

Research Considerations for the Family Physician

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The general practice heritage of today's family physician does not have a strong tradition of research in the United States. In fact, research has been a forbidden word viewed by many family physicians as irrelevant. Yet, McWhinney¹ has enumerated distinguished contributions made to the science of medicine by general practitioners such as Edward Jenner (cowpox), James MacKenzie (cardiac arrhythmias), and Will Pickles (epidemiology of country practice). That this tradition continues is demonstrated by the work of John Fry in Britain,² Bent Bentsen in Norway,³ Robert Braun in Austria,⁴ and Curtis Hames in the United States.⁵ Byrne⁶ credits Eimerl with defining research as "organized curiosity." Such a label fits the tradition well.

Family physicians who have read this far may agree that there are multiple opportunities and attractions for research in their specialty. Some may even be stimulated to engage in such "organized curiosity." This paper is dedicated to them with the hope that it may clear up some myths surrounding the nature of research, provide some caveats or warning signs to aid them in their journey, and suggest a triangulation system of navigation which may help to orient them as they begin their travels.

Some Myths about Research

Research Has Nothing to do with Patient Care

Concern for man himself and his fate must always form the chief interest of all technical endeavors.

*Albert Einstein
Address, California Institute
of Technology (1931)⁷*

The practicing physician challenged by immediate problems of patient care has difficulty understanding the motivation which keeps his basic science colleague pursuing "pure" research. It is true that the immediate relevance of such study cannot be guaranteed. Yet, the physician can appreciate these efforts as he administers polio vaccine to the well child or adjusts the insulin dosage for the vigorous young athlete before him in the examining room. Then he can ponder the question of whether to obtain a throat culture on his next patient, what to advise the overweight patient about the use of artificial sweeteners, or whether to add a part-time social worker as the next person in the office. He has identified a number of questions with direct relevance to patient care. All of them need additional research before he can make decisions with the same certainty which he just experienced with the polio vaccine and insulin dosage. Daily patient care decisions are based on the answers from prior "organized curiosity."

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I Don't Know How to Analyze Data

The whole of science is nothing more than a refinement of everyday thinking.

Albert Einstein
Physics and Reality
(1936)⁷

The practicing physician contemplating research frequently expresses dismay or is pessimistic about his potential, saying, "I don't know enough statistics to analyze the data." While it is true that biostatistics makes a vital contribution to any research effort, the sequence of papers presented in this monograph indicates that interpretation and analysis of data are not the first steps in the research process. Furthermore, it is the rare clinical investigator who has a wide command of statistical techniques. Asking the appropriate questions and gaining access to appropriate data sources are certainly within the capability of an interested family physician. Just as in clinical practice, he will want consultation with specialists in statistics, epidemiology, or another discipline. From that specialist he may gain advice and instruction in specific techniques which can be applied to his problem. As in clinical family practice, the lack of special expertise in a given field does not excuse us from approaching the researchable question before us. It merely requires that we make use of the appropriate consultant; it requires us to coordinate and integrate the efforts of a variety of research specialists in the same manner we integrate the clinical efforts of our specialty consultants in practice.

I Don't Have a Complete Data Retrieval System, Therefore, I Can't Engage in Research (or the I Don't Have a Computer Syndrome)

Science is built up with facts, as a house is with stones. But a collection of facts is no more a science than a heap of stones is a house.

Jules Henri Poincaré
La Science et l'Hypothèse
(1908)⁷

There has been much valuable effort in family practice devoted to describing and documenting

the work of the general family physician. Eimerl and others in England have pioneered with the development of the "E" book and the RCGP Coding System.⁸ Froom and colleagues,⁹ and Marsland, Wood, and Mayo¹⁰ have made extensive and valuable contributions. A system which records identifying information, problems presented, diagnoses, and therapeutic procedures for all physician-patient contacts does provide for better understanding of one's practice and population. It may improve management of the practice. The data recorded are useful for epidemiologic studies in family practice. But, it is entirely possible to conduct research in family practice without this. The recording system will identify patients and describe the population from which they are drawn. However, the actual data with which to test a research hypothesis will come from detailed record review, observation of patients, questionnaires, or some additional source of information. Questions can be defined; hypotheses can be stated; experimental and quasi-experimental situations can be created; data can be collected; analysis and interpretation can be carried out. All of these can be done with paper and pencil or card files and without computers.

Caveats or Rules of the Road

Your Question is Probably Not Entirely Unique

. . .and there is nothing new under the sun.

Ecclesiastes 1:9¹¹

Presuming you have identified a question of interest and wish to seek its answer, what is the next step? The question you stated may have unique elements and may bring your unique insight and perspective. But it is probable that someone else has thought about it or related questions. Before going further, seek out those others. Go to the literature. A medical library in your region can probably provide you a service for literature search. You may scan the *Index Medicus* for recent years. Contact colleagues whose special interests relate to your question. Test out your ideas with them. Contact your medical school or referral center to see if someone may be working on related problems. As a first step, learn as much as you can about what others have done.

Beginning a Research Project is Not a Route to Rapid Publication

Of making many books there is no end, and much study is a weariness of the flesh.

Ecclesiastes 12:12¹¹

There is much interest in family practice in developing a "body of literature." This *Journal* is dedicated to that end. Family physicians in academic settings, for better or for worse, feel the traditional pressures to "publish or perish." If your motivation to engage in research comes from this pressure, think again. Productive research takes time and painstaking work. At what you think is the end, you may only have restated the question into a more researchable one. Publication is only justified when you have clearly identified some information which will be of value to others. If your desire is to publish rapidly, do it another way. Your literature review may be of benefit to your colleagues. Consider summarizing it in print. Write from your own clinical experience—case reports. Don't anticipate *rapid* publication from a research effort.

The Scientific and Clinical Methods Are Alike, Yet Different

The progress of science and the scientific and intelligent practice of medicine employ, therefore, exactly the same technique.

Abraham Flexner (1910)¹²

Analogies are often drawn between the scientific and the clinical method. There is truth within them. Both the research scientist and the practicing clinician attempt to solve problems. They must start by formulating that problem in words. They seek out information to verify or refute those hypotheses or working diagnoses. They may reject the initial formulation and start over. But, there may be a difference. As clinicians facing an individual patient's problem, we rapidly sift, discard, and focus on multiple working diagnoses. We then accumulate physical findings, historical data, and/or laboratory data which may *support* these. If one or more of those diagnoses are *supported* by the data, we design a therapeutic plan and look for clinical signs that the problem is resolving. In doing all of this, we "play the odds." We value

most highly the treatable condition. We are always concerned for the improbable but devastating diagnosis. We may act on a diagnostic decision accepting a large probability of error. (For example, we recommend a major surgical procedure for appendicitis knowing that our diagnosis has a 15 or 20 percent chance of being in error.)

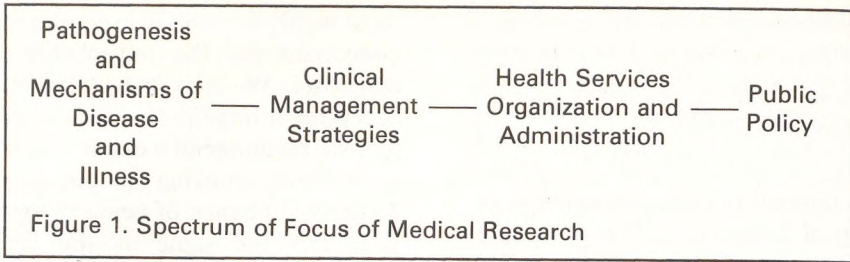
Is this the same as the scientific research method? Yes and no. The investigator likewise states his formulation of the problem as an hypothesis. In similar manner he seeks and records data. He then turns all his effort toward *refuting* that hypothesis. Only if it stands up to multiple challenges is he willing to reach a conclusion and act on it. The odds must be much heavier in support of his hypothesis than those we would accept before taking the next step in managing an individual clinical problem. Good clinical practice and good research both demand rigorous review of evidence before a conclusion may be reached. However, the history of clinical research is full of instances in which the investigator gathered anecdotal evidence in support of the hypothesis but did not demand as full a search for refuting evidence. The clinician engaging in research must be wary of this pitfall.

The Question Worth Pursuing is the One Which Interests You

It is the lone worker who makes the first advance in a subject: the details may be worked out by a team, but the prime idea is due to the enterprise, thought, and perception of an individual.

*Sir Alexander Fleming
Address, Edinburgh University
(1951)⁷*

There are many pressures today to study only questions which will have immediate payoff. Research programs are often "targeted" toward problems which are of major concern to the population. The assumption is that an organized massive effort can solve any problem. Putting man on the moon is cited as an example. Yet, the basic research which enabled that accomplishment took place because of the intellectual curiosity of individuals—the lone workers.



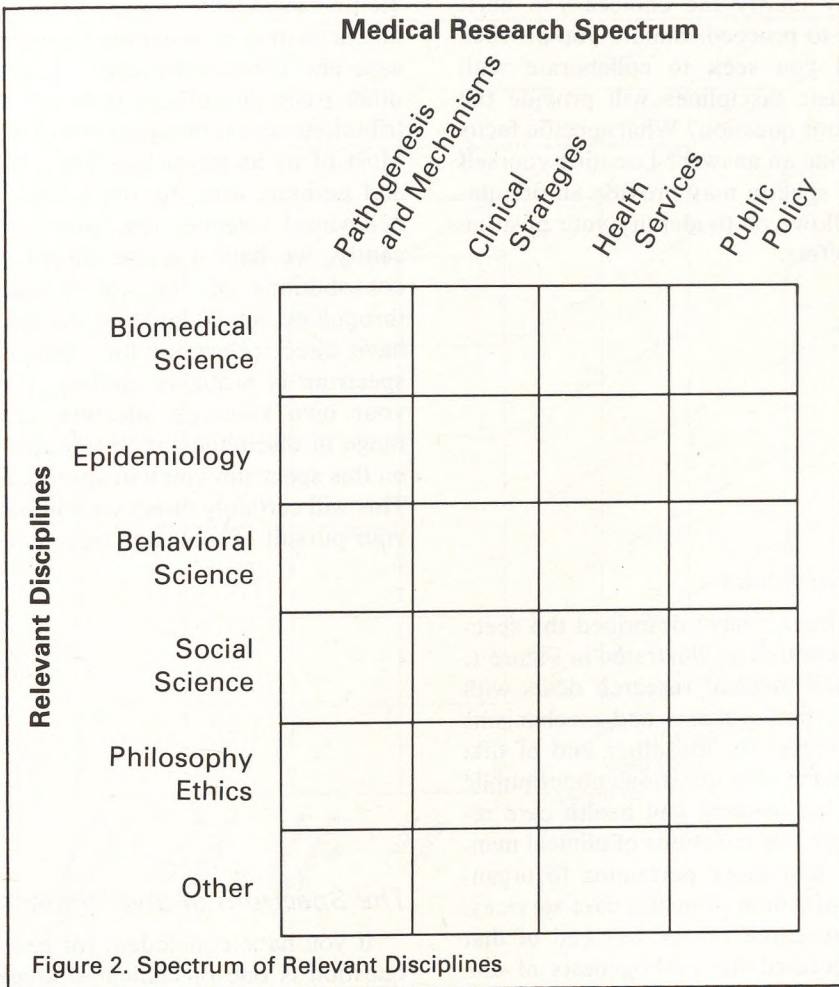
Others may not share your level of interest in the question you have defined. Some may doubt its relevance or practical application. However, you are much more likely to stay with your project and to complete it if the question you are pursuing arose from your own experience and interests you—for whatever reason. The knowledge you gain may be useful in ways entirely unexpected.

The Research Finding May Not Support The Sponsor's Views

There is only one proved method of assisting the advancement of pure science—that of picking men of genius, backing them heavily, and leaving them to direct themselves.

*James Bryant Conant
Letter to New York Times
August 13, 1945⁷*

A dilemma for the lone worker is that he must have support to continue his work. That support must come from public or private funds. Those donating the money will have their own goals and objectives. There must be a match between those and the interests of the researcher. A particular problem is encountered when professional organizations, societies, or associations undertake to conduct research programs. Most such professional organizations exist to serve as advocates for their membership in dealing with other elements of society. This is right and useful. But, it creates problems if that association undertakes to conduct scientific research. The results of that research may not be useful in serving the advocacy function for the membership. What to do in such a situation creates a problem both for the researcher and the association. We are all vulnerable to this dilemma. We all have our biases, aspirations, and concepts of the direction in which we should be heading. But, we cannot let these influence our research effort, which must be an unbiased search for answers to the problems presented.



Three Spectra of Research Opportunities

Some readers may now have decided to proceed with personal involvement in research. You will probably have a particular curiosity which you seek to organize. It may be helpful to classify that curiosity. That may direct you to the right person for assistance. It may identify what literature is pertinent. It may clarify the choices you must make about how to proceed. Should you proceed alone or should you seek to collaborate with others? What basic disciplines will provide the tools to pursue your question? What specific focus promises to provide an answer? Locating yourself on each of three spectra may provide an orientation which will allow you to identify your position in the research effort.

The Spectrum of Focus

Feinstein and Beck¹³ have described the spectrum of medical research as illustrated in Figure 1. On one end, much medical research deals with understanding the pathogenesis and mechanisms of disease and illness. On the other end of that spectrum are researchable questions about public policy for deploying medical and health care resources. In between are questions of clinical management strategy and those pertaining to organization and administration of health care services. For example, if research on the left end of that spectrum has identified the pathogenesis of diabetes and the role of insulin in glucose metabolism, there result multiple researchable questions about strategies for using insulin. Similarly, researchable questions arise concerning how to organize one's practice or a community program to ensure that proven strategies are available to patients. Finally, there are questions of relative priority for public funding and public effort. All of these are medical research questions. It may be helpful in articulating your question to identify its focus on this spectrum.

The Spectrum of Applicable Disciplines or Traditions

In an earlier paper, Gordon has discussed the multiple traditions underlying family medicine research.¹⁴ Figure 2 attempts to illustrate this spectrum of relevant disciplines which may be brought to bear along the entire spectrum of medical research. Medicine has exploited (to great advantage) the biomedical sciences (eg, anatomy, physiology, biochemistry) in exploring the mechanisms of disease and clinical strategies. Epidemiology is another basic discipline which has made great contributions across the spectrum of medical research. Most of us as physicians are familiar with these and perhaps with the third discipline listed—the behavioral sciences (eg, psychology). More recently, we have become aware of the potential contributions of the social sciences (eg, anthropology, sociology) and the fact that they may have direct relevance for clinical research. This spectrum is probably endless. As you approach your own research question, consider the full range of disciplines and traditions. Decide where on this spectrum you will approach your problem. This will certainly direct your literature search and your pursuit of tools and techniques.

The Spectrum of Involvement Style

If you have concluded, for example, that your question is one of clinical strategy, and that the techniques of the behavioral sciences offer the best chance for an answer, how will you proceed? Another range of possibilities presents itself and is added in Figure 3. What resources will you need? What resources do you have? How much time, money, and clinical resources will be necessary? The answer to these questions may dictate where you belong on this third spectrum. Are your time and facilities very limited? If so, perhaps you can seek out others working on the same problems and volunteer to collect data in your practice. The

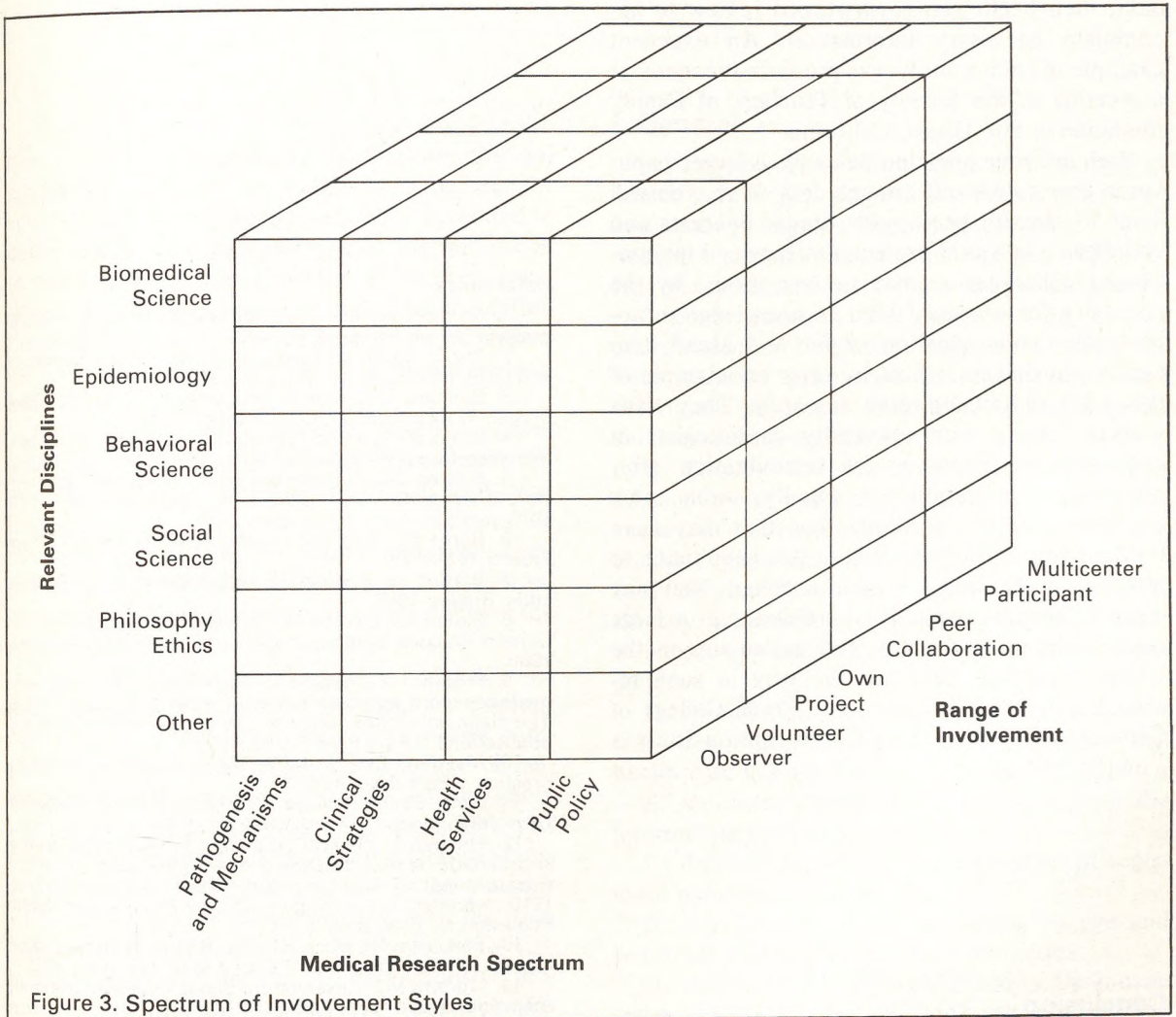


Figure 3. Spectrum of Involvement Styles

author has had one experience during which he volunteered to participate in a clinical trial. It was useful and provided understanding of the discipline involved in recording clinical observations. Perhaps the question you are approaching can be answered with data that have already been accumulated in your own practice. If so, a solo effort may be appropriate. Seek consultation with those who can assist with design and analysis of the data, then begin your own record review to accumulate necessary information. An excellent example of such a study was presented recently at a meeting of the Society of Teachers of Family Medicine in San Diego, California.¹⁵

Perhaps your question demands a larger population base to provide enough data. If so, you will want to identify peers with similar interests and collaborate in a joint project. Or, the need for continuing collaboration may be brought on by the necessity for a special kind of knowledge in approaching your question. Pyfer and Mead, two family physicians in Seattle, offer an example of this kind of collaborative research. They have worked jointly with university cardiologists in exploring the problems of rehabilitation after myocardial infarction.^{16,17} Finally, your own experience with a particular question may have brought you to the point where you have much to offer in a wider attack on that problem. You may wish to become an active participant in a large scale multicenter project. Our colleagues in the United Kingdom have led the way in such research in general practice. The Royal College of General Practitioners' Oral Contraception Study is a major example.¹⁸

Conclusion

Is your curiosity such that you now wish to proceed as a family physician investigator and researcher? If so, perhaps this paper has cleared the way by disposing some myths. Keep in mind the caveats suggested. They may help you avoid some major pitfalls. Locate yourself on the coordinates in Figure 3. Then, you may wish to sit down with the workbook included in the appendix of this

monograph. Gordon developed that workbook for the use of family practice residents affiliated with the University of Washington. It has proved a useful tool for those attempting to define and focus their research questions. It also provides a useful format for presenting those questions to others when one seeks consultation or presents his work for critique by his colleagues. Good Luck!

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