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Editorial: Ecologic Analysis as Outlook and Method

Medicine treats the individual, public health the population. Epigrammatic as it may be, this venerable idea opens above public health workers of all disciplines an umbrella of shared identity. Central to it is the notion that the health of a group, a cohort, a community, or a people is more than a summation of the health of its individual members. What makes up this "more" is public health's special province. Epidemiology, a science that public health can justifiably call its own, is from this viewpoint a fundamentally social science: not so much the study of disease and health *in* human populations as the study of disease and health *of* human populations. The epidemiologist, foremost as public health worker, does well to adopt a perspective that is "ecologic," but not in the dictionary sense of trying to understand relations between organisms and their environment or between persons and institutions. But for the unwise proliferation of neologisms, the ecologic perspective for the epidemiologist to adopt might rather be called "demologic": a focus on the population itself, on the forest and not on the trees.

These traditions notwithstanding, epidemiologists in recent times have developed another view of their work and its value. This newer view emphasizes epidemiology's roots as a type of medical research, as a way of using populations to obtain biologic knowledge about disease and health in individual persons. From this perspective, epidemiology is more a natural science than a social one. The policy guidance that epidemiologic knowledge can provide is seen as little more than applying individual-level understanding to groups. This perspective admits nothing mystical about the population; an improvement or decline in the health of a community is merely a matter of improvements and declines in the health of its individual members. No ecologic phenomenon, not even the often cited example of herd immunity, is fully understood from this standpoint until it is understood on the level of the individual. This view holds that, in its essence, public health is nothing more than medicine for the masses, with an emphasis on prevention.

We thus have two conflicts: between interests in the individual and in the individuated population, and between images of the epidemiologist as dispassionate scientist and as committed public health worker. Epidemiology as the scientific study of individuals is aptly illustrated in Greenland and Robins's classic paper, "Identifiability, Exchangeability and Epidemiological Confounding."[1] To paraphrase one of that paper's messages: We want to learn about effects in individuals, but even under a simple deterministic model of causation we cannot tell by examining an exposed case whether the exposure caused the disease or whether it was going to occur anyway. This problem creates the need to study populations and from consideration of this problem we can derive methodologic concepts such as confounding. As illustrative counterpoint, consider Susser's equally well crafted "Epidemiology Today: A Thought-Tormented World."[2] That essay may be read as a meta-lament of the acknowledged ascension of the scientific model of epidemiology and the perceived, consequent declension of the model of the epidemiologist as public health activist.

Can the two work productively in harmony, even within the individual epidemiologist, as complementary dimensions of a multiple professional personality? Rothman and I have pointed toward two obstacles in the way[3,4]: the adverse effect that one's policy preferences can have on one's scientific research and the difficulty of gaining simultaneous proficiency in epidemiologic research and public health policy analysis. Nevertheless, at least a peaceful coexistence may be essential if we are to overcome our basic inferiority complexes: epidemiology as a perceived "soft" science and the public health community as a political lightweight in comparison with the medical care establishment.

Among the many contexts within which these conflicts take place, however subliminally, is that of ecologic analysis. I dimly recall being introduced at the earliest stage of my epidemiologic training, in the mid-1970s, to something called "the ecologic fallacy." It was never made quite clear whether this fallacy was a problem of confounding or exposure misclassification, for it seemed to have elements of both. Crystal clear, however, were the two take-home messages. Ecologic analyses are not to be trusted and the ecologic fallacy, whatever it is, is potentially reducible to one or more familiar epidemiologic biases. As time went by, I was struck by how often, and in what high-profile settings, these untrustworthy analyses were considered to be of great importance. For at least the past 20 years, to cite a recurring example, ecologic analysts of an environmentalist bent have been concluding that the most recently

Editor's Note. See related articles by Schwartz (p 819), Koopman and Longini (p 836), and Susser (p 825, p 830) in this issue.

available data show that the predicted epidemic of pollution-induced cancers has finally begun. Each claim of this type invariably receives headlines, then immediate but less heralded methodologic criticism. The debate rages in the specialty journals and press for a week or two, then the controversy fades quickly from memory until another year's worth of cancer statistics becomes available and the process begins anew. One wonders if the boys and girls who, on the basis of ecologic analyses, have been crying environmental-cancer wolf for so long have inured us to a future alarm that might not be false.

What is it that makes an epidemiologic analysis ecologic? There seem to be two distinct answers to this question. One is that the population, and not the individual, is taken as the unit of study. The other is that the exposure status of each person is determined on the basis of a summary or average value of some kind for a group to which that person belongs. For example, suppose we are interested in bladder cancer in relation to the consumption of trihalomethanes and we decide to conduct a county-level, ecologic analysis of drinking water. We might use surface sources (rivers etc.) vs ground sources (wells etc.) as an exposure indicator. Surface water may be considered an indicator of chlorinated water, and chlorinated water may be considered an indicator of water with relatively high concentrations of trihalomethanes. Thus, persons who drink surface water would be expected, on average, to consume greater amounts of trihalomethanes than persons who drink ground water, everything else being equal.

At the ecologic level, however, all we know is what proportion of each of the approximately 3000 counties in the United States is served by public water supplies with surface sources. We might decide to leave the county as the unit of observation. We might conduct a linear regression analysis of, say, the 3000 proportions served by surface sources against the 3000 bladder cancer mortality rates (mutually standardized by age, race, gender, and calendar year) or we might group the counties into three groups (high, medium, and low proportions served by surface sources) and compare three averages of standardized rates. Either way, our analysis would be an ecologic analysis by both of the aforementioned criteria. The county would be the unit of study and the proportion of each county's population served by surface sources would be the indicator of trihalomethane consumption for every person in that county.

Another option would be to pool the data across all counties within each of the three groups, as Snow did with his earliest data on cholera in parishes served by the two water suppliers in 19th-century London. We would then have not 3000 bladder cancer mortality rates, but three. The analysis would no longer be ecologic by the first criterion, for the individual would now be the unit of study. However, the analysis would remain ecologic by the second criterion because each individual in, say, the "high" group still would be classified as a consumer of relatively high levels of trihalomethanes, whether or not that person lived in a home served by a public water system, whether or not that particular system used a surface water source, whether or not the system used chlorination, whether or not the chlorination created elevated trihalomethane levels, and whether or not the person actually drank tap water (as opposed to, say, bottled water).

Since the early 1980s, we have been witnessing an explosion of methodologic work in the understanding of analyses of this type. New biases, peculiar to these analyses, are being discovered and elucidated at a frequency that is either alarming or exhilarating. It is alarming if one had previously concluded that all important and useful epidemiologic methods have been known for decades. It is exhilarating if one finds it exciting to work in a realm of science in which a great deal remains to be learned.

In this month's issue of the Journal, Sharon Schwartz,[5] James Koopman,[6] and Mervyn Susser[7,8] make noteworthy contributions to the burgeoning literature on ecologic analysis. Schwartz offers a provocative deconstruction of the ecologic fallacy. To some degree, Koopman's paper is a response to Susser, but it is more important to view Koopman's work in the context of the lengthy program of methodologic development and substantive research in which he and many others have been engaged for many years in the extremely difficult area of communicable disease. Susser's pair of papers exhibit the sweeping breadth and conceptual alacrity one needs to attempt a new assemblage of first principles in such a befuddling area. (Followers of the epidemiologic literature have grown to expect no less from this particular author over the years.) Perhaps the reader of these contributions will not have to strain too hard to discern in the work of the three writers differing mixtures of the scientist interested in individual effects and of the public health worker focused on population effects.

These developments are vital because much of our work in epidemiology and public health is at least partially ecologic in character. To cite a notable example, the ecologic analysis of infant mortality rates carries great social and political significance. Progress (or lack thereof) in grand-scale policy initiatives like the war on cancer is best assessed on the ecologic level. Only ecologic analyses can gauge the magnitude of modern pandemics such as those wreaked by cigarette smoking and asbestos exposure. Despite the uncertainties, ecologic arguments will continue to be made for and against causal hypotheses involving common exposures. In the study of communicable diseases, the suitability of ecologic analysis is greatly enhanced by the nonindependence of susceptibility, infection, transmission, and disease occurrence among individuals. Even such seemingly unrelated areas as meta-analysis turn out to have ecologic aspects. Given its wide-ranging import, ecologic analysis needs the Schwartzes, Koopmans, and Sussers of the epidemiologic world to sort these matters out. It will not be easy, but, for those of us who are enlivened by regular reminders that some of the most basic concepts in our field have yet to be fully elucidated, it will be fun. □

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