Advocacy groups are not able to make their voices heard. As a difficult research area that interests very few investigators, there is no known diagnostic test, and there is no known cure. This is a problem with these symptom-based diseases: the problem of specificity. Epidemiol Rev 1998;20:97-102.

A recent issue of the Epidemiology Monitor (2) compared "circular epidemiology" with the "false-positive research cycle," which I described in an earlier article arguing for the need for a more systematic approach to epidemiologic surveillance in the 21st century (3). False-positive research cycles occur under the current unsystematic research approach in which sample-based studies are used to investigate associations. In the null situation where an exposure and a disease are not truly associated, due to bias and/or random chance an initial false-positive report may be published. This, in turn, can lead to a "hot topic bias"—namely, when a topic is "hot," more investigators will become interested in it and design more studies to "confirm" or "refute" the association. Since the usual type I error rate (significance level) is set at 0.05, 5 percent of the studies will show false-positive results. These additional false-positive studies are likely to be submitted to journals (positive result bias) and accepted by editors (editor's bias). This will lead to an even bigger hot topic bias, and another false-positive research cycle will begin.

Through this biased process, current researchers can almost always "prove" something out of nothing. Kuller's comment on "excess expenditure of resources, in terms of both personnel and money" due to circular epidemiology (1, p. 898) echoed my earlier comment that "results of such an unsystematic, non-population-based approach would likely focus efforts in certain narrow areas, ignoring some other major and real issues" (3, p. 146).

The unsystematic approach in research is tied to the unsystematic approach in research funding. As Kuller pointed out, "research support unfortunately is biased toward the continued study of already proven hypotheses. A new hypothesis for which there is a lack of substantial prior data is unlikely to be successful in terms of peer review" (1, p. 897). It is relatively easy to obtain research funding for established disease entities like lung cancer and breast cancer, because they are well-defined diseases that are usually fatal and therefore create a lot of public and political pressure for research funding. On the other hand, there are investigators struggling for research funding for certain other diseases, such as multiple chemical sensitivity, chronic fatigue syndrome, and fibromyalgia. The problem with these symptom-based diseases is that they are not "glorious diseases" (as I would describe them) for researchers. Not only are they not fatal, they even lack good clinical case definitions (4). Many of their symptoms and complaints are subjective, the cause is unknown, there is no known diagnostic test, and there is no known cure. This is a difficult research area that interests very few investigators. Advocacy groups are not able to make their voices heard. As a result, research funding for these diseases remains difficult to obtain, the cause remains unknown, there continues to be no diagnostic test available, and there is still no cure.

While "hot topic" research areas are being studied to death by repeated investigations using the same design or variations on the same theme, "cold" topics are left to freeze. Metaphorically, instead of building extensions onto a house to expand our space of knowledge, current research practices simply conduct face-lifts by applying more coats of paint in the well-lit living room of the house, where there is a hot fireplace and the room is overcrowded with researchers.

Although the source of "circular epidemiology" is obvious—namely, the lack of systematic research and a research funding approach—the solution is not readily attainable. Shall we continue with the current approach of mainly funding researcher-initiated studies or convert to the practice of targeted research? How can we make sure that research is not circular, reinventing the wheel? How can we promote a quest for new knowledge? How can we anticipate emerging health problems for research? How can we create and translate new knowledge into better preventive medicine and public health? These are all very big questions that will take time to resolve.

My recommendations for some immediate solutions are as follows. All research-granting agencies should include in their grant application forms the following questions for researchers: first, "Have the applicants performed a reasonably thorough literature review on the proposed research topic (please indicate)?" and second, "How is the proposed study different from all of the previous studies conducted on this research topic, and in what ways is the proposed study better than the previous studies?" Journal editors and peer reviewers should look for answers to the above two questions in submitted manuscripts and should also ask a third question: "What recommendations for preventive medicine and public health do the authors make based on the research study?"

REFERENCES

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