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A Conversation With George W. Comstock

Dale P. Sandler

George Wills Comstock was born in Niagara Falls, New York, January 7, 1915. He graduated from Antioch College in 1937 and received an MD from Harvard in 1941. After serving in World War II, he returned to public health in 1945, working in tuberculosis units in Ohio, Virginia, Georgia, and finally in Washington, DC, where he was Chief of Epidemiologic Studies in the Tuberculosis Program of the U.S. Public Health Service. Comstock received an MPH from the University of Michigan in 1951 and a DrPH from Johns Hopkins University in 1956. He joined the Johns Hopkins faculty in 1962 and became Alumni Centennial Professor of Epidemiology in 1965. He served as Editor-in-Chief of the *American Journal of Epidemiology* from 1979 until 1988. He is best known for his work on tuberculosis epidemiology and prevention. He established and directed the Johns Hopkins Training Center for Public Health Research in Hagerstown, Maryland. Comstock gave the Wade Hampton Frost Lecture at the American Public Health Association meeting in 1974, and was appointed Honorary Fellow in the Faculty of Public Health Medicine, Royal College of Physicians of the UK in 1987. He received the John Snow Award from the Epidemiology Section of the APHA in 1984, the Edward Livingston Trudeau Medal from the American Thoracic Society in 1980, the Abraham Lilienfeld Award from the American College of Epidemiology in 1987, and the Horace Mann Award from Antioch University in 1992.

INTERVIEW

DS: How did you get started in epidemiology?

GC: I've always said I've been lucky all of my life. I graduated from Antioch College in 1937. Students at Antioch all hold part-time jobs or internships. I worked for the pharmaceutical company, Eli Lilly, in a group that was working on pellagra. My job mostly involved washing glassware and cleaning dog cages, but it gave me an introduction to nutritional epidemiology and to public health because of Goldberger's work (on pellagra).

My boss there was directly responsible for my career direction. He encouraged me to get an MD instead of the PhD in biochemistry I had been considering. He later encouraged me to attend Harvard rather than Case Western Reserve Medical School

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George Comstock playing the recorder, 1974

where many of my friends were headed. That was much better advice than I think even he knew. Harvard was a surprisingly wonderful environment. The faculty was friendlier and more approachable than I had expected. I owe a great deal to Antioch for getting me that job, and to Harvard for giving me what I think was a remarkable education, from a social as well as from an educational point of view.

When it came time to look for an internship, I didn't have much money, and most internships paid little more than room, board, and laundry. Since I was already interested in the Public Health Service (PHS) and they paid more, I decided to apply to the PHS for my internship. If I liked it, there would be a good chance I could stay on, but if not, I'd at least have earned enough money so I could afford to take an ordinary residency.

The other bit of luck was when I came off sea duty near the end of World War II, I was immediately transferred to the newly created PHS division of TB Control. My first assignment was in Cleveland, Ohio. Morton Kramer (who eventually became a professor of Mental Hygiene at Hopkins) was there as a statistician. It was there that I wrote my first

research paper.¹ Abe Lilienfeld was also in the PHS, and he and I were the 2 who were considered for subsequent research openings. Abe got the assignment in Philadelphia. I was sent to Columbus, Georgia.

It turned out that the Philadelphia study was a bust. Abe resigned from the Public Health Service and went on to New York State where he made his reputation. The Columbus, Georgia, project turned out to be a remarkable study, largely because it was under the direction of Carroll Palmer. Palmer is virtually unknown in epidemiology because he worked in tuberculosis most of his life. Otherwise, he would be recognized as one of the world's greatest epidemiologists. His successor, Shirley Ferebee-Woolpert, was an equally remarkable person.

When I retired from the Public Health Service in 1962 I came to Hopkins, largely because of my earlier collaborations with Philip Sartwell, then Chairman of the Department of Epidemiology.

DS: When you first got started, what was epidemiology like? What did an epidemiologist do? How was it different from the way we practice it today?

GC: Statistics plays a more prominent role today. Epidemiology back then was based more on logic and whether the results made sense biologically. My early papers don't have a *P* value or significance test. The "biostatisticians" were more like today's project directors. They knew how to carry out studies and how to avoid biases.

Technology has improved. We are able to measure more things in different biologic samples. But I don't think the basic principles of epidemiology have changed at all. It's just that we now rely more on statistical significance and less on what makes sense.

DS: Do you think that is a change for the worse?

GC: It's a mixture. One positive advance is our ability to handle multiple variables in our statistical analyses. The best we could do in our early studies was stratification or analysis of variance with a limited number of variables at a time.

But I believe we pay too much attention today to the statistical significance and not enough to replication. Even in a huge study with highly significant *P* values, the results could still be due to chance. The tendency is to rely on results from one big study or from pooling studies. I think this is a mistake. I would put much more faith in results from 3 relatively small studies showing the same results in different populations and done in different ways, regardless of the significance level, than I would in any one study with a very small *P* value.

I think a good example of how a big study can lead you astray is the University Group Diabetes Project. This was a well-done study that showed an increased risk for cardiovascular death associated with one of the oral antidiabetic agents. If there hadn't already been other studies underway, it is not

likely that anyone would have initiated another study to confirm this finding, and we might not be using oral treatments for diabetes today. However, the other studies didn't confirm the UGDP's findings.

DS: Over the years there has been debate about the relationship between epidemiology and public health practice and about the failings of the discipline. Has epidemiology lost its roots in public health?

GC: I'm glad you brought that up. I think that we have moved too far from public health. Not many on the Hopkins faculty, for example, have had contact with a public health department. I think that is too bad, because epidemiology has to be the basic science of public health. Public health is both the study of populations and health services.

I think most of the criticisms epidemiology has faced are not due to the discipline per se. One factor is that there is insufficient acknowledgment that we are almost invariably looking at associations and not causes. In particular, the media want to treat every study as if it were the final answer. And we as investigators get trapped into that because of the questions reporters ask us and what gets reported. The media point us in that direction and we follow along far too readily. Even if we had said, "We really need to replicate this, we really need to look back to see what other people know," that doesn't tend to get reported. It is no great surprise that various critics jump on us. This is what they see of epidemiology.

We also are not humble enough in our written work. We often state that more research is needed without specifying that it needs to be replicated in a slightly different way or allowing that the conclusions could be wrong.

DS: Let me ask about setting up the epidemiology training center in Hagerstown, Maryland. How did that come about?

GS: Again it was luck. The year before I retired from the Public Health Service, my wife and I were camping in Washington County, Maryland. I still remember telling her when we came home, "If anybody offered me a job in place like that, I would take it in a hurry." The very next year, the National Cancer Institute decided to stop a study of background radiation that they had been carrying out in Hagerstown, despite the fact that a building had been built to house the project. They were still interested in possible cancer risk from background radiation, but they wanted the answer in a shorter period of time. Frank Lundin, who also was a Hopkins doctoral student at the time, was in charge of a small epidemiology unit in Hagerstown when I first came to Hopkins. I had some experience with censuses both in Georgia and Alaska. I realized what you could learn if you had a census with some basic data. Together, Frank and I proposed to the Cancer Institute that we carry out a private census to answer their question. They gave us a contract to do it, which gave us our first population-based project in the county. The NCI found that the reason some houses had more cancer than

others was because older people tend to live in older houses and older people get cancer. It was not the background radiation after all.

The idea of training came from John Hume, Assistant Dean of the School of Public Health at that time. He felt that we could use this research unit to train people in public health research.² Most of the students came from epidemiology, and eventually the administration of the unit fell to the Epidemiology Department. I wish that there were broader interest within the school. For example, I would have loved to have seen health policy faculty and students work with the county health department. Some students have been involved in projects that involved the health department. But, considering that the center had been in existence 40 years this past December, there has been a pitiful amount of public health research.

DS: You started the CLUE study there, and now everybody wants to have a cohort with biologic samples. You are pretty much a leader in that field. What were you hoping to accomplish? How did that project come about?

GC: Well, our primary interest (and the name of our study) was Serologic Precursors of Cancer. Abe Lilienfeld should be given a great deal of the credit. I'm not sure if it was Abe's idea or mine. It is just one of those things that suddenly came together.

My experience in Columbus, Georgia, was a training ground for how to carry out field studies. Just when I was getting ready to leave Georgia, I came to realize what a model I had, beyond just tuberculosis. By virtue of having had a census in the community, I was able to put some of those ideas from Georgia into effect here in Washington County. We gradually expanded to other samples. We learned to collect plasma and we later added toenails. And we have been sending out periodic follow-up questionnaires. Just like everyone else, our response rates have dropped from the high 90s to the low 70s [percentages]. But we are still thriving despite having much more competition than we once had.

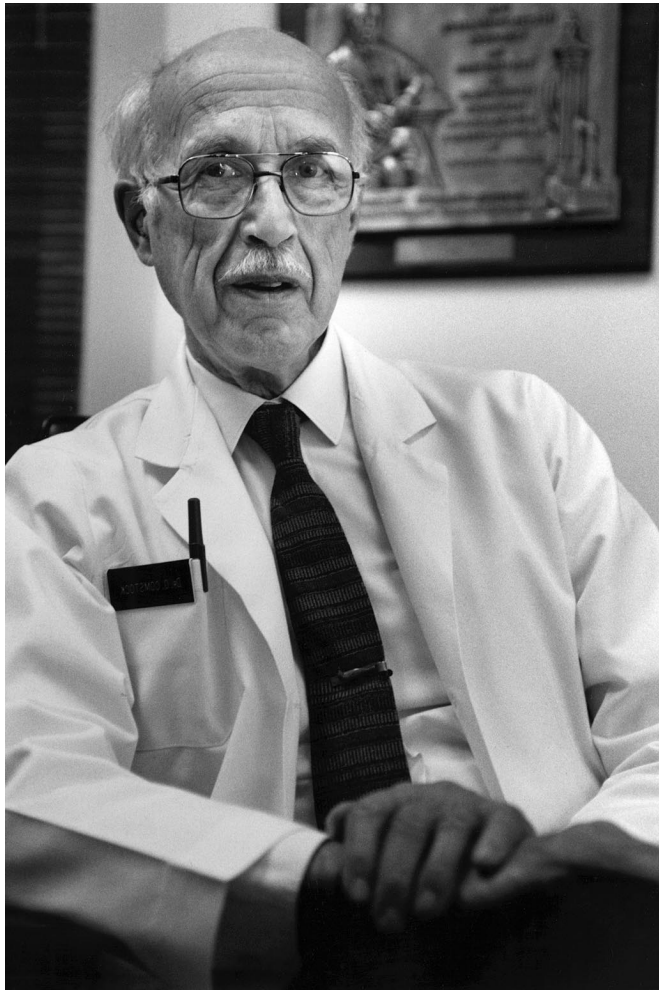
DS: Who would you single out as having the greatest influence on you career?

GC: Carroll Palmer because his instincts were so epidemiologically sound.³ I may have ended up in epidemiology without him, but he had a major impact on what I do.

Carroll wrote very few papers and gave very few presentations entirely on his own. He knew exactly what he wanted to say but he had great difficulty putting it into words. This was lucky for me because not only did I get to write papers for him, I learned, through endless rewrites, how to write exactly what he wanted to convey.

DS: Who would you regard as the most important epidemiologist during your lifetime?

GC: There are a lot of very important ones. I'm sure I will leave out half a dozen that I should have mentioned. Obviously, I think first of those I was close to. Abe Lilienfeld



George Comstock, 1999

was remarkable, not only because he was so smart and could see things so clearly, but because he was also very good at creating opportunities for other people. Walter Holland taught me the precision needed to conduct standardized surveys.

Mervyn Susser is a great leader because of his philosophical contributions. It was a tragedy when John Cassell died in his 50s in 1975. He had a major impact on behavioral and social epidemiology. Jerry Cornfield was a remarkable biostatistician with a sense for epidemiology and the ability to combine that with really good mathematical skills. He is another person we lost far too early in his career. [Jerome Cornfield died in 1979 at the age of 67.] In the younger generation, Ken Rothman has had a tremendous impact on the promulgation of methods in epidemiology.

DS: What do you think is your most influential paper?

GC: Probably the most influential was the very first paper with Palmer and Shaw reporting the results of BCG trials in Columbus and Muskogee County, Georgia, and in

Puerto Rico.⁴ I think that paper was largely responsible for the fact that BCG vaccination is not routine in this country. If it had become routine, we would have been behind in our knowledge of tuberculosis. Not having to worry about tuberculin positivity due to BCG, we have been able to do lots of things in the U.S. that the rest of the world couldn't. And, fortunately, we haven't needed BCG vaccination.

DS: Is there something you worked on that didn't get the recognition that you thought it deserved?

GC: I wrote a number of papers proposing optimal ways to evaluate new vaccines that seem to have been ignored.^{5,6} Maybe the ideas aren't as good as I thought, but I think we'd be better off now if some of my suggestions had been followed 30 years ago. For one thing, we would have saved some of the vaccine strains that had been tested so that we'd have something to test new vaccines against.

DS: Can you talk about your interests outside of epidemiology?

GC: Professionally, I still have a broad interest in medicine in general and in public health. Outside of the profession, music has been my only real hobby for years. Our recorder group, The Washington County Museum Recorder Consort, practices and performs regularly. I'm going to bassoon camp this summer and will also attend a recorder workshop, as I have been doing for years.

DS: What do you think the schools of public health should be teaching students?

GC: My statistical colleagues will not like this, but I think we turn out far too many people who think that unless something is done with a fancy mathematical model it isn't worth much. We need to think things through.

One of the major problems in the way we teach PhD students is that there is far too much focus on the thesis. Students tend to come out exceedingly well trained in that area, but many of them never pick up what I would consider an adequate biologic background. Obviously, people who are smart are going to do better at tackling new areas than others, and most of the successful PhDs outlive what I consider to be a handicap.

I think we should be giving a broader and less intense education than we get through working on a thesis. More course work and perhaps an opportunity to work on more projects in different areas would be better than one great big one.

DS: What would be the single most important piece of advice that you could give to a new person starting out in epidemiology today?

GC: I am inclined to think of my newly acquired (through marriage) grandson, who recently had a chance to talk with James Watson, [1962 Nobel Laureate in Medicine for discoveries concerning the molecular structure of DNA]. Watson told my grandson that "...my advice to you is, think."

That seems like a fairly safe thing to say. When you are working very hard on a project, it is awfully hard to see it from different perspectives. My advice would be to try to get away from it and look at it from a distance. All too often, after a paper is written and accepted, I find that I missed some little point here or there that I might have thought of if I was not in such a hurry to submit the paper.

DS: Is epidemiology in good shape? There have been articles about epidemiology reaching its limits. Are you pessimistic or optimistic?

GC: Our study subjects behave in ways we have no control over. So yes, that is a limit. But if you don't do studies on people, you are not assessing risks in the real world. While it is true that epidemiology can't do lots of things, it can come closer than any other science.

One of the things that worries me is the globalization of industry. It may very well be that industry becomes more important than government. Epidemiologic studies that make industry look bad or make their activities look bad are going to be more and more difficult, because these companies are getting more and more powerful and have more money than universities. It is possible to tie a study up for years just by taking the investigators to court.

In the face of this, it may be that epidemiologists need to pay more attention to policy. We tend to think that scientists shouldn't be advocates. If you are an advocate, you are no longer unbiased. On the other hand, your research is likely to be more focused and more useful if you do have an interest and you are an advocate. Goldberger was an advocate for good nutrition. E. V. McCollum, who discovered vitamins A and D, was the advocate par excellence. He spent a lot of his time writing for McCall's magazine, for example.

DS: I want to come back to opportunities. Are there important scientific questions that you think we ought to be asking? Research directions we ought to be taking?

GC: I have never been that forward-thinking. I have always taken the opportunity of the moment, doing what I could with what I had. I have never had a long-range plan. Obviously, for almost any disease you can think of, we are far

from knowing how best to prevent it. Take something as straightforward as lung cancer and smoking. We still don't know exactly what causes lung cancer and there are aspects of prevention we still don't understand.

Our new genetic knowledge and technology will help, but I don't think it is going to be the great solution to all problems that sometimes people think.

DS: Is there anything else you want to share?

GC: I can't think of doing anything else that would have been more rewarding and more fun.

ABOUT THE INTERVIEWER

DALE SANDLER is Chief of the Epidemiology Branch at the National Institute of Environmental Health Sciences. She received a PhD from Johns Hopkins where she worked with George Comstock and Genevieve Matanoski on a study of health effects from nasopharyngeal radium irradiation. She has since collaborated with Comstock on studies of passive smoking. She has studied risk factors for renal disease, leukemia, and breast cancer as well as health effects of radon and pesticides.

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