DISCUSSIONS

BUCK: The title of this section implies a transition from the "old" to the "new" epidemiology, and I am not exactly sure what we mean by transition. In the first section we discussed early works that represented the old epidemiology. By new do we mean, then, the application of epidemiology to new problems? Perhaps a good beginning for this section would be for us to try and define this transition.

NAJERA: In this second section, I see us starting in the early twentieth century, when there already was a fairly well established, scientifically sound, infectious disease epidemiology, and then moving on to a broader application of epidemiology to all health problems. This would be what I would call the difference between the "old" and the "new" epidemiology: the shift of interest and concern that occurred in the 1940s and 1950s.

LLOPIS: In my opinion, this transition may have peaked in the 1940s, but it had already started in the previous decades. A case in point is that, as early as 1914, Goldberger's work was more rigorously scientific, more methodologically sound, than the work of any of the people we mentioned in the historical discussion.

TERRIS: I agree with you. The truth is, if you really look at it closely, that a lot of things were already happening before the 1940s. As you mentioned, Goldberger's work on pellagra began in 1914. Even earlier, beginning in 1910, the Public Health Service in the United States did a good deal of work on occupational disease epidemiology. In the late 1920s, the Massachusetts State Legislature, responding to the concerns of the public, actually ordered the State Health Department to begin studies in chronic disease epidemiology. And the National Cancer Institute was organized in the United States in the thirties, before the war. I am sure that if you looked at England, you would find that they, too, were doing a fair amount of cancer epidemiology in the twenties and thirties, which is why Major Greenwood could include the subject in his text in 1935. We don't have any occupational health specialists here, but if we did, they would undoubtedly point out some key occupational stud-

ies. I know that Winslow and Greenburg were doing studies of occupational disease in the twenties. So there was a lot of work leading up to this transition, and what happened is that it came to fruition in the forties. It was like Pasteur and Koch. If you read carefully, you find out that all during the 1850s and 1860s there was a tremendous development of animal microbiology, and that this was the basis on which Pasteur and Koch arrived at their epoch-making discoveries.

I think we need to discuss all the factors that came together to influence the transition. For instance, I would like to comment on the ideological aspect because I think it is rather interesting. In both England and the United States there was a rediscovery of the sociological school of epidemiology. It was a rebirth of the views of Villermé, Virchow, and the others who thought there was much more to health problems than sanitation, that poverty was important. The reason they had failed to demonstrate their point was that in this century they didn't have the methodology; the movement ended up with rhetoric. It was only in the twentieth century, when sociological epidemiology developed further, that the necessary methodological tools were available to carry through the needed research.

I would like to suggest that what is happening in Latin America today—this ideological ferment of social epidemiology which is somewhat political in orientation— is, in a sense, a preparation for work. Another example is the South African school of social medicine, a group of liberal and radical young people who were very much influenced by Henry Sigerist. Unfortunately, South Africa did not have an epidemiological tradition, so they turned to sociology and anthropology. When a number of them emigrated to the United States, they had to learn their epidemiology here.

My view is that the period of transition starts as a real movement in 1943 with John Ryle. His story is a dramatic one. Here was a distinguished British professor of medicine who resigned his position as the Regius Professor at Cambridge to become the first Professor of Social Medicine at Oxford. He stated his concept of social medicine very clearly: a transition to noninfectious disease epidemiology. As I said earlier, Ryle represented a throwback to the sociological school of the nineteenth century. Like Alison in Scotland and Virchow in Germany, he believed that disease is caused by poverty and other social conditions. The British school in the forties thought that there must be something in society that causes infectious diseases. It was this simple logic that led to the shift in epidemiology from the

study of infectious diseases to the study of noninfectious diseases.

Now we are concerned with the epidemiology of injuries, with occupational diseases, with environmental hazards, and we are beginning to use epidemiology to evaluate the validity of clinical procedures and the effectiveness of health services. There has been a tremendous growth in epidemiology, and I think that this book has to give a sense of all this change, development, and expansion.

BUCK: I would like to go back one generation before Ryle, to a work whose content would be very appropriate. I am thinking of Major Greenwood, who preceded Bradford Hill at the London School of Hygiene. In 1935, he published a book called *Epidemics and Crowd Diseases*, and that book not only contains chapters on tuberculosis and other contagious diseases of great concern, but also chapters on cancer and psychological causes of illness. Greenwood made it clear that epidemiological concepts were transferable from one kind of disease to another.

TERRIS: You are right about Major Greenwood; the movement had already started in the thirties.

I mentioned Ryle because it was such a dramatic thing, one of the outstanding clinicians of Great Britain deciding to leave clinical medicine to work in epidemiology. It was unheard of. I might add, as a postscript, the curious irony that many years later, Richard Doll, one of the world's outstanding epidemiologists, was appointed Regius Professor of Medicine at Oxford. Isn't that a marvelous turnabout?

NAJERA: Defining a transition period is very complicated. It started long before the forties and developed quite slowly. We should also try to address the reasons behind Ryle's change of mind. I think the transition came as a consequence of looking at health problems comprehensively. People like Ryle, who had a comprehensive knowledge of medicine, and people who knew statistics started to realize that the social aspects of most diseases were more important than either the specific agent that caused them, or whether they were classified as infectious or noninfectious. I think that is what Ryle said in the preface of his book, that infectious diseases also have sociological roots. Therefore, to be a doctor he had to do social medicine; he had to consider the social factors because they were more important.

I think that the development of health services was also a major factor in the transition. This was also the major

difference between the rest of Europe and England. France, Italy, Denmark, the Scandinavian countries, and everyone copied the German insurance system, but the British developed their own. For centuries, England has had a tradition of providing services for everyone that, in my opinion, no other country has had. This tradition probably stemmed from the fact that Henry VIII took for the state the social responsibilities that the church used to have. Also, statistics developed in England around the seventeenth century. They were called "political arithmetic," and they were a way to use mathematics to make information available to the state, to the ruling power. And so, the use of statistics as a way to evaluate health conditions (which started with Petty and Graunt who were the first to look at mortality) prompted people to think that the state should provide health care for everybody. From there, people went on to suggest that the state, the government, be organized in ministries, including a ministry of health. Of course, these ideas weren't fully developed until later, but this is where one sees the earliest signs of the concept that the state has to care for the health of everybody.

The socialist movement in England was also different from the socialist movement in the rest of Europe. You see, I believe that Bismarck introduced the health insurance system in Germany not as a means of developing social services, but as a way of curtailing the development of social ideology. John Peter Frank had done the same a century before. But in England, the development of health services was profoundly rooted in a social ideology. In this sense, British politics played an important role in the development of health services by establishing participation by the people much earlier than in other countries of Europe. One could say that this political development started in Spain, but that the Catholic Church prevented it from continuing. That is why neither Spain nor France continued to develop along these lines, whereas England did. The English health service development began a long time ago and continued unabated. This is probably why, although health services in other European countries may have been comprehensive by the beginning of this century, the way in which they developed in England was different.

Another influencing factor which should be considered in the early twentieth century is the Russian Revolution. The Russians also developed, for the first time in their history, a comprehensive health service. What Semashko, the Soviet Union and the world's first minister of health, did in 1918 was closely watched and commented on by radicals in Europe. In 1919, as a result of what was happening in the Soviet Union, England improved its health serv-

ices by reorganizing its Ministry of Health. So, my contention is that the development of public health services greatly influenced the transition in epidemiology and that this, too, is probably what sets England and the United States apart from the rest of Europe. A country's political development serves as background.

Spain is somewhere in between, because at the beginning of the twentieth century it was influenced by the Rockefeller Foundation and the development of public health services along American or English lines. In 1924, Spain established its National School of Public Health, one of the first in Europe (the second, I believe, after the London School of Hygiene), and introduced a public health component into its already comprehensive rural medical care network. A real school of thought in epidemiology started there between the late 1920s and the Spanish Civil War in the late 1930s. That was quite a development. This could help explain why there is more epidemiology in Spain than in many other countries in Europe.

BUCK: Going back to the comments on Ryle, I feel I should point out that John Cassel's work is another influential example of the important principle that a variety of diseases can have a common cause. I think that several of his papers were landmarks in the sense that they made that point and illustrated it with fairly convincing evidence.

TERRIS: I have often puzzled over why it was England that pioneered in the noninfectious diseases. Why not Sweden, where the problems of noninfectious disease were felt earlier because of an aging population. Yet Sweden never developed this field. The big development was in Britain and the United States. The question is why, and I am not sure I have all the answers to that. I once discussed this with Abe Lilienfeld, who thought that it occurred because of the development of vital statistics in England. My own interpretation is that political factors are important. The reason the movement began in Britain, and it is difficult to say why it did not happen elsewhere, was that much of the leadership of the British movement for social medicine was influenced by labor and socialist ideology. Major Greenwood was a founding member of the Socialist Medical Association (SMA) in 1930; Richard Doll was an active member; and Ryle himself had close ties to the SMA. Jerry Morris was certainly pro-labor. J.A.H. Lee once told me at an International Epidemiological Association meeting in Yugoslavia that those who went into social medicine in Great Britain fulfilled at least two of three criteria: one, they were pro-labor; two, they were Scots; and three, they had done some-

thing else before going into medicine. I do not know if this is true, but he was making the point that the sociological orientation really came out of a political consciousness. This was also true to some extent in the United States, but more so in Britain. Why it did not happen in the rest of Europe would be the subject of a long discussion.

Now, these were not new ideas. Alison, a Scottish epidemiologist at the time of Farr and Chadwick who was very critical of Chadwick's work, agreed with Virchow, Villermé, and others of the sociological school of the 1840s and 1850s in Europe. They all believed that disease was not just caused by emanations, bad sanitation, miasma; it was also caused by poverty, the miserable social conditions. The difference, as I have said before, is that in the nineteenth century they didn't have the methodological tools to get at the specific agents. It is not enough to come up with the general statement that society is the cause of disease. I keep hearing that all diseases are social, and that if the social order could be changed then disease would disappear. We already know that this is not true, and this rhetoric has to be replaced by specific research and action. In the 1840s Virchow said it was the old reactionary system and the attendant misery of the peasants that caused the epidemics of typhoid fever in Silesia. This was true, but it wasn't enough to say that it was the social system; he could not get at the specific agents. After all, in the 1840s, they did not have the background of a hundred years of science, statistics, and methods in epidemiology. In the twentieth century they were able to find specific factors such as cigarettes, toxins, and saturated fats.

BUCK: Just to add a comment to the idea of the specifics, I think it is important to stress that the theory was very general and evidence was hard to come by. The specifics are just now beginning to flower, especially in the area of psychoimmunology. Cassel, for example, looked at the effects of abrupt cultural dislocation on such physiological specifics as blood pressure and blood lipids. He recognized that there could be even more subtle influences than the purely dietary. That was one of the beginnings, which happened to be contemporaneous with Seeley work on stress. People then began to go back to the physiological work of Walter Cannon and to see it in an epidemiological context for the first time.

As you say, the notion that societal forces cause disease does not necessarily mean that a simple reordering of something as vague as society would provide the solution. In other words, the term "societal causes of disease" has to

be refined so that it includes specific mechanisms and specific individual responses, perhaps even analogous to the immune reactions to infectious disease.

NAJERA: Isn't it also possible that an important factor in the transition rested with the development of trade unions in England, since they developed alongside the socialism of labor? What I think played an important part in the shift toward chronic or noninfectious diseases, was that when the union members started to demand their rights, their health problems were not chickenpox nor any of the infectious diseases with the exception of, perhaps, cholera. Consequently, people started concentrating on those diseases that affected adults, or almost only adults, since infectious diseases were mostly restricted to children. This is what I think caused noninfectious diseases to gain importance. And this, in turn, allowed Ryle and others to consider the importance of sociological factors even for infectious diseases.

BUCK: How much was the transition to chronic disease epidemiology, apart from the obvious rising importance of chronic disease, due to the fact that a lot of these physician-epidemiologists of the period had been internists?

NAJERA: It is possible that this was a factor, but I think that the key was this radical or socialist ideological thrust, and that its impact was the extent of health services coverage that began with the insurance system.

Health service coverage, along with the sanitation movement of the nineteenth century, seemed to be enough. The nineteenth century sanitation theory postulated that providing safe water was enough sanitation for disease control. At the same time, the workers' conditions forced the expansion of health coverage to include all diseases. At first, only accidents were covered, but by the forties, most diseases were covered by the insurance system. By the twenties or thirties, those countries that had better services, like England, looked beyond infectious diseases.

TERRIS: I don't agree. Most countries with highly developed insurance systems didn't do anything in this area. Sweden didn't experience this change, neither did France or Germany. Yet the United States, which had no government health insurance, played a leading role in the development of noninfectious disease epidemiology.

NAJERA: Yes, but England did the most. They started the whole thing.

TERRIS: They may have started the movement, but I don't think it was because of insurance. If this were true, it should have happened all over Europe, but it did not. In Germany, national health insurance for all diseases started in 1884. It began in England in 1911, and included only wage earners, not their families. It was a limited program. The German system was more comprehensive.

No, it wasn't insurance that led to the new epidemiology. The European countries which had insurance did not develop chronic disease epidemiology. It happened in England and the United States for reasons which had nothing to do with insurance but had a great deal to do with an independent public health movement as exemplified both by the School of Hygiene in London and the Public Health Service in the United States. It had nothing to do with medical care insurance. If it had, why didn't it develop in the Soviet Union? They had a comprehensive national health service which covered all diseases, and yet they did not develop chronic disease epidemiology. And that was true of the other socialist countries.

NAJERA: Perhaps in the Soviet Union the system evolved too early—it was at the beginning of the century. And the other socialist countries followed the Soviet Union's model.

TERRIS: No, I think the reason is very different. What the socialist countries have done is to develop a very powerful medical care system which has come to dominate the health services. Why did the movement for noninfectious disease epidemiology occur essentially in England and the United States? I believe the reason it did not happen elsewhere in Europe was that the health services were all clinically dominated. There was no strong independent tradition of epidemiology and public health. This was true of Sweden, France, Germany, the Soviet Union—all of Europe, both East and West. Medicine overshadowed public health. But then why did it happen in England, which also was dominated by clinical medicine except for the London School of Hygiene? Remember the field was called social medicine; the movement for noninfectious disease epidemiology developed within the medical schools.

There are two other issues, two revolutions in thinking, which I think may also have influenced this transition. The first was the discovery that infection is not the same as disease, that there are inapparent infections. Early in this century, Chapin got rid of fumigation in the United States because he pointed out that epidemiology teaches us that there is no use in fumigating. Disease is spread mainly by healthy carriers, not by cases. That was a tremendous leap

forward, based on the understanding that infection is not synonymous with disease. The second big revolution was the discovery—which resulted from mass X-ray surveys and other screening procedures—that disease and illness are not synonymous. Through X rays we found out that people could have a chest full of disease and yet look perfectly healthy, without illness or symptoms. Another example is the Pap smear: a woman seems perfectly healthy but is found to have carcinoma in situ.

BUCK: You could say the same for hypertension.

TERRIS: Well, you can argue whether hypertension is a disease. But I think that those two discoveries were terribly important in the way we think about the natural history of disease. Our concept of disease was changed by microbiology and epidemiology, by mass surveys and screening. The whole idea of finding disease before it resulted in illness was a discovery of the twentieth century.

BUCK: I work a lot with academic people who practice family medicine, and, of course, they have opened my eyes to the fact that a substantial part of what the primary care physician treats is not even a diagnosable condition. I do not mean it is imaginary; it is genuine enough ill health. It just does not fit our mode of disease classification at all, but is much more compatible with the psychosocial taxonomy. This, of course, takes us back to Ryle, Cassel, and the others.

TERRIS: Going back to the transition, in the United States the schools of public health succeeded in making the change. When I studied at the Johns Hopkins School of Hygiene in 1943, not a single noninfectious disease was mentioned in the course in epidemiology. This was the country where Goldberger had done his classic studies on pellagra, but his work was never discussed at Hopkins then. It was only infectious disease that was studied. But now epidemiology at Hopkins is concerned primarily with noninfectious diseases, perhaps too much so. The same was true of medical care. In 1943 we had all of three sessions on medical care by a visiting lecturer, while now this area dominates the field of health administration at Hopkins. The same transition took place in all of the United States schools of public health.

BUCK: We were wondering why England seems to have been ahead. When I was taking my D.P.H. at the London School of Hygiene in 1950–1951, we had chronic disease epi-

demiology, we had public health administration, and we had what I guess you would call medical care topics, but we had no laboratories in bacteriology. They had already made the switch by 1950. There was almost nothing that came up later in North America that they had not forecast for me in the London School of Hygiene.

It is also necessary to recognize that statistics certainly helped to influence this transition. But, as you say, once the political momentum got rolling, the London School of Hygiene was to medicine what the London School of Economics was to some other fields. They are only about four blocks apart. There was an *avant garde* spirit there which went beyond the political side of medicine and into the epidemiological side. You see, Bradford Hill, Doll, and Donald Reid already were working in the Department of Epidemiology and Medical Statistics in 1950; they weren't in any medical school. Most of the medical schools in London were small and none of them at that time had a department of preventive medicine. They did not want to, I am quite sure. They were strictly clinical, hospital schools, and they made little attempt to teach anything in the line of public health. Nobody was worried about that because the big positions in public health could be taken by the bright young people who were being attracted into the London School of Hygiene.

TERRIS: In their epidemiologic studies, as I recall, the British worked very closely with the health departments. They did not have money, like we had in the United States, to set up their own studies, so they had to go to the health department of the hospital for their study populations.

This section should emphasize that in this transition, epidemiology moved from concentrating exclusively on infectious diseases to looking at all disease and injury, even "positive" health. In other words, that this transition resulted in the expansion of epidemiology. Methodological problems were being solved on the way, and they are not all solved as yet, by any means. Confounding variables still confound us. The important point was the shift in interest and concern to a whole new area. Epidemiology stopped being limited to infectious disease and became concerned with all the factors that influence the health of populations. And other than the deficiency theory of disease which, as mentioned earlier, was first developed by Casimir Funk, in general it was Great Britain and the United States that developed noninfectious disease epidemiology.

Take cigarette smoking and lung cancer. The first papers appeared in the United States early in 1950, when Wynder

and Graham, and Levin, Goldstein, and Gerhardt published their work in the *Journal of the American Medical Association*. Then, in England, in September of the same year, Doll and Hill's paper appeared in the *Lancet*. That was the starter gun for noninfectious disease epidemiology. It developed almost simultaneously in the United States and Great Britain. These two countries became the center, and from there noninfectious disease epidemiology began to spread everywhere.

My theory about why it happened first in England and the United States and not in the rest of Europe is that in other countries they had no public health as an independent discipline. They never worked with statisticians, they never developed groups of interdisciplinary teams. In England, the London School of Hygiene was the focal point of the epidemiologic revolution: Major Greenwood had been both an epidemiologist and a statistician; they had epidemiologists like Richard Doll, Jerry Morris, and Donald Reid, and statisticians like Bradford Hill and Peter Armitage. Theirs was an interdisciplinary group made up of more than just physicians. And the United States, too, had such a powerful group of epidemiologists coming out of the Public Health Service: Rosenau, Goldberger, McCoy, Anderson, Frost, and many others. There was a fantastic growth of epidemiology in the United States; that is why the United States became a leader in this field.

NAJERA: This is a good point. In other countries there was no true *public* health in the sense of a discipline with profound community objectives.

TERRIS: I must say that I am biased on this question, because I am convinced of the need for both an independent public health movement and for schools of public health. I want to get the field out from under the medical milieu; it gets stifled in the medical profession. It has to be multidisciplinary, even though physicians play a tremendous role.

BUCK: I could propose another theory to you: that it is almost chance that determined in which country epidemiology flourished. Maybe it wasn't the things you were mentioning about politics and so on. It may be that it was a kind of rare phenomenon that arose in a few places, akin to a genetic mutation. Seriously, though, I am not sure we have the answer.

TERRIS: Well, think of it. There was a tremendous need for noninfectious disease epidemiology because of the aging popula-

tion, and because infectious diseases were being conquered. It became very clear that noninfectious diseases were the major problems.

BUCK: If we take that tack, I see no problem with it. If we limit ourselves to defining the transition as the application of epidemiology to noninfectious diseases, with the methodological implications of that switch, then I think we are on solid ground. But if we try to account for it rather than just to describe it, we may be over our heads.

TERRIS: But it is very important to discuss this whole question. I think the future depends on this. We now see a retrogressive movement in the world. The medical profession is trying to recoup its fortunes. That is why we have clinical epidemiology; they are trying to stop this development of prevention and public health. Europe was held back because it was under the domination of physicians; they had no independent epidemiology and no public health. I'm willing to take the position that this is what really happened.

NAJERA: If you begin with the last century, there is a logical flow when you trace where and why public health started, how it developed, and why some countries were left behind.

TERRIS: Why are the Latin American countries now becoming interested in noninfectious disease? It's very simple. Heart disease is the leading cause of death in 28 countries of the Americas, cerebrovascular disease in 3, and cancer in 1, compared with diarrhea and enteritis in only 5, and influenza and pneumonia in only 2. Injuries are second in Costa Rica and fourth in Mexico. Latin American countries now have the same problems as the industrial nations. That is why they are becoming more interested in the noninfectious diseases.

BUCK: Leaving that aside for the moment, couldn't you still argue that the new epidemiology developed in countries in which noninfectious diseases first began to rise in prominence?

TERRIS: Not really. If that were the case, France and Sweden should have been the first because they had the most old people.

BUCK: I think it's a multicausal thing. I don't think there is a single explanation. I think, first, that the rise in noninfectious diseases had to occur. Second, as you are saying, there had to be some structure for encouraging this new interest. I

am wondering, in many of these countries that went ahead, if that structure that had served them very well during the infectious disease era—the Public Health Service being an example in the United States—was for some reason flexible enough to take on and lead in the new issues. Now, I am not sure what the British counterpart was. It may have been the School of Hygiene and Tropical Medicine. So I am putting it to you that the countries which had both the early rise of chronic diseases and the structure would perhaps be the leaders.

TERRIS: I agree. That is true. It's both. They had the problem and they had the structure, the capability to deal with it.

You know what is crucial in the whole thing? I am now convinced that it was the collaboration of epidemiologists and statisticians. At the London School of Hygiene and Tropical Medicine they had Doll and Hill, and Major Greenwood himself was both epidemiologist and statistician. In the United States Harold Dorn set up a statistical unit at the National Cancer Institute which included half-a-dozen of the most brilliant young statisticians in the country— including Jerry Cornfield and Nathan Mantel—who were put to work to develop the methodology.

BUCK: If we attribute what happened in England to their School of Hygiene and Tropical Medicine can we find any counterpart institutions in Germany and France?

TERRIS: No, all they had were medical schools. They didn't do epidemiology. It was mainly legal medicine.

LLOPIS: I agree. For example, in France they tended to look at demographic problems, and statistical work was, therefore, mostly related to demography. They were always concerned with maintaining a demographic balance with neighboring countries. So, when epidemiology began its development in the 1950s, they used statistics and demography as a starting point. They had no counterpart institutions.

NAJERA: That's right; they didn't exist. If you look at some of the German textbooks of epidemiology, they deal exclusively with infectious diseases: they explore all modes of transmission, including 20 or more ways to spread respiratory disease. As an aside, I would like to suggest that we do not use the term "chronic," but instead use the term noninfectious, because tuberculosis is a chronic disease and so are syphilis and leprosy.

BUCK: In 1979, Elizabeth Barrett-Connor wrote a paper on the epidemiology of infectious and noninfectious disease, where she made the point that the difference between the epidemiology of infectious and noninfectious diseases is not that big, since we have one epidemiology that allows us to study both kinds of disease. I think that this idea, that the difference is more quantitative than qualitative, is an important one, because it shows how the epidemiological transition was possible. Part of her argument also was that noninfectious disease epidemiologists tend to look down on infectious disease epidemiologists. Everyone notices, for example, that the infectious disease papers rarely come first in the *American Journal of Epidemiology.*

TERRIS: It didn't use to be that way.

BUCK: Well, it's been that way for about 10 years. I notice it every month. Barrett-Connor pleaded for a stop to this two-class system, pointing out that infectious disease epidemiology takes some know-how too.

TERRIS: We should include her paper in this section, perhaps as the last paper, to say, "look, you have made the transition, but don't go too far." Because what has happened is that most of the current American textbooks of epidemiology do not even discuss infectious disease.

BUCK: The real truth of the matter is that the complexity of the epidemiology of some infectious diseases such as leishmaniasis, schistosomiasis, and leprosy makes the epidemiology of cancer and heart disease look simple. Actually, couldn't tuberculosis represent the transitional disease, the one infection that had so much in common with noninfectious chronic disease that it required chronic disease methods?