

# William Stephenson at the University of Chicago: Subjectivity/Objectivity Revisited<sup>1</sup>

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*[EDITOR'S NOTE: In this address, delivered as the keynote to the Tenth Annual Q Conference, Bob Lipgar reflects upon his days as a graduate student at the University of Chicago during the time William Stephenson was a member of the faculty there. It was during his Chicago years, of course, that Stephenson's seminal statement on Q methodology, The Study of Behavior, was issued and an awareness of the magnitude of Professor Stephenson's innovations and ideas began to take shape. These and related aspects of Stephenson's Chicago Years are well captured—along with a sense of the rich intellectual atmosphere of that time and place—in Dr. Lipgar's remarks. His address is reprinted here in the exact form in which it was prepared and delivered. DBT]*

The task I've accepted tonight is more than slightly daunting. It is to talk to you about a man I knew more than 40 years ago—a man whom some of you have compared to Galileo, Jefferson, Freud and Einstein! It is awesome to speak to a group that holds a man in such high regard. It is also a bit intimidating to consider the extent of his writing and teaching in his nearly 40 post-Chicago years. And I am further impressed by the fact that this is the 10th Annual Meeting of the IAS and co-sponsored by the Research Center named in his honor here at the University of Missouri.

I am more than a little anxious. I am also delighted, deeply touched, and very grateful to you for inviting me to be here with you

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tonight.

What makes my task manageable as well as pleasurable is simply that I have the sense of revisiting an old friend. I was one of Will's first research assistants in Chicago and still have the sense that I really understood what he was about. I was drawn to his spirit and his ideas then in 1949—I was 21 and he was 47—and I feel some sadness now for the time lost. To become reacquainted with his thinking in preparation for tonight and to meet so many of his colleagues and students, is indeed most gratifying.

We who knew him during his Chicago years remember him as affable, even jolly, creative and very generous in sharing his time and his ideas. He seemed to us very British; he wore business suits and often a bow-tie (which I wear tonight in his honor).

Stephenson, of course, is remembered also as a very independent spirit—always poised for the pleasures of a good intellectual joust! A phrase from one of his own articles, "Concourse Theory of Communication" (1978), will convey something of his personality: he was "the white-knight, with prancing courser, tilting in playful quarrel with his peers!" As you might suppose, his jousting and tilting were not always seen as playful. Some found him difficult to follow, and some were put off by his insistence that Q could address all their problems. But for myself and some others, he was a very significant and positive influence—and, I would add, inspirational.

I asked one of my classmates of 1949, Marshall Edelson, to comment for tonight's talk. Marshall was the first among us, in 1950, to make a comprehensive application of Q methodology to the study of a single case; he worked together with Arthur E. Jones, another of our small core group of 24 new graduate students in psychology. (This work, incidentally, is cited in *The Study of Behavior*, the major work of Stephenson's Chicago years.) Marshall recalled how most of our faculty stressed a philosophy of science and an experimental methodology which rejected studying anything which could not be measured; how we had been taught to consider only those matters that could be seen from the outside and only those events that could be shown to be repeatable and constant, or reliable. In this climate, of course, Stephenson stood out—daring to draw us into a science of subjectivity where changes in individual subjects' responses from one condition or circumstance to another were matters of great interest.

Stephenson stood our concepts of reliability and validity on their heads. He challenged us, and himself, to meet the standards of quantification and scientific rigor, but to do so without moving outside the subject's psychological world.

Central to his teaching then, and his legacy now, is the fact that he taught a psychology which would attend to the desires and imaginings, needs and values of individuals; and he taught as well the operational means by which such a psychology could be achieved. His inventiveness opened our eyes to ways in which we could explore vast domains of human behavior previously thought unapproachable.

Well-schooled, quick-witted, gifted with intelligence, energy and a generous spirit, and notable also for the color of his hair—"red gold" as Maimie has named it with unflinching fidelity—Will Stephenson held constant to his vision of a science of subjectivity. About this he was passionate. His mind leaped with pleasure over such an immense range of sources and issues that some of us became discouraged or skeptical. Yet, whenever I mustered the energy and diligence to check his sources, I was always satisfied with his reading of other people's work.

As a teacher he was charismatic and intellectually formidable. More than this, he took our questions seriously and engaged and encouraged our abilities to think critically and independently. He inspired many of us to think in new ways about how to advance and enrich psychology.

Stephenson had been recruited to the University of Chicago in 1948 by the new chair of the Department of Psychology, James G. Miller, a 31-year-old psychiatrist. Miller aggressively recruited Stephenson from Oxford where he was established as a leader in factor analysis. Let me now give you some sense of the University of Chicago when Will Stephenson arrived.

World War II had ended only three years earlier. The post-war intellectual climate at the University of Chicago was filled with serious optimism. There was an air of "everything is possible"—no intellectual challenge was too great. Only a few years earlier, working in a make-shift laboratory, in space converted from a handball/squash court beneath the Stagg Field football stands, Enrico Fermi and his colleagues had achieved the first self-sustaining nuclear chain reaction. That this scientific breakthrough at the University had made possible the atomic bomb—the horrific destruction as well as the great promise of the "nuclear age"—led to vigorous and searching re-examinations on campus of morality, philosophy, science, and public policy. Faculty and graduate students in different disciplines earnestly argued the merits of their particular intellectual pursuits, the place of values in relation to science, and the merits of various philosophies of science and

epistemology. Operationalism and logical positivism, phenomenology and behaviorism, and the unification of science were all "hot" topics. The research work underway in the early 1950s in physiology, medicine, physics, chemistry, economics and the humanities by both faculty and students would lead, in subsequent decades, to a rash of Nobel Laureates associated with the University of Chicago. These were heady times on the Midway—many regard the 1950s as our "golden years." Intellectual pursuits were not yet disrupted by the social and political upheavals of the '60s. (Nor had the sophisticated satires of Mike Nichols and Elaine May, fellow Hyde Parkers, yet been discovered by Hollywood and Broadway and taken from us.)

Under Miller's chairmanship and in this climate, the Psychology Department set out to build a strong faculty and recruit only the most promising students, admitting only those with excellent academic records who also scored in the top percentiles on the Graduate Record Exam and on the Miller (no relation) Analogies Test of Intellectual Ability, and undertaking their preparation only for doctoral degrees.

When Stephenson came to teach at the University of Chicago, the Psychology faculty were very self-conscious about their status as scientists and wanted their graduates to be able to hold their own with physicists, mathematicians, astronomers, physiologists and the like. We were to be "real" scientists capable of contributing original and independent research to psychology as a science. Such assumptions and aspirations permeated the Department. Many of the young faculty, newly recruited and ambitious, and many of the new students, for instance, never accepted Carl Rogers as a "real" psychologist. He was perceived as a minister, despite his trail-blazing empirical studies of therapeutic change and his international renown. And the graduate students drawn to him (and I was one of these) were looked upon as "soft-headed"—little better than cultists.

William Stephenson, with his intellect (and his red hair), was much more difficult to put down. With a Ph.D. in physics as well as one in psychology, and his considerable sophistication in the history and philosophy of science and in matters of experimental design and statistics, he was much more difficult to depreciate or dismiss. But they did!

Dr. Miller recalls recommending Stephenson for tenure "two or three times," and each time this appointment was denied. Votes defeating him were probably cast by young colleagues with narrow biological and physiological orientations to psychology. (Although L.L. Thurstone was tenured and senior in the Department and a polar opposite of Will in most respects, I have no knowledge of his actually

having served on the tenure review committees.) The very qualities of character and insight which for us defined Stephenson's originality and genius were, I believe, the very qualities which doomed to failure his bids for tenure. As Miller now puts it, "they had never seen anyone like him before and, at the time, didn't know quite what to make of him."

Stephenson had boundless enthusiasm for learning and discovery; the faculty who would prevail as the dominant force in Psychology at that time were more interested in prediction and control. He was committed to making the inner world available for exploration through public and objectifiable procedures; they were more interested in measurement and scales. He believed it worthwhile to explain the interplay of diverse psychological forces and variables; they were inclined to seek general laws, pinning down single traits one at a time. He found frontiers of the unknown challenging and exciting; they responded to the challenges of the unknown only in very narrow and proscribed contexts. He was at home with science, statistics, the arts and humanities, philosophy and history; they approached the contributions of other academic disciplines with suspicion, diffidence, or quiet arrogance. He was enthusiastic about applying his creativity to many problems and joined in collaborative efforts with vim and vigor; they tended to be self-protective and picayune about turf and territory. He was interested in society in the broadest sense, in what current issues were being learned and taught at-large and not only among the elite; they focused more on parochial academic and scientific matters—not seeing themselves so directly involved as players in civilization's destiny.

When I spoke with James G. Miller a few weeks ago by phone, he recalled Stephenson and Maimie fondly and told me that Will's work with Q was "absolutely original and valuable—a major, important new development."

Stephenson's accomplishments at the University of Chicago include much more than his major book, *The Study of Behavior: Q-Technique and Its Methodology*. There are more than two dozen articles and presentations and at least two book-length unpublished manuscripts. He also found time to sponsor several ground-breaking doctoral dissertations. I will cite here only those by Fred Fielder, Ralph Heine, Margaret Hartley, and J.C. Nunnally. (I want, of course, to include on this list my own dissertation, *Subjective Probability Notions and their Personality Correlates* [1965] which was not completed until after Will left Chicago.) I've already mentioned the unpublished, detailed study

of a single case in 1950 by Edelson and Jones, later published as a monograph (1954).

Fiedler's dissertation was the first using Q methodology. Remembering that time, Fiedler writes, [Stephenson] "made me rewrite my dissertation the week before my final orals.... I wasn't too happy at the time.... As it turned out, my thesis made quite a splash, and I was glad that he took the trouble ... he was a tremendous help" (personal correspondence, August 8, 1994).

Beyond all this were many important and timely collaborations with faculty. Probably Carl Rogers' most lasting contribution to psychology was the empirical study of therapeutic change. This could not have been accomplished without Q technique and Stephenson's help. Moe Stein, who was on the faculty then, pointed out to me recently how timely this collaboration was for both Rogers and Stephenson (personal correspondence, September, 1994).

Samuel Beck, the Rorschach expert, was also on campus at this time. His insightful and systematic studies of adaptational styles in schizophrenia probably could not have been accomplished without Stephenson, Q methodology, and J.C. Nunnally—another of my 1949 classmates and arguably Stephenson's most dedicated and productive research assistant during these Chicago years. (Jum unfortunately has passed away, but has left us a lucid body of work in psychometrics and psychological research.)

Among Stephenson's other colleagues during these years were: Riesman in sociology, Carnap in philosophy, Thelen in group dynamics, Moe Stein in the study of creativity, Bolgar in psychoanalysis, Butler, Cartwright, Dymond, Grummon, Gordon, Shlein in client-centered therapy, Janowitz in the social sciences. He also drew on many who had taught earlier at the University, notably John Dewey and George Mead. His array of source material was always impressive. As you know, he happily took on all comers—arguing from Shakespeare, James Joyce, Kierkegaard, and quantum theory to daytime radio, Forbes magazine, and, if the inspiration struck, all in one breath!

Stephenson was energized by the most advanced insights of his time, and yet was ahead of his time. He drew on the work of Kurt Koffka, Freud, Jung and Klein, Langer, Popper, Bridgman and Northrop, Lasswell, and so many others. He was a voracious learner and yet a fearless leader, intent on advancing his vision but also intent on making us his peers. He was often impatient with us for not keeping up, for not "getting it." He often implored us simply "to get on with it!"

The foundation for Stephenson's intellectual range may have been

in his native Northumberland countryside or in the colleges of Britain and not among the imported Gothic structures of our campus on the Midway. A review of his writing during his time at the University, however, makes it abundantly clear that he found his years in Chicago profoundly stimulating.

What we overlooked, I believe, during his Chicago years was the second frontier that Q methodology would open up. We thought then of subjectivity and Q primarily as these pertained to psychology. We concentrated then on how single-case research could be made respectable, how all manner of psychological theories from Spranger to Jung to Henry Murray could be tested with Q methodology, how factor analysis, the analysis of variance and Fisherian designs could test hypotheses derived from postulates, and how Q could be used to identify salient variables, generate meaningful hypotheses and thereby build theory.

In retrospect, it seems clear to me that all along Will had even "bigger fish to fry." He would lead the charge to explore even grander confluences of subjectivities—i.e., the social interactions from which cultures and civilizations are created! I remember from those years the many technical discussions we had about "universes" of items. Some classmates snickered, as a few of us obsessed at length about these "universes"—how and why to select a "universe" and how best to structure a Q sort to represent it. As you probably know, Stephenson's theory of "concourses" came later.

We were enticed by prospects of investigating "universes"—universes of perceptions, conceptions, convictions, preferences, prejudices, attitudes, objects, roles, thoughts, shapes, textures, and so forth into infinity and all under varying conditions. In our more sober moments, these were discussed as "populations" for which we would construct "samples." You see, we had not quite made the full turn to "Q"—the inoculation against R methodology and the whole test-and-measurement virus had not yet taken full effect. The "universes" to be "represented"—not "populations" to be sampled—all became, after the Chicago years, "concourses" to be studied with Q. More significant "concourses" more clearly came to refer to public and social matters, not only personal and psychological ones. Subjectivity in political science, public relations, communications, and group psychology are now all open for scientific inquiry with Q methodology.

In Stephenson's view, "concourses" contain the rich ore of creativity. It is in these "concourses" that we can see how civilization pursues its pleasures and goes about the business of sustaining its humanity. Through Q we can examine exactly how matters of pleasure

and learning from experience, adaptation and change take place.

Even though his critics on campus found him like Don Quixote, single-minded and preoccupied with Q methodology, Stephenson was moved, even at that time, by a deeper and more compelling vision. Even then, I believe, Will understood that a free and nurturing civilization requires us to make room for and to appreciate personal pleasures (i.e., tastes and values) and public "play" (i.e., the daily "fill" of sociability and expressiveness). But more than this, he understood that maintaining such a civilization requires us to accept the challenge of exploring and explaining ourselves.

It was Stephenson's vision that by operating with Q methodology on the concourses of subjectivity, we are creating new facts which will challenge and expand our scientific understanding of ourselves, individually and collectively. With this methodology, the dialogue among scientists will be greatly enlarged. For some, this quest will still seem "Quixotic."

For me, however, William Stephenson remains a very supportive, creative and positive presence. Unfortunately he left the University before I was able to complete my dissertation. I feel he would have been proud of it, as I am. We exchanged letters once but I regret not having sought out opportunities to reconnect with him after he left Chicago. Thanks to Steve Brown, who is doing such a remarkable job of carrying forward Will's work; thanks to Karen Dennis and members of the ISSSS Program Committee for the invitation to share these remembrances; and thanks to all of you for listening. You have given me a very special opportunity tonight to feel reconnected with one of my most important teachers. And thank you, Will—however late, I feel you would understand and appreciate my comments. Thank you all again for being here tonight.

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### References

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