COMMENTARY

Epidemiology and the Elusive Nobel Prize

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Many epidemiologists have been concerned and frustrated that our scholarly activities have never been honored by a Nobel Prize for physiology or medicine. Why has this not happened, and why may it be unlikely ever to happen? As I am recently retired following 11 years of service on the Nobel Prize Awarding Assembly, no secrets can pass my lips for another 50 years. However, when the embargo is lifted, no big secrets may be revealed because much of what epidemiologists want to know is already in the public domain.

Alfred Nobel’s will, dated 27 November 1895 (one year before he died), lays the groundwork for the prize-awarding process. Essentially, all 5 prizes (physics, chemistry, physiology or medicine, literature and peace) shall be given to those who, “during the preceding year, shall have conferred the greatest benefit on humankind.” One of the Nobel Prizes is given “to the person who shall have made the most important discovery within the domain of physiology or medicine.” Although timeliness remains an ambition, it may nowadays take many years or often decades before the impact of a discovery can be reliably assessed. According to §4 of the rules of the Nobel Foundation from 1900, no more than 3 individuals can share the prize and no posthumous awards are granted.

Discovery

The element of discovery, emphasized in Nobel’s will, is a fundamental criterion to be met by any Nobel laureate. Evidence from the 109 years that the prize has been awarded clearly emphasizes this. Hence, when the history of novel scientific development is reviewed, a quantum leap is linked to a specific year or even date of a catalyzing or eye-opening observation, often published in a prestigious journal. The prizes for science have essentially nothing to do with lifetime accomplishments, not necessarily with scholarly competence or extensive research following the initial discovery, even less with intelligence. It is “the height” of scientific discovery that matters, often something that reveals fundamental biologic mechanisms, changes a paradigm, explains a set of existing enigmas, opens an entirely new field of scientific inquiry or provides novel opportunities for diagnosis, treatment, or prevention of human disease.

As an observational discipline with little opportunity for controlled experimentation on humans, epidemiology has a hard time meeting the criterion of ground-breaking discovery, sharply defined in time and assignable to no more than 3 individuals. Ethically and practically, trials in humans can often be done only after the basic science discoveries have been firmly established. As illustrated by the story of HPV and cervix cancer in this issue of the journal, knowledge grows gradually (and not always linearly), methodologies are challenging and never perfect, and it generally takes one or several confirmatory studies before we conclude with confidence that an association is causal.
As epidemiologists, we assert that causal inference for humans can hardly, if ever, be accomplished without epidemiologic evidence. But our often slow scientific process does not accommodate the criterion for a Nobel Prize well. Rather than being generated suddenly by one or a few scholars, epidemiologic discoveries emerge gradually through involvement of many investigators. And this barrier to the Nobel Prize is likely to grow rather than diminish in an era when our research is quickly becoming more complex, transdisciplinary, and multiauthored.

Epidemiology and Basic Research

But there is another perhaps more important barrier, namely the lack of appreciation of our discipline among many basic scientists. Paradoxically, at a time when epidemiologists struggle hard to understand and interact with basic scientists, to accommodate advanced laboratory techniques in their research and to move into the middle of biomedical research, there is apparently no symmetry in ambition between them and their colleagues at the laboratory bench. Indeed, still in 2009, it remains perfectly acceptable even for a distinguished basic scientist to know essentially nothing about epidemiology, and yet have strong, often vociferously expressed negative opinions about our discipline and little ambition to learn. The assumption that only basic research is “true” science does of course deny the continuity between mechanistic studies and primary prevention and may indeed “reflect a certain degree of intellectual snobbery.” This situation will never be improved by complaints or defensive-ness on our part. Only successful collaboration, open discourse, methodologic rigor, self criticism, fewer alarms based on lousy “science,” and more careful promotion of findings that need confirmation will foster the mutual collegial respect that we now are often missing.

Causality

As we all know, the epidemiologic tool box is powerful but not trivial to master. The reliance on observation rather than experimentation, complex methodologic theory, advanced statistical techniques, and a separate terminology may alienate basic researchers who dominate biomedical research in influence as well as numbers. But their lack of appreciation of epidemiology may have deeper roots, related to the criteria for causal inference. In the laboratory sciences, mechanistic evidence from experimental animals is often considered both necessary and sufficient to document causal associations. In contrast, epidemiologists contend that generalization from animals to humans (or from one species to another) must be done cautiously, and has indeed often been misleading. Further, epidemiologists assert that causality can be established without mechanistic understanding—with smoking as a cause of lung cancer as one preeminent example—and that most, if not all, causes of human disease have been documented epidemiologically. Until resolved, this controversy about the criteria for causal inference will continue to penalize epidemiologic candidates for the Nobel Prize and other scientific awards as well.

CONCLUSION

The Nobel Prize in physiology or medicine is arguably the highest distinction in biomedical research. There is no question in my mind that epidemiologists have done work worthy of such distinction. Nonetheless, there are at least 3 factors that hamper epidemiologists from being considered: the rarity of distinct discoveries, unequivocally linked to 3 or fewer scholars; the lack of understanding and appreciation of epidemiology among many influential biomedical researchers; and the widespread perception that our discipline can document associations but not causality. Because none of these barriers can be easily eliminated, our expectations for a Nobel Prize in epidemiology anytime soon should be modest.

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