

Viewpoint

Fundamental research at primary care level*

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Two Canadian medical schools may have appointed a family physician as dean, but in most of the developed world primary care is held in low esteem in academic establishments. If, as I believe, primary care provides the essential underpinning for any rationally balanced health service why do so many in positions of academic power and influence have a negative opinion of general practitioners and family physicians?

In my view, this negativity is because primary care investigators, as opposed to their biomedical colleagues, have contributed little fundamental knowledge on the origins and natural history of disease. Studies of physicians' activities, problem distributions, training schemes, prescribing patterns, appointment strategies, office staffing, specimen collection, and management strategies are helpful. They are health services research, an enterprise with which I have been involved for decades; I welcome them and we need more.

Health services research, however, does not throw much light on the interacting factors that predispose to, precipitate, and perpetuate ill-health and disease. Such studies rarely expand our knowledge of prevention, treatment, and amelioration of disease to the extent that biomedical research does, although I believe primary care research could have an impact in these areas.

Truly important studies contribute to our understanding of causality; their insights endure as essential components of medical theory and practice and are applied in many settings. At best, the results of most studies in the past few years by primary care investigators have had only a slight impact. Only rarely does primary care research have a lasting influence on the practice of medicine or the education of health professionals.

First-rate fundamental research at the primary care level should contribute substantially to buttressing the intellectual credibility of general practice and family medicine in the eyes of the medical establishment. To accomplish this, priorities and aspirations for primary care research need to change. Is there any valid reason why a primary care investigator should not contribute landmark knowledge worthy of a Nobel Prize?

How should one start? The first criterion for selecting any research problem is that it can be an important one. There is little justification for wasting time on unimportant matters. But how does one define important? A very important question is whether there is life after death. Unfortunately, few people are working on it although it is

apparently researchable.¹ Other important problems are those involving large numbers of people, many days in pain or suffering, many days lost from work or school, and those requiring extensive human and material resources or money that could be put to use more productively elsewhere. Less common problems are also important if their origins, when better understood, result in more effective prevention or resolution. Important, fundamental, and usually complex questions should be the hallmark of primary care research.

The second essential ingredient for first-rate research of any kind is curiosity. Little of note is accomplished without a burning curiosity. But what kind of curiosity is needed for primary care research? It is the curiosity of the naturalist concerned with first causes, diversity, and patterns of growth and senescence rather than with structures and processes. John Ryle (1889–1950), the former professor of medicine who was the first director of Oxford's Institute of Social Medicine, observed that naturalists have "the desire to establish the truth of things by observing and recording, by classification and analysis."

"Certain sciences, such as chemistry, physics, and modern physiology",¹¹ wrote Ryle, "are essentially experimental. Others, like zoology and astronomy, are observational. In the biological sciences as a whole it would seem we can dispense with neither method . . . It is well to remember, however, that nearly all experiments have developed on the basis of earlier, painstaking observations of natural phenomena, and that there is actually no great dividing line between the methods . . . The observer uses the slow, vast, and difficult experiments of nature".² Unfortunately, naturalists in medical research are now in short supply, especially in primary care.

Experimental methods, buttressed by ever more complex statistical analyses, have contributed mightily to the generation of effective pharmacological and technological interventions; we should be very grateful. As engineers' close attention to the wiring enables them to detect malfunctioning of a radio's transmitters, and receivers, so the biomedical scientists' focus on neurological, humoral, and chemical pathways enables them to detect malfunctioning of the patient's neurotransmitters and receptors. Primary care scientists, on the other hand, should be concerned with the music and messages transmitted and received over the wiring and through the ether. There is a vast difference between the two but neither is good or bad, right or wrong, hard or soft. For effective understanding of health, disease, and suffering both the wiring and messages deserve investigation. The role of naturalists in medicine has been lost sight of in the wake of biomedicine's growing hegemony. It is time to restore the balance western medicine needs now, more than ever, the wonder, awe, and observational instincts of the naturalist.

Another development has accompanied the evolution and dominance of biomedicine. That is the myth of the single "cause" of each disease. Western medicine's failure to

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distinguish between necessary and sufficient factors in the genesis of ill-health distorts both its theoretical base and clinical practice. The public harbours the notion that most physical ills are due principally to genes or germs. Both are important but rarely sufficient to cause disease. Undue focus on them tends to stifle further thought about the myriad factors that impinge on each individual's disease. Let me illustrate the point.

Years ago I accompanied an Indonesian doctor making rounds in a village. A distraught mother brought him her feverish, coughing infant. "What seems to be the trouble?", I asked. The physician replied: "The child seems to have bronchitis but the mother is depressed. The mother is depressed because her husband is chronically drunk. The husband is drinking because the pig, the family's main source of wealth, is dying. The pig is dying because it is the rainy season and the roof is leaking. The roof can't be repaired because there is no money. So what is the problem", he asked "the rain pouring, the roof leaking, the pig dying, the husband's drinking, the wife's depression, or the infant's bronchitis?" What is the point of investigating the "wiring" in such a situation when so many unspoken poignant messages are ricocheting back and forth from rain to roof to pig to husband to mother to child? Would the child have developed bronchitis if a kindly neighbour had repaired the roof?

Here is another example. One Monday an irate medical student assigned to a home-care service demanded to see me. "I've had it with this family medicine business," he stormed. "Over the weekend I made twelve house calls to Mrs Jones and her eight kids. It was just one thing after another; there were colds, coughs, fevers, vomiting, pains—just no end to the problems." I said: "Did it ever occur to you that there was something else going on in the family that was upsetting everyone?" I sent him back to discover the problem. Sure enough he returned with word that the father had lost his job and was drinking heavily; there was no money and the mother was distraught. The children were receiving the parents' desperate non-verbal messages; their immune systems were impaired allowing "bugs" to wreak their harm by manifesting assorted physical illnesses. These are anecdotes, however, they are not research. If such tales are to have any impact on medical practice and education, they require support with credible research.

Walter B Cannon's 1942 article on "Voodoo death",³ bolstered by George Engel's brilliant description of the "biopsychosocial" paradigm in 1977,⁴ led to increasing calls for the expansion of the 17th century world view that dominates Western medical thinking.⁵ The potential role of epigenetic phenomena in modifying the substrate of many diseases provides additional support for the need to broaden our notions of causation.^{6,7} No longer can we ignore vital information describing the circumstances surrounding the onset of each individual's disease.

It is hard to deny the outstanding success and effective interventions of our present medical paradigm. But if a broader model can accommodate a wider array of clinical and historical evidence and generate more enlightened understanding of illness, disease, and health, is it not preferable? As with any hypothesis, however, acceptance is unlikely in the absence of credible research. Research is needed that will persuade the sceptics in the medical establishment that changes in the emphasis and content of medical education and scientific thinking are fully warranted, if not long overdue.

Mere talk about the centrality of the biopsychosocial or any other paradigm is simply inadequate. That is the challenge and the opportunity facing primary care investigators. Two public figures provide additional examples. The late William Casey, then director of the US Central Intelligence Agency, on the day before being questioned by a congressional committee about the Iran-Contra imbroglio, had a major convulsion due to a previously undiagnosed malignant brain tumour. In response to a reporter's question, his physician, a prominent academician, stated on national television that the timing of the seizure was "just a coincidence". Maybe so, if you believe in coincidences. A different view, however, was expressed by Woody Allen's character Isaac Davis in Manhattan when he said: "I can't express anger. That's one of the problems I have. I grow a tumour instead!" What's going on here?

To investigate such matters at the primary care level, we should consider addressing five types of generic question.

Onset circumstances—What precisely was the situation surrounding the initial signs or symptoms of the patient's discomfort or illness? Where was the patient? Who was he or she with? What was he or she doing or thinking? What was new or different and what did he or she think and feel about it? What were other persons in the home, at work, in the family, in the neighbourhood doing or saying? What were the unspoken messages he or she was receiving? Were there more or fewer messages than usual? Were they more or less intense and emotional?

Concomitant factors—Was there a constellation of two, three, or more interacting or re-enforcing circumstances or encounters surrounding the onset of the patient's discomfort or illness? For example: unusual job stress, damp weather or a "chill", and exposure to a "bug", before the onset of a common cold; the threat of unemployment, the presence of a sick child or relative, and undue fatigue from physical exertion on the job, before the onset of rheumatoid arthritis; consumption of an extra cup of coffee and a caffeine-laden chocolate biscuit, followed, when late for an appointment, by a stressful drive through dense traffic, immediately before an episode of atrial fibrillation?

Predisposing factors—What is known about the patient's genetic, familial, and cultural backgrounds and "belief system"? Does the patient have or believe he or she has any particular vulnerability or susceptibility or what used to be called a *locus minoris resistentiae*? Why me? What is the patient's theory about the problem?

Precipitation of help-seeking—What events, comments, thoughts, or behaviour triggered the patient's decision to consult a particular physician at this precise time? What, if anything, changed? Why now? Why here?

Therapeutic environment—What did the patient feel, perceive, imagine, and think about the physician's and nurse's behaviour, the technology, procedures, medications, and general ambience of the setting? What were the characteristics, hallmarks, and reputation of the health-care personnel, institution, system, or clinic?

Patients' responses to these kinds of questions usually are best elicited at the primary care level and should go a long way toward understanding the webs of causality.

The ten problems described below are illustrative; there are scores of others that cry out for deeper understanding. All of these problems have been studied previously, some decades ago, others by several investigators. To provide external validity, to say nothing of generalisability leading to acceptance by the medical establishment, their initial

findings require replication with large numbers in diverse settings. Only then are the results likely to be incorporated widely in medical education and practice. Each problem deserves critical thought, careful refinement, and several pilot studies. Like eating an elephant, you take one bite at a time.

Collaboration with immunologists, neuroscientists, psychologists, epidemiologists, sociologists, clinical specialists, and other scientific colleagues is essential. Research designs will necessitate the development of generic protocols, including survey instruments that enable the reporting clinicians to record categories of responses derived from conversations with each patient and probably one or more family members or friends. In addition to a wide range of quantitative methods, a substantial armamentarium of qualitative research methods is available for use in primary care.⁸ Generation of adequate numbers—large numbers—for studies of each clinical entity and its explanatory patterns at the primary care level needs substantial networks of primary care practitioner/investigators who report to a central co-ordinating office. Successful examples include those sponsored by the Netherlands Institute of Primary Health Care's Continuous Morbidity Registration Sentinel Stations,⁹ and the American Association of Family Physicians' Ambulatory Sentinel Practice Network.¹⁰

Here are common clinical questions that I believe merit further investigation:

- If *Helicobacter pylori* is a necessary factor in the causation of peptic ulcer, is it sufficient? Why do so many "carriers" of the bug not develop the disease? What other factors or experiences does it take to evoke clinical symptoms?¹¹
- What role does separation from a family member, neighbour, job, or even a pet, have on the precipitation of an illness such as congestive heart failure?¹²
- How frequently is the manifestation of tuberculosis preceded by "two years of increasingly disturbing occurrences"?¹³
- How frequently is the perception of inability to control fundamental aspects of one's job associated with the development of an illness such as coronary heart disease?¹⁴
- Is the depression associated with pneumonia a precursor or a sequela of the illness?¹⁵
- Are there illnesses that come from being "caught in a trap"?¹⁶
- Why do patients recover more rapidly in some hospitals than others?¹⁷
- Why do wounds heal more quickly on some clinical services than others?¹⁸
- Why do healing rates for both placebo and active ingredients vary widely in different study centres during clinical trials using identical protocols?¹⁹
- Why do mortality rates increase substantially after personal, religious, ethnic, and statutory holidays, and anniversaries?²⁰

This is difficult research but then all really important research is difficult. If it is not difficult, it may not be worth doing. Others have tackled the easy problems.

Here is another suggestion: if the biopsychosocial, or, better still, just the "broader" paradigm is an accurate formulation of reality that can accommodate a wide array of factors impinging on the patient's life and health, why not find out how useful it is clinically? I suggest that some group develop a simple but carefully designed postal survey of a credible stratified probability sample of all primary care clinicians in two or three countries; the response rate should be at least 85%. Ask this: Is the biopsychosocial paradigm useful in the management of most (over 50%) of your

patients? Answer yes or no. If "yes" provide one brief case history showing how it was useful.

Positive results should provide a host of researchable questions as well as put primary care research on the medical establishment's map.

Sir James Mackenzie (1853–1925) is the patron saint of general practitioners and family physicians the world over. By following patients in his general practice for decades he revolutionised cardiology and concluded that much was to be learned by meticulously studying the origins of symptoms and describing the natural history of disease. In 1919 Mackenzie retired from his world-renowned cardiology practice in London to establish an institute for medical research in St Andrews, Scotland. Among its purposes was: "To investigate disease before the occurrence of any structural change in any organ of the body, with the view of providing a diagnosis at a period earlier than is possible by the methods now in use and in order to obtain a knowledge of the circumstances that favour the onset of disease".²¹

In my view, primary care can best achieve the academic stature to which it aspires by internalising the clinical wisdom bequeathed us by Ryle and Mackenzie and undertaking the serious investigation of important clinical problems.

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