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978-0-521-09894-6 - The Applicability of Organizational Sociology

Chris Argyris

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PREFACE

During the past several years I have been especially involved in trying to understand some of the theoretical and applied problems of relating thought to effective action. This interest has led me to explore the degree to which our methods of rigorous research may, if used well, inhibit one from obtaining valid knowledge that could become the basis for effective action. I concluded that concepts of rigorous research may be identical to those that are used to design the assembly lines in large corporations. Subjects may react like workers and knowingly or unknowingly give invalid information (Argyris 1968). In short, the conceptions of rigorous research may be subject to question, as are the research procedures that are derived from these conceptions.

The next step was to look at examples of the knowledge produced by behavioral scientists to see how relevant they were to effective action and change. Explorations were completed of key theories and research in social psychology (Argyris 1969) and industrial psychology (1970a).

In the case of the former, dissonance and attribution theories were found to be applicable in the world as it now exists; a world dominated by low interpersonal trust and openness. The proponents of these theories have shown little interest, however, in exploring how behavioral science research could help to design and thus become appropriate to a world of high interpersonal trust and openness. It would not be surprising to learn that a reader, especially the lay practitioner, might conclude that in the present world dissonance and attribution activities are givens. They may therefore act to continue these activities. Thus these theories may help to form the basis for maintaining the *status quo*.

In the case of the latter an examination of industrial psychology suggested that many psychological researchers tended to place the environment (social structure, norms etc.), in a black box and act as if they did not exist or, if they did, were benign or trivial variables. The unintended consequences in the field of industrial psychology were (1) internally consistent and empirically valid

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theory were rarely developed, (2) the practical contributions ranged from being incomplete, to misleading, to dangerous, (3) the field accepted and reinforced the *status quo*, and therefore (4) produced much knowledge which was neither valid nor useful, (5) which may someday set the stage for society to decide to attempt to manage and re-orient the activities of industrial psychologists.

One day I received an invitation from Professor William Whyte to participate in a conference planned by Professor Ned Rosen and himself to explore issues emerging from recent empirical research in the field of sociology or organizations. He informed me that, among others, Professors Blau, Thompson, and Perrow had been invited and were planning to attend. I accepted the invitation with a strong sense of pleasure and gratitude. As I contemplated what I would write, it occurred to me that, in my inquiries about the relationship between thought and action, I always had difficulty with the work of these three renowned sociologists. All seemed to believe strongly in a 'sociological approach'. They were committed to developing theories about organizations that apparently ignored much of the research in personality, interpersonal relationships, and group dynamics.

As long as I was not concerned with the issue of effective action, then my bewilderment about how they could maintain, what seemed to me to be an unreal differentiation, never led to action on my part. After all, I kept saying to myself, all scholars have a right, indeed it is a necessity, to place limits and boundaries on their work. I, too, place boundaries in my work thereby making my work incomplete and lopsided.

However, once I became interested in the applicability of knowledge and in the idea that behavioral scientists ought to study organizations by creating new ones or changes in the existing ones, then the issue of where one drew the boundaries became very important. I had to confront myself and others about how we chose the variables we ignored.

In re-reading the work of these authors, I became even more convinced that their theories would tend to emulate and reinforce the *status quo*, and if an activist were to use these theories as a basis for change, he would become an authoritarian manipulator (again, the *status quo* in change processes). I also concluded that the exclusion of the variables listed above tended to make their sociological theory less effective.

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I decided that one way to make the conference more of a learning experience for me and, at the same time, discuss some of the emerging questions, would be to raise the issues that concerned me by illustrating them with the work of Blau, Thompson, and Perrow. Not only were these scholars central figures in their respective fields; not only were they influencing many graduate students and other scholars; but, for my immediate purposes, they would have read the manuscript and could confront me directly on these issues.

As I began to write my paper, I decided to expand it to include other writers who were also active in the field but who expressed these sociological biases in different forms. I chose John Goldthorpe and David Lockwood because of their recent research and because it represents an approach that is somewhat different from Blau, Thompson and Perrow.

There are other sociologists whose works are similar and those whose works are dissimilar from these men. It seemed to me useful to note some of these people especially during the discussion of what might be done about these problems.

Part II, for several reasons, was not presented at the conference. Part I was already too long and common decency dictated that I ought not to impose myself on those present even more. But decency was not the only reason. The truth is my cognitive maps about these issues are not very clear. I believe that the framework developed so far, and presented in Part I is a useful one. However, I also see it as incomplete, its parts underdeveloped and as yet loosely connected. The primary reason for this is my lack of knowledge and my limitations. A secondary reason is the lack of research available, especially studies describing sociologists who attempt to test parts of theories by creating changes in on-going systems or organizations.

It was the questions of my fellow conferees and the enthusiastic encouragement and challenge of the younger faculty and graduate student observer-participants that led me to include my views on the subject even though they are primitive. Also, there is the hope that the material might stir up interest among scholars about the exacting intellectual problems involved in applying behavioral science knowledge effectively to the problems of our society as well as to redesigning it.

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*To Di, Dart and Dianne Ellen –
I love them all*