ball rolling by his seminal paper on the induced fit theory. In this work he examined the ability of beta-amylase to hydrolyze the penultimate linkage of linear starch molecule. Koshland's fascination with this enzyme most certainly began with the observation that this enzyme was able to digest different polymers of sugars such as maltopentose, maltohexose, and maltotetrose. He is to this day envious and respectful of any body or molecule that like himself has the capacity to digest large quantities of sugar in any shape or form. An examination of the data presented in figures 2–5 of his classic paper, "Competitive Inhibition by Substrate during Enzyme Action: Evidence for the Induced-Fit Theory" shows a beginning of a trend in Koshland's career; in figure 2 there are 24 data points, but by figure 5 only 3 data points are present. Despite the paucity of data, these experiments as well as others in earlier publications led Koshland to propose his model of induced fit in which a protein changes its shape when presented with a substrate. While scientific historians still debate what gave Koshland this flash of brilliance, two theories have been proposed. Several renowned historians have noticed that at this point in his life he began to lose the stick figure that he was blessed with. That is to say, an infinitely small but detectable pot appeared near his belt line. These historians suggest that one day Koshland was sitting eating his standard lunch, a peanut butter and jelly sandwich, a chocolate bar and a coke, when he looked down at his belly and noticed that as he bound these different substrates (like the sandwich) his shape (the pot) began to change (grow). "Aha," he shrieked, "maybe enzymes do the same thing when they bind sugar as I do." The second theory suggests that during this period of his life he participated in the conception of his five children with the express purpose of producing an audience for his puns which nobody else seemed to appreciate. To his dismay, he quickly noticed that after each pun his children writhed in a fit of pain; hence, the notion of "induced fit" was born. In any case it was a good idea.

Next came negative cooperativity. We had been fighting for some time as to whether the positive cooperative binding of oxygen to hemoglobin reflects a sequential change in each of the subunits of hemoglobin or a concerted change in all the subunits at once. So he comes up with this hair brain prediction from his sequential model of cooperativity that some enzymes might show negative cooperativity, that is, the binding of a ligand to one subunit might slow down the rate of the neighboring subunit. This kind of perverse scientific thought probably reflects the fact that he let his most perverse daughter work on this project. It should be noted that the trend that began in earlier publications is now very pronounced in his publication "Negative Cooperativity in Regulatory Enzymes" (Levitzki and Koshland, 1969). In this publication there are 9 graphs; only 3 have any data points at all. In the discussion of this paper Koshland notices that "in all cases negative cooperativity is indicated for at least one effector. In many of these cases positive cooperativity with other effectors is also observed." He goes on to suggest that a single unifying theory to explain the diversity of response of enzymes is that a given conformational change will depend upon the ligand that is presented to the enzyme. This theory was even more profound than Koshland first realized because it is

applicable to social as well as hard science when it is stated in its most general form; the diverse states of y can be explained by the fact that the change in the state of y will depend upon the factors to which it is exposed. An example of how this theory can explain many phenomena is evident when applied to Koshland himself. A single unifying theory to explain Koshland's diverse (erratic) political stances is that his political stance changes depending upon with whom he is arguing.

The next seminal contribution of Koshland's to enzyme mechanism came with the discovery of orbital steering. In his paper "Theoretical Aspects of Orbital Steering" (Dafforn and Koshland, 1970), Koshland elaborates on his theory of orbital steering. In order for a reaction to take place, the atoms of the two reacting molecules must collide in a very specific manner. In Koshland's model, enzymes accelerate the rate of reactions by orienting the two reactants such that they collide in an optimal manner compared with random collisions of normal chemical reactions. This paper of Koshland and Dafforn is a landmark paper for several reasons. First, the second paragraph begins with the statement "Orbital steering is defined in operational terms . . . ." This may be the last time that Koshland has begun an argument by defining his terms. In addition, this paper marks the completion of a trend; all graphs in this paper lack data points. Some historians suggest that this model arose during a period in Koshland's life when he would drive into the lab on Saturday mornings and listen to the comedy hour on the radio. The humor so distracted his mind (putting him in another orbit) that he almost caused many collisions with other cars because he was driving directly at them while spaced out on humor. A child (who chose to remain nameless) confided in me that his driving was so horrendous on these days that they coined the term orbital steering. When his driving problem was finally revealed to him by one of his brave children, it dawned on Koshland that, if he could induce the rate of collision of cars by driving directly at them, so an enzyme could induce the rate of a reaction.

CHEMOTAXIS BY MEL SIMON:

Dan's contributions to chemotaxis are so numerous that one can barely begin to chronicle them. He has helped to understand the function of many of the gene products of the CHE genes by biochemistry and even with a little bit of genetics. One of Dan's most brilliant traits is his ability to reduce to very simple terms complex phenomena like the response of an organism to stimuli and its adaptation to stimuli. One of my favorite examples of this genius is in a paper by MacNab and Koshland entitled "Bacterial Motility and Chemotaxis: Light-induced Tumbling Response and Visualization of Individual Flagella." This paper has no tables and no graphs. In fact there is no data. It contains just one cartoon. This kind of science leads to simple science that is very easy to understand. Through this type of rigorous experimentation, Koshland and MacNab "demonstrate" that straight swimming occurs when the flagella remain coordinated as a single bundle and that tumbling occurs when the flagella become uncoordinated and unravel. As Koshland has pointed out himself, the behavior of bacteria and Koshland are not so different. He generally proceeds in a straight line towards attractants like hard sauce, his wife, his sisters, chocolate, a good argument, Cal football, an ugly tie, and the Marx brothers in that order. Similarly, with the appropriate stimuli he can have a spasm (the human equivalent of tumbling); such stimuli include the press, his wife spending money, and lawyers, or one of his children crossing the street without his permission. Today, Dan continues to strive to learn more about behavior but he has now moved on to humans. Given the success he has had in bacteria, it is not clear why he has made this switch. However, some historians have suggested that he is after the ever elusive laughter receptor. With this receptor he could design the appropriate agonist which could be applied to his audiences to ensure their laughter at his atrocious puns.

Science: In fairness, Koshland was given this article to preview so that he could rebut any point. He refused to cooperate. In fact, he became so angry at the contents of the article that he began to yell. He screamed that he would send all of his future papers on the CHE genes to *Nature* so loud that you could hear it all the way to the outer orbits of the solar system. The moral of the story is that Koshland's negative cooperativity induced a fit of orbital jeering that is the CHE to bad SCIENCE.

Similarities in Behavior Among Professors, Graduate Students, and Bacteria: A Case Study

BY CATHERINE P. KOSHLAND
EDITED BY JAMES M. KOSHLAND

This article reports results of a case study, conducted as part of a larger study (1) undertaken to examine the behavior of graduate students engaged in doctoral research. During the early stages of the study, it became obvious that it was essential to examine the behavior of their professors (faculty advisors) as well. In this case study from the lab of Dr. Daniel E. Koshland, Jr., renowned for its study of the behavior of bacteria, we observed an extraordinary parallelism among the behaviors of the professor, his students, and the bacteria. The bacteria exhibit various tumbling and swimming motions that, while appearing random, actually allow them to explore their environment, moving towards concentration gradients of nutrients and avoiding less beneficial environments (2). As this study will indicate, bacteria are an excellent guide to understanding behavior.

IN A RETROSPECTIVE STUDY TO IDENTIFY THE ANTECEDENTS of current behaviors (*3*), we learned that in the early days of his marriage, Professor Koshland was given the opportunity to work with his wife in the same lab. However, the behavior of each in the lab was the antithesis of the other. Professor M. Koshland was meticulous and precise, adding just the right "spice"; Professor D. Koshland throwing this or that in, and drawing sweeping conclusions. Needless to say, the chemistry in the lab wasn't right and Dr. Dan Koshland concluded that it was time to retreat from the lab or "kitchen," a behavior that persists to this day.

From this and other observations, we have discerned that faculty fall into two categories. The first group includes those who venture into the laboratory occasionally to ask a few questions and make a few suggestions. (It is observed that such behaviors while acceptable in the lab are not acceptable in the garden or in the kitchen.) The second group includes those who are very active in the lab and pursue some project of their own. Both groups appear to be healthy and to produce results, although it is not clear why the laboratory environment is preferable to some faculty and less so to others.

Laboratory approach-avoidance behaviors were also observed among students. Students who had not completed a task or were at risk of the faculty member's displeasure went to great lengths to avoid environments where the advisor might be located. In the extreme case this produced a bimodal distribution: those in the lab and those not. Among those present in the lab, more subtle approach-avoidance behaviors were observed. The student wishing to go from point A (the lab) to point C (the bathroom) would take the longest path, zigging and zagging to avoid point B (DEK's office). Similar behaviors have been observed in other laboratories where students will use the stairs farthest from a desirable location such as the coffee pot to avoid passing the door to the advisor's office.

In contrast, it was observed that students seeking an advisor's signature on anything from purchase orders to theses had extreme difficulty locating the advisor. Two explanations are plausible: (i) avoidance behaviors learned as graduate students are employed by faculty themselves or (ii) extensive use of avoidance behavior by students interferes with the normal approach mechanisms.

To test the health of student and faculty approach mechanisms, we took our clue from the studies of bacteria (*2*). We conducted a series of experiments in which various nutrients were placed on the lunch/conference/work table. Professor D. Koshland would avoid the lunch table on the days that tofu and sprout sandwiches from the GoodEarth or low-calorie cottage cheese and simple green salad were offered. However, rapid swimming and tumbling, ending in an appearance of euphoria were observed, if peanut butter, frozen waffles with maple syrup, chocolate milk shakes and avocados were the fare. Graduate students showed some mild preferences. Some responded most favorably to the bean sprouts, and others to the peanut butter. In general it was observed that the graduate students were always hungry, and the highest percentage of them were in the lab at lunchtime. This suggested that there were no problems with the students' approach mechanisms.

We speculated that there may be a correlation between the numbers of students and post-docs in the laboratory and the time spent by the professor in the lab, with the larger labs receiving less time. We hypothesized that the absence of the professor might be related to competition for resources to support the lab. On reanalysis of the Koshland data, it became evident that he did not always appear at lunch, even if Hershey bars were added to the peanut butter and avocados. Patterns of up to ten-day absences were observed. On return, he did not appear more fit, tan, or relaxed, suggesting that vacations in Tahiti or Italy or Paris were not the explanation. A greater availability for signature signing was observed during the first few days after returning. In other labs similar behaviors were observed, with shorter absences correlating with smaller labs.

To locate Koshland during the unexplained absences, we had Mrs. Willie Mae Barrett (*4*) tag his shirts with a radioactive label. The data revealed that he followed a fairly predictable pattern during

Catherine P. Koshland is a professor in The Building Next Door, Blue and Gold University. James M. Koshland is an attorney for Unaware and Filthyrich who edited the manuscript to remove libelous statements.

the 10-day absences. He flew to Washington and spent his time in the offices of a scientific journal, asking a few questions and making a few suggestions. Here too, rapid swimming and tumbling were observed when "letters to the editor" were delivered. We decided that an interview with the subject might shed light on the behaviors. When confronted with our observations, Koshland confirmed our hypothesis about resource competition. Koshland said: "I had to get a second job to feed my lab."

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3. M. E. Koshland, personal communication.
4. Our thanks to Mrs. Barrett for tagging the shirts and throwing out several too-short pairs of green polyester pants.

Long-term Memory in a High-Molecular Weight Organism

HANNIBAL, RINGLING, BARNUM, BAILEY, AND D. E. KOSHLAND, JR.

The enormous brain of the elephant is calculated to hold up to 100^{100} separate memories, with suspected capabilities ranging from higher-ordered differential equations to complex language skills. Current dogma places the elephant very high on the evolutionary ladder with regard to memory prowess. In contrast, we find in the carefully controlled study reported here that the memory of the elephant is no greater than that of a single tethered bacterium which has been flamed for 30 minutes on a sterile loop. To reconcile these contrasting results, we show that upon immersion in 12*M* L-aspartic acid, the elephant sinks to the bottom of the pipet, explaining its failure to swim to the top where it is offered a handful of peanuts. This, then, explains just about everything anyone needs to know about memory, and so new intellectual areas will have to be pursued in the decades to come.

LONG-TERM MEMORY HAS BEEN FOUND IN ORGANISMS AS small as the bacterium and as large as the overweight midwesterner. However, until recently, memory in the largest vertebrate land-mammal, the African elephant (Pachydermus nasokoshlandus) has only been documented in apocryphal scripts (*1*), assorted old-wives' tales (*2*), and in petty fictional works (*3*).

Our laboratory's main interest (besides pizza) is in how the brain works as well as it does, in spite of how gooey it looks when frapped in an expensive laboratory blender (*4*). We therefore dreamt up, during a nine-hour delay at Kennedy Airport (*5*), a series of pie-in-the-sky experiments designed to experimentally address the question, does an elephant ever forget?

African elephants were obtained by poisoned-dart capture at the San Diego Wild Animal Park, and were immediately shipped on ice to the cell culture facility at the Hughes Medical Research Institute in Nairobi, Kenya. In several experiments conducted in our Berkeley laboratory, the African environment was effectively simulated by playing continuous reruns of Mutual of Omaha's Wild Kingdom (*6*) as well as a quadrophonic tape recording of the bongo drummers on Telegraph Avenue. The elephants were then probed by the well-known telephone number test, in which the subject is asked to look up a telephone number and then dial that number by memory at some future time.

To adapt this test to the peculiar physique of the elephant, the Washington phone directory was entered into the computer by the typesetters at *Science* Magazine, and then printed at a 24 pt. font setting. Each letter i was dotted. The elephants were then handed the now oversized phone directory, asked to open it to a random page, close their eyes, and then point to a random listing (*7*). The phone number selected was then recorded on kapok tree bark (*8*),

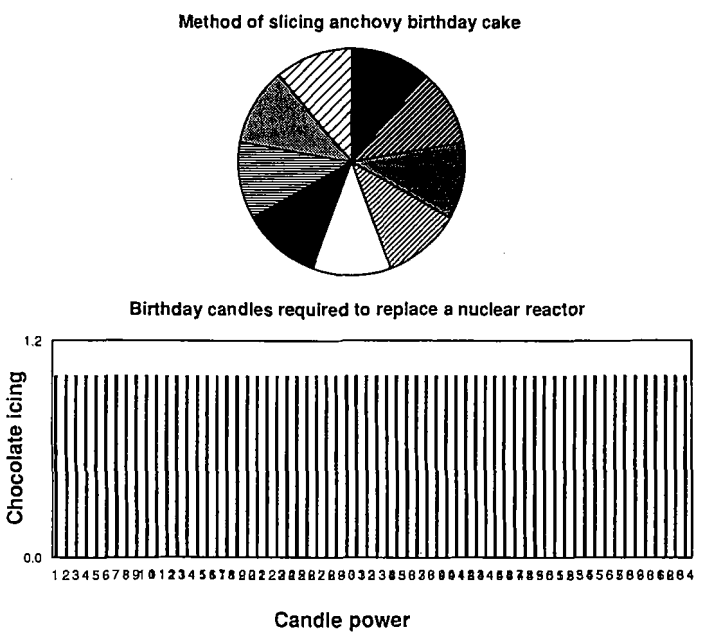


Fig. 1. The elephant really does forget. The x-axis shows something (we forget), while the y-axis shows something about, uh, Snickers bars, or something like that. Oh, yeah . . . . we are only supposed to eat our sandwich and a little slice of an apple, and not a Snickers bar for lunch. And during peach season, we can eat one peach.

and the elephant was told to dial that phone number (*9*). If the phone number reached was a working number, the person at the other end of the line was given an NIH grant, and his household was requisitioned for the duration of the experiment.

To now test the length of time that the elephant could remember the telephone number, we simulated the passage of three-hundred years by having them read several reprints of articles on the subject of orbital steering. After this time, we again asked the elephant to dial the telephone number from memory. Figure 1 shows that no elephant was successful in completing the second call, even though a brown bear was fully capable of doing so. The irrevocable conclusion, then, is that elephants are dumb animals, incapable of remembering even the simplest of modern-day tasks. On a scale of one to ten, ten being equal to E.T.'s ability to phone home, we are forced to make the scientific judgment that the elephant is a big fat zero.

Finally, we note that a possible artifact could creep into a study such as ours if rigorous controls are not applied: for some unexplained reason, a large number of telephone calls are placed through the circuits of AT&T each day by a caller identifying herself as a "bunny". It is therefore of paramount importance to interpret with caution any past reports of repetitive dialing by any warm, furry animal.

REFERENCES AND NOTES

1. Rushdie, The Satanic Verses.
2. Divorced wives have even longer memories.
3. Kipling, The Jungle Book.
4. Lo-fat, sugar-free brain whip is practically tasteless, and so is not considered here.
5. I was *not* late and missed the plane because I ran out of gas and then left my tickets in my other coat pocket. Besides, there are no phones that can reach me when I'm sitting in the frequent flier waiting room, and so I can dictate editorials without harrassment.
6. We thank the estate of Marlin Perkins for the "Unedited Version of the Wild Kingdom", showing the sex scenes of water buffalo which were cut from the original series.
7. Some animals were allowed to say "eeny-meeny-miney-moe," and this was found to have no effect on the important results described here.
8. The kind elephants like to scratch their backs on.
9. Any porno numbers reached by accident were forwarded to the Senate Committee on Fraud.
10. We thank the Oakland A's for the original suggestion that the elephant logo on the players baseball uniforms is there to help the players to better remember just exactly who is paying their salary.

Expression of the *Negotiator* Gene in *D. koshland**

G. M. RUBIN AS TOLD# to L. M. Rubin

IT WAS EIGHT YEARS AGO. I WAS SITTING IN MY OFFICE, staring at the rings my coffee cup had left on my desk, and thinking about flies. Just another day in Baltimore. The phone rang. It was my old friend, Tij, calling from Berkeley. He had an interesting offer. We would discuss it further at a Gordon Conference in a few weeks, but even the bare bones sounded pretty exciting. I went home to tell my wife the news. It was our big opportunity. The big leagues. Like playing for the Celtics, pitching for the A's. MIT, the MRC, Stanford Biochem., Harvard Med. and Carnegie had all been leading up to this. It was kind of like joining the Marines (you know, the few, the proud. . .), an elite. It would take all of our resources, all our skills, but we were ready to try. We were going to negotiate with Koshland.

This was a big one for Dan. His reputation was on the line. The MacArthur Professorship was up for grabs, and it was his turn. The Physics Department had first crack at it, but their candidate was on his way to Texas. Their chief negotiator was back to arguing with freshmen over grades on midterms.

We were already in training; our son had recently celebrated his second birthday, and just getting through each day was one long negotiation. But Lynn thought we needed outside help. After all, the biology buildings at Cal were full of scientists sitting in damp basement closets with four bags of petri plates and a work-study student from the wrestling team, fine scientists who had gone up against Koshland and been vanquished. This was not a negotiation to be entered lightly. The price of failure was too high. I'd been in the Biochemistry Building and seen the toxic waste room.

Lynn headed for the bookstore and returned with *You Can Negotiate Anything* by Herb Cohen. We practiced on the butcher, the kid who mowed the lawn, the babysitter, the egg man. We would have to leave Baltimore soon—everyone was starting to hate us.

I went to the Gordon Conference to confer with Tij. He'd already been through his trial by fire, so to speak, and might be able to provide some useful insights. He made me drive him around the New Hampshire countryside looking for a fishing hole while I picked his brains. Should we hire a private investigator to dig up some dirt? No, Tij had already tried that; the man was a regular Mr. Clean. What about his weak points? Tij said there weren't any. What about Bunny? She'd been getting the best of Koshland for years, but she wasn't talking. I returned to Baltimore with three dead minnows, a sunburn, and very little information. We were on our own.

I'd been reading my book, honing my skills on my son for long enough. It was time to negotiate for real. I headed for California. We decided that Lynn should stay behind. She was so desperate to leave Baltimore she would have agreed to anything, and if Koshland somehow found this out, it would seriously undermine our position. I told him she wasn't at all interested in moving, she loved Baltimore, she didn't see any point in even visiting Berkeley. He bought it. I guess Dan has never been to Baltimore. Anyway, this remained our official negotiating posture—I was all for moving, but Lynn would have to be convinced. I could only hope she wouldn't blow it.

I arrived in Berkeley and checked into the Faculty Club. I had my

*with apologies to Sam Spade
#we all know what that really means

book in my pocket, I was loaded for Bear. We toured Stanley Hall, Dan showed me a closet next to the receiving dock. We toured Barker Hall, Dan showed me the radioactive waste facility. LSB had a broom closet that wasn't in use, and there was some space under the stairs in Mulford.

Dinner that night was at Chez Panisse. He was trying to soften me up. They served barbecued pigeon. I grew up in a Jewish neighborhood in Boston; pigeon was not something I had ever associated with my plate. Things could get ugly. I was glad I had left Lynn at home.

The next night I dined with the Koshlands at their home. Somehow I let my guard down and made what could have been a fatal mistake. I asked Bunny for advice about gardening books to take home to Lynn. Would Bunny infer that Lynn was already planning her new garden? This could ruin everything. The slip seemed to pass unnoticed.

I was getting tired. This negotiation was taking a lot out of me. I would have to watch my step until I went home. That moment came just in time. I was so rattled that I left all my underwear in a drawer in my room at the Faculty Club. Could we rely on the discretion of the housekeeping staff? If this somehow leaked back to Koshland he might sense my weakness and fatigue, and go in for the kill.

I got home in one piece, barely. Then the phone calls started. Dan would call Lynn and tell her stories about Berkeley, and how wonderful it was to live there. (Of course, Dan lives in Lafayette, but a master negotiator never gets hung up on details.) It was always a fight for control. I would watch her turn blue as she tried to keep from laughing. One slip and he would see through her facade. She almost blew it when he called to tell her about the Big Game. Had he somehow learned that she grew up in Boulder, watching Big Eight games all through her childhood, and was fed up with what passed for football at Hopkins? Lacrosse was no substitute. He was finding our soft spots.

Other people were getting involved. Dan wrote to my boss, Don Brown, asking for a letter of recommendation for his file on me. Don responded by offering to recommend Dan for a leadership position in the Institute for the Advancement of Creation Science. Things were heating up.

One night Dan called with a memorable story about his own negotiations with Bunny before they moved to Berkeley many years ago. It seems that Dan wanted to move, and Bunny wanted to stay on the east coast. One night, after weeks of "discussion", Bunny woke Dan out of a sound sleep with an elbow to the ribs. She'd been thinking, she told him, and had realized that if they stayed she would spend the rest of her life making it up to him, but if they moved he would spend the rest of his life making it up to her. They were moving.

Lynn wasn't quite sure how this story related to our situation, but she liked the sound of it. Dan had found the crack in her armor. She was changing sides. I realized I'd better conclude these negotiations soon or I'd be betrayed from within my own camp. Dan seemed as anxious as I was to wrap things up. It was tough, we both fought hard, but I'd like to think we both got what we wanted.

It's been eight years now, and there have been many other negotiations with Dan. But it's like they say. The first time is always the best.

Galvanic Skin Response and the Gustatory Enhancement of Peanut Butter

PHYLLIS A. KOSHLAND,* ROSEMARY A. M. LUKER,
BRUCE A. PUSSELL, SALLY F. ROBINSON

A study of the basal receptor loci in the tongue for maximum appreciation of peanut butter revealed on-site chemical perception of the means of conveyance of the test material. A strong correlation between type of material of implement and appreciation was found. Clinical trials, chemical tests and linear acceleration provided the model of transferred particle excitement by galvanic skin response from the implement to the substrate and then to the basal receptor loci.

In order to maximize the gustatory sensation from the ingestion of peanut butter, basal receptor loci also known as taste buds (BRL) were studied for initial interaction with the substance in order to determine fastest and most effective interface. All brands and styles of homogenates were sampled but a chaotic pattern of results emerged that suggested a preemptive chemical reaction was intervening between the BRL and the induction of the substance. Further tests using a highly selected, homogeneous cohort (*1*) determined that the source of the pre-emptive chemical reaction was the implements used to transfer the substance from its container to the subject. The effort then concentrated on determining the nature of the interference and if it had a positive or negative effect on the reactions of the BRL.

A new test team of rigorously selected, statistically neutralized, living subjects was chosen to participate in the clinical trials. Every age decade, income tax bracket, racial amalgamation and weight grouping (*2*) was represented equally. Further unethical and hereditary factors were deemed to be prejudicial to the precision of the testing and the original 3000 selections were checked for genetic predispositions for peanut butter, i.e., if one or more grandparent

P. A. Koshland, Silknit Institute of Applied Arts, Surrey Hills, N.S.W.
R.A.M. Luker, Museum of Contemporary Art, The Rocks, N.S.W.
Bruce A. Pussell, Prince Henry Hospital, Camperdown, N.S.W.
S. R. Robinson, Creative Wardrobes, Woollahra, N.S.W.

TABLE 1

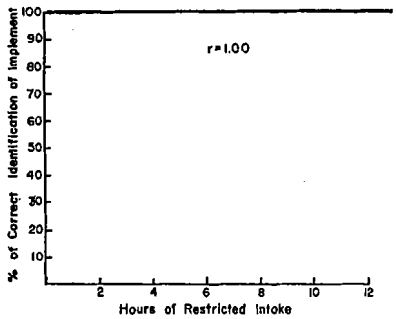
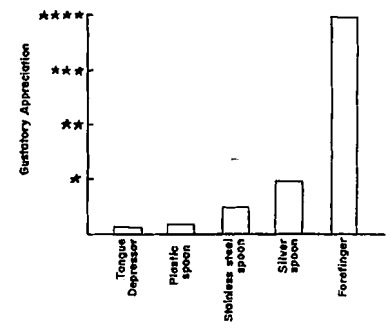


TABLE 2



ate more than one jar of peanut butter in a week; and for unethical uses of peanut butter, i.e., putting the substrate on rival go-cart axles. Environmental conditioning was not considered important except in extreme cases of deprivation or forced ingestion (*3*) but the subjects were tested for adequate short-term memory (*4*) although long-term memory was considered undesirable. The final test group numbering 307 (*5*) were sequestered in the laboratory and subjected, with consent, to rigid dietary controls and minor medical inconveniences and the trials commenced.

Four utensils—heirloom silver spoon, domestic stainless steel spoon, plastic spoon (supplied in bulk by MacDonald's), wooden tongue depressor, and the subject's right forefinger—were compared after 2, 4, 6 and 12 hours of restricted intake consisting of water and/or chocolate. As demonstrated in Table 1, there was little correlation between length of fast and correct identification of the implement. The remaining 301 (6 testees were eliminated for refusing to use their forefingers) then repeated the accuracy trial after having their tongues superficially scraped and then neutralized with Listerine. The testees maintained the level of accuracy, 93.032% with a correlation coefficient of variation of 1%, and the testors then proceeded to assessing gustatory appreciation.

Table 2 demonstrates the vital importance of the proper selection of implement when sampling peanut butter. Subjectively the reasons given by the testees for the enhanced appreciation ranged from the ease of removal of the substance, to dislike of the texture of the implement, to the likelihood of the implement being at hand when needed, and most important of all to a subliminal differentiation in the gustatory essence of the substance. This was the next object of investigation.

Variations of amount of substrate to length of time on implement were examined in order to determine the magnitude and amplitude of the change. However, the inferences were essentially negative: neither duration of contact nor substrate overload altered the order of preference or the degree of perceived enhancement of taste (*6*). Samples of peanut butter from each implement of each testee were then chemically analyzed for the presence of aflatoxins, free radical deterioration, contamination by PCPs, Ag-antioxidants, inadequate H_2O flushing, ligneous splinters and cosmic irradiation. As all of the above were found in each sample, including the control, the research team concluded that a change in the chemical structure of the substrate was being induced by the nature of the material used to convey the substrate and that this change produced the gustatory enhancement.

To access the chemical and molecular structure the peanut butter samples were separated into constituent components by Sephadex 200 columns (*7*). Owing to the large size of the molecular structures involved and the adhesive nature of the bonding it proved difficult to attain handlable and regularly comparable components but eventually with the use of lasers, special reflective glass structures and centripital force a linear and alphabetical configuration was achieved. To the consternation of the test team there seemed to be no chemical differences between any of the samples. However electrophoretic analysis exposed a charge difference in the forefinger samples only, which led to the conclusion that the change was at the atomic or subatomic level and was the result of transfer of energy from the implement, i.e., the forefinger.

To test the exact nature of the charge or particle difference the samples of peanut butter were sent in sealed containers to prevent handler misappropriation of substrate to the Stanford Linear Accelerator. There the samples were bombarded with the primary beam and split into primary particles which were then sorted, collected and compared for energy output (*8*). When the hadrons of the forefinger samples were secondarily accelerated it was noted that they were emitting unusual numbers of quarks and this was the source of the energy differential.

It was then clear that the mechanism for the enhanced gustatory sensation was galvanic skin response (GSR) due to dipping one's finger in the peanut butter. This specific excitement is transferred to the peanut butter in the form of electrical energy, thus propelling the hadrons to superhype states causing them to expel quarks. The quarks are perceived by the BRL producing the subliminal enhancement of the taste of peanut butter.

It was decided to explore further the galvanic skin response in respect to specificity of digit and importance of the fullness of the jar of peanut butter. All digits proved positive for GSR but fore and middle scored best (*9*). The importance of the fractal geometry of the pattern of the dips became clear as the GSR was highest in the middle third of the jar. It is postulated that a negative expression of guilt at the first violation of the pristine condition of the substrate diminished the GSR at the beginning of the jar. Similarly the law of diminishing marginal utility effected the GSR in the lowest third.

In summary life would be much more exciting if everyone ate peanut butter with his/her finger or the quirk is in the quarks for a quick dip in the peanut butter.

REFERENCES AND NOTES

1. All male, aged 14, from middle-class background and no previous experience eating peanut butter or participating in scientific experiments; unfortunately only 5 were found in continental USA, which restricted the use of statistical analysis.
2. Met. Life Insur., "Cohorts, Breakdowns and Stats," vol. 263, no. 8 (1988).
3. Most of the latter had died of dental cancer anyway.
4. D. E. Koshland, The Burnt Coffee test, *J. Ab. Mind Prof.* **70**, 326.
5. This number represents the total years since peanuts were discovered.
6. In the 2-hour trial, testees had to be strapped down for the forefinger test as early tests showed the subjects lacked self-discipline and succumbed to temptation unless restrained. This may explain the slight drop off in appreciation for the forefinger-conveyed substance in the 2-hour trial.
7. This took 2 years, 3 months and 13 days which had a slightly demoralizing effect on the test team.
8. H. R. McCaughey, Festie, Measuring Minimum Energy Expressions, Tallarook Quotidien, 2, 31.
9. The pinkie was eliminated from the trials after 3 of the testees dislocated their fingers from overdipping.

Going My Way

Bacterial Chemotaxis as a Model Behavioral System. DANIEL E. KOSHLAND, JR. Raven, New York, 1980, xiv, 194 pp., illus. $18. Distinguished Lecture Series of the Society of General Physiologists, vol. 2.

In this commemorative issue regarding Daniel Koshland, it would seem appropriate to look back and review this book written by Professor Koshland ten years ago. It also seems appropriate to analyze Koshland's past and present behavior to determine if any of the reasoning and conclusions of the book may be applied to his own behavior.

In Daniel E. Koshland, Jr., the field of bacterial chemotaxis has one of its most visible and vocal advocates. The strength of this book derives from the irresistible enthusiasm about the beauty of biological mechanisms and the direct applicability of observations in bacterial systems to the complex problems of behavior in higher organisms. It is believed that Koshland's theories and enthusiasm have evolved as the result of biochemical reactions to childhood experiments. The two most famous experiments were attempts to shoot a beebee through his new bedroom oak door and his observation of the eating of manure by a manacled female cousin. Other early significant experiments include napkin tossing of butter balls to the roofs of elegant dining rooms and the water pistol shooting of kneecaps of unexpecting women riding cable cars in San Francisco.

The first two chapters of the book introduce bacterial chemotaxis and may well seduce the reader into venturing into subsequent chapters. The final chapter, "Bacteria and higher behavior," discusses observations showing how behavior includes genetic and biochemical components and how defects in these components can result in abnormal behavior. Here, Koshland's scientific observations mirror his real life experiences. Due to the absence of certain genetic and biochemical components, Koshland is unable to cook (frozen peas are his specialty), to garden, to repair any broken appliance or to be generally useful and productive around the household.

The bulk of the volume (chapters 3 through 7) is a description of the current understanding of bacterial chemotaxis, emphasizing the interests and contributions of the author and most importantly, his laboratory. These chapters are directed toward readers with biological training, who will likely find much of interest in them. A chapter on adaptation, a central feature of most sensory phenomena, is weighted toward consideration of mathematical models for adaptation, which may slow down some readers. In lieu of the mathematical models, Koshland should have related to the reader his own adaptative behavior. For instance, Koshland learned earlier in his career after many traumatic events that the laboratory and he did not agree. Moreover, it became evident that the more he stayed away from the laboratory, the more productive were his students. Now, his laboratories are equipped with loud sirens which blare out if Professor Koshland enters the laboratory. This siren both reminds Koshland that he is not where he is supposed to be and reminds the students to watch out for and protect their mentor.

In conclusion, Koshland does a service in trying to communicate to the general reader that the strategy of deducing principles from the study of simple biological systems and applying these principles to the understanding of more complex systems is likely to prove powerful in the study of behavior. However the book would have been more meaningful if the author would have inserted some of his own real life experiences as examples of biological processes affecting behavior. We can only encourage Koshland to write a follow-up volume, relating these examples.

JAMES M. KOSHLAND,
Doctor of Law,
DEK School of Life

SCIENCE FROM THE INSIDE

(Science *is pleased to bring a new feature to its readers to keep in touch with inside developments.)*

We members in good standing of the *Escherichia coli* Koshland strain object to all the publicity we've received lately. In particular the rising interest in chemotaxes is keeping us spinning our flagellae looking for less taxing shelters. If the unprecedented verbal diarrhea continues we will seek alternate pathways and rely on Nature as a second messenger. Will Koshland ever learn that overexpression is equivalent to absence!

Despite this phenotype, we continue to be attracted by his intestinal fortitude. We look forward to many more cultured editorials, meaty papers and positive gradients to lead us. Koshland, we love your guts.

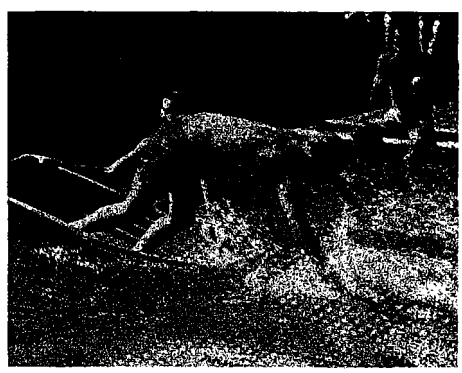
Congratulations from
A.P. and T.H. Geballe,
Coligues

Bloopers to Study By

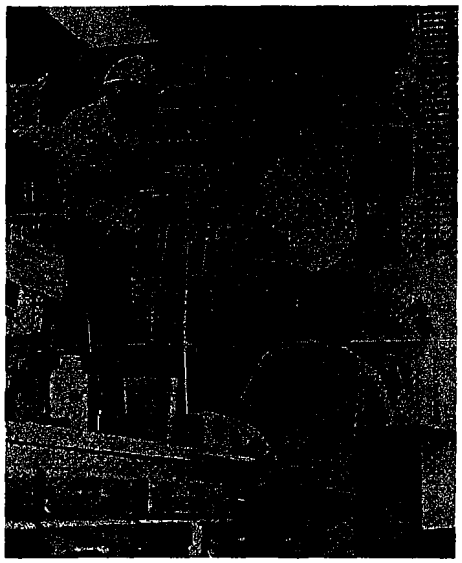
The Koshland Sports Bloopers Science Video. Joseph Six Pack Productions, Inc., Hollywood, CA Color. 27 min. $19.95.

As part of the recently announced AAAS policy to address global competitiveness concerns by bringing science to the masses, an imaginative sports video has just been released. Nobel laureates and Monday night couch potatoes alike will welcome this hilarious addition to their video libraries. The astonishing breadth of Koshland's sideline athletic career is revealed in a compendium of clips which is used to illustrate basic physical principles. The footage spans 25 years beginning with his unsuccessful two-person co-ed bobsled trial for the 1964 Olympics with his long-time partner Marian "The back-seat-driver" Koshland. Recent

Koshland warms up with the Oakland A's: F=ma.



Swimming pool acrobatics: Archimides' Principle.



With skydiving manual in the lab: g=9.8 m/s^2.

takes include a bloody fencing match with his rival John Madmax, Duke of Nurture, at Lord's Cricket Ground, London.

Each embarrassing clip is accompanied by a voice-over explaining a physical principle in play. Vector forces, buoyancy, and acceleration are just some of the topics succinctly covered by the narrator, ex-sportcaster Ron Reagan. Perhaps the most memorable moment is an excerpt from his 1986 interview with Howard Cosell. When asked why he pursues so many sports he responded. "It beats silly hobbies like book collecting and it keep me slim and chic."

H.-P. AIRMANN

Memory solved!

Global Cosmic Memory. DANIEL E. KOSHLAND, JR. Enquirer Press, New York, 1990. iii xxx, 70 pp., illus., $19.20.

The search to understand human memory has preoccupied countless lost souls this past year. Warped individuals have attempted to solve memory by looking in the deepest, darkest corners of the universe. All those foolish fellows studying brain slices, invertebrate worms, and animal behavior were all simpletons not grasping reality.

Koshland's new-age treatise on global cosmic memory has very simply explained every aspect of human memory and consciousness. "It's like remembering a phone number. The more times you say it, the better you *think* you remember it," Koshland expounds. The key to Koshland's success in solving memory is positive feedback, remembering to remember. Koshland's memory theory demonstrates how repetitive, repetitive reinforcement is important important in remembering remembering. "Suppose someone tells you your brother died and at the same time hit you over the head with a hammer, don't you think you'd remember?!?"

It is astonishing how closely his theoretical model ~~confuses~~ explains experimental data. Professor Oscar B. Urger at the Cholesterol Research Institute has done extensive Snickers analysis on Koshland's model and found a correlation coefficient of 0.987 to all ~~un~~known data. "This is the best fit of a model to experimental results, or was that experimental results to a model. . .huh, I forgot, oh well, what was the question again?" declared Dr. Urger.

Packed full of easy-to-relate-to analogies, the beauty of *Global Cosmic Memory* is reflected in one of Koshland's own life experiences. At the age of 5, Koshland discovered that pounding a square peg into a round hole produced tight binding and then realized that fits could be induced. It is this type of insightful wit that destines his book, *Global Cosmic Memory* to be a classic.

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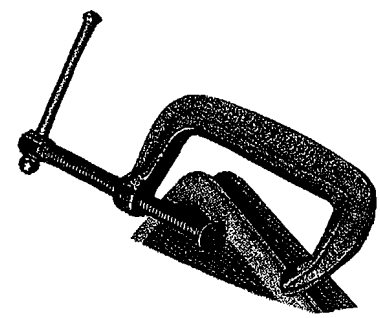
Products & Materials

Eye of the Beholder

A California–Washington firm has introduced the all-purpose laboratory calibrator for all disciplines and all types of scientific experiments. The new Niner (Model



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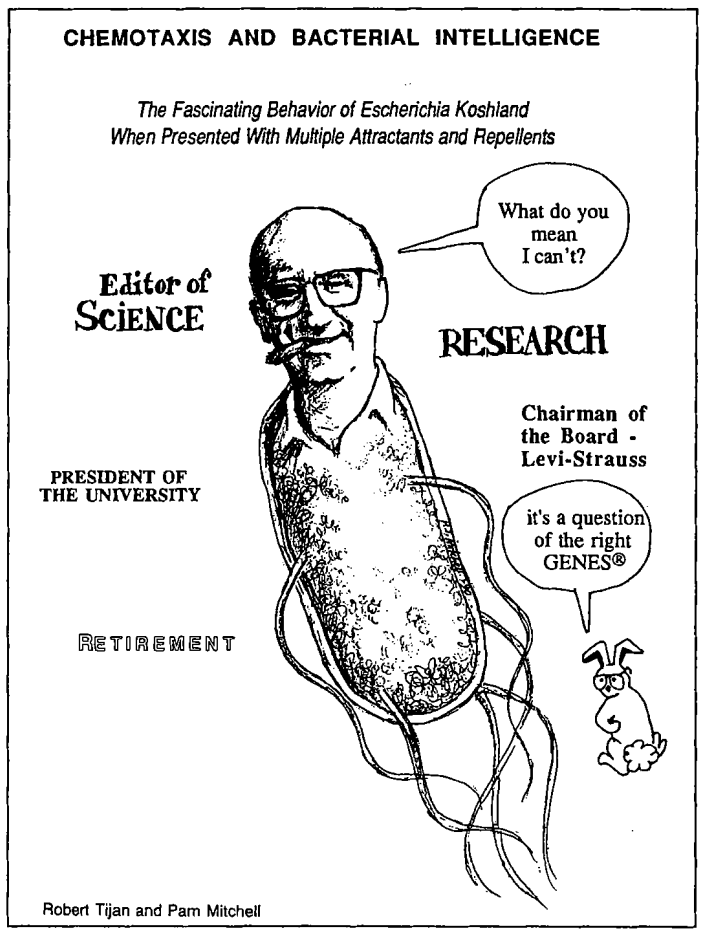

THE BEHAVIOR OF FATHERS

Toward maple syrup he would swim
no matter what you plunged him in.

Through noxious opera or disapproving stares
he'd not tumble or spin
but go on to chocolate sauce or peanut butter.

The power of "the code" this clearly proves
since there's been no impact
from a lifetime of good food.

Now the choosing of his mate
also went like substrate to template.

She's neat; he's disorderly,
he's gush, she takes no bunk,
she's behind the scenes, he's upfront.

A complementary binding, like hand to mitt,
You could say it was an induced fit.

If all his behavior went like this
we'd be in scientific bliss.

But such a man of logic, reason, progressions
must, it seems, have moments of regression.

I can't explain these lapses
But they sure did cause mishap-es.

Like the time our science wizard
, forgets his daughter
at square-dance in a *blizzard.*

Or what about the strange taxi ride
cruising Manhattan, hoping
the hotel forgotten, his memory to chide.

As you'd expect, he was a great math coach
teaching probability beyond reproach.

Then why sailing in light wind, no rain
did we go prepared for a hurricane?

Sure, we got morals morals, MORALS
in practice for all those editorials

The Science staff may think he's straight
But I've seen him in a game of license plates

Then, to win, rules go out the doors
as he takes little airport detours!

Now that we've come to maturing,
some signs are reassuring.

Success, honor, respect
as befits his age, it's correct.

But beware of that look that's wild
for he's still got the mind of a child.

Using books to build ramps,
speeding down slides on greasy pants

I conclude there's only one explanation
for all these aberrations.

He's an ENZYME-for-FUN
a catalyst of life's occasions.

You can argue my sample is only one,
but I wouldn't trade him for anyone.

Love
Ellen K

Personnel Placement

POSITIONS OPEN

RESEARCH ASSISTANT. Publication executive needs researcher to identify and document controversial issues for use as topics for editorials in a prestigious weekly scientific magazine. Position allows latitude in choice of research area, but specific emphasis will be placed on those topics generating maximum public outcry and mobilizing conspicuously vocal special interest groups. Successful projects in the past have included animal rights, euthanasia, nuclear energy plants, legalization of drugs, and radioactive waste disposal. Please send letter of application and proof of participation in police-monitored protest rallies to: **The Editor, SCIENCE, 1333 H Street, N.W., Washington, DC 20005.** *An Equal Opportunity Employer. Members of lunatic fringe groups are encouraged to apply.*

CHAIR
ORBITAL STEERING COMMITTEE

The National Aeronautics and Space Administration (NASA) invites applications for the position of chair of the newly formed orbital steering committee. The candidate will be responsible for overseeing a variety of research programs ranging from planetary chemotaxis to nutritional sciences (induced fat). In addition, the candidate will give a yearly Strate of the Committee report and monthly sub-strate reports. Excellence in administration and leadership is important, but the ability to smooth-talk senators on the Ways and Means Committee is required.

For further information regarding these positions, telephone: 555-SNI-CKERS. For prompt consideration, send curriculum vitae, pertinent reprints, names, addresses and telephone numbers of forty-seven references by 1 April 2001 to:

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Major scientific journal has immediate opening for an experienced statistical consultant to augment our present staff. We are seeking in individual with specific expertise in adapting sociological samples (i.e., control groups) into a format resembling scientific support data, in order to justify publication of lengthy social science manuscripts solicited by and insisted upon by the editor. Position requires a high degree of creativity in rendering such data acceptable to readers in more traditional scientific disciplines. Incumbent reports directly to the editor, as lesser authorities on the staff have declined responsibility for this project. Some editing skills necessary, but on-the-job training can be negotiated. Objective myopia a plus. Reply with curriculum vitae, reprints of at least five original works of fiction, and names and addresses of two anthropologists who can supply references to: **Science, 1333 H Street, N.W., Washington, DC 20005.**

MEETINGS

A. G. BELL INSTITUTE

MARCH 30, 1990 LEARNING TO LISTEN WORKSHOP

The A. G. Bell Institute will again offer a Learning to Listen Workshop, March 30, 1990. This course, sponsored by the Society of the Unheard, has been designed to provide an "ears-on" introduction to the methodology and concepts of current listening procedures for professionals who need to exploit these techniques in order to round out their careers. Included in the curriculum are:

- How to listen to psychiatrists, psychologists, sociologists and other nonprofessionals (extend your range to several minutes beyond your breath-holding capacity);
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Soup kitchens, welfare, shelters are patchworks that fool
Let me you give you a kinder, gentler, more solid rule.

For everything social, psychological, economic
I can handle with ease—even improve the Boston Philharmonic.

My voice is strong; my tongue does blister
I'm chronically fatigued convincing my little sister.

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Is how consistently she doeth love me.

Phyllis Friedman

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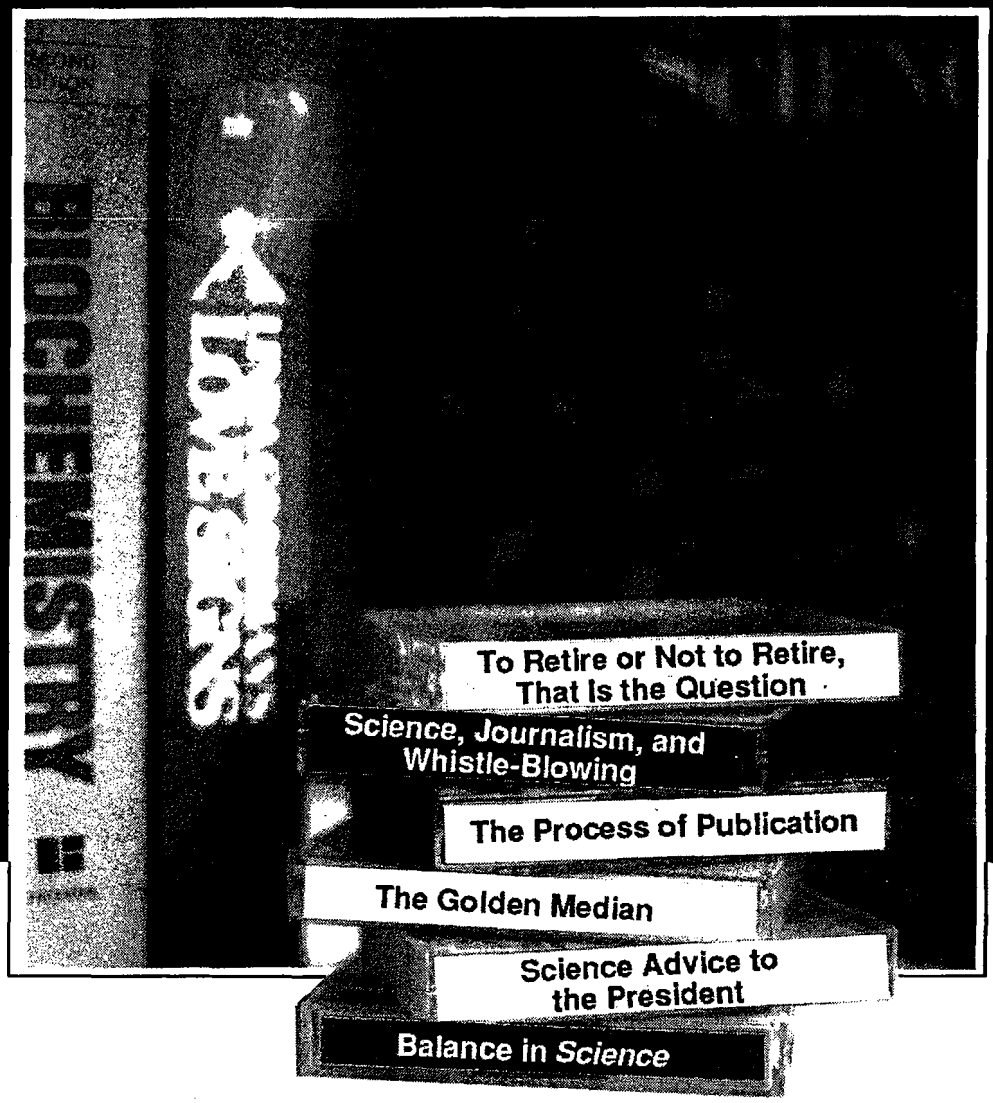

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Submitted by *Charles Ray Wachtel*

James M. Koshland
Partner

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Community/Professional Activities:
- Member, Board of Directors, Foundation for Future, Menlo/Atherton High School
- Member, Board of Directors, Senior Coordinating Council of Palo Alto
- Member, Executive Board, Stanford Law School
- Member, Board of Directors, Levi Strauss & Co.

Education:
J.D. - Stanford Law School (1978)
B.A. - Haverford College (History, 1973)

Joined Firm: 1978

CURRICULUM VITAE

Hugh O'Neill McDevitt, M.D.

PRESENT ADDRESS

Department of Microbiology & Immunology
Stanford University School of Medicine
Sherman Fairchild Science Building, Room D345
Stanford, California 94305-5124
Phone: 650-723-5893 Fax: 650-723-9180

EDUCATION

1952	B.A., Stanford University, Biology (with honors)
1955	M.D., Harvard University Medical School

TRAINING & EXPERIENCE

1955-1956	Intern, Peter Bent Brigham Hospital, Boston, MA
1956-1957	Assistant Resident in Medicine, First Medical Division, Bellevue Hospital, New York, NY
1957-1959	Captain, M.C., USAR, U.S. Army, Camp Zama, Japan
1959-1961	Postdoctoral Research Fellow, Department of Bacteriology & Immunology, Harvard Medical School, Boston, MA
1961-1962	Senior Assistant Resident in Medicine, Peter Bent Brigham Hospital, Boston, MA
1962-1964	U.S. Public Health Service Special Fellow, Medical Research Council, National Institutes for Medical Research, Mill Hill, London, England
1964-1966	Instructor, Department of Bacteriology & Immunology, Harvard Medical School, Boston, MA
1966-1969	Assistant Professor of Medicine (Immunology), Department of Medicine, Stanford University School of Medicine, Stanford, CA
1969-1972	Associate Professor of Medicine (Immunology), Department of Medicine, Stanford University School of Medicine, Stanford, CA
1970-1976	Chief, Division of Immunology, Department of Medicine, Stanford University School of Medicine, Stanford, CA
1971-1978	Director, Clinical Immunology Laboratory, Stanford University Hospital, Stanford, CA
1972-Present	Professor of Medicine, Department of Medicine (Immunology), Stanford University School of Medicine, Stanford, CA
1978-Present	Professor of Microbiology & Immunology, Stanford University School of Medicine, Stanford, CA
1986-1990	Chairman, Department of Microbiology & Immunology, Stanford University School of Medicine, Stanford, CA
1988-Present	Burt and Marion Avery Professor of Immunology

MEMBERSHIP OF SOCIETIES

American Academy of Arts and Science
American Association for the Advancement of Science
American Association of Immunologists
American College of Rheumatologists
American Diabetes Foundation International
American Federation for Clinical Research
American Society for Clinical Investigation
Association of American Physicians
Clinical Immunological Society
Institute of Medicine
National Academy of Sciences
National Institutes of Health
Western Association of Physicians
Transplantation Society
Foreign Member - Royal Society

HONORS & AWARDS

1968-1972	Senior Investigator, The Arthritis Foundation
1972-1973	Russell Cecil Fellow, Arthritis Foundation
1975	Dyer Lecturer, National Institutes of Health
1977	Borden Award for Outstanding Research, Association of American Medical Colleges
1980	Alena Lengerova Memorial Lecturer
1980-1988	Joseph D. Grant Professorship, Stanford University
1981	Passano Foundation Award
1983	Albion O. Bernstein Award
1984	American College of Physicians Award for Research in Medical Sciences
1985	Lita Annenberg Hazen Award for Excellence in Clinical Research
1985	Theodore Lynen Lecturer, Miami Mid-Winter Conference on Immunology
1986	Campbell Lecturer, Asilomar Mid-Winter Conference on Immunology
1986	3M Life Sciences Award
1986	Lee C. Howley, Sr. Prize for Research in Arthritis
1987	Paul Erlich Prize
1987	Rose Payne Distinguished Scientist
1988-present	Burt & Marion Avery Professor in Immunology, Stanford University
1989-1996	Outstanding Investigator, National Cancer Institute
1990	Doctor Honoris Causa, University of Paris VI, Paris, France
1991	K.P. Chang Visiting Professor, University of Hong Kong
1991	J. Allyn Taylor International Prize in Medicine
1992	Paul E. Lacy Lecturer, Washington University
1992	John & Margaret Cochrane Visiting Professor, University of Alabama
1992	Barbara Davis Diabetes Award
1994	Paul Klemperer Award, New York Academy of Science
1995	Elected to Royal Society, London, England
1995	The Albion Walter Hewlett Award
1996	American College of Rheumatologists, Master's Award
1998	Jessie Stevenson Kovalenko Award, National Academy of Sciences

EDITORSHIP

1971-1975	Associate Editor, The Journal of Immunology
Present	Editorial Board: The Journal of Clinical Immunology & Immunopathology, Immunogenetics
Present	Editorial Board, Molecular Medicine

ADVISORY APPOINTMENTS

1968	Diplomat, American Board of Internal Medicine
1968-1972	Member of Allergy & Immunology Study Section, DRG, National Institutes of Health
1972	Member, Panel of Immunologic Intervention, Task Force of Immunology & Disease, NIAID, NIH
1972-1973	Member, Ad Hoc Committee on Tumor Immunology, National Cancer Institute
1973-1975	Member, National Large Bowel Cancer Project, Immunology & Immunotherapy Subcommittee
1974-1975	Member, National Cancer Institute, Immunotherapy Contract Review Committee
1975-1977	Member, National Multiple Sclerosis Society Advisory Committee on Fundamental Research related to Multiple Sclerosis
1975-1977	Member, American Cancer Society, California Division, Fellowship Selection Committee
1975-1981	Member, Jane Coffin Childs Fellowships Selection Committee
1975-1976	Member, Research Work Group, National Commission on Arthritis
1976-1982	Counselor, American Association of Immunologists
1976-1979	Member, Advisory Board: Journal of Experimental Medicine
1978-1982	Council Member, National Advisory Council, Allergy & Infectious Diseases
1981-1982	President, American Association of Immunologists

BIOGRAPHICAL SKETCH

Name: Gail Frances Koshland Title: Lecturer

Education:

University of California, San Diego, CA	B.A.	Sociology	1973
Stanford University, Stanford, CA	M.A.	Physical Therapy	1976
University of California, Los Angeles, CA	Ph.D.	Kinesiology	1988

Post-doctoral training:
1988-1990 With Dr. Ziaul Hasan, Department of Physiology, University of Arizona, Tucson, AZ.

Academic and professional appointments:

1976-1979: Physical therapist, clinical research instructor, chairman research committee, Helen Hayes Hospital, West Haverstraw, NY
1980-1981: Natural science researcher, Rand Corporation, Santa Monica, CA
1983-1988: Doctoral studies supported by NIH funded post-graduate researcher position, Dr.J.L. Smith's laboratory, University of California, Los Angeles, CA
1988-1990: Postdoctoral NIH Fellowship, Motor Control Training Program, University of Arizona, Tucson, AZ
1990-1991: Research Associate, funded by NIH grant awarded to Dr. Hasan, University of Arizona, Tucson, AZ
1991: Research Associate Professor, Department of Physiology, University of Arizona, Tucson, AZ
1990-1995: Research Coordinator at the Arizona Movement and Balance Lab, Rehab Institutute of Tucson, Tucson, AZ.
1992-2000: Assistant Professor, Department of Physiology, University of Arizona, Tucson, AZ
2000-present: Lecturer, Department of Physiology, University of Arizona, Tucson, AZ

Honors and Awards

NIH-- RO1 Award, Musculoskeletal & Orthopedics Study Section	1991-1995
Received a Vernon and Virgina Furrow "Innovations in Medical Education Grant"	1995
Outstanding first-year basic science course--Human Neuroscience	1997-9
Dean's Research Council Grant, Univ. Arizona	1999
Dean's Teaching Scholar, Univ. Arizona	2000

Major Fields of Research

Neural control of multijointed arm movements.
The interface of biomechanics, robotics, and prosthetics with the neurophysiology of multijointed movements.
Alterations in neural control and movement of patients with movement disorders; cervical spinal cord injury, stroke, Parkinson's disease.

Recent Publications:

Koshland, G.F. and J.L. Smith (1989) Paw-shake responses with joint immobilization: EMG changes with atypical feedback. *Exp. Brain Res.* 77: 361-373.

Koshland, G.F., M.G. Hoy, J.L. Smith, and R.F. Zernicke (1991) Coupled and uncoupled limb oscillations during paw-shake response. *Exp. Brain Res.* 83:587-597.

Koshland, G.F., L. Gerilovsky, and Z. Hasan (1991) Activity of wrist muscles elicited during imposed or voluntary movements about the elbow joint. *J Motor Behav.* 23:91-100.

Koshland, G.F. and Z. Hasan (1994) Selection of muscles for initiation of planar, three-joint arm movements with different final orientations of the hand. *Exp. Brain Res.* 98: 157-162.

Miller K.J., Garland S. J. & G.F. Koshland (1998) Techniques and efficacy of physiotherapy poststroke. *Phys Med and Rehab (State of the Art Reviews)* 13: 473-487.

Koshland G.F, B. Marasli & A. Arabyan (1999) Directional effects of changes in muscle torques on initial path during simulated reaching movements. *Exp. Brain Res.* 128: 353-368.

Koshland, G.F. and Z. Hasan (2000) Electromyographic response to mechanical perturbation applied during impending arm movements in different directions: one-joint and two-joint conditions. *Exp. Brain Res* 132: 485-499.

Koshland G.F., J.C. Galloway & C.J. Nevoret-Bell (2000) Control of the wrist in three-joint arm movements to multiple directions in the horizontal plane. *J Neurophysiol.* 83: 3188-3195.

Sherman, S., G.F. Koshland., T.J. Joganich & J. Laguna (2000) Hyper-reflexia without spasticity after infarction of the medullary pyramid. *J Neurological Sci.* 175: 145-155.

Farley, B.F. and G.F. Koshland. (2000) Trunk muscle activity during the simultaneous performance of two voluntary tasks. *Exp Brain Res* (in press)

INDEX—Marian Elliott Koshland

Sally Smith Hughes

Graduated from the University of California, Berkeley, in 1963 with an A.B. degree in zoology, and from the University of California, San Francisco, in 1966 with an M.A. degree in anatomy. She received a Ph.D. degree in the history of science and medicine from the Royal Postgraduate Medical School, University of London, in 1972.

Postgraduate research histologist, the Cardiovascular Research Institute, University of California, San Francisco, 1966-1969; science historian for the History of Science and Technology Program, The Bancroft Library, 1978-1980.

Presently research historian and principal editor on medical and scientific topics for the Regional Oral History Office, University of California, Berkeley. Author of *The Virus: A History of the Concept*, Sally Smith Hughes is currently interviewing and writing in the fields of AIDS and molecular biology/biotechnology.