Kenneth Boulding in his foreword writes that this book "should cheer a lot of people up who are undeservedly rather low on the totem pole of academia" (p xv), that is, people working on narrow problems of the real world rather than working on theories of their discipline. These researchers also do, or can do, orderly thinking. Moreover, they are usually less encumbered with methodological orthodoxy than those working on theories whose concepts have been transported from the physical to the social sciences.

Johnson’s book offers an antidote to the emphasis in economics on the methods of the physical sciences. Johnson distinguishes between three types of research—disciplinary, subject-matter, and problem-solving—recognizing that economic research often blends these types. Subject-matter research and problem-solving research (together termed "applied research") are invariably multidisciplinary. They differ mainly in their span of coverage over problems and decision makers. Most research on human migration, for example, would be classified as subject-matter research, whereas most research on costs and benefits of importing workers for a specific employer would be classified as problem-solving research.

The three categories of research are said to be conceptually separate because they differ in the kinds of information acquired, in the methods used to acquire such information, and in their "philosophical underpinnings." This view leads Johnson to examine the nature of information, particularly the degree to which it can be objectively determined (whether or not the information is value-free), the possibility of obtaining accurate knowledge of human values (values defined here as the degree of "goodness" or "badness" that people hold with respect to "things, conditions or situations"), and the possibilities of acquiring prescriptive knowledge about the rightness or wrongness of decisions that one might make.

A third of the book is given over to explanation of the major philosophical underpinnings—positivism, normativism, and pragmatism—and to how each philosophical view is reflected in economics as a discipline. An excursion into philosophy seems necessary in a treatise on research methods, but important ideas may be obscured in the language of philosophy.

Some main ideas are these. Logical positivism is helpful, not because it always reflects the real world (which it does not), but because it can correct its mistakes. Strict adherence to positivism tends to restrict the scope of what can be usefully researched.

Some normativists have come to believe that knowledge of values can be researched in the same sense that knowledge of value-free information can be researched. Indeed, many of the basic values in a society are experienced directly by most of its members (for example, a full stomach is better than an empty one) and do not need proof. This is a key proposition that leads to investigation of the most relevant problems of a human society. On this matter, Johnson concludes that our first task as disciplinarians and applied subject-matter researchers and problem solvers is to free ourselves from the shackles of logical positivism and of Pareto optimality and conditional normativism. We need to do this to get on with the important task of increasing our stock of objective disciplinary knowledge about the goodnesses and badnesses experienced as characteristics of the real world. More such objective knowledge of values is crucial if we are to improve and prevent deterioration in the structure of our economies and societies. Without this knowledge, we cannot know objectively whether improvement or deterioration is taking place (p 97).
As for the pragmatic approach (wherein the truth of propositions is judged by their consequences), its strength in research is its concern with real world problems and its dependence for their solution on insights from all relevant disciplines. Pragmatism's main weakness, however, is in its very complexity and the general refusal of its proponents to subdivide problems into manageable parts as positivists and normativists do. Pragmatists' rejection of simplifying methods may prevent them from making headway in illuminating specific value-free and value-laden problems.

After this excursion into philosophy, Johnson considers six case studies (two in each of the three research categories) to illustrate the nature of information sought and the way that the research effort was mounted, administered, monitored, reviewed, and funded. He examines Leontief's input-output matrix and Schultz's studies of the role of investment in human capital in economic development for their contributions to economics as a discipline, recognizing that each effort was mounted from a concern with a real world problem.

In subject-matter research, Johnson shows that Fox's measurement of how people in a community value their time is an outgrowth of Fox's early interest in sociology and mathematics. The other study, made by the U.S. National Academy of Sciences at the request of President Ford, asks how U.S. research should be organized to help solve the world's food and nutrition problems. It cuts across many disciplines and thereby reflects the philosophical positivism of biological and technical scientists who commonly believe that knowledge of real values cannot be known. Johnson says that this 1977 report may have contributed to subsequent erosion of support for social science research.

Problem-solving research is illustrated by a Michigan Pickle Study done by university researchers under continuing dialog with the main pickle interests and by an investigation by the Michigan Public Service Commission into a rate increase that had been requested by the Consumer Power Company. The latter study elicited information mainly through adversarial hearings. The problem in both studies was to find an acceptable price for services that could be publicly controlled: Mexican nationals' wages in one case and the price of kilowatts in the other. Thus, the two studies differed in the rules under which prescribed prices were arrived at.

The remaining chapters draw together the implications of the philosophical underpinnings, methodologies, and case studies for research support, accountability, administration, conduct, review, evaluation, durability, and practical importance of economic research.

Johnson intended this treatise mainly as a textbook for graduate students in the social sciences. It is purposely repetitious in language and presentation and a bit tedious on this count. The required readings at the end of the first nine chapters, averaging about six readings per chapter covering about 50 pages, should help students, particularly those who are uninitiated in the language of philosophy, to grasp essential ideas.

It is important, I think, for economic researchers to develop their own philosophy of research just as Johnson has done. To this end, the book should serve as a sounding board against which to identify one's own biases and blind spots. It should be studied by persons concerned with promoting, administering, evaluating, or funding research in the social sciences. It is possible, as Boulding notes in the book's foreword, that the taxonomy of research commended to us by Johnson will become more widely used to advance an understanding of human society.