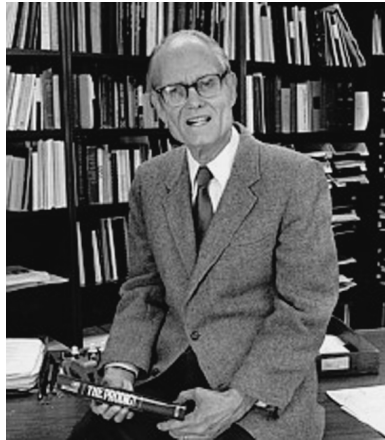


## Profiles in Research

### Julian Cecil Stanley

*Interview by Howard Wainer and Dan Robinson*



### *Biography*

Julian Cecil Stanley was born in Macon, Georgia on July 9, 1918. He earned his undergraduate degree from Georgia Teachers College and an Ed.D. from Harvard in 1950. He began his career as a high school math teacher but subsequently taught at Vanderbilt (Peabody College) and Wisconsin before moving to The Johns Hopkins University in 1967, where he began his influential work on mathematically precocious children. His study, which focused on students age 13 or younger who score 700 or more on the SAT-Math, led to the establishment of Hopkin's Center for Talented Youth in which these young students took college-level courses on weekends and during the summer. The Johns Hopkins program became a model for subsequent programs established at Duke, Northwestern, and many other institutions. He was also the coauthor (with Donald Campbell) of the influential book *Experimental and Quasi-Experimental Designs for Research* (Houghton-Mifflin, 1966). He was past president of AERA and NCME and was awarded a lifetime achievement award by Mensa. This interview was begun in 2004 and was ended, not-quite-complete, by his death on August 12, 2005.

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Howard Wainer/Dan Robinson: *In reviewing your professional career I got the distinct impression that you were not one person but at least two or three because your work is so broad, so deep, and in so many disparate areas, it is hard to imagine all of it coming from the same person. It also spans 60 years. Could we begin with a brief summary of the path you took and the forces that pulled you along that path?*

Julian C. Stanley: I have had, in effect, four different “careers”—(1) a very young (age 19–23) teacher of science and mathematics in Atlanta high schools just before World War II; (2) a participant in that war, January 6, 1942–September 6, 1945, 44 months, almost the exact duration of the war displaced by 1 month; (3) a “dry bones methodologist,” virtually a fanatic about statistics, the design of experiments, test theory, and testing, and research design, 1949–1971; and since then (4) deeply involved in finding “boy and girls who reason exceptionally well mathematically and/or verbally” and helping them educationally. Graduate school intervened, 1945–1949, on the G. I. Bill.

Not all my time has been spent slaving over a hot typewriter. In between, I’ve visited 73 foreign countries, quite a few of them several times each. For 8½ months I was a Fulbright Research Scholar in Belgium, 1948–1949, and for 3½ months in 1974 in New Zealand. A year was spent as a postdoctoral fellow with the Committee on Statistics (now a department) at the University of Chicago, 1955–1956, after studying mathematics intensively for 8 weeks in a Social Science Research Council Institute on Mathematics for Social Scientists at the University of Michigan.

The years 1965–1967 were spent in academic “heaven” as a Fellow of the Center for Advanced Study in the Behavioral Sciences at Stanford University. I spent an academic year (1948–1949) as a full-time instructor in the Harvard Graduate School of Education, summer teaching there in 1963, being a visiting lecturer at the University of North Texas, the University of New South Wales in Australia, and the People’s Republic of China in 1986.

There’s much more, of course, but I’ll spare the reader further details. It’s been a very busy life since I enrolled in the Harvard Graduate School of Education in the fall of 1945, only a few days after being discharged from the U.S. Army Air Corps following my service in World War II.

Wainer/Robinson: *Aristotle tells us that we understand best those things that we see grow from their very beginnings. Perhaps we should begin by you describing your early life.*

Stanley: I was born in Macon, Georgia, on 9 July 1918, shortly before World War I ended. At that time my father was a hotel clerk and my mother a registered nurse. When I was about 2 years old we moved to East Point, Georgia, a contiguous incorporated suburb of Atlanta, near what is now the Atlanta Airport.

My early education was in a three-grade neighborhood school a block from home. I was a C student in the first grade (there were no kindergartens or other preschools then). The teacher told my mother that I tended to keep my feet on my desk. Miraculously, in the next two grades I was an all-A student, to the extent that my third-grade teacher strongly recommended that I skip the fourth grade (most unusual in my hometown) and go directly into the fifth grade of the large local elementary school. I did this with no appreciable academic problems but discomfited socially by having to leave my girl friend and my best boy friend behind. It took me several years to adjust to the skip. In retrospect, if I had been offered a choice, I'd probably have declined the teacher's offer. In the long run I may have benefited by the more challenging curriculum and being seen by my classmates as a "brain." Who knows? I had no control-group identical twin who was not accelerated.

A peculiarity of the impoverished Deep South at the time I entered the local high school was that there were only 11 years of school, not the usual 12 found elsewhere. Thus, with a late birthday and having skipped the grade, I graduated from high school while still 15 years old, the youngest in the 1934 class of 200. I had made virtually all A's without much effort and graduated third in the class, having taken 4 years of Latin and the usual required courses. They were all academic because we had no other options.

Then, because of financial reasons, I enrolled in a small, 1-year-old residential state junior college (now the rather large State University of West Georgia), graduating in 1936 and going on to the state teacher's college. It proved so unchallenging that I was glad to complete Bachelor's degree requirements quickly.

Then, less than 2 months past my 19th birthday, I became a regular, full-time teacher in an Atlanta high school enrolling mostly students from lower socioeconomic-level parents. I was large and not conspicuously young looking, but not mature enough to be an outstanding teacher, although for 4½ years perhaps I did a passable job of teaching whatever was asked of me. Chemistry was my favorite subject, but this financially poor school had only one section of it. Therefore, at times I taught general business training, commercial arithmetic, ninth-grade

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English, spelling, remedial general mathematics, general science, and several other subjects. That thorough exposure as a tenured high-school teacher proved over the years to have been invaluable experience. I never fault the harried, overworked teacher trying to teach several different subjects to 150 students daily, as my daughter now does. It's very demanding work.

Wainer/Robinson: *I assume that you left teaching to join the military and only then, after World War II, did you decide to go to graduate school. What was Harvard like then?*

Stanley: The Harvard Graduate School of Education, 1945–1949, was an exciting holding pattern for outstanding academics recently discharged from the service. Nearly all of them moved on soon. Among those with whom I worked were O. Hobart Mowrer and Bob Sears, both later presidents of the American Psychological Association. Truman Lee Kelley, “Stanford University’s Pearson,” and that intellectual giant Phillip Justin Rulon were among my mentors. I had five major professors in 4 years. Nominally, I completed my doctorate under Rulon, but he had no interest in my rats-in-a-T-maze early study of partial reinforcement, chosen while Mowrer was my adviser, so I was fortunate to run across a very research-oriented postdoctoral fellow, William Oliver Jenkins.

Bill Jenkins helped me some with my dissertation. We coauthored a major review of the field (Jenkins & Stanley, 1950). For 10 years it was the basic source of history and theory, until it was superseded by a new review.

To my surprise, during my first year of graduate school I discovered my great interest in test theory and statistics. Rulon’s design of experiments course furthered that. He, then acting dean, was at the very forefront of statistics but published little during his lifetime.

Mowrer left for the University of Illinois in 1948, leaving me feeling a bit odd with my learning study. As noted above, Jenkins saved the day for me. Soon thereafter he shifted to clinical psychology at the University of Alabama, so we collaborated no more.

Postwar university positions were just beginning to open up. I taught at the University of Georgia during the summer of 1947 and received an offer of a tenured associate professorship. Perhaps I should have accepted it, but my role model, Frederick B. Davis, was chair of the Department of Psychology at George Peabody College for Teachers, so I went to Nashville in 1949 as a nontenured associate professor of educational psychology. (Peabody is now part of Vanderbilt.)

Wainer/Robinson: *After completing your degree you began a period of prodigious scholarly productivity that continued for the rest of your life. Yet some very important papers were buried in obscure jour-*

*nals. Would you describe a little of what happened in this early period in your career?*

Stanley: Fred left for Hunter College after the summer session of that year. Nevertheless, Peabody proved a good training ground. I was treated as a big fish in a little pond and had much freedom to do whatever I wished, which was to work myself very hard teaching and supervising admissions testing at both undergraduate and graduate levels.

I did, however, begin publishing, though with little understanding about which journals would be best for my articles. After 2 years I had tenure, with a promise of a full professorship in 2 more years.

My first article with Jenkins was followed by two statistical articles buried in obscure journals. That same year one more data-oriented article appeared in a widely circulated journal. By the end of 1951 I had published 10 articles, a huge outpouring considering that my doctoral dissertation was completed during the summer of 1949.

In 1952 there were “just” a review and an article. By 1953 I was back on stride with six articles, reviews, and technical notes. There were nine in 1954, including my first book (Stanley, 1954) and my first foray into the gifted-child field.

Of the seven in 1955, I single out one. “Statistical Analysis of Scores From Counterbalanced Tests” (1955) was my most technical article yet. I had begun to master the esoteric aspects of analysis of variance.

The eight in 1956 include only one I consider worth singling out. It is a brief expository technical note arising from my three-quarter statistics course under Professor Alexander Brownlee at the University of Chicago during my postdoctoral year there.

The 16 (!) published items in 1957 must have kept me busy day and night. A few of them might still have value. I still mourn wasting several of these in sources not known to most of the appropriate readers. Clearly, I needed guidance from a mentor far wiser about publishing than I. I must have been in too much of a hurry to get on to the next project.

Of the eight items in 1958 a few were wasted again in obscure publications. About this time several articles by me about the gifted appeared, foretelling my virtually total switch to that area from 1971 onward. More about that later.

Probably there is more detail about my first 2 years out of graduate school than the typical reader might want, so from here on I’ll merely single out what seems to me one of my best publications. “Analysis of Unreplicated Three-Way Classifications, With Applications to Rater Bias and Trait Independence” (Stanley 1961) is the most technical article, deeply involving the algebra of the analysis variance, that I’ve done. Fortunately, I had the good judgment to get it into *Psychometrika*, where for many years since it has been cited as a statistical extension of Camp-

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bell and Fiske (1959). This led Don Campbell to invite me to work with him on a chapter for a handbook that later was revised a bit and reprinted by Rand McNally (Campbell & Stanley, 1966) as a little book that sold about a quarter of a million copies.

Wainer/Robinson: *Your work with Donald Campbell is indeed well known. Would you talk a little about Campbell and your collaboration, and perhaps discuss your other work on quasi-experimental designs?*

Stanley: Don Campbell was a great psychologist of sociological and anthropological leanings, a truly brilliant mind. It was my pleasure to work closely with him for several months in 1960. In 1998 a memoir prepared by me about Don Campbell was published in the *Proceedings of the American Philosophical Society*. Although I consider it one of my finest publications, of course it received little attention because most other psychologists and educationists aren't members of that society.

Pella, Stanley, Wedemeyer, and Wittich (1962) was a large-scale field study of teaching physics by means of an excellent film. Stanley (1966) explored "A Common Class of Pseudo-Experiments."

In 1966 I chaired ETS's Invitational Conference on Testing Problems and edited its proceedings. That same year I organized a symposium and edited the ensuing volume.

Stanley and Porter (1967) studied predictability for Blacks versus Whites. Also see Thomas and Stanley (1969), Hills and Stanley (1970), and especially Stanley (1971b), which reflect my interest in predicting the academic achievement of African American students versus that of Caucasians and Asian Americans.

Wainer/Robinson: *It was at this time that Arthur Jensen's 1969 Harvard Educational Review article on Black/White IQ differences came out.*

Stanley: Art Jensen is my hero. We were Fellows, 1965–1966, at the Institute for Advanced Study in the Behavioral Sciences at Stanford. He was then doing his first write-up of Black–White differences, which resulted in a carefully prepared 40-page article in the 1968 *American Educational Research Journal* that attracted virtually no attention, being overshadowed by the 1969 controversy.

I can testify personally to the reason why Art doesn't get awards or honors. At a meeting of a very prestigious national society we were nominating persons to become members. I nominated Art, at which a prominent psychologist said that would not be politically wise. I insisted, so we took a vote, a ranking of the 20 nominees. I was the ballot counter. Jensen got a 1-to-5 rating from all but the objector. He ranked him dead last, 20th, and thereby killed his chances. That was about 20 years ago, and he still isn't a member.

I, too, have suffered because of my *Science* article (Stanley, 1971b) that showed persuasively that the SAT predicted the college achievement of Blacks as well as it did for Whites. Actually, Blacks were a bit over-predicted; they didn't do quite as well as predicted.

My empirical gender-differences article about SAT-M (Benbow & Stanley, 1980) in *Science* got a countrywide hysterical reaction from feminists and many psychologists. Eight years later, all but one reviewer of my NSF grant application savaged me and my "unscientific" reputation, etc. They were still very angry because we had helped destroy the fictions they were using to get large government grants. Nevertheless, I have since published five more gender-difference articles. Needless to say, they aren't popular in certain quarters. Nowadays, almost no one else, least of all ETS, does such research. Miraculously, I did get them published, one in the *Journal of Educational Psychology* (Stumpf & Stanley, 1996).

Wainer/Robinson: *Was your interest in measurement-related issues beginning to wane after this?*

Stanley: Oh, no, my interest in classical test reliability was manifest in Stanley (1971c), as was the design of experiments (Stanley, 1971b).

Around this time I authored or coauthored a number of technical measurement and statistics studies, too numerous to detail here. Typical were Stanley and Wang (1970) and Wang and Stanley (1970), concerning the differential weighting of test-item options. Marilyn Wang (now Marilyn Demorest) was a graduate student in psychology who pursued measurement issues with me intensively during the first summer after I arrived at The Johns Hopkins University in 1967.

After its third edition, my first book (Stanley, 1954) was continued for many more editions by Prentice Hall as *Educational and Psychological Measurement and Evaluation*. In 1970 my former graduate student Gene V. Glass and I coauthored an intermediate statistics textbook (Stanley & Glass, 1970), based appreciably on the course we had taught together at the University of Wisconsin.

University of Chicago statistician Professor William Kruskal invited me to prepare the entry of the analysis of variance for the *International Encyclopedia of the Social Sciences* and helped me considerably to improve my submission (Stanley, 1968). Although I had been a post-doctoral fellow, 1955–1956, in statistics at Chicago, there still were niceties of statistical notation that I was missing. Happily this gap was filled from what I was taught by the generous Dr. Kruskal. My grasp of formal statistics thus improved greatly since my rather meager formal statistical training as a graduate student. How I loved the analysis of variance, which seemed to me an elegant approach to designing experiments and analyzing the data thus engendered.

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Wainer/Robinson: *How did your interest in gifted students develop?*

Stanley: In 1969 I began to discover mathematically highly precocious youth and by 1971 had developed a passion for finding them and helping them get the special, supplemental educational opportunities they sorely needed for their own benefit and that of society. I created the Study of Mathematically Precocious Youth (SMPY) at The Johns Hopkins University in 1971. By 1979, the unit working directly with talented youth and conducting educational programs became the Center for Talented Youth (CTY). [SMPY became focused on research through a longitudinal study of over 5,000 individuals, which is now located at Peabody College, Vanderbilt University (Benbow et al., 2000; Lubinski et al., 2006).] Soon we also helped create the Talent Identification Program (TIP) at Duke University, the Center for Talent Development (CTD) at Northwestern University, and the Rocky Mountain Talent Search (RMTS) at the University of Denver, thereby blanketing the United States and also involving several foreign countries. For the first results of this, see Stanley, Keating, and Fox (1974). This was followed by many books and articles such as Benbow and Stanley (1983).

From the creation of SMPY in 1971, the Center for Talented Youth (CTY), the three other regional programs, based at Duke University, Northwestern University, and the University of Denver, to the present coverage of the entire United States and a number of foreign countries with annual talent searches and academic summer programs is a long story of successful development of the special educational opportunities we did not have in 1971. A glimpse of this can be obtained from Benbow and Lubinski (1996).

Wainer/Robinson: *Your work on sex differences brought controversy. What has been your reaction to the response to Harvard President Summers' recent remarks concerning sex differences in math and science?*

Stanley: Women tend to score somewhat lower on mathematical reasoning, about a standard deviation lower on mechanical reasoning, about a standard deviation lower on three-dimensional (but not two-dimensional) spatial relationships, about the same difference on theoretical evaluative attitudes, and at least a standard deviation *higher* than men on social service and aesthetic evaluative attitudes, etc. Last time I checked, the top score on a College Board physics exam was earned by 43 men for every woman. Thus, it's no wonder that bright women gravitate toward medicine, biology, biophysics, and biochemistry, rather than electrical engineering, physics, chemistry, or (especially) computer science. Summers is right,

but gravely incorrect politically. In the sociopolitical context, gender differences are almost as much a hazard as racial ones. I know. I've ventured into both areas and been strongly criticized. Being a stubborn sort, however, I've persisted in studying gender differences on aptitude and achievement tests.

Wainer/Robinson: *It seems clear that the key elements in your success, besides talent, were a powerful curiosity and a lot of energy. Would you agree?*

Stanley: Yes, it's been very stimulating all the way, and still is. Many honors have come my way, vastly beyond what I could have expected at age 19 when I graduated from college or even much later. I was a late bloomer, with no publications until age 31. During my 87 years of existence, despite this slow start, I have published thus far 564 items (books, book reviews, professional articles, technical comments, letters to editors, etc.). A book I coedited was recently published (Boothe & Stanley, 2004), and I currently have two articles and a book review in press. I continue to pursue several projects.

How well the Zeitgeist, serendipity, and chance worked for me! I'm not a religious person, but it does seem that some guardian angel protected me from my many mistakes and provided me the abundant energy needed to pursue my interests.

*In the months before his death Julian Stanley, then living in an assisted-care facility in Columbia, Maryland, still worked two 8-hour days per week on developing and administering programs for the gifted. He is survived by his wife of 3 years, the former Dorothy Fahey, and his daughter, Susan Willhoft of Tacoma, Washington.*

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