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## Lyle V. Jones: Biography

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## **Lyle V. Jones**

### *Biography*

Lyle V. Jones was born on March 11, 1924 in Grandview, Washington. Lyle began his college experience at Reed College in 1942 but was immediately called into active duty in the U.S. Army Air Corps, where he served until 1946. He finished his bachelors and masters in mathematics and psychology at the University of Washington and received his Ph.D. in psychology and statistics from Stanford in 1950. He began teaching at the University of Chicago and moved on to the University of North Carolina in 1957 where he served as director of the Thurstone Psychometric Laboratory and also Vice Chancellor and Dean of the Graduate School. He has been a Research Professor at UNC since 1992. Lyle served as managing editor of *Psychometrika* from 1956-1961, and president of both the Psychometric Society from 1962-63 and APA Division 5 from 1963-64. He has had a very active research life.

*JEBS: How did you get involved with the Thurstone Psychometric Lab at North Carolina?*

**LJ:** The answer to that entails a series of happy accidents, some of which occurred several years prior to such involvement. Let me explain. In the fall of 1949, I had begun dissertation research in my second year of graduate study at Stanford, and I wrote to L. L. Thurstone at the University of Chicago, proposing to apply for a National Research Council postdoctoral fellowship if he would sponsor my visit to his Psychometric Lab at Chicago for the following year. My master's thesis with Lloyd Humphreys at the University of Washington in 1948 had entailed a series of factor analyses, and I wanted to learn more. Thurstone replied that I would be welcome, and I applied for the NRC award.

By early spring, 1950, I had not heard from the NRC. I was weighing offers of Assistant Professorships at the Universities of Hawaii and Kansas, and of an instructorship at Harvard. Each offered an annual salary between \$3300 and \$3600. Each had attractive features and some drawbacks. What a relief it was when a letter from the NRC finally arrived, for it contained good news and I accepted the \$3500 fellowship award.

Arriving at Chicago, I learned that I was one of eight postdoctoral visitors to Mr. Thurstone for that year. As I recall, others included James Birren, on leave from the University of Southern California, Allen Edwards, on leave from the University of Washington, Julian Stanley from (at that time) Wisconsin, Francis Gendre from Lausanne, Switzerland, Sten Henrysson from Sweden, Horace Rimoldi from Argentina, and Father Lawlor, a Jesuit priest. Each of us enrolled in courses, engaged in research, and exchanged

views with each other; it was a year of memorable excitement, stimulation, and good fun.

As the year progressed, I again considered employment offers, this time from Cornell, Johns Hopkins, and Michigan, when Mr. Thurstone surprised me by asking whether I'd like to join the Chicago faculty. "Indeed I would," I replied. Apparently, he had cleared the appointment in advance, because it turned out that my reply was all that was needed.

Yet another surprise awaited me the next year as I learned that Thurstone was leaving the University of Chicago, having reached the mandatory retirement age of 65. He was courted by California-Berkeley and the University of Washington, but he chose to accept an offer from the University of North Carolina. The deciding factor was that it was the only institution of the three that agreed to a professorship for his wife, Thelma Gwinn Thurstone, as Professor in the School of Education. (Nepotism rules at Chicago had prevented her from holding a faculty appointment there.)

When in 1952 the Thurstones moved to Chapel Hill, I inherited Mr. Thurstone's Psychometric Lab at Chicago. I continued to work with him on sponsored research as we had at Chicago. We kept in touch by mail, and I visited his North Carolina Lab on several occasions. I admired not only the two-story Nash Hall that had become the home of his new Laboratory, but also (as a house guest) the Thurstones' spacious new residence near the campus and the seminar room in that residence, much like the one in their Chicago home that had served for so many years for evening research presentations by distinguished visitors.

In early summer, 1955, I accepted an invitation from Mr. Thurstone to visit for a couple of days in August at the Thurstone family summer home near Traverse City in Michigan's Upper Peninsula. Between my efforts to learn to water ski and pleasant social times with the Thurstone family, Mr. T took me aside to plan research priorities, and also to give me some fatherly advice on matters academic. I've never forgotten his comments about research grants and contracts. By then I had some at Chicago. He warned me, as a Principal Investigator, to be sure that the authority for financial expenditures be mine alone, and never shared with a department chair, a college dean, or any other academic officer.

A month later, in September of 1955, I was both saddened and astonished to learn that Mr. Thurstone had died at his summer home. Thelma Thurstone agreed to serve as Acting Director of the Psychometric Lab in Chapel Hill, but only for a couple of years, or until commitments were fulfilled for all existing research grants and contracts.

I was a visiting Associate Professor at the University of Texas for the academic year of 1956-57, and had decided to accept an offer to return to Austin as a permanent faculty member there the following year. Indeed, one evening in the spring of 1957, my wife and I met in our rented house with a realtor, preparing to sign a contract to purchase a house in Austin. Unexpectedly, the

phone rang and I excused myself to take the call in the bedroom. It was Dorothy Adkins, at that time Chair of the Psychology Department at UNC, asking me to visit Chapel Hill to consider accepting a position as Director of the Psychometric Lab. I told her that I would consider it, noting that had the phone call come ten minutes later, it would have been too late. (Despite my disclaimer, the realtor remained convinced that he had been tricked, set up for this joke on him.)

I did visit Chapel Hill, accepted the offer from UNC, and that's how I became involved in the Psychometric Lab at Chapel Hill. Incidentally, in 1977 on its 25<sup>th</sup> anniversary, we re-named it the L. L. Thurstone Psychometric Laboratory in honor of its founder.

*JEBS: What were some highlights at the Psychometric Lab since you moved to UNC?*

*LJ:* Highlights include a couple of rather formal celebrations. One, the Lab's 25<sup>th</sup> anniversary in 1977, was celebrated by hosting the annual meeting of the Psychometric Society (that had been founded by L.L. Thurstone in 1935). Thelma Thurstone was honored then and gave a memorable after-dinner address. R. Duncan Luce was President of the Psychometric Society that year. His Presidential address recognized some of L.L. Thurstone's contributions to mathematical psychology and their influence on more modern developments. The celebration of the 50<sup>th</sup> anniversary of the Lab involved another meeting of the Psychometric Society at Chapel Hill in 2002.

The most important highlights, though, have been people—the faculty, graduate students, and postdoctoral visitors—who were influenced and influenced others while at Chapel Hill. More than 100 graduate students earned PhDs in the Lab. Nearly 50 postdoctoral scholars have visited the Lab for a semester or longer. Their presence enlivened both educational and research activities.

The means by which support for visitors was found may be of some interest. In its early years, support for Laboratory personnel came largely from federal research awards. I was the PI for five or six separate grants and contracts that ran simultaneously. Some required quarterly progress reports, and all entailed periodic renewal applications. I discovered that writing progress reports and preparing applications took a large portion of my time. Gradually, salaries for faculty in the Lab were shifted from soft money to permanent positions funded by the University. Also, in the early 1960s, we were fortunate to consolidate research programs into a single program-project grant from the National Institute of Mental Health (NIMH). At about the same time, we were awarded an NIMH training grant that supported our proposal to provide stipends for graduate students, and also for two kinds of postdoctoral fellow each year, two freshly minted PhDs on the one hand, and one "senior" fellow, often a visiting faculty member from another institution. These visitors served to stimulate both faculty and students in highly constructive ways. I firmly

believe that more such interchange of personnel among universities would enhance the educational and research efforts of any graduate program.

*JEBS: Would you talk about your role in the development of NAEP?*

*LJ:* Yes, that clearly did involve the happy accident of being in the right place at the right time.

After 14 years of faculty duties without an academic leave, in 1964-65 I was fortunate to be a fellow at the Center for Advanced Study in the Behavioral Sciences at Stanford. My major project that year was writing sections of a forthcoming book on Thurstonian scaling methods, My co-author was R. Darrell Bock, who remained that year as Associate Professor at the Psychometric Lab in Chapel Hill.

Ralph Tyler was the charter Director of the Center and continued in that role the year I was there. I had known him a decade before when he was the Dean of Arts and Sciences at the University of Chicago. Lee Cronbach recently had moved to Stanford from Illinois. John Tukey from Princeton had been a fellow at the Center earlier and remained an active advisor, and Robert Abelson was, like me, a fellow at the Center that year.

I mention these names, because in February of 1965 Ralph Tyler asked if I would join with Abelson, Cronbach, and Tukey to advise Tyler and the Exploratory Committee for Assessing the Progress of Education (ECAPE) on the design of a project to periodically monitor levels of achievement and change in achievement over time of the nation's children and young adults. Tyler served as Chair of ECAPE, formed in late 1964 with a grant from John Gardner's Carnegie Corporation of New York, upon the initiative of Francis Keppel, at that time the U.S. Commissioner of Education. Both Cronbach and Tukey had engaged in earlier meetings that had supported the project.

I did accept the assignment. The group became the Technical Advisory Committee (TAC) with Tukey as Chair. We began meeting immediately with Ralph Tyler at the Center to formulate plans for a national assessment. While still at the Center, we met every three or four weeks. Then we continued to meet six or seven times a year until the first NAEP operational assessment in 1969. (ECAPE had become CAPE which in 1969 became NAEP, the National Assessment of Educational Progress.) Some of those meetings were at the Center some were at Princeton, or Chapel Hill, or near John Tukey's summer home in Westport, MA, or at hotels convenient to airports, e.g., at Chicago or New York or Dallas. The four of us soon were well acquainted and then became close friends.

Typically, a meeting would begin on a Friday evening, continue all day Saturday and after dinner Saturday evening, ending about noon on Sunday. The agenda would be prepared by John Tukey with assistance from the ECAPE staff director, Jack Merwin from Minnesota and later Frank Womer from Michigan. Early meetings focused on what subjects to assess, what conditions

should be adopted for administering assessment exercises, what were the appropriate target populations, and what sampling procedures should be employed. Later, we hammered out specifications for the construction of assessment instruments, evaluated proposals from potential contractors, critiqued exercises created by contractors, and analyzed results from pilot studies. After the initial national assessment of 1969-70, we wrote NAEP Report No. 1, often working together by telephone conference call far into the night. That report provided a model for reporting assessment findings that served as the template for later NAEP reports, as did NAEP Reports 4 and 7 that we prepared somewhat later.

In the summer of 1969, after the first assessment had been completed, NAEP found a home with the Education Commission of the States (ECS) in Denver. TAC was converted to ANAC, the Analysis Advisory Committee, and the composition of the group changed over succeeding years. John Tukey and I did remain members of ANAC until 1983, when the contract for NAEP shifted from ECS in Denver to the Educational Testing Service in Princeton. Happily for me, John and I still continued to meet occasionally in person and often by phone, working together on a variety of activities until his untimely death in late July of 2000.

A critical history of NAEP is in press, to be published by Phi Delta Kappa International in early 2004, the 40th anniversary of the formation of ECAPE. Ingram Olkin at Stanford and I are the co-editors, and we are pleased that the book is sponsored both by AERA and NCES. Among the contributors are many who facilitated NAEP in both early and later periods, including Lee Cronbach and John Gardner, both of whom are recently deceased. Even though I was deeply involved in NAEP during its early years, it was not until I read draft chapters of this book that I realized how close NAEP was to being discontinued, both before it was mounted in 1969 (because of opposition from school administrators) and in the mid-1970s (because of political and federal budgetary issues). At those times, TAC and ANAC had been largely protected from such concerns.

*JEBS: What are your thoughts concerning the entire debate over significance testing that has intensified recently?*

LJ: That question actually sends my thoughts back about 50 years. Let me explain.

For the *Annual Review of Psychology*, Volume 6, of 1955, I prepared a chapter on Statistical Theory and Research Design. Having reviewed relevant literature for the year 1953-54, I noted "an increased awareness that the usefulness of conventional hypothesis testing methods is severely limited. The typical procedure of testing a null hypothesis against all possible alternatives, so prevalent in psychology, is often inadequate." I went on to suggest that the investigator should formulate "experimental problems in terms of the estima-

tion of population parameters, with the establishment of confidence intervals about the estimated values, rather than in terms of the test of a null hypothesis against all possible alternatives.”

That suggestion at that time had little or no impact. However, I believe it to have been sound advice then and still to be sound advice now. I’m pleased, then, about the recent debate if it helped to stimulate consideration of pertinent issues by the APA Task Force on Statistical Inference. The Task Force recommended procedures that seem to echo my theme from 1955.

Often, the parameter of greatest inference is an effect size, so the outcome of an experiment is appropriately expressed as an estimate of the effect size (frequently standardized rather than in arbitrary units), together with a  $(1 - \alpha)$  confidence interval about the point estimate. If zero is included in the confidence interval, we know that a traditional significance test at significance level  $\alpha$  would fail to reject the null hypothesis. So, this approach always yields the result of a test of significance, but provides additional useful information as well.

I took courses in psychological statistics from Allen Edwards at the University of Washington and from Quinn McNemar at Stanford, just as each had published a highly successful textbook for the course. To both of them, I am grateful, for I became infected by their enthusiasm for the content of their courses. However, in both of their books, as in many later texts, principles of statistical inference were introduced in the context of significance testing. Interval estimation was presented more or less as an afterthought. Because most psychologists learned their statistics this way, an effect may have been to inculcate bad statistical habits so that authors and editors alike expected significance tests to be a standard part of articles on empirical research. Surely, it is past time for reform; the report from the APA Task Force presents a constructive framework for more reasonable statistical practice.

Despite my strong preference for interval estimation, there are situations for which a test of significance still may be appropriate. One is multiple comparisons, such as comparisons between all pairs of states for average student achievement scale scores in NAEP. A related application is assessing the goodness of fit of a model to an array of values. In these cases, interval estimation is not easily employed and the careful application of significance tests may continue to serve about as well as any alternative.