

A Conversation with John C. Bailar III

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Abstract. John Christian Bailar III was born on October 9, 1932, in Urbana, Illinois. He received his B.A. degree from the University of Colorado in 1953, an M.D. from Yale University in 1955 and a Ph.D. in statistics from American University in 1973. He is a Fellow of the American Statistical Association, an elected member of the International Statistical Institute, a Fellow of the American College of Epidemiology, a Fellow of the American Association for the Advancement of Science, an elected member of the Collegium Ramazzini and a MacArthur Fellow (1990–1995). He was Editor-in-Chief of the *Journal of the National Cancer Institute* and has been on the Editorial Board of *Cancer Research*. He has served as Statistical Consultant for the *New England Journal of Medicine* and is currently a member of its Editorial Board. He has served as Chair of the Biometrics Section of the American Statistical Association, was Founding Chair of the Boston Chapter of the Society for Risk Analysis and was President of the Council of Biology Editors. His tenure at NIH included the years 1956–1970 and 1972–1980 on staff at the National Cancer Institute, with a stint at the Veterans Administration from 1970–1972. He began as a Field Investigator in the Biometry Branch, was appointed Head of the Demography Section and then Director of the Third National Cancer Survey. His last appointment at NCI was Deputy Associate Director for Cancer Control. He was awarded the Commendation Medal from the United States Public Health Service for his work on breast cancer screening. Since leaving NIH, he was a Senior Scientist at the Environmental Protection Agency and the Department of Health and Human Services, a Lecturer in Biostatistics at the Harvard School of Public Health, on staff at the Health Effects Institute and Professor and Chair of the Department of Epidemiology and Biostatistics at McGill University Faculty of Medicine. Since 1995, he has been Professor and Chair of the Department of Health Studies at the University of Chicago.

Ellenberg: What brought you to NIH and when did you come?

Bailar: I took a reading course in statistics while I was an undergraduate. I found it very interesting, but there was no way to pursue the subject at that time. Later, I thought I had to make a choice

between statistics and something else. I ended up choosing medicine. I was well through most of medical school before it occurred to me I didn't really have to make a choice. I learned from the example of Dr. Colin White. I was at Yale then, and he was on the faculty. He was a marvelous teacher of statistics, and doing some rather interesting research on medicine and statistics. So I did my required medical dissertation with him on some aspects of 2×2 tables and, particularly, biases that occur when one of the 27 cells in a $3 \times 3 \times 3$ table is missing or undercounted. It is one way to look at what is called Berkson's bias [7].

Because of my growing interest in statistics, I thought it might be worth coming to Washington

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FIG. 1. *John C. Bailar III.*

briefly. It seemed to me that the National Cancer Institute (NCI) was the place I wanted to work. I had been very much impressed by Mike Shimkin and Bill Haenszel, and one or two others there. I went off and did my medical internship; then I came straight to NCI in 1956.

Ellenberg: When you came to NCI, were you in Mike Shimkin's group?

Bailar: I think that technically I was assigned to Bill Haenszel, who was Chief of a Section under Shimkin. At that time, that was the only statistical group at NCI. There was already some differentiation of the statistical groups at NIH, but people got together every day for lunch. They didn't much care whose Institute you worked for.

Ellenberg: Were you the only statistician there with an M.D. at that time?

Bailar: I think I was the only person at NIH who had both an M.D. and a strong interest in statistics, with the exception of David Alling. It was years and years and years before there were any others with medical training.

I started working there on June 24, 1956, and had several very small assignments. And then I talked with Haenszel at his request about being re-assigned to the state of Connecticut to work with the Connecticut Tumor Registry. I spent probably seven or eight months getting ready to go to Hartford to do that. I had become interested in cancer of the uterus, both cervix and corpus. I did a great deal of reading. Of course there was not as much literature then as there is now. I devised a form and, basically, a very primitive computer system for handling the data.

Ellenberg: A computer system in 1956?

Bailar: Well, in the sense that a form was printed by computer from the old-fashioned punched cards the Registry had. And then I marked boxes on the form, which were coded and made into new cards, which I could then use in statistical analysis. So, I guess you shouldn't call it a computer system; it was a punch-card system. NIH had computers then, and we thought that I might use them. I think that was when they had the old IBM 650.

I went off to Hartford and began abstracting records for the cases in the Registry. I found that the Connecticut Cancer Registry had very accurate identification of individuals, but not very much detail about individuals or about their cancers. So, I basically used the Registry as an index and spent the next year and a half going to all the general hospitals in the state to fill out my abstract form. I believe there were 35 hospitals and a total of about 6,000 patients. I started early in the morning Fri-

day and would work until late at night and then drive back to Hartford.

It was a solo project. My goal was 25 records a day and there was a certain administrative overhead. When I first wanted to go to a hospital I would have to call the hospital director, arrange an appointment, tell that person what I wanted to do and why. I had letters of support from the State Health Department, the State Medical Society and elsewhere. I had no difficulty. This was in 1957. I'm sure that nobody could do this today, even at the hospital level. People would be much too nervous about letting a stranger look at their records. But it wasn't a problem in the late 1950s and so I developed a very substantial database, which I used for probably half a dozen original articles over the following several years [1-3].

Ellenberg: Did you work with Haenszel or Shimkin on this project?

Bailar: I was working in their group, but not with them. I have always been pretty much a loner in my work, and that has some advantages as well as disadvantages; but it is just the way I do things. I had a desk; I could come and go; nobody kept track of my time. I was working very, very hard, and very long hours—both before I started the abstracting of records and during that time; but I guess they figured Bailar works best alone.

At the end of this time, I went to a summer program at the University of Michigan. I took courses from Bill Cochran and Helen Abbey. And then in 1958, when I went back to Connecticut, I resettled in New Haven rather than Hartford. I had an academic appointment at the Yale Medical School in the Department of Public Health.

Ellenberg: In addition to your appointment at the National Cancer Institute?

Bailar: Yes. This was all done as an NCI employee, but Yale gave me desk space. I was in Colin White's group. I taught courses and spent most of my time in analysis of the data that I had collected. In addition I took a course in statistics from Chester Bliss that Cochran had told me about when I was at Michigan. It was fascinating. It was a two-semester course, three credit hours per semester. Cochran had told me what to expect, but the other students didn't know. After the first week or so, Professor Bliss told us that things were going a bit slowly, that we would have to meet for another hour or so each week. Pretty soon it was an extra two hours, three hours. Before long, we had found that he had added a lab. Of course, in those days it was with the desk calculators and they were *very* slow. The homework assignments got bigger and bigger. By mid-November, it was virtually a full-time job. Ex-

ams were assigned over long holidays, like Christmas, so that we would have plenty of time to work on them. We got through the school year doing almost nothing else.

In May he said he was not yet finished, that we would continue and have an exam and grades when he was finished. And he wound up the course just before it was time to start teaching the following year's class. It was marvelous training. Bliss claimed he had no interest in statistical theory, but he had an instinctive grasp of many things that other people have to work out by means of theory. He had just an uncanny knack for understanding degrees of freedom in complex kinds of models. The whole course was taught from real examples that he had collected over a lifetime of scientific work. I remember those examples, and I still use some of them, because they were so cogent.

I should mention that this course was taught formally within the School of Forestry. It was not the kind of course that sat very high with the Statistics Department because it was all applications, flamboyant applications, with a real disdain for any kind of theory. It was a marvelous experience, and I trace much of my skill in statistical analysis back to that one course.

Ellenberg: At that same time you were taking this full-time course, you were continuing to analyze your data from the uterus cancer project.

Bailar: I was trying to, but it went slowly for that 12-month period. I finished the course and shortly after that moved back to Bethesda. I started working more closely with Haenszel's group and stayed there doing various kinds of projects, mostly self-generated, for several years. And one day in 1962, Haenszel called me in and said they would like to make me head of the Demography Section.

I had been making fairly extensive use of the National Cancer Surveys done in 1937–1938 and 1947. The difference from the first survey to the second was quite substantial. It was not clear how much of this was because of the improvement of methods and how much was real. There had been a separate cancer survey in the state of Iowa in 1950, which again showed a different pattern; yet somehow they never got themselves organized for a 1957 survey. In 1967, I wrote a couple of long memos about how we ought to have another cancer survey. I proposed a three-year survey, centered around the 1970 Census. This was approved and led to the third National Cancer Survey. We carried over most of the same geographic areas that had been in the two earlier surveys, but made some changes in boundaries. I collected some staff, two people who worked with me very intensely: John Young and Susan Devesa.

The organization and implementation of the third National Cancer Survey was backbreaking work. Planning began in March 1967 and data collection started January 1, 1969, and was to end December 31, 1971. The planning continued in 1969, even though we were already collecting data. I was with it long enough to make sure that it was running well, but by the summer of 1969 I was simply burned out.

I wasn't looking for a job, but one came my way at the Veterans Administration, where I became Chief of their national Research Program (1970–1972)—pure administration, nothing to do with statistics, but I learned a great deal.

To finish the story of the National Cancer Survey, I thought the third survey as a whole turned out quite well. When I went to the VA, of course, I left behind this Cancer Survey effort. Case material had been collected through 1971; 1972 was a year of consolidation. And then the Surveillance, Epidemiology and End Results (SEER) Program began in 1973. Basically, it was a continuation of the Cancer Survey.

Ellenberg: So, in some sense, you might be considered the father of the SEER Program?

Bailar: Or maybe better, the grandfather! They continued with the areas that I had selected for the National Cancer Survey, pretty much the same data items, and definitions, and procedures. Of course, all of this has evolved since then, but there was not much in the way of identifiable differences between what we started in 1969 and what the SEER started in 1973. I know that both groups tried to do things right up to standard; standards don't change that fast.

Ellenberg: When did Dave Byar join your group?

Bailar: I don't remember exactly when, but it was after I had been teaching a course at the U.S. Department of Agriculture Graduate School for several years. Byar was then at the Armed Forces Institute of Pathology, working with Cas Mustaffe, as a pathologist. Mustaffe called me one day and said "I've got this guy who wants to be a student." I said "Fine, send him along." And he turned out to be just a superb student in what was basically a first graduate-level course in mathematical statistics. I thought we could use him, so I talked to Mustaffe again, made sure he didn't have any objections, offered Dave a job and pretty soon we had him on board.

Ellenberg: But his interest did not lie in the general area of demography or surveys.

Bailar: His interest was more in clinical trials. He worked with me to some extent on the VA trials on therapy of cancer of the prostate and bladder.

He knew what he wanted, how to go about it, and I was very happy to have him do that. And he turned out to be a wonderful addition to the NCI and NIH staff.

Ellenberg: So you left for the VA for two years and then you returned to NIH in 1972?

Bailar: In September 1972, I returned to the Cancer Institute, but in a completely different role, as Acting Director of the new Cancer Control Program in the NCI Director's Office. The program was set up by the National Cancer Act of 1971, and by September 1972 they were ready to put themselves in business, with a director and small staff. So I was the Acting Director for a year and a half. It didn't work out as well as I had hoped, primarily because of serious disagreements with other parts of NCI about the mammography screening program. As soon as I got there, the program was presented to me as a *fait accompli*. I asked to see some of the program documents and I had some questions, but I couldn't get answers at all.

Ellenberg: Could you summarize the major issues?

Bailar: Major issues had to do with radiation risk related to the mammography; the size of the benefit that would be conferred; the effectiveness of follow-up; and delivery of needed services. As I recall, even at that very early time I was also concerned about the difference in apparent effectiveness of screening between older and younger women and, of course, it still dominates the discussion.

I accept, and I think almost everybody accepts, that something about screening (maybe not from mammography per se), can reduce breast cancer mortality by about a third in women past 50. But many of us still see no evidence that it is beneficial at younger ages [4, 5].

Ellenberg: Had these issues been considered?

Bailar: I felt they had not been adequately considered. There was a memo in the record stating that the radiation risk probably wasn't a serious matter, but it was not clear that this was meant as an authoritative statement, nor is it clear that anybody paid much attention to it at the time. The fundamental problem with the mammography screening program is one common in highly interdisciplinary work: nobody sees the whole. This was an issue that involved epidemiology and biostatistics, but it also involved the practice of radiology, plus radiation biology in relation to the cancer risk, plus radiation physics because the nature of the quality of the radiation is not like it is in most diagnostic x-rays. It also involved pathology, certainly public health and the delivery of health services, and even such matters as economics and law. The

list of areas of expertise just seemed almost endless. And people who were expert in each part of it had been involved, or soon became involved, but nobody was looking at the whole thing.

I finally asked for an appointment with Frank Rauscher (Director of the NCI at that time). I went through my concerns, my calculations of radiation risk (now, this was in the old days when doses were far higher than they are now) and benefits and explained why I was concerned. And Rauscher said that he would certainly look into it. As I left his office, I heard him say he would have to talk to the American Cancer Society (ACS). Nothing happened. I called his office two or three times, and still nothing happened.

I had suggested a set of three committees to look at different areas of concern. And I even proposed chairs for the committees, and after some delay Rauscher followed through. The three committees reviewing the mammography screening program then had a steering committee that basically agreed with the committees's conclusions. As a result, NCI changed its policy on screening, restricting the recommendation for annual mammography almost completely to women past the age of 50.

Ellenberg: What do you think made Rauscher more interested?

Bailar: The timing of things is a little fuzzy now, but somehow a story got into Jack Anderson's column that quoted me. And then things began to happen. I can't remember for sure that Jack Anderson's story came before the committees were established. It was a *very* interesting time.

The ACS looked on the Cancer Control Program as an extension of their activities and they wanted to direct what went on. I did not agree. I had difficulty with the other Division directors also. By the time I got back to NCI from the VA they had already divided up the budget to the four Divisions in approximately equal amounts. They thought the Cancer Control Program should be one person with a secretary to supervise the distribution of money. For example, in the area of cause and prevention they felt that the Cancer Control Program would be used as a conduit for funding epidemiology studies. Similarly, the Division of Cancer Treatment would be used to fund treatment clinical trials.

The Division of Cancer Biology and Diagnosis was going to go into this screening program, and the fourth division, which had to do with training and education, planned to support the training of practitioners. So they knew just what to do with every bit of the money; and, again, that was not my idea of the role of Cancer Control. Because of the controversies, eventually, I took on the job of Editor of *The Jour-*

nal of the National Cancer Institute (*JNCI*), still in the office of the Director.

Ellenberg: Did editing *JNCI* occupy you full-time, or were you involved in research projects as well?

Bailar: Running the journal was a full-time job for about the first month. Nobody is ever trained to be an editor, so I had a lot of learning to do. But within a few months it was taking about an hour a day and I had a lot of time to continue working on breast cancer screening. So, I worked up some papers, spoke at national meetings and did other kinds of writing. I essentially had a free hand to pursue this, which was very intensive work for me. It was the hardest work I have ever done, for a period of several years. Eventually, it resulted in the very first of the NIH Consensus Conferences, "The Role of Mammography in Breast Cancer Screening," that came down almost a hundred percent where I had hoped they would [6].

Ellenberg: And how long did you remain at NCI?

Bailar: I actually left NCI on November 1, 1980. It was clear that I had stomped on a lot of toes, and I didn't want to spend the rest of my days editing a journal that wasn't taking me more than an hour a day. Shortly after I had gone to the *Journal* in 1974, I was assigned to work with the Statistical Center for The Eastern Cooperative Oncology Group (ECOG), which was Marvin Zelen's operation, then in Buffalo.

Ellenberg: How did that get arranged?

Bailar: I can't remember exactly, but I was talking with Marvin Zelen about it and the idea arose.

Frank Rauscher said, "Fine, go ahead." I tried to spend a couple of days a week in the ECOG offices in Buffalo, sometimes three days a week, going over problems. I gave courses for the staff, mostly on medical matters. They could turn to me as a medical consultant, except for very highly specialized questions, which had to go back to the group chairman. Even though I was functioning more as a medical consultant, the knowledge of the statistical issues was important. I felt like I was really working on the boundary between medicine and statistics, which is where I spent most of my career, and continue to do so.

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