

Public Health Services and Systems Research An Afterword

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The effort to establish a research agenda in public health services and systems research (PHSSR) has provided some insights and useful lessons. Our efforts would not have been possible without key contributions from academic researchers and practitioners representing a variety of public health work settings and insight from policymakers. From this process, there has been a series of insights that might prove beneficial to both our current colleagues in PHSSR and those who intend to join our research efforts. This afterword is an effort to summarize some of those insights for individuals who are or would be colleagues in our attempt to improve public health's labors to "create conditions in which people can be healthy."

One of the initial steps in the creation of the PHSSR agenda, published in this supplement to the *American Journal of Preventive Medicine*, was the development of a series of systematic reviews,¹⁻⁵ attempting to capture the current PHSSR literature. The thin nature of the systematic reviews was surprising. We had anticipated a richer literature that allowed for more inferences to be drawn. For example, the quality improvement (QI) literature, even with gray literature included, provided a limited set of lessons published. This paucity exists in the face of several initiatives. The new Public Health Accreditation Board requires, as a prerequisite to accreditation, a QI plan.

In addition, several organizations have begun to provide assistance in the development and implementation of public health quality improvement plans, including the Public Health Foundation, the National Association of City and County Health Officials, and the Robert Wood Johnson Foundation, in collaboration with the University of Minnesota. Dr. Peggy Honoré, in the Assistant Secretary of Health's office, has spearheaded a major effort to establish QI in public health. As a member of the

Assistant Secretary of Health's office, Dr. Honoré coordinated the development of the *Consensus Statement on Quality in the Public Health System*⁶ and she authored *Priority Areas for Improvement of Quality in Public Health*,⁷ two pivotal productions regarding public health and QI. It is clear from these reports that QI efforts are critical to improving the public's health, and there are increasing political demands for greater integration of these efforts into the delivery of public health services. Given the attention that QI in public health has received, one would expect the development of a more robust literature.

A potential explanation for the lack of published QI information is the persistent divide between academia and local public health practitioners, an issue that was raised as long ago as the 1988 IOM report *The Future of Public Health*. We are aware of active efforts to establish QI programs in local health departments, but many of these are not recognized by health departments or their potential academic partners for their ability to move forward the research in this area. This seems to suggest a few lessons. First, health departments may not see themselves in the academic business, even when they are implementing cutting-edge changes in processes and procedures. Additionally, health departments may not feel the necessity to share their experiences and assist others seeking to develop or implement similar processes. How can these practitioners begin to see themselves as practical researchers who can and should help their colleagues by sharing with others their experience and the lessons they have learned?

Perhaps equally importantly, these observations seem to suggest that local health departments could benefit from formal working relationships with academic institutions that could facilitate the process of designing, implementing, evaluating, and disseminating information related to changes in processes and procedures. The Practice-Based Research Networks (PBRNs) in Public Health is a response to this and provides an impetus for those in practice and academia to engage in collaborative practice-relevant research that is shared in the literature.

A research agenda seems to be most relevant for researchers, guiding their efforts to gain new knowledge

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and to provide direction for their efforts at generating new knowledge. However, the process of developing this agenda prompts an awareness of the importance of the research agenda to at least two other communities, namely, practitioners and policymakers. Unless the questions posed for research are likely to result in improved practice, with improved health outcomes, then the work is sterile and of little consequence.

This fact calls for a dialogue between practice and research to ensure that the questions asked and investigated are those that represent real problems for practitioners. Our own experience suggests that even for the academic researcher, the recognition that his or her work can and will make a difference in practice makes the work more pleasant and gives a sense of accomplishment that research for its own sake is unable to duplicate. The current buzzword of *translational research* would suggest that this issue is not confined to public health but is a broader issue in the biomedical research community. We anticipate the newly crafted agenda to prompt more and better collaboration between practice and research to ensure that this dialogue is encouraged, at least in public health. Again, an area where PBRNs can make a difference.

The other community is policymakers. Public health departments are governmental entities and thus are open to the push and pull of politics. In fact, public health is at the nexus of science and politics. In the current environment of governmental cuts, the issues that PHSSR can and should address are those that are likely to provide information to policymakers. Everyone is seeking more-efficient and effective ways to provide public services, and public health is no exception. It is imperative that, to the extent possible, we attempt to ensure that policymakers make decisions based on evidence of the most-effective and efficient mechanisms to provide public health services.

We are aware that many health departments across the U.S. are experimenting with new organizational patterns of doing business in response to budget and political changes. There is nothing inherently evil in new organizational patterns or structures; however, we are better served, nationally, if natural experiments in PHSSR are seen as productive mechanisms to investigate the impact of these new ways of doing business. Again, this affords us the opportunity to guide, with evidence, the decisions that policymakers consider, instead of passively allowing them to make public health policy and budget decisions based on a gut reaction.

The current research in PHSSR is in the early stages of development. We are still at the descriptive stage in the search for answers to questions that have been posed, as indicated in the systematic review of methods used in

PHSSR.⁴ It is clear that we must move beyond this relatively primitive set of research activities to those that allow for inferences to be drawn.

The associations spelled out by regression analysis must give way to long-term longitudinal studies that allow for evidence-based conclusions about public health services and systems. That, in turn, requires longitudinal databases for prospective studies. Where is the Framingham study of PHSSR? Can we develop and deploy mechanisms for capturing data that allow us to study variations in services and systems, the Dartmouth Atlas of PHSSR? Is there a way to develop the public health equivalent of the medical outcomes study that contributed so much to health services research knowledge? There is a need for some of these long-range, large, and expensive examinations of our discipline to answer the questions that are raised in the agenda we have developed. This occurs in other areas of biomedical and epidemiologic research, and the health of communities is at least as important as the issues that draw funding for these sorts of large, involved, and expensive projects.

One of the wonderful features of the public health profession is the array of problems that confront the practitioner. A public health director's day may bring a problem with a dog bite, a case of shigellosis in a day care center, a major confrontation over a residential development and its septic system, and a call from the chair of the board of health to fire one of the department's employees for a personal reason. Public health changes, and one only need think of 9/11, anthrax, SARS, H1N1, and the drastic economic changes of the last several years to realize that ours is an evolving discipline, with new problems piled on top of old ones.

This suggests several things, first that longitudinal studies are necessary, as they will allow for the examination of changing public health conditions and the environment within which we function. Moreover, the research agenda we have developed will change as the problems and issues that we must face change.⁸ What is published in this supplement reflects what is prevalent, where we are at a point in time. This agenda must be an evolving one and it must not remain static but mechanisms found to ensure that it stays current with contemporary public health problems. We are committed to efforts that ensure that this agenda remains current and that it reflects the state of the art in practice, policy, and research.

This leads, in turn, to another point. The agenda serves another function, that of a tracking and sorting tool. It can be used to track research efforts focused on the questions in this list.⁸ The agenda, properly used, will allow us to promptly assess what research is occurring on each of the listed issues, and this information can guide our ef-

forts to establish priorities for new research endeavors. For instance, areas receiving an abundance of research attention shall have a lower priority for new funds, compared with areas lacking an equivalent amount of research activity. Moreover, by using the agenda as a mechanism for research tracking we can more rapidly compile our contemporary knowledge of an issue as it is developed. It is hoped that this compiled knowledge then also can be used as a tool for systematic reviews regarding budding public health systems research and as a tool to provide evidence for either practice or policy.

The agenda is long and detailed.⁸ It poses questions and opens issues that require thoughtful and careful research. This research will not be accomplished without some requisite items. These items include funding. Sutton's law operates in more than just bank robberies: Researchers go where the money is. So, resources to invest in this research are crucial if the agenda is going to have any meaning.

In addition, even with money, we must have those who are prepared to do this research. The development of new researchers in PHSSR is imperative. Investment in the development of young and vigorous new faculty is obviously an area where we will succeed or fail with this agenda. Finally, new data and methods have to be brought to bear on the questions we have raised. As we have pointed out, the long-term collection of data and research, similar to either the Framingham study, the Dartmouth Atlas, or the medical outcomes study, is a vital part of the work that needs to be accomplished. Without these crucial resources, funding, young research faculty, and data, the work that this agenda suggests will never be accomplished. That is a challenge and an opportunity for those who provide support for this sort of effort; we hope they accept it.

So the challenge is offered of an agenda that reflects the current thoughts about where PHSSR is heading over the

next years. That challenge requires our attention and demands our commitment if we are to achieve the mission of improving both public health service delivery and the health of the communities we serve.

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