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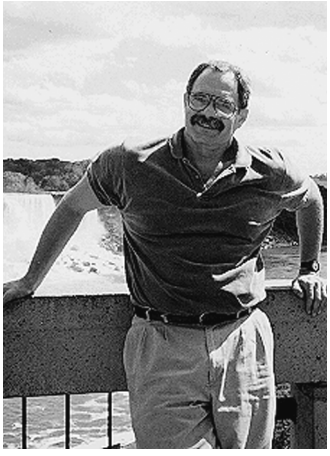
# Profiles in Research

**Howard Wainer**

*Interview by Dan Robinson*



### *Biography*

Howard Wainer was born Howard Charles Goldhaber in Brooklyn New York, on October 26, 1943. He received a B.S. in mathematics from Rensselaer Polytechnic Institute in 1965 and an A.M. and Ph.D. from Princeton in psychometrics in 1967 and 1968, respectively. He taught at Temple University and The University of Chicago before moving to the Bureau of Social Science Research during the Carter Administration. He was a Principal Research Scientist at ETS for 21 years before assuming his current position as Distinguished Research Scientist at the National Board of Medical Examiners. He is also an adjunct Professor of Statistics at the Wharton School of the University of Pennsylvania. He has published more than 300 articles and chapters in scholarly journals and books; his 15th book, *Graphic Discovery,* was published by Princeton University Press in January of 2005. He was editor of the *Journal of Educational and Behavioral Statistics* from 2002 until 2004.

Dan Robinson: *At an early age it seems your career path was forged. What led you to interests in science and mathematics?*

Howard Wainer: My life was changed profoundly when I was 4 years old. At that time my father, Meyer Goldhaber, died of complications from a

bleeding ulcer. He was 35. My father was an anatomist who, because of the depression, had trouble finding work, so he became a dentist and was successfully building a practice when he died. He left my mother with my 6-month-old brother and me. My grandparents then bought a house suitable for holding us all, and we moved in with them. My mother went back to school to get a teacher's credential to support the three of us. About 2 years later she married Sam Wainer, who adopted us; hence, I became "Wainer." My stepfather was a small businessman whose formal education was limited by the exigencies of the depression and his participation in World War II. But he had high regard for education and insisted that we do our best and go as far as we could. We also stayed in touch with my father's family, many of whom were eminent physicists. Thus, both nature and nurture pushed me in the direction of science and mathematics.

Robinson: *You are widely known for your literary and entertaining writing style, why did you decide to attend Rensselaer Polytechnic Institute, which is so narrowly technical?*

Wainer: After my mother's remarriage, we moved to a working-class neighborhood on Long Island. It was a pleasant development of a vast number of identical houses constructed after the war to hold the returning veterans and their families (a generation we have come to call the "baby boomers"). I was mostly miserable throughout high school. My unhappiness could be directly traced to high school cheerleaders not having much interest in classmates whose sole asset was that they were really good at math. But, at the end of junior year of high school I took a test to gain admission to a recently inaugurated NSF project called the Science Honors Program. The test selected 50 or 60 high school seniors from the greater New York City area to participate in advanced training in science and math at Columbia University. I was chosen, and it was the highlight of my adolescent life. To get to Columbia required 2 hours of bus and subway travel each way—no problem. At that age I could read math while standing on a moving subway. In the mornings I took classes in abstract algebra, Markov chains, and number theory, and spent my afternoons in Columbia's Watson Laboratory on 116th street using the university's IBM 650 computer.

This was 1960, and the concepts of computing were still very new and foreign to most people. The 650 was an enormous machine—roughly the size of three large refrigerators, sitting back-to-back-to-back, with a panel of blinking lights that let you know exactly what was going on in its innards. Attached to it, through thick electrical umbilicals, was a matching box that was the power supply, and another that was a card-reading

and card-punching machine. Programs had to be written in machine language and transcribed onto Hollerith cards. The cards were then read into the machine, and the output was punched out, 80 columns at a time, on other cards. You could then take the cards to a printer and see what happened. I was enthralled with this, and each afternoon would write a program to try out some of the things I had learned in the morning. Once we learned about perfect numbers (numbers that equal the sum of their factors; e.g., $6 = 1 + 2 + 3$, or $28 = 1 + 2 + 4 + 7 + 14$). We were told that there were only four known perfect numbers, and so I wrote a program to look for a fifth one. My program found the four known ones, but ran out of time before finding a fifth.

From a modern prospective the 650, as wonderful as it seemed at the time, was a very tiny machine—less powerful than the chip in a microwave oven. It had 2,000 words of storage on a rotating drum, and its computational speed was laughable in modern terms. However, it could do things easily that had been incredibly tedious up until then.

To justify NSF's faith in us, we were supposed to prepare a final project to show some of what we learned. I did something on Markov chains on my own and also collaborated with some friends to build another machine that would do most of what the 650 typically did, but do it faster. Using a cardboard box and an erector set motor we built what we called a "sick 50" that would read your program and, in one step automatically dog-ear the cards and throw them in the garbage. One of my co-conspirators in this project was a 15-year-old prodigy named Robert Strom. He had achieved fame about a year earlier by winning $64,000 on the eponymous TV quiz show.

My experience at Columbia motivated me to stay interested in mathematics despite the best efforts of my college instructors to wring it out of me. I went to Rensselaer Polytechnic Institute because I could study mathematics there and because they had very limited humanities requirements. On the administration building there was a big sign that said "Increased earning power makes a good education a fine investment." That pretty much set the stage for my intellectual involvement at college. Most of college was a blur; with the only things remaining firmly in memory was my participation on the varsity swimming and water polo teams. This selective memory seems to go both ways. When I returned to RPI for my 25th reunion I met several of my math instructors as well as a physics teacher I had for a couple of courses. Although I was a pretty good student they had no memory of me whatsoever. However despite being very much a run-of-the-mill swimmer, my swim coach remembered me well. Indeed, he saw my son Laurent swim at a regional meet, went to speak with him, and after discovering his paternity, asked after me.

Robinson: *And then what events led you to psychometrics and Princeton?*

Wainer: As is often the case, the vital importance of what turn out to be key events in our lives are rarely recognized at the time. Three such events occurred while I was at RPI. The first was a psychology course I took from George Boguslavsky (who, in 1955, had published a paper on a mathematical learning model in *Psychometrika*) and was fascinated. Boguslavsky showed how vague ideas could be codified and through the use of mathematics, yield rigor and unanticipated implications. I suspect that he was unused to the sorts of enthusiasm I displayed and so, a couple of years later he recommended me to Harold Gulliksen for a Psychometric Fellowship at Princeton.

The second key event developed from my inability to learn German. Three semesters of German were required for math majors. I failed German I, and so second semester, when I retook it I also took an extra course (so I would not be a course short for graduation). The same thing happened sophomore year when I failed German II, and had to take six courses second semester. The extra course was an introduction to mathematical statistics. Unfortunately this course was given at the same time as German, so I never went to class (I really had to go to German). My friend Michael Dimen went, and he took careful notes and kept me up-to-date on the assignments. He also handed in my assignments for me. When the exams came we would study together—or more accurately, he would teach me what was to be on the test. I passed German II but got an A in statistics. Either Dimen was a terrific teacher or I was pretty good at statistics. I took more statistics courses and did well on them, and I suspect that this was important in getting Princeton to overlook my German grades.

The third of the key events occurred toward the end of my junior year. I shared an apartment with two friends. A family named Elfenbein lived next door, and they had a very attractive daughter. She was a freshman at Barnard, and we became friends. I only got to see her on those occasions when she was on holiday, and I was not. Fortunately, I often stayed at school during holidays to train and so during the 1964 spring break she came home with her roommate, Linda Steinberg, whose family lived in Albany. I was completely smitten by Ms Steinberg, who was both the most beautiful girl I had ever seen as well as the smartest; in 1989 after an extended, albeit intermittent, courtship, she finally agreed to marry me. I am nothing if not persistent.

On April 1, 1965 (note the date) I received two letters. One was from Harold Gulliksen telling me that I had been awarded an ETS Psychometric Fellowship for graduate study at Princeton (conditional, of course, on the pro forma admission to the graduate school). I was delighted because it offered the opportunity to participate in a bridge program in both mathematics and psychology. The second letter was from Princeton telling me that I was wait-listed for admission. I thought sending a pair

of letters like this on April Fool's Day was a pretty poor joke, but I had also been accepted at the Harvard Business School and reckoned that was a reasonable back-up plan. So I went for a beer and forgot about it. A week or two later I got a call from Gulliksen asking if I was going to accept the fellowship. I told him that I would have liked to but had not been accepted by the University. He asked me to wait by the 'phone, and he would call me back shortly. Within 15 minutes the 'phone rang and Harold told me that I was accepted, and would I come. I said yes, and that was that.

Princeton was everything that RPI was not. The goal was scholarship and undirected curiosity was rewarded. Over the mantel in Proctor Hall at the Graduate College is the inscription "*Bonus intra, mellior exi*"—enter good, leave better—a long way from increased earning power. Graduate students were treated as junior colleagues—we were not on trial to see if we could make it, but rather it was assumed that we would succeed, but it was up to us to find a direction that would allow us to make a contribution. The other ETS Psychometric Fellow my year was Charlie Lewis, with whom I shared an office. If you look up "Oxford Manner" in the dictionary you will find a picture of Charlie. Nothing seemed to bother him, and his mastery of the courses we took seemed effortless.

In the first 2 years of graduate study Psychometric Fellows were expected to take the usual departmental requirements as well as two special psychometric courses each year (we referred to them as the "Four Courses of the Apocalypse"). The first year we had test theory taught by Fred Lord and Mel Novick—we learned it from drafts of what eventually became Lord and Novick (1968). I hated it. In retrospect, when I pick up L&N and try to retrieve something from it I am reminded why. Gulliksen's (1950) *Test Theory* was, and remains, a lot easier for a novice. I hope that some of the lessons I learned from this were carried over in our preparation of the *CAT Primer* (Wainer, 1990/2000) and *Test Scoring* (Thissen & Wainer, 2001). I was very fortunate in the make-up of that class. There were only four students who took it—Charlie Lewis, Jim Ramsay, Sam Reed, and I. After getting to know Charlie and Jim, I realized that if they were a representative sample of the talent in this field, I was in for a rough career. However, happily there was Sam, who blissfully smoked his pipe in class and always managed to keep me in the top 75% of the class—although sometimes only barely.

The other course was on mathematical models of learning—Harold taught the first semester. In it I learned how to sleep with my eyes open. Harold was a wonderful, wise, kind, and gentle man, but he could make a 2-hour class seem like a week-long endurance contest. Swimming the English Channel was easier. Robert Bush was imported from Penn to teach the second semester. The material was not any more interesting but the course was. From the kinds of misfit of the simple linear models that were described in Bush and Mosteller, it was clear to me that characterizing

the path of learning in more complex situations might be more accurately modeled with a stochastic generalization of the stimulus sampling models that William Estes had developed. My eventual pursuit of this topic led to my dissertation. In fact, the Markovian character of this model drew directly from what I had done in my high school project at Columbia.

Second year Harold taught us scaling. Reading Torgerson was a combination of measurement and the philosophy of science. It represented, for me, a real consilience. At that time, nonmetric multidimensional scaling (Shepard, 1962; Kruskal, 1964) was only just becoming popular, and we were all interested in trying it out, for it seemed to offer the possibility of measuring things that were untouchable otherwise. My fascination with this method lasted many years and resulted in my using it to study poetry, French stories, Rorschach inkblots, and any number of other phenomena. In addition, ETS imported a very young Karl Joreskog from Sweden to, among other things, teach us factor analysis. It was a brilliant course, full of important new ideas that could immediately find use. We all learned the new lexicon (UMLFA, RMLFA, and ROTSIM), which eventually grew to become LISREL and its brethren. My life and Karl's became inextricably tied together on January 20, 1968, when the two of us paced around a waiting room of Princeton Hospital for a couple of hours together. His daughter Karin and my son Laurent were born a few minutes apart.

Graduate school finished with an explosion of activity. I finally passed the required German exam (after four tries) and wrote my dissertation. My son Laurent was born, and I met John Tukey. Tukey represented a point of view quite different from any I had learned before. It seemed too free-and-easy to be legit, but Tukey's reputation forced one to consider the stuff seriously. Issues of robustness were everywhere, and draft copies of EDA were pored over. It was a whole new way to think about things. Moreover, although we were all skeptical, as soon as we got into the habit of using EDA methods, we were hooked. Even Ramsay, who was not easily impressed, became a convert. The connections with Tukey that were established then grew and lasted for more than 30 years. Tukey taught me more than statistics; he was the only genius I ever knew well. He caught on to new things lightning fast and was able to see deeply into them. Although I could certainly not duplicate his feats, just watching how a first-class mind works helped to shape my own approach to problem solving. I learned from his "axiom number one" that "people are different" and so listening seriously to other's thoughts is an invaluable aid in seeing problems more broadly.

Robinson: *You didn't go right to ETS immediately after graduate school. Why did you decide instead to enter the academy?*

Wainer: After I finished my degree I was pretty clueless about academic careers. I was offered a position at Temple University in Philadelphia, and,

because it was close to my friends in Princeton and my family in New York, I accepted. It didn't take very long for even someone as dense as I was to figure out that Temple wasn't Princeton. As the newest assistant professor I was low man on the totem pole and shared an office with an assortment of mops and brooms; my teaching load had me doing Intro Stats at 8:00 AM on Monday, Wednesday, and Friday and a Monday evening version from 6:00 to 9:00 PM. I also had a more advanced course on Tuesdays and Thursdays. There wasn't a lot of spare time to think about research that year. Although part of the blame for that was mine. Soon after I arrived, a parade of students and faculty came to my door asking for help. This never happened at Princeton, and I was flattered. It wasn't long before every spare minute was used up doing analyses for others. I felt useful, but a bit overwhelmed. About mid-year I was back in Princeton having lunch with Harold, and when he asked about my research, I grimaced and told him that there was no time. He asked what was taking it all up, and I explained. His advice was sage and practical. He told me that I should remember that my goal was not to help the students get their projects done, but rather help them learn something. He suggested a 4-step solution:

1. Ask all who come for a consultation to prepare first a one-paragraph description of their problem and give it to me a day or two in advance, so I might be able to think about it (this alone cut back on the line by 30–50%).
2. Prepare an annotated bibliography.
3. Check off the appropriate reading on the bibliography and give that to the student.
4. Only if I didn't know an appropriate reading should I meet face-to-face with the student.

I followed this advice and found that, once students realized that they would have to do something themselves, the torrent of help-seekers shrank to a trickle.

Robinson: *What took you from Temple to Chicago?*

Wainer: Before the end of my first year at Temple I decided that the right career path for me almost surely lay elsewhere. So when a suitable opening occurred at Chicago, I was keen to switch. The only regret I had in leaving was that after I had made up mind to leave, Leona Aiken arrived. Her enthusiasm for the field more than matched mine, and had I stayed, we would certainly have been able to sustain one another. As it is, we did manage to write one paper together that used multidimensional scaling of Rorschach inkblots to diagnose patients (Wainer, Hurt, & Aiken, 1976). However, Chicago had a rich reputation—after all, that was where Gulliksen and Tucker had begun, and Thurstone before them. But that was in the past; for me the reason to go was Darrell Bock.

I first arrived at the University of Chicago as a new assistant professor in September of 1970. As I said before, I ventured into the Midwest primarily because Darrell Bock was there (who I have always thought of as the IBM of psychometrics), and I knew that I could learn more psychometrics from him than from anyone else in the world. The problem I faced was in uncovering a mechanism that would allow me to get close enough to learn.

We met at the usual "get acquainted" gatherings that are common for new faculty. I made a point of asking what he was most interested in at the time, and he told me about a remarkable longitudinal data set that had been gathered over the past 40 years at the Fels Institute in Yellow Springs, Ohio. Part of this data set was a substantial number of complete records of growth in stature. He had invited Alex Roche, of Fels, to visit Chicago and speak about the Fels data. Darrell asked if this interested me and I enthusiastically nodded assent.[1]

Thus began a commitment to an avenue of research that actively continued for more than a decade and resulted in dozens of papers and at least three books. This work began modestly enough with an informal seminar on human growth that Darrell organized. We met one evening a week. Darrell and I were the only faculty that attended regularly, but we were joined by a group of remarkably able students. Among them were Jim Murray, Anne Petersen, and David Thissen. Our first project was to try out a new parameterization for human growth that Darrell had dreamed up that was a simplification of an idea of Cyril Burt's. Darrell's idea was to represent human growth in stature as the sum of two logistic components. Once he convinced himself that this would work, I was given the task of writing a computer program to estimate its parameters. I teamed with Anne Petersen to do this (Wainer & Petersen, 1972), and once the program was completed, we tried it on almost any longitudinal data that we could find. The fits it yielded were almost perfect; at least almost perfect for the range of people whose stature was in the normal range and whose data record began no earlier than age 1 year.

This seminar was the beginning of my 30-year collaboration with David Thissen. Dave has been protecting me for years. For example, because he understood JCL,[2] I never had to learn it. I remained grateful for this for many years. I have taken many things from my long relationship with Thissen, but most important was the realization of how attractive his enormous generosity of spirit makes him. I have tried to copy him, but never as successfully as I would like.

While at Chicago, my primary teaching responsibility was a two-term statistics course for psychology graduate students. My original idea was to divide the course in half, with the first half covering exploratory data analysis and the second confirmatory procedures. This intuitively attractive approach was a complete flop. First because EDA was almost

unheard of in 1970, and so most students thought I was wasting time that could more profitably be spent doing ANOVA and chi-squares. But my course on EDA was one of the very few in the country at that time and so when Fred Mosteller and John Tukey organized a session on it at the 1972 annual AAAS meeting they asked me if I would agree to be a discussant. I agreed (who could refuse that sort of invitation?) but found the prospect of discussing the work of the leading people in the field more than a little daunting. So instead I decided that I would do some simple shoebox experiments on the efficacy of the, mostly graphic, methods proposed. That study (Wainer, 1974) would have profound implications because in 1976, Al Biderman was looking for someone interested in statistical graphics to lead an NSF project on graphic social reporting. Tukey mentioned me, and I spent the 4 years of the Carter Administration in Washington. My 4 years at the Bureau of Social Science Research taught me a great deal about how successful scientists worked. One key idea, that I'd like to share, is that the best (perhaps the only) way to write a successful proposal to a funding agency is to have a lag of one. What I mean by that is (a) first do some research, then (b) write a proposal to fund that research (that way you can be very precise about what you will do, what you will find, etc.), (c) after you get the money you work on the next project, and (d) go back to (b). The hard parts are getting started and controlling your desire to publish prematurely. While in Washington, I worked with other social scientists whose interests ranged broadly. This led me to apply psychometric methods in unlikely places (e.g., robust IRT to study parole recidivism) and learn about survey methods.

Robinson: *It sounds as if between working with Bock at Chicago and dabbling in consulting projects, you had found the right home. Why, then, did you decide to abandon this career path for ETS?*

Wainer: After 4 years in Washington, I met with Don Rubin, who had accepted a position with the Environmental Protection Agency.[3] He was trying to put together a statistics research group, and toward this end recruited Paul Rosenbaum and Rod Little. We discussed my joining in, but after a little while, it became clear to both of us that my interests made me more suitable for a position in ETS's Research Statistics Group. I believe that Don spoke with Paul Holland who invited me to join ETS. I did, and the 21 years I spent there were as fruitful as I could have hoped for. Working with Paul was, and remains, a great joy. Our arguments brought out the best in me. I have long been a fan of latent variables. I believe that the concept of a latent variable allows the construction of powerful models suitable when data are insufficient. Paul is an observed score guy. When the problem of detecting items that function differently in different examinee subgroups became important, Paul and I, predictably, went in different directions for a solution. After a few years of working on it, we

agreed that we knew enough to anthologize the results, and our DIF book (Holland & Wainer, 1993) was born. From Paul I learned many things, but prominent among these was his advice that "any memo to a vice president should contain, at most, one point." It is remarkable how often this advice has proved useful.

If Holland had been my only colleague at ETS, I would have been satisfied, but in fact ETS is overflowing in talent. During my time there, I was fortunate to be able to work closely with Henry Braun, Sam Messick, Bill Angoff, Bob Mislevy, Charlie Lewis, Eric Bradlow, Xiaohui Wang, and a host of others whose names are prominent on the reference lists of the psychometric literature.

Charlie Lewis is very special in many ways. One of these is that he can listen to you describe your problem and then he can discuss plausible solutions in your language. Most of us have to translate into our own terms before thinking about them. Charlie doesn't have to (or he translates out and back in so quickly that you can't tell he's done it). I have never met anyone who could do this as well as he does. In addition, he is an enormously caring person. Charlie enlisted me to join him for lunch with Harold Gulliksen every 2 weeks. We did this regularly for several years before Harold's death at the age of 93. Harold was an empiricist down to his soul. One afternoon we brought Harold to ETS to have lunch with a crew of ETS summer pre-doctoral students. They knew Harold's work, but it was a thrill for them to actually meet him. At this time Harold was very infirm and often did not remember either of us. As I was driving him home from this luncheon shindig he observed that there seemed to be a lot more traffic on Rosedale Road than he remembered in the past. Two miles later, when we turned off onto Cleveland Lane, Harold said, "96 cars."

John Tukey was another well-known empiricist. Charlie arranged for John to come to ETS regularly to consult with whomever had a problem. Although the queue to see him was very long, there was always time for us to speak about the future of graphics. John's ideas on this formed the basis of the last third of my book *Graphic Discoveries.* Charlie and I joined with John to form a modest version of Tukey's famed "Marching and Chowder Society." I remain grateful to Charlie for arranging the meals that we shared with Tukey and for helping me to understand his sometime Delphic pronouncements. We did this up until Tukey's very last day, when the three of us shared a mince pie in his hospital room and discussed the role of multiple comparisons in data mining.

During the 21 years I spent at ETS, I was permitted time and resources to pursue the graphic interests that had begun during my stint in Washington. In 1991, I began writing a column on graphics in the statistics magazine *Chance.* This hobby has continued unabated for 14 years and has brought me great joy. Every 6 or 7 years I've gathered the columns into a book (Wainer, 1997, 2000, 2005) that my mother can read. More-

over, writing for *Chance* provided the sort of broad audience that allowed me to collaborate with my younger son, Sam. His 9th grade math project grew into an article on self-selection (Wainer, Palmer, & Bradlow, 1998), and his participation on the high school swim team led to two joint *Chance* papers on sex differences in sports (Wainer, Palmer, & Njue, 2000a, 2000b). Sam's admission letter to Princeton sparked a *Chance* article on score reports (Wainer, 2002). This is yet another example of how I was pulled along on someone else's coattails.

Robinson: *It seems as if the story should end at ETS but it does not. Why did you decide to leave ETS for NBME?*

Wainer: ETS became a less happy place in 1993 when Holland left for Berkeley, and the administration's focus on computerizing tests had profound, negative, consequences for the resources available to do research. Happily, the deterioration that characterized the ETS environment for the latter part of the 1990s ended abruptly in 2000, when after an effort to unionize ETS research staff only narrowly failed, ETS's trustees appointed Kurt Landgraf to replace Nancy Cole as president, and Paul Holland retired from Berkeley and returned to ETS. I was delighted with the turnaround, and probably would have finished my career at ETS had I not run into Brian Clauser, of the National Board of Medical Examiners at a Psychometric Society Meeting in Valley Forge. Brian and his colleague Dave Swanson took me to lunch and spoke glowingly of the work going on at NBME and the role that I might play in it.

Eventually the Board made me an offer I couldn't refuse, and so I joined up in 2001—a move that has delighted me in every way. In addition, I found that I could keep my hand in academics by teaching a little in Penn's statistics department, where I have continued the collaboration with Eric Bradlow that we began at ETS so long ago and renewed my friendship with Paul Rosenbaum.

I erred badly in one aspect of my career. I had always thought that when you pass your 60th birthday the pace of life would ease; that frantic deadlines would no longer loom so frequently. Alas, I could not have been more wrong.

## Notes

[1]Of course I would have shown equal enthusiasm for mule skinning if that would have gotten me in closer contact.

[2]JCL (Job Control Language) was an arcane IBM invention that required a lot of work to get a computer to do something very simple. Dave prepared labeled packets of JCL for me to stick on the front of programs. Happily, JCL is now gone and I am still here.

[3]Don was an old friend, and the prospect of working with him and the team of young statistical all-stars he assembled was very tempting. But in the end I didn't

trust the newly elected Reagan administration to maintain support for the sort of group Don imagined. It didn't take Don long to share my view—he quit EPA after being employed there for about 2 weeks.

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