Point: Is There a Future for Innovative Epidemiology?

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Point-Counterpoint

Editor’s note: Counterpoints to this article appear on pages 281 and 283.

Recently, the American Journal of Epidemiology published 3 very important contributions on the future of epidemiology. Ness (1) explained that innovative thinking is critical for successful research accomplishments. Sortie et al. (2) discussed how the National Heart, Lung, and Blood Institute’s epidemiology program is evolving for the future, and Manolio et al. (3) described a new paradigm for large-scale prospective studies. The Journal should be commended for publishing these commentaries. Epidemiologic societies should give very high priority to responding to these key articles.

Ness (1) is right on the mark and, in fact, the other 2 articles are irrelevant unless epidemiologists conduct innovative research. Sortie et al. (2) note that cardiovascular epidemiology programs have to change. I would agree. Coronary heart disease is preventable (4, 5). There needs to be a major shift from etiologic research to preventive research. Obviously, highly innovative etiologic research is still of great importance, but not measuring the same attributes in many different ways or discovering again and again that coronary atherosclerosis is the major determinant of clinical coronary heart disease. There are obviously still important issues. Would good cardiovascular health among young women reduce rates of prematurity and low birth weight? We still cannot predict very short term cardiovascular risk or the determinants of such risk—that is, why does any individual have a heart attack at X time on Y day? How do we improve evaluation of thrombosis or acute plaque abnormalities?

Clinical coronary heart disease is rapidly becoming a disease of older age. By age 70 years, atherosclerotic disease is practically universal in the United States. Therefore, the focus of much epidemiologic research has been on the sick heart—congestive heart failure, atrial fibrillation, ventricular arrhythmia, sudden death, etc. New technologies, such as magnetic resonance imaging, may provide a window to better study of the “sick heart” in epidemiologic research (6).

The paper by Manolio et al. (3) presents a most serious challenge to epidemiology. The proposed approach of centralized data collection may result in lower cost but also in poorer-quality science. Approaches such as the UK Biobank lack specific hypotheses and therefore unique baseline testing and selection of populations. This lack of novel and innovative new measurements at baseline will probably severely limit the utility of this study and similar studies.

It is very easy and low-cost to collect large samples, as was done in the Multiple Risk Factor Intervention Trial, a study of 360,000 middle-aged men (7), and the Women’s Health Initiative, which recruited about 160,000 women (8). The mass mailing approaches were the same as those of the biobank project. The major costs are not necessarily in recruitment but rather are specific to data collection, such as magnetic resonance imaging, positron emission tomography, computed tomography, and specific assays that require unique methods of data collection and storage. A simple food frequency questionnaire is not going to answer whether eating spinach in an individual with a specific genotype reduces risk of cancer. Future epidemiologic studies of dementia or Alzheimer’s disease will require magnetic resonance imaging and positron emission tomography to evaluate amyloid plaques and cerebrospinal fluid, as well as traditional measures of cognition and dementia outcomes (9).

Successful large prospective studies need specific hypotheses and substantial input from innovative investigators who have a vested interest in the study from the beginning, who represent the distribution of disciplinary skills necessary for implementing the study design (as was done successfully in prior National Heart, Lung, and Blood Institute studies (10–12)), and who are not paid based on how many pieces of data they submit to a central data collection agency or how many papers they publish in a journal.

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Major issues have not been discussed. A growing top-down approach and complex administrative regulations in epidemiology and population research have likely stymied innovative hypothesis-testing. There is also increased emphasis on mechanistic approaches rather than on testing of etiologic hypotheses. Most past successes, however, have been based on identification of specific etiologic agents—that is, cigarette smoking, serum cholesterol, blood pressure levels, specific viruses related to cancer, nutritional deficiencies and excesses, etc.

There will be more money for the National Institutes of Health and for epidemiology when we succeed in identifying new “big winners” that will specifically prevent or improve treatment of important diseases in the population. The public pays for what it gets. Innovative research is the key to success (1).

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REFERENCES