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A Conversation with W. Allen Wallis

Ingram Olkin

Abstract. Wilson Allen Wallis was born on November 5, 1912 in Philadelphia. He was an undergraduate at the University of Minnesota and continued graduate studies there, at the University of Chicago, and at Columbia University. He held faculty positions at Yale University, Stanford University, and the University of Chicago and administrative positions at Columbia University, the University of Chicago and the University of Rochester, where he was President or Chancellor from 1962 until 1982, when he became Under Secretary for Economic Affairs in the U.S. Department of State, a position he held until 1989. He is now a Resident Scholar at the American Enterprise Institute for Public Policy Research in Washington.

He held appointments of a year or two each at the U.S. National Resources Committee, the National Bureau of Economic Research, Inc., the Ford Foundation, the Center for Advanced Study in the Behavioral Sciences and The White House, where he was Special Assistant to President Eisenhower from 1959 to 1961. He has been a member of numerous government and foundation panels and advisory committees and a trustee or director of many business, educational and eleemosynary institutions.

He was Editor of the *Journal of the American Statistical Association* from 1950 to 1959, was President of the American Statistical Association in 1965, received the Association's Wilks Memorial Medal in 1980 and was elected a Fellow of the American Academy of Arts and Sciences in 1964.

The following conversation took place in his office at the American Enterprise Institute for Public Policy Research in Washington, D.C.

STATISTICS AT AGE 6

Olkin: Allen, thank you very much for agreeing to be interviewed. You've had an illustrious career as statistician, as university teacher and administrator, as a corporation director and as government servant, and I thought that we might divide these areas in our discussion. Perhaps we can start with your background and with the statistical part and how you came into the field of statistics.

Wallis: I never came directly into the field of statistics. It has always been a sideline for me, a secondary interest. I got into it because something else I was interested in brought me to statistics.

I remember several incidents when I was quite young. When I was six or seven years old and lived in Fresno, California, I noticed that Ghirardelli chocolate had two versions, one with sugar and one without, and the one with sugar added was cheaper than the one without. I remember puzzling about why it would cost less with something added, and finally figuring out something about weighted averages. To keep the weight constant, when you add sugar you delete cocoa and cocoa is more expensive than sugar.

Still later—I must have been about 10 years old—I had a newspaper route in Portland, Oregon. They gave a prize for expanding your subscriptions, and I had one of the biggest jumps in the city. My route covered the Reed College area, East Moreland, which was sparsely populated in those days. Most of the subscriptions came from the College students and faculty and the base for this contest was in August. I had about 20 subscriptions then. When the college opened, I had about 80, so I had a big percentage gain. It really meant nothing about my salesmanship, but was a reflection of the poor measure they were using. Anyway, I won a turkey—live.

When I was about 12, Richard Scammon, who has become prominent in election statistics and was director of the Census for a few years, lived three doors from me in Minneapolis. We used to

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make little gadgets that we called "yonnies." They were made with a spool and a piece of soap for a washer and a rubber band and match sticks. You wound them up and they would run along. We had a stable of these and we used to race them, and we would rank them. I learned some things there about the behavior of ranks.

When I was in the third undergraduate year at the University of Minnesota, I majored in psychology. A professor of experimental psychology had done an experiment, which I was a subject in, to determine the effect of color on apparent size. I thought it was stupidly designed and executed and wouldn't prove anything. That raised the question, "How would you do it?" I designed an experiment in which I was able to measure the effect, how much bigger red looked than black, or white than red, and so forth. I didn't know anything about statistics and certainly never heard of experimental design, but the things I did were common-sensical and consistent with what a statistician would tell you. For instance, I randomized the order of presentation. I used the same order for every subject, however, having gotten one random sequence; and I reversed the sequence for alternate subjects to balance out the effect of learning or other trends during a trial. I would present a standard and a series that differed in color and size, and the subjects would say which looked larger, the standard or the other. A probit analysis should have been used to find the 50% point—the LD 50 as they say in pharmaceutical work—which would be the size that appeared the same as the standard. I didn't know about probit analysis. I took successive groups of three points and fitted parabolas to them and then smoothed them by hand. I probably got the right point for measuring the average effect but, of course, it gave me no estimate of uncertainty. Incidentally, that work was my first published article (except that I had published articles and editorials in my high school and college papers). It was published about four years later in the *Journal of General Psychology* under the title "The Influence of Color on Apparent Size."

FIG. 1. *W. Allen Wallis at age 19, upon graduating from the University of Minnesota.*

Olkin: It sounds like you were one of our youngest statisticians if you started at the age of six or seven.

Wallis: When you notice little details, later you can interpret them as having a statistical element.

Olkin: Then you majored in psychology at the University of Minnesota?

Wallis: I was an undergraduate psychology major with minors in sociology and mathematics, but I stayed at Minnesota for a year as a graduate student majoring in economics. The only degree I ever got is a Bachelor's degree at Minnesota, except for the four honorary doctorates.

Olkin: Your first position was at Yale, is that right?

Wallis: That was my first teaching position. From Chicago I came to Washington and worked for an organization that changed its name frequently, but was generally known as the National Resources Committee. Congress abolished it every year and the administration would change the name and transfer the files to the new organization and everything went on just as if Congress had never abolished it. Originally it had been called the National Planning Board.

Olkin: You worked as an economist and statistician?

Wallis: Yes. We planned a large sample survey to study consumer incomes and expenditures. It involved sampling designs and questionnaire designs, analysis and so on. Milton Friedman was working there at the same time. That's when he did his work on analysis of variance by ranks.

Olkin: That work must be his 1937 *Journal of the American Statistical Association* paper, I think. After the '36–'37 year, you went to Yale?

Wallis: Yes. During the summer of '35 I was in Washington. Then in the '35–'36 academic year I was at Columbia doing graduate work in statistics. I went there to study with Harold Hotelling. I had gone to Chicago in 1933 interested in mathematical economics and statistics. Henry Schultz was away the first year I was there. He came back the second year and I thought he was hopeless as far as my learning anything about statistics was concerned. He hadn't been back more than a few weeks before I was looking into where to go to study statistics the next year, and that's how I first met Milton Friedman. Homer Jones suggested that I talk to him. He had been at Chicago in '32–'33 and at Columbia in '33–'34, the first year I was in Chicago, and returned to Chicago in '34–'35. So I looked him up. He agreed that Chicago wasn't the place to study statistics and that Columbia was. At that time Hotelling was practically the only person in the U.S. teaching statistics as we think of statistics today; so I went to Columbia for the academic year '35–'36 and then was back in Washington for '36–'37. I went to Yale in the fall of '37, turning down a higher salary at Indiana. I was at Yale only one year. Soon after Christmas Bernard Haley came east to the annual meetings, but I didn't get to them because of flu. The meetings were in Philadelphia, as I recall, but he and I arranged to meet in New York, and that resulted in my going to Stanford in the fall of '38.

Olkin: You went to Stanford on the faculty of the Economics Department?

Wallis: Yes. I taught an "advanced" course in statistics and an intermediate course in economic theory. Most of the students were graduates.

Olkin: This was '38 and that was shortly before the war. You left Stanford in '46. During the time that the war was on, I know that you were with the Statistical Research Group at Columbia.

Wallis: Well, before that I'd been at Stanford only 1 year when I was invited to spend a year at the National Bureau of Economic Research as a Carnegie Research Associate. Most places wouldn't give you a leave of absence when you weren't on a permanent appointment, but even though I'd only been there a year, Stanford—or anyway Bernard Haley—was very accommodating in doing things to encourage the professional development of the faculty. They did give me leave so I went to the National Bureau for 1939–40. That's when I worked with Geoffrey Moore on runs up-and-down. He was interested in fluctuations in agricultural output and whether they were random. Wesley Mitchell had a draft manuscript which suggested that if cycles in agricultural output were just random you'd have 2 year cycles. Moore thought maybe that wasn't right, which it wasn't. He took the actual numbers from a series on corn production and put them in all possible permutations to see what kind of cycles you'd get. Then he quickly saw you didn't need to deal with the actual numbers. By replacing actual numbers by ranks you get the same pattern of rises and falls.

When he told me that, I saw immediately—having studied with Hotelling, just after his article on rank correlation as a nonparametric test had been published, and having been working with Friedman when he developed his rank analysis of variance—that if you do that, you are on the track of a nonparametric test. So that's what got me interested in nonparametric analysis. Geoff and I published a monograph and two other articles at the Bureau. After that I was back at Stanford for 1940–41, then back at the Bureau for the last 6 months of '41, then back at Stanford again for 6 months.

Olkin: Then you went to the Statistical Research Group?

Wallis: Yes, in July of 1942.

THE STATISTICAL RESEARCH GROUP

Olkin: There is an article in the June 1980 issue of the *Journal of the American Statistical Association* that describes what went on at the Statistical Research Group from 1942 to 1945, but perhaps you can recapitulate some of the highlights on how the SRG started and who were the people there.

Wallis: Well, the SRG was started by Warren Weaver. He was Vice President for Natural Sciences at the Rockefeller Foundation and was working during the war for the Office of Scientific Research and Development. To begin with he was head of the Fire Control Division and later of the Applied Mathematics Panel. They were interested in aiming weapons, especially anti-aircraft guns against moving targets, but also any kind of fire control. As an officer of the Rockefeller Foundation, he was one of the first people to help finance the development of mathematical statistics. He had provided a Rockefeller Fellowship for Sam Wilks to study with Hotelling and also abroad. Wilks had suggested to Weaver that he get Hotelling to head up a group of statisticians. He wasn't too sure what they'd do, but there was a war and there would be a lot to be done; the idea was to put a group together, confident that there would be war work for it. Hotelling suggested me and Jack Wolfowitz; both of us had been students of his. I had already agreed to take on something for the Office of Price Administration, but I backed out of that because Weaver's work was obviously more important for the war. Even before that I had agreed to start a project at Stanford on statistical quality control, which Holbrook Working then organized and directed.

So the SRG met on July 1, 1942. That first

FIG. 2. *W. Allen Wallis in his White House office in 1959, age 46.*

summer we didn't have clearances; even during the war they delayed getting clearances, but it was not as bad as it is now. Hotelling lived in Mountain Lakes, New Jersey, so we lived and worked out there. There was one other person, whose name I don't remember, and he wasn't with us more than 3 or 4 months. It became clear to me that summer, though it wasn't clear when I went, that I would have to be the person in charge of planning and running the group. I can remember what a sinking feeling I had when I realized that, because I didn't have the background for that, but it became clear that the other people were pretty demoralized because clearly we weren't getting anywhere. So we moved into New York City in the fall and for offices rented an apartment on 118th Street and Morningside Drive, overlooking Harlem. Gradually, as time went on, the SRG grew. We rented other apartments; in fact, we and another one of Weaver's groups took over almost the whole building before the war was over. I think Columbia bought it, as a matter of fact.

Olkin: There was really a galaxy of stars at the Statistical Research Group.

Wallis: Yes. It probably was as big and brilliant a galaxy as has been collected at any one place. Of course, we didn't know that then. Most of the people were unknown, even people like Kenneth Arnold, Rollin Bennett, Julian Bigelow, Albert Bowker, Churchill Eisenhart, Harold Freeman, Milton Friedman, Abe Girshick, Millard Hastay, Fred Mosteller, Ed Paulson, Jimmie Savage, Herb Solomon, George Stigler, Abraham Wald and Jack Wolfowitz, who are big names now. They were all unknowns then. Two of them have won Nobel prizes, Milton Friedman and George Stigler. One of them was president of the American Association for the Advancement of Science. Six or eight or maybe more have been presidents of major professional organizations, and two have been presidents of major universities.

Olkin: I seem to recall that one of the main problems at SRG dealt with firing weapons.

Wallis: Well, the first serious problem we had was to decide whether it would be better to have eight 50 caliber machine guns on a fighter plane or four 20 millimeter guns. The 20 millimeter gun shoots a bigger shell and if it gets a hit, the probability of a kill is higher than for a 50 caliber bullet. On the other hand, the 50 caliber guns shoot so many more rounds—eight guns and each at a faster rate of fire—that it's more likely to get hits, though they're less likely to be lethal. So the problem is to figure out which would be better.

Olkin: Did you carry out experiments?

Wallis: We had some data on how fast machine guns fire and on accuracy; also on vulnerability. We didn't actually take airplanes up and shoot at other airplanes.

Olkin: But there were places where they simulated shooting?

Wallis: At places like Eglin Field they fired at towed targets. We got involved with some of that. The physicists told us that the accuracy falls off as the square of the distance. In fact it doesn't. It's got some other relation to distance, which we estimated. We also studied tactics—what kind of acceleration you would encounter in trying to get behind the other airplane. And we got involved in analyzing combat records. There were a lot of data from the Battle of Midway, the battle narratives of Marine fighter pilots. It was interesting to see the difference between statistical analysis and anecdotes. The general anecdotal analysis was that American fighter planes were no good and the Zeros were marvelous because they were so maneuverable. In these combat accounts you'd have the pilot telling how the Zero outmaneuvered him and he could feel the bullets hitting the armor plate back of his seat which, of course, the Zero didn't have. That's one reason it was so maneuverable. Finally our pilot got one burst into the Zero and it exploded. When you put all these stories together, you saw that in fact his plane was doing better than the Zero by sacrificing some maneuverability to gain some ruggedness. It showed the difference between impressionistic evidence and statistical evidence. That's an example of the kind of studies we got into.

Olkin: Most of the statistical material from that period was written up in the book *Techniques of Statistical Analysis* edited by yourself with Churchill Eisenhart and Millard Hastay.

Wallis: No, only a small part of it. The great bulk of what we did was concrete and specific—also classified. We did a tremendous amount of work on rocket propellants, for example. We collected data every month from all the rocket propellant factories, and we started a monthly report which years after the war was still being continued by the people in the rocket propellant business.

Olkin: Were the statistical analyses and recommendations by your group implemented? Did they have an effect on policy?

Wallis: Yes, I think they had an effect. Usually though, it wouldn't be just on policy. For example, fighter planes used to mix different kinds of ammunition, some incendiary, some armor piercing, and some high explosive. Jack Wolfowitz pointed out that we should find out which is most effective and that's the only one to use; and that was pretty quickly adopted. During the Battle of the Bulge the Army was given proximity fuses for the first time, and they didn't know how to use them effectively. Proximity fuses have a little radar that sends out a signal that bounces back from the target. The Army used them for air bursts of artillery shells against ground troops. That doesn't work very well with time fuses because the timing is too sensitive. But a proximity fuse measures how far the shell is from the ground and explodes it at the right height. The physicists had theories about how the fuses behaved, but we had a lot of data from actual tests. So the Army flew some people over here in the middle of the Battle of the Bulge for a meeting in Washington. Milton Friedman had been working on proximity fuses and had lots of well-analyzed data on them, so he went to the meeting and helped to determine the setting to be used on the fuses.

One thing that was clearly carried out according to our work was the inspection system for the Army Quartermaster Corps. They used sequential analysis which, as the *JASA* article relates, was originated at SRG. We also prepared a manual on *Sampling Inspection* for the Navy that included single, double and multiple sampling. In fact, I guess that descendents of that sampling manual are still in effect. I don't know if there's any trace of the one we wrote. After the war they kept revising and amending. The people who worked on it at SRG were good theoretically as well as practically, and our book had a consistency that disappeared later as different bureaucrats compromised over revisions.

THE CHICAGO PERIOD

Olkin: In 1946 you left the Statistical Research Group. The war was over by this time. Did you go to the University of Chicago?

Wallis: I went back to Stanford for about 5 months, the spring and summer of '46.

Olkin: When you went to the University of Chicago in 1946 there was no statistics department. Was there a program at the time?

Wallis: No, not really. There were courses in different departments. I'm not sure about the economics department then. I remember that Tjallings Koopmans was there; also Jacob Marshak, but I don't remember if he taught statistics.

Olkin: This was with the Cowles Commission?

Wallis: Yes. L. L. Thurstone was in Psychology, Sewall Wright in Genetics, Walter Bartky in Astronomy and Dean of the Physical Sciences. I don't remember who else. There really wasn't anybody else in the Business School.

Olkin: Tell me about the origins of the Department of Statistics at the University of Chicago. I gather it was formed in 1949.

Wallis: Yes. Well, Jimmie Savage, of course, had been in the Statistical Research Group and he had gone to Chicago just after I did, the fall of 1947—though neither of us knew about the other. (Milton Friedman went there in 1946, also.) Jimmie had gone to be in the Institute of Radiobiology and Biophysics. That group all felt that there ought to be some organized work in statistics. The field was developing, it was one that universities needed to cover, and it was an appropriate field for a separate department. So we had a committee appointed to study it and they recommended that a department be established, which it was in '49. Originally it had the name "Committee." In Chicago there were two or three, maybe more, groups that were essentially departments, but were called committees, and had all the functions of departments. Chancellor Robert Maynard Hutchins had been proclaiming that there was too much specialization in universities, there shouldn't be so many separate departments, so he was unwilling to create new departments. At the final meeting where this was discussed, Hutchins said it was all right with him as long as we didn't call it a department. We took the line, "a rose by any other name," et cetera, but we rather soon found out that wasn't true, that people in statistics then were generally insecure and people weren't confident that something called a committee was really a department and permanent.

We got some good people, as you know, so we were obviously able to satisfy them, but we spent a lot of time on that. Eventually when Hutchins left, his successor, Lawrence A. Kimpton, agreed to call us a department.

Olkin: In addition to the group that you've mentioned, who were some of the faculty who joined the department in the early days?

Wallis: Bill Kruskal, I think, may have been our first appointment. Raj Bahadur was certainly there early, and Dave Wallace came quite soon. Alec Brownlee was there, but not at first. I don't remember how soon he came, but I think it was a year or two later. Murray Rosenblatt and Leo Goodman were also there early.

Olkin: So the Department of Statistics began to thrive. It had a lot of very good people at the time. You were Chair and then in 1956 became Dean of the School of Business. At that time what relation did the Business School have with Statistics or was the deanship your economics hat?

Wallis: The deanship was an administrative job, and it was not necessarily an economics hat. We changed the character of the Business School drastically in ways that other business schools have subsequently followed to some degree, but at that time it was considered either radical or screwball—or both. There were other people in the Business School who were interested in the Statistics Department, most notably Harry Roberts. He was very active in the Statistics Department. In 1982, 20 years after I left Chicago, they established a W. Allen Wallis Professorship, which George Tsiao holds.

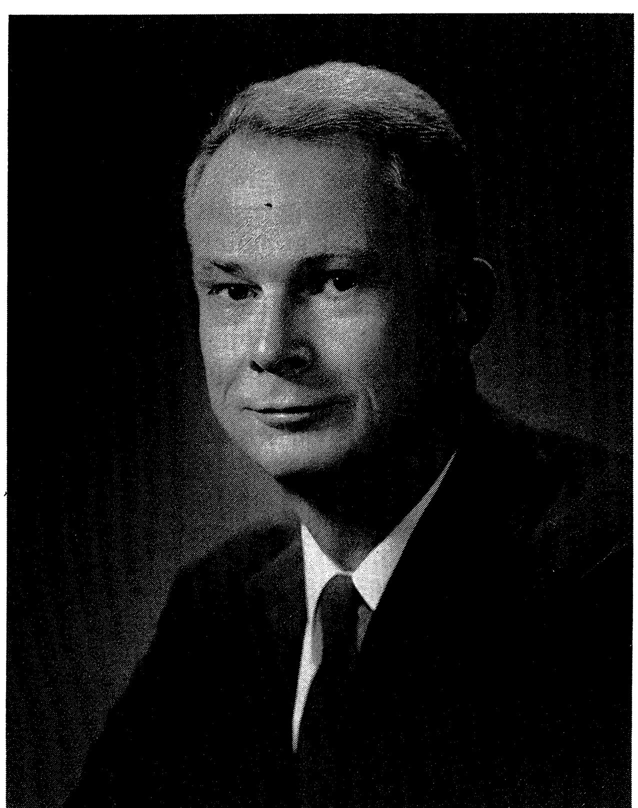

FIG. 3. *W. Allen Wallis upon becoming the sixth President of the University of Rochester in 1962, age 49.*

Olkin: They now have a very strong statistics group in the Business School. It is virtually a separate department.

Wallis: I don't know how much the two groups work together. Before there was a department I taught a course that was taken by students from lots of different departments. Students from as many as 12 or 15 different departments, including even areas such as theology, the natural sciences and all the social sciences. That was just one more reason why we needed a department to organize and serve this kind of function.

Olkin: Allen, I want to go back for a moment to the department because I remember that the University of Chicago had a Rockefeller Foundation grant to bring people in from other fields to study statistics. I thought that was an important educational function that the department served. Did you start that?

Wallis: Yes. Warren Weaver was the one who gave us the money. He was very sympathetic to the project. I remember once when we got a renewal, each of the Fellows who had been in that program had written a letter evaluating his experience, and I remember going over those with Weaver. There was one of them who just talked up to the sky all the wonders he'd accomplished. Weaver was going over this and said, well, the others all seem good, but this guy sounds like a phony. I was impressed that Weaver spotted that, because on the face of it, it was the most enthusiastic report on how great the experience had been. It would be interesting to find out now what's become of those people. I know one of them, Julian Stanley, is very productive in education.

Olkin: He was there when I visited Chicago in 1955–56 and I've known him since then. Julian is at The Johns Hopkins University; he runs the gifted mathematics student program where they search out young children who have very high scores and follow them.

This type of Fellows program is certainly one that we could do well to repeat, now that there is such an emphasis on cross-disciplinary research.

Wallis: If you get good people it should be beneficial to the Statistics department too by bringing in new problems and new points of view on what were thought to be solutions but aren't, and that kind of thing.

THE UNIVERSITY OF ROCHESTER

Olkin: Let us move to a next stage that may be further away from statistics. You became Chancellor at the University of Rochester for a period of years.

Wallis: Twenty.

Olkin: I do know that during your chancellorship at Rochester the departments of Biostatistics and Statistics were formed. How did statistics enter into your dealings at Rochester?

Wallis: A department was created while I was president, but steps had been under way before I got there. I think they had even written to me for advice a couple of years earlier. After we set up the Statistics department at Chicago we had lots of inquiries from other universities about setting up a department, the pros and cons and so forth, and I think Rochester was one of them. At any rate, when I got there, there had been an effort to set up a department but the previous president had vetoed it, but I did get it set up. I didn't play any real role in that. Nothing happened for a long time because they kept thinking that I'd organize it, but I didn't think it was appropriate for the President to organize a department. If he did, he'd have problems in relations with the department later. Once it dawned on them that I was not going to do it, that they'd have to work it out themselves, they got busy and set up the department.

FIG. 4. *W. Allen Wallis taking the oath of office as Special Assistant to the President, March 18, 1959, with Vice President Nixon to his right and President Eisenhower to his left.*

THE FEDERAL GOVERNMENT

Olkin: Now, from that time on, you were involved in a lot of governmental activities at the national level. I'd like to go through some of those, in particular to discuss whether statistics played a role in policy questions. So perhaps we could review some of the important positions that you've held. From 1959 to 1961 you were executive vice chairman of a Cabinet Committee on Price Stability for Economic Growth. Can you describe that activity?

Wallis: That was a committee of cabinet members with Vice President Nixon as the chairman. I ran it for Nixon. We wrote a series of papers on various aspects of economic growth or price control. Several of these papers are published in a book of essays I got out in 1976 called *An Over-governed Society*, a phrase of Walter Lippmann's. One chapter in the book is on economic growth, and mostly deals with problems of measurement, pitfalls of comparisons, difficulties of interpretation, and that kind of thing. There was a chapter on the relation of prices to productivity. There are many references to the accuracy of the measures of inflation, especially the consumer price index, which, as you know, has been considerably improved since those days, but even so, will never be perfect.

Olkin: After that in 1969 and '70 you were on the President's Commission on An All-volunteer Armed Force.

Wallis: In the first place, we at Rochester had a major part in getting that set up. In December 1968 I talked about it to Arthur Burns, whom I had known well and worked with since the late '30's. He was to become the domestic counselor to the President in the new administration. I don't think he had given any real thought to the draft and the all-volunteer army. I eventually persuaded him that he ought to think about it. He promised that he would propose it to the President if we could show how it could be done at a cost of a billion dollars or less the first year. So Martin Bailey, Harry Gilman, Bill Meckling and Walter Oi, all of the Business School at Rochester, prepared some studies and quickly completed a report on this. They had it down to one page, as Arthur requested, but they included about 20 pages of appendices.

When the new administration came in, we saw to it that they all got copies. Several of the cabinet members phoned me about personnel problems, and I would end the conversations saying, "Can I take three or four more minutes of your time for something I want to bring up?" So I'd bring up the volunteer army. All the copies seemed to descend on Mel Laird, who was the Secretary of Defense.

He got to wondering what was going on. Well, there are a lot of other in's and out's of the story. The Defense Department had a plan for raising pay in the armed forces and it was a plan that would raise the pay for the highly paid, the permanent people, and wouldn't leave any money for the draftees. After all, you get draftees free, so why pay them? That was the attitude. Also they were cheap, and they were being used wastefully. We told Arthur about the study that was coming up from the previous administration. George Shultz got into this at one point, too. Burns called in Martin Anderson, who worked for him at that time, and said, "Go over to the Office of Management and Budget and tell them what to think when they get that study." Then Arthur said, well, the only way we're ever going to make any headway on this is to get a high level, prestigious commission whose names will be taken seriously and are not precommitted for or against the draft.

So they established the Gates Commission. It had two four-star generals on it, two or three academics, the head of a big munitions maker, and a couple of Blacks (Blacks were believed to be against the all-volunteer army). Thomas Gates, the Chairman, was a former Secretary of Defense. I've forgotten what the total composition was, but at the start it probably was evenly divided for or against the draft or for or against the volunteer army. It wound up unanimously in favor of an all-volunteer army. From then on things began to move in that direction. The weekend after our report came out, Senator Stennis was on a Sunday talk show and he said, I notice the names of the people on the report and they're all people with their heads screwed on real tight so I guess we're going to have to take it seriously. All the Republican presidential candidates since Eisenhower have opposed the draft, so we at Rochester were not the only, or even the main, advocates of an all-volunteer force. But we did happen to get to the right people with facts and figures at the right time.

Olkin: That really had a major effect on policy?

Wallis: Yes, I think it had a real effect. But there were a lot of forces moving that way. Mel Laird was against the draft; he had been one of the Congressmen who published a paperback book against it. You had other sympathetic people in the new administration, including Nixon.

There was a curious kind of a division on the draft. It was supported basically by the extreme left and extreme right. The right thinks that the army will teach those guys to behave, make them shape up and instill patriotism. We looked into that argument, incidentally, and found that there is no evidence for it. The left generally favors the draft because they are predisposed to government control in general. They like the idea of the government grabbing people and holding them for a couple of years, telling them what to do, moving them around. They don't like the idea of people voluntarily deciding what to do because that goes against the grain. So you get these two extremes. It's an odd coalition.

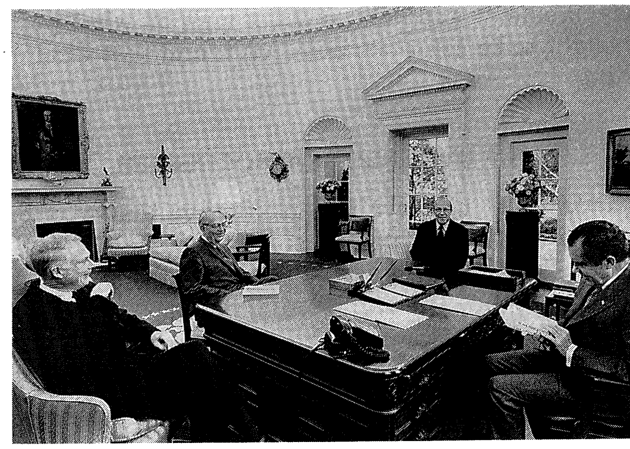

FIG. 5. *President Nixon discussing the report of the President's Commission on Federal Statistics with W. Allen Wallis, Frederick Mosteller, and George P. Schultz (Director of the Office of Management and Budget), February 4, 1972.*

Olkin: Not one that you would have expected?

Wallis: I was surprised about the left. I would have thought that since they're antimilitary, they'd be against the draft, but, no, they regarded it as a socialistic kind of program, a national service.

Olkin: One of your most important activities in terms of statistics was the President's Commission on Federal Statistics which led to the two volumes. *Federal Statistics, A Report of the President's Commission*, that came out in 1971. I think this had a profound effect in lots of other ways. In particular, it generated the Committee on National Statistics. Can you comment more about this project?

Wallis: I see that you have a copy of the report. My favorite page is page 106, that contains a very clever Steinberg cartoon. I think Julius Shiskin was the person responsible for forming that commission. Nixon suggested me to head it because he had known me and he knew I was a statistician, and then I participated in picking the other members. Fred Mosteller was on the commission, and I designated him vice chairman. I used to have him preside part of the time, and I'd preside the rest of the time. Richard Scammon was a member; also John Tukey, Bill Kruskal and several other people I had worked with in the past. Hickman died before we got going. Of course, as with all commissions, a lot depends on the staff. Dan Rathbun headed ours. Subsequently, he was executive vice-president of the American Petroleum Institute.

Olkin: What were some of the recommendations that came out of the commission?

Wallis: Our general emphasis was not on picking specific series, evaluating them and recommending improvements, though the appendices did some of that. We focused on how to design the system so that it would be self-improving. We didn't deal with the current problems that were being faced by government statisticians at the time. When the report was completed, Julius Shiskin was annoyed with it for that reason. He wanted it to deal with his current problems. And I remember that Phil Hauser was apoplectic about it. Partly, he is so political that he opposes anything done under a Republican administration, but the thing that really infuriated him was that we refused to recommend a census every five years instead of ten. Almost all statisticians were in favor of that. The commission I think, first looked at that proposal sympathetically; but some spoil-sport asked, well, just exactly how much difference would it make in what census data are used for? The arguments for the proposal emphasized what large amounts of government money are allocated on the basis of census data, and the arguments against emphasized how little the large amounts would be changed.

Olkin: One of the interesting recommendations deals with confidentiality, a problem which is still important.

Wallis: When the federal government asks you for some information they tell you that it will be treated confidentially. Well, usually they're lying. They don't have the legal power to hold it completely confidential. The Census is one agency that does have that power. Even there it was jeopardized just before the Commission met. The Census asked respondents to the Census of Manufacturers to keep file copies of what they sent so that the next time they could answer questions about changes. Then some agency that couldn't see the census data decided to subpoena those carbon copies from the private companies. In the end either the courts or Congress blocked that. Where confidentiality is respected, it prevents a lot of what would be useful linkages.

Olkin: That's a critical point, namely, longitudinal linkages. In order to answer more complex questions, you need to be able to track or trace individuals and that's one of the problems.

Wallis: You don't care who it is, just that it is the same individual. The problem is to handle the data in such a way that you don't know who the individual is. You just know that some individual has a certain combination of characteristics.

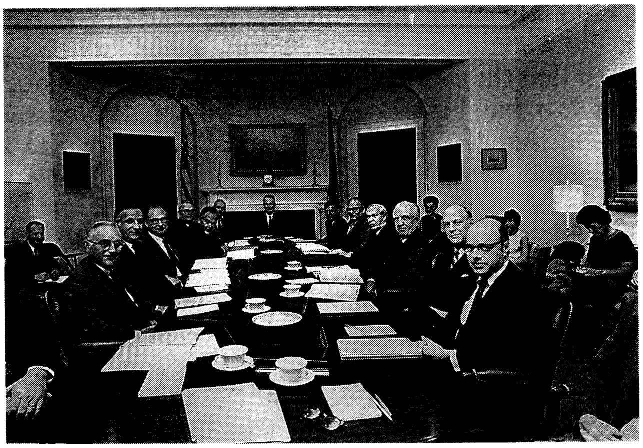

FIG. 6. *First meeting of the President's Commission on Federal Statistics in the Roosevelt Room of the White House, September 26, 1970. Around the table from left to right: Richard M. Scammon (only hands showing), William H. Shaw, Frank D. Stella, James A. Suffridge, John W. Tukey, Frederick Mosteller (Vice Chairman), Daniel B. Rathbun (Executive Director), W. Allen Wallis (Chairman), Ansley J. Coale, Paul M. Densen, Solomon Fabricant, Robert D. Fisher, W. Braccock Hickman, Stanley Lebergott, William H. Kruskal (only hands showing). In the rear on the left: Paul Feldman (Deputy Executive Director).*

THE ENCYCLOPEDIA

Olkin: Allen, to move on to a different type of activity, the *International Encyclopedia of the Social Sciences*. I notice that you were intimately involved with that project from 1960 to 1968.

Wallis: Yes. Barney Berelson, who was a friend of mine from when I first joined the faculty at Chicago in '46 and then later was on our faculty in the Business School, when he was at the Ford Foundation, got me interested in that project. Alvin Johnson, who edited the first *Encyclopedia of the Social Sciences* in the early '30s, had said that it was out of date and there ought to be a new one. He made that proposal to a number of foundations, but only Berelson gave him the time of day. Berelson asked Frank Sutton, who worked for him, to check it out, and Frank talked to some librarians and college teachers and concluded that there really was a need. So Berelson arranged a small grant for a study in the summer of 1955 on whether there was a need for a new encyclopedic treatment of the social sciences, and if so what it should be like.

We had a group of seven very eminent people. Clyde Kluckhone was one; also Jacob Viner, Arthur Schlesinger, Sr., Fred Mosteller, Lyle Lanier and Kingley Davis.

Olkin: So this project was centered at the University of Chicago?

Wallis: Yes. I was the chairman, and Bert Hoselitz was the staff director. Generally, the top scholars in a field would say, no, you shouldn't do

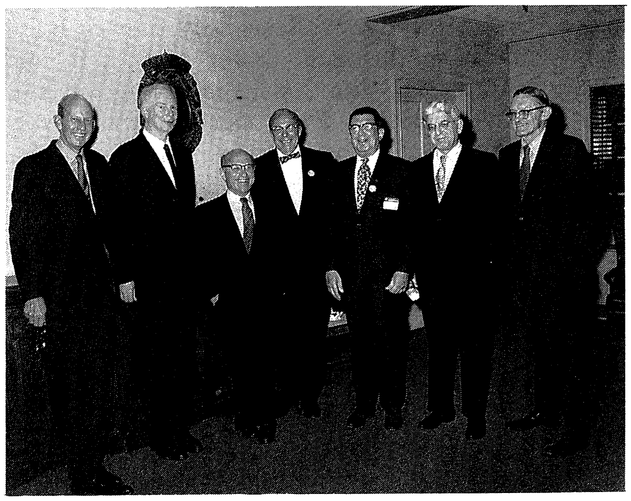

FIG. 7. *Long-time friends meet at the University of Virginia in 1972. From left to right: George J. Stigler, W. Allen Wallis, Milton Friedman, George P. Schultz, Herbert Stein, Arthur Burns, and Homer Jones.*

that. The new knowledge gets out of date so fast that it wouldn't be worth having it in an encyclopedia and it would tie up the top talent in the field writing encyclopedia articles instead of developing new knowledge. Small colleges and other places said, we desperately need it, by all means, do it. Librarians everywhere were enthusiastic.

So eventually, the summer group got out a study of what it should be like. Then nothing happened because the social scientists took the attitude that, we want it financed by somebody in such a way that he doesn't have any control over it; we want to control it totally. Nobody would put up the money on those terms.

In 1962, Jeremiah Kaplan sold the Free Press to Macmillan. He knew about this project, and within a week or two, maybe it was a month, but almost immediately he said, well, we'll publish it. We're prepared to set aside $2 million. We think we'll make money on it. And as I understand it, they did make money on it. As you know, they spun off an *Encyclopedia of Statistics* from it. I don't know how well that has sold.

Olkin: A need must have been reassessed since there's now a new 10-volume *Encyclopedia of Statistical Sciences* published by Wiley.

Wallis: That's as big as the whole encyclopedia of social sciences, whereas this one consisted of two volumes taken from the *Encyclopedia of Social Sciences*, plus some new material. Bill Kruskal and Judith Tanur did it.

STATISTICAL WORK

Olkin: Let me come back to more of the actual statistical part of your life. If your name is known in any general way in terms of technical work, it's in connection with the Kruskal-Wallis test and the Wallis-Roberts book. I wonder if we could go back and discuss some of the statistical material. Do you have particular papers you want to talk about?

Wallis: As I said earlier, statistics was never my main interest, but it's been the center of my attention for considerable periods. It's always been secondary in that I've gotten into statistics through other interests. If you look at my substantive articles in statistics, each of them was stimulated by some outside influence. In your own field, one that's really the heart of your work, one thing leads to another and is sort of self-directing, in a self-perpetuating way, whereas the articles I've done in statistics grew from outside influences. I mentioned the article on the influence of color on apparent size. That was a psychology article but it involved design of experiments. It should have involved probit analysis. It was almost a classical type of problem for statistics, from design to analysis and interpretation.

When I was a graduate student at Columbia I had to write a term paper for Hotelling. I had been in Washington the previous summer, and I was much interested in the Supreme Court decisions on the National Recovery Act and the Agricultural Adjustment Act. Those decisions threw out the basic New Deal economic program. In Washington, the attitude was that the Administration had made a mistake in stalling to delay a Supreme court test. Monday morning quarterbacks said if they'd gotten it to the Court quickly in the first flush of the New Deal enthusiasm and while the depression was still at its worst, then the psychology would have been such that the court would probably have gone along; but this way both laws got thrown out. It occurred to me that the Administration might have had a different strategy. They might have said, it's a close call and if we can stall, we might get to appoint a Justice, and if we stall sufficiently long, maybe we can appoint two. So the question in my mind was, if they could stall for three years, as they did, what number of Justices could they expect to be able to pick?

So I looked up the data on the Supreme Court and decided that a Poisson Distribution would fit the data and did my term paper on that. It was published in the *JASA* in 1936, entitled "The Poisson distribution and the Supreme Court." Recently I received an article from Thomas Casstevens at Oakland University in Rochester, Michigan, citing that article and saying that it opened up a whole new field in political science and citing a series of related articles on the turnover of elites. I noted that the next one after mine was 35 or 40 years later, so I'm a bit doubtful that mine is what stimulated them, but anyway it was a forerunner of the field.

Olkin: It would be interesting to test how well the Poisson distribution fits subsequent data.

Wallis: Somebody brought my data up to date and carried out a test. It fites with the same parameter, one Justice every two years roughly.

When I moved to Stanford in 1938, there was a young psychologist there, John Lyon Kennedy, who was working on extrasensory perception. He talked to me about some statistical data he had, and I saw that what he really wanted was a measure of the degree of correlation when he was using rank correlation. I published an article called "The Correlation Ratio for Rank Data" in the 1940 *JASA*. In the course of working on that with Kennedy I realized that the Friedman rank analysis is of limited usefulness because you have to have two variables. For a single variable, you ought to be able to rank all the data, compute the mean rank in each group and see if the means vary more than by chance. I figured intuitively that it would be a chi-square type of distribution, but the question was what sort of multiple you're going to have to put on the basic computation. Later I tried to interest people in helping me with the mathematics, but I couldn't get anybody to take the bait. When Charles Stein was at Chicago I tried it on him. But when I tried it on Bill Kruskal, he picked it up right away and worked it out. I don't think he found it very difficult, although by the time you read his article in *The Annals of Mathematical Statistics* it's pretty complicated. We published a more heuristic version in the *JASA*. It's one of those things where, once you know the answer, you can plainly see that this is the way it should be. So that is what led to that article with Kruskal. He tells something about that in the article itself. The article itself doesn't say how it originated between us, but it was listed as one of the *Citation Classics* a couple of years ago because it had 650 citations in the citations index. Then we were asked to write a little article on the history of it, which basically Bill drafted.

Olkin: Your paper on the correlation ratio proposed a procedure that's the same as Kendall's tau.

Wallis: Kendall's article in the *Annals* and mine in the *JASA* appeared simultaneously. Maurice Kendall and I had not met at that time. There was no contact between us or any similarity in what stimulated the work. It is interesting to speculate on why his article is well-known and usually cited in appropriate contexts, but mine is unknown and never cited. I can't think of even a paranoid type of explanation.

Olkin: What was your paper called "Statistics of the Kinsey Report" about?

Wallis: There was a group at Chicago that met informally once a month or so. I've forgotten what we called it, but it may have been Social Research Group. We took up different topics and when the Kinsey Report came out I was asked to do a report on it, which I did. Then the American Statistical Association had a session on the Kinsey Report where again I was on the program, so I elaborated what I'd done previously. Then it was suggested that I publish it. It's a terribly long review article, minutely detailed, and hypertechnical. The notes are longer than the text. The text itself isn't so bad, but I made a point of being meticulous in backing up my criticisms in the notes. Kinsey's book is abominable from the point of view of statistics.

That review had an impact. The Rockefeller Foundation had helped support Kinsey's work. Warren Weaver was at first provoked by my review, but then he checked it with the book and said, well, it's even worse than you said. So they held up further funding for Kinsey until his statistics could be reviewed. Mosteller, Tukey and Bill Cochran were appointed to review the work, and they published a book on it. I don't recall that they took exception to any of my criticisms, but there were one or two where they thought that I was pretty picky.

Olkin: On the other hand, it's important for reports such as the Kinsey Report to be very careful.

Wallis: Yes, the book was promoted commerically in a big way and they were taking sort of a holier-than-thou attitude, saying we're not promoting it at all, it's just that the public is naturally interested in the subject. When you read it you get the feeling that Kinsey had some kind of missionary zeal. It isn't clear in what direction. He was a geneticist and knew my next door neighbor in Chicago, Sewall Wright. I talked to Wright once about Kinsey and he said, well, he's a pretty competent, very detailed workman. I don't think he used the word pedestrian, but he gave me that impression. So then I said, well, the data in his book don't hang together at all, and he said, oh, well, Kinsey was never one to count his wasps twice. (Kinsey had specialized in gall wasps.) He'll have a table that says, distribution of 235 men by age, but when you add it up, it's maybe 211. Kinsey explained that to Mosteller, Tukey and Cochran by saying, well, new data came in after the table had been completed. It was an extremely sloppy piece of work.

Olkin: That's an interesting sidelight to the report.

FIG. 8. *W. Allen Wallis in 1978, age 65, with Milton and Rose Friedman, friends since 1934 and 1933, respectively.*

Wallis: There's another example of a paper that was stimulated by outside activity. This one was on compounding probabilities from independent significance tests.

Olkin: This is a hot topic today under the name meta-analysis.

Wallis: The paper appeared in *Econometrica* (volume 10 (1942), 229–248) and was done while I was at the National Bureau of Economic Research. I was working with a lot of economic time series and I was combining significance tests by the method that Fisher gives. I had a feel for the substance, and the results of those tests just did not conform with my feel for the subject matter. So I was inclined to reject the test result. I remember getting into a discussion about it with Rollin Bennett. I was saying, well, there must be something wrong with that test that makes it inapplicable here, and he was saying, it's ok, what could possibly be wrong with it?

Trying to answer that question I said, well, for one thing maybe discontinuity has something to do with it. And in fact that's what it was. That makes a lot of difference in the test. When you have only four or five possible probabilities from a test, the discontinuity makes a big difference. In the course of that, I discovered that what Fisher says in his book, while true, is highly misleading. It gives you the idea that you can take each probability and convert it to a chi-square with any arbitrary number of degrees of freedom that has the same probability, do that for each of the probabilities to be combined, and add them up. This led to ingenious ideas about weighting the probabilities. Actually, all Fisher really has in mind is sort of a mathematical coincidence. When you try to calculate the probability of the product of separate probabilities, the mathematics boils down to a partial sum of a Poisson distribution, and that basically is tabled in the chi-square tables.

That's really an expository article except that it does go beyond Fisher to deal with discontinuities and, as I say, that makes a real difference. The first time I met Churchill Eisenhart, before he even said, how do you do, he said, oh, you wrote one of the few articles that I wish I had written. So I said, I can see you and I are going to get along fine.

THE BEGINNINGS OF SEQUENTIAL ANALYSIS

Olkin: It is well known that sequential analysis originated at the Statistical Research Group. Tell us your recollections of its beginnings.

Wallis: That was dealt with at great length in that article "The Statistical Research Group, 1942–1945" (*Journal of the American Statistical Association*, 75 (1980) 320–330) that you mentioned earlier. Milton Friedman and I got the idea and turned it over to Abraham Wald. We got the idea that a sequential test would be more powerful or more economical than a single sample test.

Olkin: Did that come from the practical problems that you were dealing with?

Wallis: Yes. I was at the Navy Department talking to Admiral Schuyler—he was Captain Schuyler at that time—who had asked us to work on the question of how many rounds you have to fire at Dahlgren Proving Grounds to determine whether one method is superior to another. Ed Paulson and I had worked up a memorandum on double dichotomies, and I went down to see Schuyler, whom I'd gotten to know earlier, to tell him about it. In the course of the conversation he said, you know, one trouble with those people at Dahlgren, they don't have any experience, they don't have any feel for ordnance. If you tell them to fire 2000 rounds they fire 2000 rounds, while maybe after a hundred, one method would miss every time and the other hit every time, and any fool could see it isn't worth finishing the 2000 rounds. There ought to be some formula to tell them to stop.

Later I got to thinking about that and talking to Milton Friedman. Milton was still living in Washington—he had been working for the Treasury—and was commuting to New York. Going between New York and Washington we'd usually get a drawing room on the train so we could talk confidentially. I brought this idea up with him and said, I think it makes a lot of sense and might have a lot of applications. We got the idea that, yes, it would really be a more powerful test than the so-called most powerful test, so we called it "super colossal" —which offended Jack Wolfowitz because he had seen a mathematical proof that existing tests were most powerful, so how could there be a more powerful one?

We tried to sell that idea to Jack after we explored it ourselves. We decided it was taking too much of our time from things that we were supposed to be doing. We had one long lunch with him that went on at least three hours. He wouldn't admit there was any sense to the idea at all. Jack was very smart but he was also very obtuse. He didn't catch on to things orally very well, but he wasn't dumb the way some of this story could make him sound. At any rate, we asked him whether he thought it was a good idea to try it on Wald. His attitude was that we would be wasting his time, but it was up to us. So when we got Wald over, I think Jack had almost certainly prejudiced him against it, and he said he didn't think it made any sense. But Wald phoned me the next day and said he had thought about it some more and would admit that it made sense. However, he didn't think it would work, it wouldn't really lead to more powerful tests. Then the day after that, two days after we presented the idea to Wald, he called up and said, not only will it work, but I can show you how to do it. He came over to my office and explained the probability ratio test to us. Hotelling suggested the name "sequential" for the test, and from then on we began working on practical applications.

Olkin: Had Wald proved any properties of the sequential probability ratio test at that time or had he just constructed the test?

Wallis: As far as I know he had just constructed the test. This was only after 48 hours, and 24 hours after he was saying it wouldn't work, so I can't believe he had probed it very far. He derived it by assuming prior probabilities of the two conditions he wanted to discriminate between and then showing that it doesn't matter what prior probabilities you use, you get the same answer independent of the prior probabilities.

I argued that if that's the case, the prior probabilities didn't have anything to do with it in the first place and there must be some more direct way to get the result. He didn't take to that very much at first. Later he worked it out without reference to prior probabilities, but even then when he wrote up the book—the version distributed during the war—he started with the prior probabilities and showed that they cancel out. In those days that was not a way to get to the intuition of a statistician. There was overwhelming prejudice against prior probabilities. E. C. Molina kept publishing things that used them, and Sam Wilks, who was then Editor of the *Annals of Mathematical Statistics*, finally told him not to send any more manuscripts like that to the *Annals*.

Olkin: Was Wald's article prepared for the *Annals*?

Wallis: Yes. Walter Bartky had done something along somewhat similar lines, which Wilks had declined for publication. Wilks heard about sequential analysis, and he quickly rushed Bartky's article into print so it appeared before Wald's. Bartky's paper would have been dead forever otherwise. It is not very similar and wouldn't have been any great loss. Molina's stuff, I think, made some sense. Too bad Molina didn't live long enough to see what happened to Bayesian statistics. I guess Fisher, Neyman and Pearson are the ones who really did in the use of prior probabilities.

OTHER RESEARCH AREAS

Olkin: That was an exciting period in the history of statistics. Is the article on "Time Series Significance Tests Based on Signs of Differences" based on your work with Geoffrey Moore on runs up and down?

Wallis: Yes, and there are two more articles with that kind of title. You can get the results there if you consider each of the variables distributed from 0 to 1 uniformly and then integrate over the appropriate part of the n-dimensional cube. Or you can do it as Fisher does it, by permutations. That is, you can say that the probability that three points will be in rising order is one over three factorial. If you subtract 1 over 4 factorial from 1 over 3 factorial you have the probability of a run up of exactly 3 and no more. We came across Fisher's method much later, after we had done it all by the probability transformations.

Those tests, I found, were often very useful as rough-and-ready tests. Right after the war I was at the Inyokern Naval Ordnance Test Station on the atomic bomb project and they had some data that were suspicious. So I quickly checked them on the runs up and down and was able to tell that they weren't right. They'd been rounded some way. I've forgotten what the actual circumstances were, but I was there without any equipment or tables. In fact, it was a fluke that I was there. They'd sent for three of us to consult. When we arrived they said

FIG. 9. *W. Allen Wallis with Prime Minister Margaret Thatcher at dinner at 10 Downing Street, December 1982. This snapshot was taken by George P. Schultz.*

we didn't have the right clearances because we needed what were called Q clearances because the problem was related to the nuclear bomb, and we didn't have those. They'd call us when we had them. In almost no time they called me. This was in the summer of '46 when I was still at Stanford. I went down. I was the only one of the three there. Nothing was said to explain that, but in the end the guy in charge said, were you born in 1910? I was not, I was born in 1912. And did you work at the Richland Nuclear Project during the war? No, I didn't. And he said, I didn't think so but I wanted to have you here so I didn't question it. Your clearance came because of somebody with the same name.

Incidentally, I did a favor for them, a statistical one. Maybe the most highly guarded secret in the country at that time was the size of our stockpile of nuclear bombs. This was in 1946. So after I was through down there I got hold of the Admiral in charge of the whole thing and said I estimate that we have x-hundred of these bombs. He didn't say anything. He was someone I knew very well, and he was completely poker faced when he wanted to be. He later became a Vice Admiral in charge of one of the long-range missile projects. Years later, I asked him about that. He said, I remember that because you had it almost on the nose. The thing required x parts, 14 I think it was, that went around the actual bomb. (The technical details have been in the newspapers from some of the spy trials.) They were made up like a sphere. They had a fairly high percent of defects in manufacturing them. That's what I was actually working on, a quality control guide. But they put a serial number on each part. Knowing the defect rate, which was pretty high, knowing what the latest serial number was, and knowing how many parts were in each bomb, it was no trick to estimate the stock. I don't know whether spies ever got information on the size of our stockpile. This experience and some articles I came across led me to suggest to Leo Goodman the work that resulted in his papers on serial number analysis.

We were running down this list of articles in my bibliography. There are two that I haven't mentioned. One was on sampling inspection by variables. I remember that Abe Girshick came across an article by N. L. Johnson and B. L. Welch (*Biometrika*, 31 (1939) 362–389). It gave a way of dealing with what are essentially noncentral t problems by using the fact that the mean plus a multiple of the standard deviation is a linear combination whose distribution approaches normality quite rapidly. Using that, you can get around the complications of the noncentral t distribution. We used that during the war and most of what's in the first chapter of *Techniques of Statistical Analysis* was worked out by Girshick. At the end of the chapter there is a section on sequential analysis that I worked out when I was writing up the book. It was one of those situations where you had a nuisance parameter and could eliminate it by selecting some other variables properly. I forget the trick now, but I remember that after I'd gotten that worked out, I got Wald to look it over and had quite a hard time persuading him that it was correct, but he did finally concede that it was. Also I had some exact figures from the noncentral t tables that I could compare with my approximations. That's in the appendix or in a separate section of that chapter.

One reviewer commented that the book was amazingly uniform for a book written by so many different authors. The truth was that I went over every word of it except Hotelling's chapter, and did extensive editing. I felt completely free to edit it. I had stayed in New York for 6 months after the group ended, working on those two books. So you'll find much more extensive cross-referencing and uniformity of style than in most books by many authors.

Then the other article I mentioned is the one on tolerance limits that I did in the Berkeley Symposium, I think in 1950. I've forgotten now how I got on to that idea. It's based on a combination of two different approximations. One was Ed Paulson's approximation to the F distribution. The other was Wilson and Hilferty's approximation to the chi-squared distribution. I've had a number of mathematicians tearing their hair out about combining those two approximations.

EDITORIAL WORK

Olkin: I want to come back to another big part of your role in the statistical profession, namely, as editor of the *Journal of the American Statistical Association*. You were editor for almost a decade. Is it true that you started the Associate Editor format, or had that been in existence?

Wallis: I'm not sure whether they had Associate Editors before, but I certainly started the present system. There were about six associate editors. I referred each article to one of them, and he got reports from two referees. The associate editor drafted the letter to the author and as editor I checked it and sent it, usually without change. We published annually a list of the referees, called "Editorial Collaborators." That would occasionally let somebody guess who refereed his article, but not often.

Olkin: Were there any special highlights during that decade? I think that was a period of real growth for the journal.

Wallis: Yes. One volume was enormous, as a result of the explosion after the war of people finishing up doctoral dissertations or things they had worked on during the war. The *Journal* was becoming an unsupportable financial burden for the Association. It diminished later, but not because we rejected good papers. One cause of the decline was the rise of additional journals—*Biometrics, Technometrics, Industrial Quality Control* and *The American Statistician*, for example. I remember that I was always having to defend the *Journal* at meetings of the board of the American Statistical Association, who said it was too mathematical, and of course what it was then is nothing compared to what it is now. Even *The American Statistician* is more mathematical than *JASA* was in the '50s. Board members would complain, I'm not interested in more than 20% of the articles in an issue. I told them, well, you're boasting if you claim to be interested in that large a proportion of the articles. The field is pretty diverse.

Olkin: It's interesting that you're mentioning this at this time because from 1959 to now, it's over 30 years later, and we hear the same comments that the journal is too mathematical and does not serve the complete spectrum of the society.

Wallis: Well, the complaint I mentioned was more that it's got too much stuff in it. Any given reader finds most of the stuff in it doesn't interest him. Well, that struck me as natural in a field of so much diversity.

Olkin: Do you have any opinions about the fact that the American Statistical Association has only the one publication whereas many other societies have more publications? IEEE has approximately 80 publications for its membership.

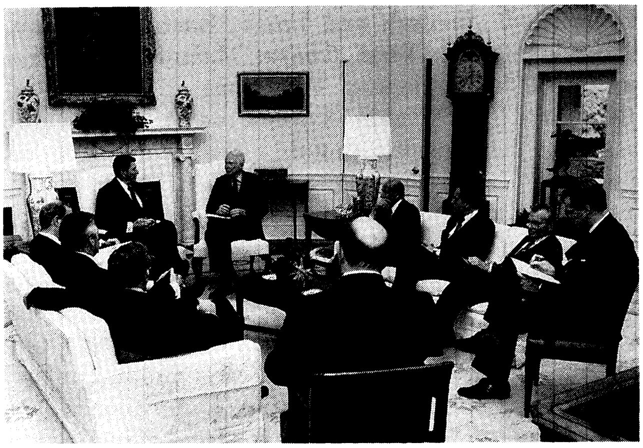

FIG. 10. *W. Allen Wallis and President Reagan discussing on April 24, 1987 plans for the Economic Summit of Industrial Nations (Canada, Federal Republic of Germany, France, Italy, Japan, United Kingdom, United States), which was held in Venice in June. Clockwise from the President and Wallis are: George P. Schultz (Secretary of State), Stephen Danzansky (Senior Advisor for Economics, National Security Council), Frank Carlucci (National Security Advisor to the President), Alexander Platt (Note Taker), Marlin Fitzwater (Press Spokesman for the White House), Howard Baker (Chief of Staff to the President), David Mulford (Assistant Secretary of the Treasury), and James A. Baker (Secretary to the Treasury).*

Wallis: Don't forget that ASA has started or helped start a number of other publications. They helped start *Biometrics, Technometrics* and *The Annals of Mathematical Statistics*, but I don't think they have any control over any of those anymore. I think it better to have diversity of control, rather than a lot of journals under one monopolistic control.

Olkin: Do you have any suggestions for the society or statistical community in general?

Wallis: No, not offhand. It seemed to me that there were a lot of people in the ASA who wanted to use the Association for commercial purposes. They wanted to have licensing. Maybe not quite, but you know, authentication of people's qualifications.

Olkin: Certification?

Wallis: Yes. They wanted to take various steps to improve the economic welfare of the profession by artificially limiting supply, which I never favored.

RELATIONS WITH THE BUSINESS WORLD

Olkin: Are there other subject areas that we should discuss?

Wallis: One subject we haven't covered is business. I did only two small consulting assignments for corporations, but I was a director of nine corporations, Bausch and Lomb, Eastman Kodak, Esmark, Lincoln First Banks, Macmillan, Metropolitan Life, Rochester Telephone, Standard Oil (Ohio) and TransUnion. I was a member of several business associations (notably the Committee on Economic Development), and I was Dean of the University of Chicago's Graduate School of Business at a critical turning point in its history. Also, I made numerous public speeches, mostly to business and professional associations; about 30 of these were published in 1976 in a book entitled *An Overgoverned Society*, a phrase of Walter Lippmann's.

The first business consulting I did involved a critique of a study using least-squares regression and correlation, both of which were mysteries previously unheard of by the lawyers involved—and even by my fellow faculty member who recommended me. The study had correlated X/Z with Y/Z and interpreted the positive result as a relationship between X and Y. The most interesting aspect of this job for me was the look it gave me at lawyers and government regulation—really appalling. (The regulations involved in that case were eliminated two or three decades later, which resulted in spectacular gains in efficiency in the industry.)

The only other business consulting that I recall had something to do with index numbers used in calculations related to some complex tax laws about excess profits, but I don't remember any more about it than that.

Of course, I frequently consulted with academic colleagues, both at Stanford and at Chicago, on their research. Through this, I got intriguing peeks at a number of fascinating subjects, not only in the social sciences but also in such fields as obstetrics, radiology, hematology, physiology, chemistry, physics, geology and meteorology, among others.

When I became Dean of the business school at Chicago its period of great distinction was long past. It was nearly defunct and the university administration was considering abolishing it. At the time, I was considering an offer of a professorship at Harvard but turned it down when two things happened: Jim Lorie agreed to be my Associate Dean (in reality, he became Co-Dean) and Larry Kimpton (Chicago's Chancellor) agreed to double the budget, to transfer the Walgreen Foundation (which was moribund) to the business school and to give me a free hand on appointments. With those conditions, we were quite successful in revising the curriculum, initiating significant research and acquiring faculty—Barney Berelson, Norman Bradburn, Yale Brozen, Sidney Davidson, Paul McAvoy, John McGee, Manning Nash, George Shultz, George Stigler, Lester Telser, Arnold Weber—well, I shouldn't have started naming them because surely I have omitted some stars. We also got a Univac and three mathematicians. We made quite a few joint appointments because if a person wasn't good enough in his field to be welcome in the appropriate department, we didn't want him either. The downside of that policy was that later the school lost some of the joint appointees—Telser to Economics, Bradburn to the National Opinion Research Center, Janowitz to Sociology, two mathematicians to Computer Science, Nash to Anthropology.

FIG. 11. *W. Allen Wallis meeting with President Corazon Aquino of The Phillipines, December 10, 1986.*

The corporate boards fitted in with my interests in economics, business and public policy, but relatively little in statistics—except the oil company, where the geologists did sophisticated mathematical modeling and probability calculations. Hundreds of millions of dollars were invested, or at least offered, on the basis of these calculations. I was first invited, and first became interested, to join a board when I became Dean of the business school.

STATISTICS: A NEW APPROACH

Olkin: I want to bring up two areas, not necessarily in this order, but one is whether you have suggestions for the future of statistics, and the other is I want to come back to your book with Harry Roberts, *Statistics: A New Approach*, which had an impact on the field when it first appeared. We mentioned it but haven't really discussed it. What was your goal in writing that book?

Wallis: Well, basically to try to make statistics more interesting. It seemed to me it was an important subject. I never took a course in elementary statistics. I once started an introductory course at

Chicago, but after two or three weeks, it was so deadly dull that I didn't go any more. It seemed to me that was the way it was usually taught. The people who taught statistics usually didn't know the first thing about it anyway. As Hotelling said in his famous article, (*The Annals of Mathematical Statistics*, 11 (1940) 457–470), it was the blind leading the blind. It seems to me that statistics is something people use every day if they think about it, and could use more often if they had a better grasp of it, and that it has a lot of interesting applications and ought to be taught that way. The book is an attempt to do that. I never taught elementary statistics but I shared an office at Stanford with somebody who did and we used to talk about it a good bit. I tried to talk him into writing a book along those lines.

When I got to Chicago, I taught statistics in the Executive Program. In all my statistics courses I had an advanced graduate student take notes, write them up and distribute them at the next class meeting. Otherwise students were so frantically writing notes that they couldn't concentrate on what I was saying. They were trying to get it down on paper and maybe getting a lot of it down wrong, so when they'd study, they'd study it wrong. I'd go over the notes and edit them before they were mimeographed (later they were dittoed). I did that for several years, so I had these sets of notes of lectures. Students in the Executive Program were middle or upper middle management types, and I had them bring in examples. A lot of the examples in the book were brought in by those people. Harry Roberts was a note taker one year—maybe more than one—and his wife, June Roberts, did one year too. So Harry got working on it that way. We took those notes and worked them up into the book. Harry did most of the collating of the original sets of notes into a coherent first draft. We used it in the Executive Program for several years, continually revising it. Then I did the final version. In the end I spent the entire summer of 1955 doing nothing else but working on it from roughly 10 in the morning until 3 or 4 the next morning. I'd mail stuff to Chicago every morning early and get stuff back in two or three days typed, then revise it again. I started on the 4th of July and finished on the 4th of October. That way I was able to keep the whole book in mind all the time, make cross-references and maintain consistency. We were at our summer cottage on Lake Michigan and I'd go down to the beach for half or three quarters of an hour every day.

Olkin: Well, the book has certainly had an impact in that so many of the examples have been taken over and have become basic examples in other textbooks, in teaching, or as folklore. So in that sense the book has been extremely successful.

Wallis: It sold well and stayed in print for 29 years—a paperback of the first quarter of the book still sells under the title *The Nature of Statistics*. About 15 years ago I picked up one book whose preface was almost the same as ours. I don't know if it was plagiarism, but it was surprising. If you were going to plagiarize anything, the preface would be the last thing. But a lot of people plagiarize, unconsciously, unintentionally.

I had an article plagiarized once. It was on economic growth, essentially the chapter in *An Overgoverned Society*. I picked up a copy of the *Michigan Business Review* from Michigan State and found an article on economic growth. Since I'd been interested in that, I looked at it. The more I looked, the better I thought it was. Even the wording seemed felicitous. So I got Bud Fackler to take it and my article, which had been published in the *Wall Street Journal* and also somewhere else, and compare them. He prepared one of those *New Yorker* style "Funny Coincidences" things in parallel columns running five or six pages. I sent a copy of this to the author, who was at a university. The first thing I heard was from the Chairman of his department, who wrote me that this young man had come in to see him absolutely thunderstruck. He had this thing of mine, and he just couldn't believe it was plagiarism. The head of the department said, after all, if you're going to write about economic growth, there are just certain sensible things to say, and naturally two people writing on that subject would say the same things. That was so absurd that I didn't even answer, I waited to see if the author would reply. Eventually he wrote. He said he was shocked to see this and he said, I don't remember ever seeing your article but it's clear that I did. I still don't know where I could have seen it, or remember seeing it. He explained that during the '60 election campaign he had been asked to speak to a women's club on economic growth. The speech seemed to go over well and he was asked to give it a few more times. It seemed to go over so well that his colleagues said that he ought to include it as a lecture in the elementary economics course. So he worked it up for that. Then they said, it's so good you ought to publish it. So he wrote it up for publication. And that's all he could tell me. It's obviously based on your article, he said, but I still don't remember ever reading it. George Stigler said, well, he must have spent at least 45 minutes to an hour copying it, and if I had spent that much time copying something, I might not remember it at my age, but at his age I would. A publisher told me once that that's how most of the plagiarism cases they have arise. Somebody puts things in his lecture notes and doesn't put in citations. He uses these notes year after year, and they come to seem like his own material. Later he writes a textbook, fully confident that the material is strictly his own. Personally, I am convinced that the young man did not consciously plagiarize my article, so I just dropped the matter. The editor of the *Review* was quite complacent about the matter.

Olkin: Well, in general, I think the notion of plagiarism isn't that clear-cut because you may hear a talk and there may be some aspects of the talk that you don't pay that much attention to but they go into you subconscious. You don't quite need the idea at that time. Years later suddenly you have this idea because that was exactly a point that you needed and you use it and it seems original at the time.

Wallis: Let me mention one thing that I thought of earlier. I was trying to think of who all I had studied statistics under. When I was an undergraduate at Minnesota I took a course called mathematical statistics from Dunham Jackson. It was a classical course on probability, Pearson frequency distributions, the method of moments and things like that. Two years later the same course was given by William Hart. I was still around so I took that too, but I can't say anything of real interest came out of either course. Then at Chicago Theodore Yntema gave a course that I attended. Yntema's technique was to assign an article in some journal to a student and have him report on it while Yntema sat in back doing something else, and I can't say I ever got much out of that.

But then I went to Columbia and Hotelling was very different. In lots of ways he was unique; he would have lectured identically the same way if the class hadn't been there, I think, but nevertheless he was full of ideas and information. And he had a lucidity in exposition that shows in his writing. But on the whole, I'd say I learned more about statistics from working with other statisticians—Milton Friedman most of all, and some from Jimmie Savage and Abe Girshick. When Jimmie came to SRG he didn't know anything about statistics and was eager to learn, but in the process of tutoring him I learned a lot from him. I'd tell him something and he'd tell me it didn't make sense or it wasn't right. Then it would turn out he was correct, or maybe just that my explanation was too superficial. He and Milton spent a lot of time that way, and he and I spent a lot too. Anyway, I'd say I learned more statistics informally that way than in formal courses.

Earlier I said a statistician has to have very good grounding in mathematics these days, so that after the war when I went back to Stanford, there was some thought that because I'd been running a big group, if there was going to be an organization at Stanford, I should organize it. I remember saying it wouldn't make sense for me to do that because I had no mathematical background, and nobody without a mathematical background was going to go very far in statistics. I still think that is true. A lot of what I did was of the "let's you and him do something" type. For example, I got Bill Kruskal and Leo Goodman started on that series of papers on measures of association. I got them going on it because L. L. Thurstone had come to me with a problem. I talked to him about it and got them together with him. I think I gave them at least one idea that they used, although I think the idea wasn't original with me. I didn't really have anything to do with that work other than to get it started.

Olkin: Well, serving as a catalytic agent is really very important.

Wallis: Yes, I'm not being modest or underrating that function.

INTERNATIONAL AFFAIRS

Olkin: Allen, you have been really at the forefront of being a public servant in so many different ways in the federal government in particular. How has statistics arisen in policy in your various commissions or as Under Secretary of State?

Wallis: Well, it's very hard to say. Things come up. George Schultz, when he was Secretary of State and I was Under Secretary, became enthusiastic about a mass screening program for AIDS. I used Example 328 in the Wallis and Roberts book to disillusion him. The best estimate at that time of the incidence of AIDS was about 0.015. That was for recruits to the military, but was similar for some other groups. So unless your test is practically errorless, false positives will swamp the true positives by a huge factor. Of course the people that matter, the people in the Center for Control for Communicable Diseases, know that kind of thing perfectly well.

In my first Presidential Invited Address to the ASA in Detroit in 1970 I discussed nontechnical uses of statistics. Let me give an example. It came up when I was head of the University of Rochester. There was a lot of talk at that time about universities having the student body vote to determine everything under the sun. I made the point that if you do that, universities will become quite homogeneous, on account of the regression phenomenon. That is, if you select people according to one characteristic, such as academic aptitude, you'll get pretty much a representative sample of the population with respect to baseball ability or political views—not completely, but more or less. So every university will turn out to have much the same views on such political issues as how you should organize the university and what you should teach. So you'll have uniformity among the universities. But if you're going to have effective freedom of choice, you need variety among universities. I tried to expound that view a couple of times in speeches at universities, Rochester and others. I don't know how well I expounded it—better than I just did, because I assume you're familiar with it. I think some people catch on to it. I'm not sure it sticks though.

The regression fallacy is one of the hardest things in the world for people to get. You explain it to them and 20 minutes later they're falling into the fallacy in a different example. Milton Friedman claims his master's thesis about railroad bonds is based on the regression fallacy, but the faculty accepted it. I haven't looked at the thesis, but if I am quoting Milton correctly, I'm sure it's true.

Olkin: Can you comment on statistics as a profession?

Wallis: I think there are a lot of ways in which statistics is a good profession for certain people. If you're interested in a lot of different subjects and have a lot of curiosity about things, you can get into almost anything through statistics. Through statistics I've gotten a little exposure to engineering, physics and chemistry. I don't know anything about those subjects, but I have some feel for the people. I can look at issues of *Science* and have some idea of what many of the articles are about. Usually, the biological stuff I don't get, though I did work on one experiment that had to do with cholesterol, vitamin C and so forth.

Olkin: But now you're touching on a point that's critical at the present time, namely, we're not getting as many people into the field as we might. Do you have any thoughts on how we might begin to attract people?

Wallis: Well, how does anybody ever hear of statistics? When I was in college I never heard of anybody aiming to be a statistician, and if I had I would have thought of a green-eyeshade type.

Olkin: That's part of our problem, I think. Or the football stats, as they say.

Wallis: I don't know how you cope with that. I know there have been efforts by Fred Mosteller and others. The book by Bill Kruskal and Judith Tanur *Statistics, a Guide to the Unknown* illustrates a wide variety of uses of statistics. Otherwise, I guess people frequently get into statistics the way I did. If they come in through mathematics, they tend to be interested only in formal problems, not in applications, and not to be very good at finding out what kind of problems would be worth working on or what are useful solutions. Hotelling was an exception to that, a striking exception. You know, Hotelling started out aiming to be a journalist and later was converted to mathematics. He definitely had broad interests.

How many of these interviews have you done and has anybody looked for a common pattern in them?

Olkin: There have now been about 15 published interviews, but no one has studied them for patterns.

Wallis: When I was formally inaugurated at Rochester we gave a number of honorary degrees to people that I'd been closely associated with. Jimmie Savage was one, Harold Hotelling, Frank Knight, Warren Weaver, Dwight Eisenhower, George Beadle and Arthur Burns were the others. Rather than have them just be clothes horses that stand up and have a hood put on them and receive a diploma, we decided to ask each to give a ten-minute autobiographical sketch, telling how he got interested in what he was interested in. One of the things that was interesting about the whole set was that most of them were mainly influenced by their mothers. Hotelling and Weaver had been given by their mothers a small working steam engine. They got interested in how it worked and one thing led to another.

Olkin: Curiosity in some sense.

Wallis: Yes, but it was that particular thing that started them off. In the case of Jimmie Savage, his mother gave him a children's encyclopedia, *The Book of Knowledge*, and he read practically all of it. At any rate there were some patterns among people of quite diverse interests as to how they'd gotten going.

One of the things I didn't mention that I don't think had a lot to do with statistics: My stepmother—my mother died when I was 17 and my father remarried a woman who had a Ph.D. in archeology and physical anthropology—had a large volume of data on the skulls of Merovigians from France. I did a tremendous amount of calculating for her for 50 cents an hour. It was almost all simple correlations or means and standard deviations. It wasn't the kind of thing to get you interested in statistics as such, and could even have had the opposite effect.

Here at the American Enterprise Institute I am somewhat involved with statistics. I am interested in the quality of economic statistics and some of the people here are working on it. Ben Wattenberg is interested in establishing measures of social well-being, and Herb Stein, of course, is interested in national economic statistics. So I've been going to the meetings of the Committee on National Statistics at the National Academy of Sciences. They've had a number of things on the agenda of interest to me that I think are of interest around here, so to that degree I guess I may still be involved with statistics.

Olkin: The statistics profession really appreciates all you've done for it over the very many years during the wartime, during your stay at Chicago, Stanford, and Rochester, and for your efforts in the federal government. Thank you.

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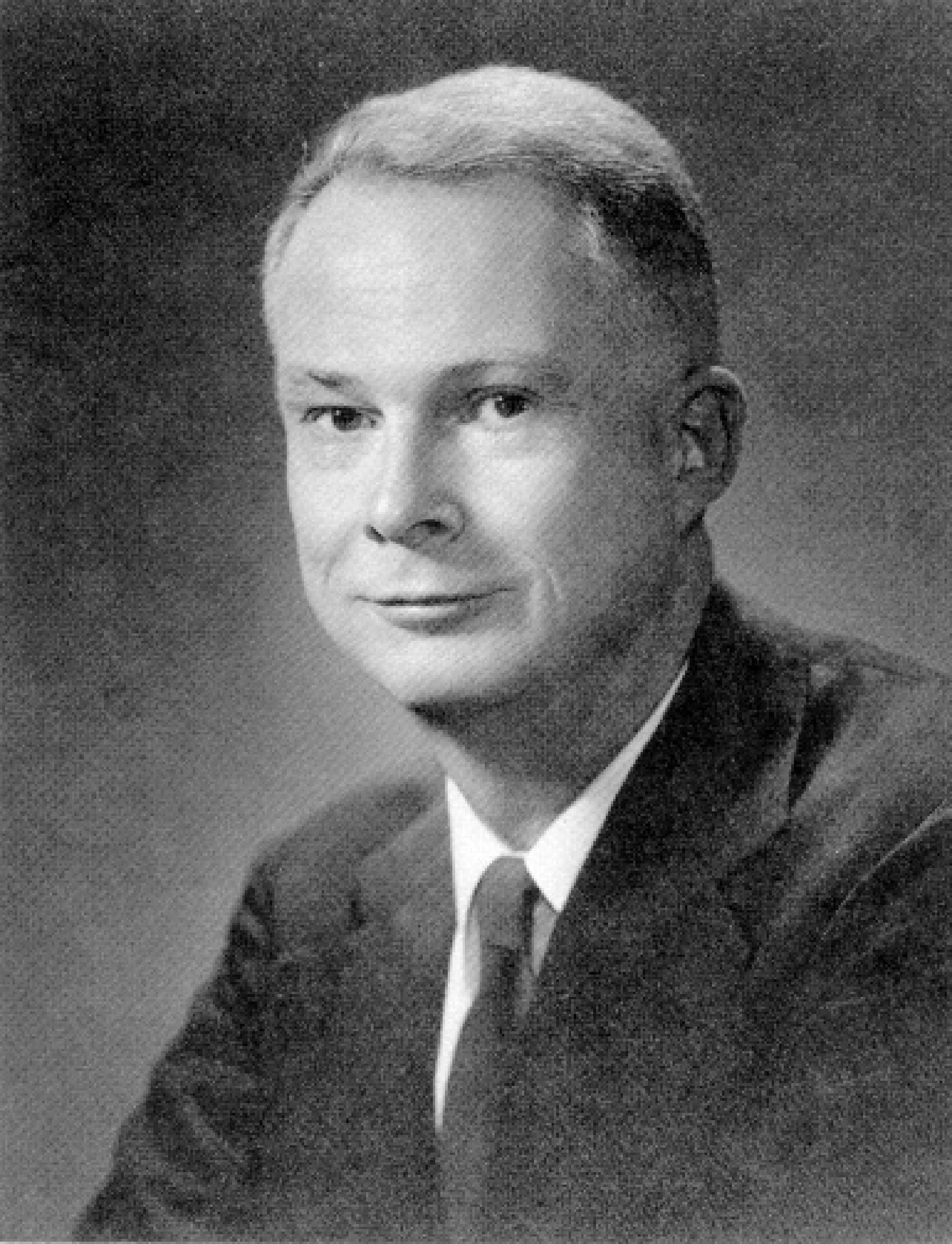


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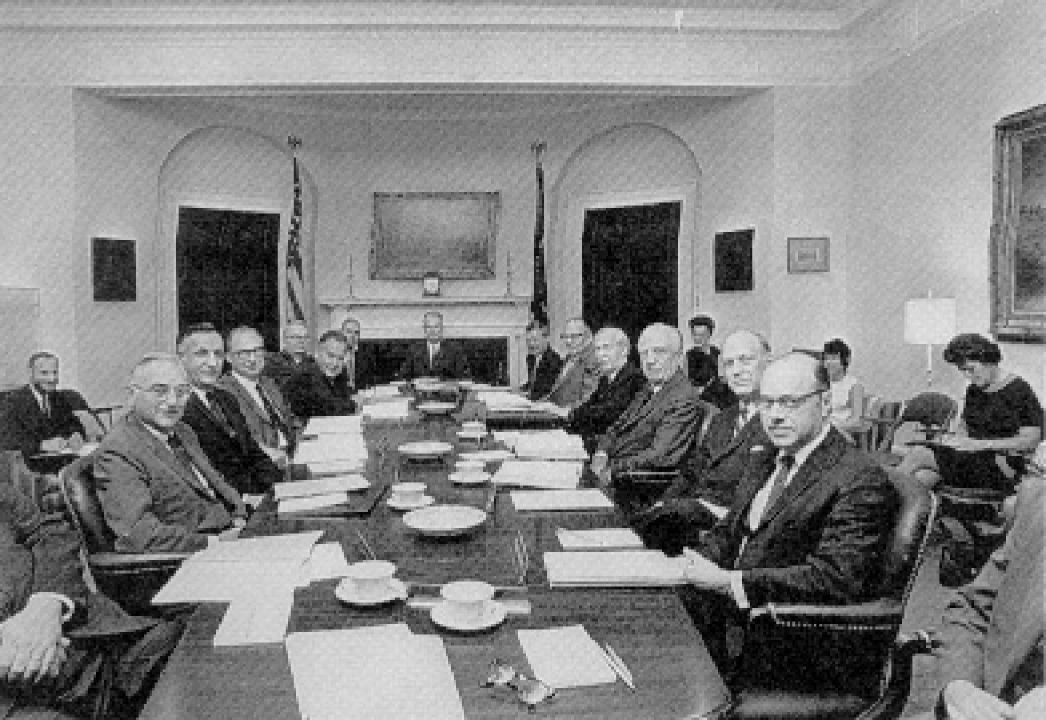

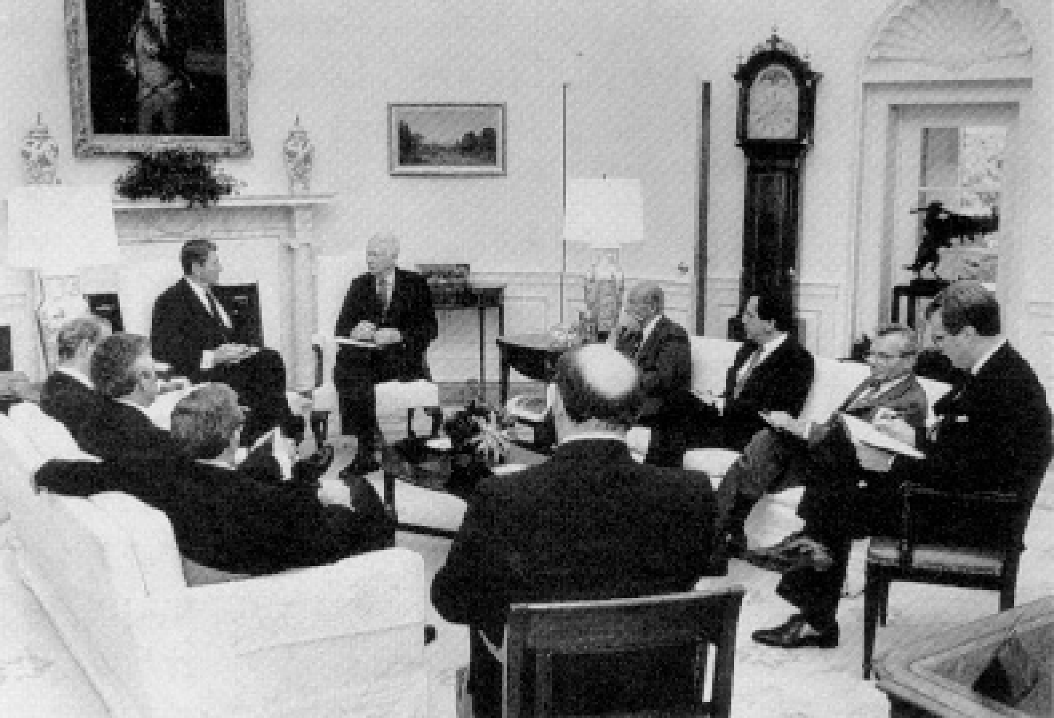


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