and man acts within the realm of possibilities the productive system provides. In primitive society man applied distinctions he perceived in nature to himself as a means to create artificial distinctions between men and provide a basis of social organization. In modern societies the mode of production and the system of exchange creates economic inequalities that create real distinctions between men. These distinctions are used as a basis to define social relations and the structure of society. Whereas primitive man determined the superstructure, and exchange provided a system to link men together, the superstructure in modern societies is determined by the mode of production. Economic inequalities created by the system of exchange are used to differentiate among men.

Differentiation and exchange serve as the bases of social cohesion in *both* types of society, but obey entirely different "historical logics." The totemic society is stablized by an artificial homology with nature, whereby man becomes subject through the application of natural objectivity. In caste society production for exchange becomes the center of the system, but only at the expense of displacing men from their determinative role as the subject of history.

REFERENCES

Ekeh, Peter P.
  1974   Social Exchange Theory. London and Edinburgh: Morrison and Gibb Ltd.
Glucksmann, Miriam
  1974   Structuralist Analysis in Contemporary Social Thought. London and Boston: Routledge and Kegan Paul.
Levi-Strauss, Claude
  1963   Totemism. Boston: Beacon Press.
  1966   The Savage Mind. Chicago: The University of Chicago Press.
  1969   The Elementary Structure of Kinship. London: Eyre and Spottiswoode.
  1976   Structural Anthropology Vol. II. New York: Basic Books.
Marx, Karl
  n.d.   Capital Vol. I. Moscow: Progress Publishers.
Turner, Jonathan H.
  1974   The Structure of Sociological Theory. Homewood, Illinois: The Dorsey Press.

NOTES AND COMMENTS

"WHY DOESN'T HE MENTION SO-AND-SO?"

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It is especially telling, I think, that a chapter entitled 'Responses to Death and Stages of Grieving' could be written without reference to either Freud or Erich Lindemann. Or, as another example, chapter 4 (a rather peculiar section, reviewing some of the various 'contexts' in which death occurs, i.e., homicide, disease, accident, etc.) manages to discuss situations of mass death in war and natural disaster without so much as a nod in the direction of Robert Lifton (Lofland, 1979).

The above is a typical specimen of the genre of academic comment that I shall refer to here as "why doesn't he mention so-and-so?" Taken at face value, the purpose of such remarks is to set straight a piece of unsound academic business. But, I shall argue, the tone of righteous intellectual indignation that appears in comments of this order is misplaced, because what they really signify is a profound "epistemological pathology" of the social sciences. On the deepest level of its consequences, this pathology condemns the social sciences to a collective existence characterized by the profitless rise and fall of mutually hostile and exclusive schools of thought.

As an index of this pathology, I shall focus on the use of citations in social science, a use that is generally, as concerns its rationale, "taken-for-granted." For our purposes, let us initially consider citations in the broad category of "primary" and "secondary" citations. The first concerns the use of "data" or "evidence," and their purpose *appears* to be the documentation of such "evidence," i.e., the rooting of the arguments in the "facts" of social reality. I shall consider citations of this order in due course, but, for the moment, let us focus on the "secondary citations." These are citations that refer, not to the "data," "evidence," or

"facts" of the "real world," but to other writings in social science. I ask then: What is the purpose of such "secondary" citations?

An immediate, and obvious answer that comes to mind is that such secondary citations serve to integrate the work at hand into the context of related ideas in the social sciences. According to this view, the secondary citation is the essential link between on-going work and the "edifice of thought" that is constituted by the cumulative endeavors of social science. Without such a link there could be no continuity, no development, no progress in the social sciences. Let us then subject this elegant, and as it turns out, entirely specious rationale, to closer scrutiny.

We must first ask, if the purpose of such secondary citations is that which is stated above, how is it that today we encounter little or no reference to famous names in the earlier history of the disciplines. For example, in sociology who today refers to Von Wiese, Vierkandt, Spann, Mosca, Spencer, Pareto, Sorokin, Giddings, Hobhouse, Schmalenbach, Ross, Cooley, Thomas, Sumner, Tarde, etc.? If secondary citations are used to maintain the continuity of thought in the social sciences, why have these names been substantially lost from view?

A related consideration is what appears to be the pattern of an on-going temporal shift in the fashionability of citations. Thus, some years ago the names of Parsons, Mills, Mannheim, and Riesman were prominent in American sociology, and now seem to have yielded to the newer lights of Gouldner, Goffman, Blau, and Berger. All of this is "impressionistic" to be sure, but it at least suggests that secondary citations are not linked to a progressively expanding "edifice of knowledge" in the social sciences.[1]

There is also the problem of the parochialism of citations. In a recent review, for example, it was noted that of 405 references in a Russian book on "systems theory," most of the citations were to "Russian or East European writers" (Bredemier, 1979). One can imagine the flurry of "why doesn't he mentions . . .? that will greet this work from American, English, French and German readers. And yet, as is well-known, such a pattern of citations is by no means exceptional. American social scientists, poorly schooled in foreign languages, rarely cite works in other languages (unless they are translated) and European writers, albeit

to a considerably lesser degree, often return the compliment. The extent to which the nationalist parochialism of citations can be carried is illustrated by the fact that Durkheim and Weber, living and writing at the same time, did not directly cite one another in their works.[2] How then can we reconcile such a pattern of cultural and nationalistic in-breeding of secondary citations with their supposedly universalistic function of building and maintaing the progressive "edifice of knowledge" of the social sciences?

Let us try still another approach to this problem of the "manifest" theory of the use of secondary citations. Now imagine that we are interested in placing the term "ideology" in its proper context in the social sciences, and we are determined to use secondary citations for this purpose. Who then shall we cite: Mannheim, Marx, Hegel, Helvétius, Bacon, or Destutt de Tracy (who originated the term in Eléments d'Ideology)? And will any or all of these citations accomplish the purpose of integrating the concept of "ideology" into the history of the social sciences? If we are honest about the matter we shall have to concede that such an integration can only be effected by way of a study of the history of the idea of "ideology" (e.g., Lichtheim, 1967) and, in general, the satisfactory location of any idea in the context of earlier work would require a similar undertaking, e.g., Talmon (1970), Collingwood (1946), Cassirer (1971), Meinecke (1965), Lovejoy (1936). From this point of view, then, the naive explanation of the purpose of secondary citations must again be rejected.

THE "LATENT FUNCTION" OF CITATIONS

For reasons that shall become clear in the following discussion, I shall not suppose that I have here persuaded the reader that the idea of the "manifest functions" of secondary citations as relating to the "progress" of the social sciences is untenable. I hope, however, to effect this persuasion by outlining a theory of the "latent functions" of citations that will better account for the actual practices in use. My theory concerns ideas of the

"conceptual spheres" of the social sciences, and the process of scholarly communication in the social sciences.

The "conceptual sphere" denotes the complex of ideas held in common by academic disciplines, by sub-disciplinary areas of specialization, and by "schools of thought" in the disciplines. Thus, within the discipline of sociology these commonly-held ideas are demarcated by the names of Weber, Durkheim, Mead, Simmel, Mannheim, Freud, Marx, Tönnies, Parsons, Merton, and Goffman, among others, All sociologists, whatever their particular dispositions to focus on one or another of these writers, are familiar with their ideas as constituting the "common heritage" of the discipline. In addition, there are subdisciplinary specializations, e.g., political sociology, stratification, deviance, formal organizations, marriage and the family, the sociologies of knowledge, religion, art, medicine, etc., which are each demarcated by other names, whose ideas constitute the core of the "conceptual spheres" of these areas. There are also the "schools of thought:" Marxist sociology, Weberian sociology, phenomenological sociology, ethnomethodology, structural-functionalism, structuralism, etc. For each of these as well, a core set of names and ideas could be provided. The "conceptual sphere" thus approximates what is elsewhere described as the "collective consciousness," the "stock of knowledge," "basic paradigms," or "ideology," although I emphasize that it is not *exactly* equivalent to any of these. I shall allow the exact meaning of this term to "unfold" in the following discussion.

My argument is that the process of scholarly communication is oriented towards these conceptual spheres. To put it more strongly, it is *rooted in them,* and dominated by them. This process of communication hinges around three *implicit* questions that are posed by the scholar as he confronts a book, monograph, or article. These questions are: 1) why should I read this? 2) what does this mean? and 3) why should I believe what is stated here? Each of these questions is implicitly posed in relation to the conceptual sphere.

Now, as we shall see, the author implicitly addresses himself to these questions, but before we consider how this actually proceeds we must examine the *types of arguments* that are made in the social sciences, because these determine exactly how the questions of "relevance," "assimilation," and "credibility" are answered. These arguments can be classified as: scholarly arguments, applications, synthetic arguments, and new arguments.

Scholarly arguments are those that move entirely within the realm of the conceptual sphere, and are thus of the familiar species of exegeses that comment upon the ideas of authors whose work constitutes the material of that sphere, e.g., Aron's *German Sociology* (1964), and Parsons' *Structure of Social Action* (1968).

Applications are arguments that make empirical applications of theory, e.g., Smelser's *Social Change in the Industrial Revolution* (1959) is aptly subtitled "An Application of (Parsonian) Theory to the Industrial Revolution." Bellah's *Tokugawa Religion* (1957) is an application of Weberian and Parsonian ideas to Japanese society, and Parsons applies his own ideas to the "empirical case" of the modernization of the world in *The System of Modern Societies* (1971).

Synthetic Arguments are those that extend and develop the ideas of the conceptual sphere, e.g., Berger and Luckmann's development of the Schutzian paradigm (1967), Erikson's development of Freudian theory into an ego psychology (1950), Parsons' and Smelser's extension of Parsonian ideas (1956) and, of course, the numberless extensions of Marxist theory (for an overview see, e.g., Anderson, 1976 and Lichtheim, 1970). These arguments may be said to exploit the "logic of possibilities" of the conceptual sphere as it stands.

New Arguments are those that transcend existing conceptual spheres. Thus, the work of Marx, Freud, Mead, Schutz, and Parsons, when it first appeared, was of this genre, and more recently, the work of Foucault (1970, 1976) must strike Anglo-Saxon sociologists, in particular, as coming from somewhere in outer space.

Let us now consider how the "three questions" implicitly posed by the reader are implicitly answered by the writer who wishes to communicate a message to the community of social science.

THE PROBLEM OF RELEVANCE[3]

The problem of relevance is posed by the questions, "why should I read this?", and is answered by reference to the conceptual spheres that establish the ground rules of relevance. For an argument to be "relevant" to social scientists it must be rooted in the conceptual spheres that they intellectually inhabit. An argument that is outside of this conceptual sphere will be dismissed as "irrelevant." Thus, a sociologist who peruses journals of history, psychology, economics, or political science (considering that he is so eccentric as to do this in the first place) will find nothing "of interest" there unless it can be related to the conceptual spheres of sociology. The conceptual sphere, therefore, defines what can be *safely ignored.*

Now the problem of relevance is automatically solved for scholarly arguments, because these dwell entirely in established conceptual spheres and their very titles declare their relevance to those who inhabit these spheres, and their irrelevance to those who do not. Thus, an article titled: "Weber and Freud: On the Nature and Sources of Authority" (McIntosh, 1970) immediately announces its relevance or irrelevance to the sociological reader on the basis of the reader's conceptual sphere. The same is more-or-less true of applications and synthetic arguments that brandish conceptual spheres in their titles, for example: "Foundations of Parental Influence on Adolescents: An Application of Social Power Theory" (Smith, 1970); "Societal Reaction as an Explanation of Mental Illness: An Evaluation" (Gove, 1970).

We can see now how the use of secondary citations is associated with the writer's response to the problem of relevance. Specifically, what may be called "documentary citations" are here employed to *point* to the conceptual sphere. Thus, in the opening passages the writer cites the names associated with the conceptual sphere by way of rooting the argument in it, for example:

Regarding primary and secondary deviance, Lemert (1967:17) says. . ." (Gove, 1970).

Social power theorists of the past decade (Bannester, 1969; Emerson, 1962; French and Raven, 1959; Harsanyi, 1962; Nagel, 1968; Raven, 1965; Secord and Backman, 1964:273-293; Tannenbaum, 1962; Thibaut and Kelley, 1959:100-125; Wrong, 1968) have disagreed about various aspects of the conceptualization of power (Smith, 1970).

By this device the writer "displays" the conceptual sphere, saying in effect, "this is why you should be interested in what I have to say." It should be obvious that "new arguments" face special problems in this regard, which I shall refer to later.

Another aspect of this "problem of relevance" is that empirical problems are not "interesting" in themselves, but only become so when they are *related to the conceptual sphere,* thus making the argument a "synthetic empirical argument." The writer then begins the presentation by "displaying" (documenting) the conceptual sphere in relation to the empirical subject, for example:

The recurrently higher official arrest rate of negroes over whites poses a persistent issue in the study of deviance relative to ethnicity. . . the extent to which the differential accounts for the racial variance in crime rates remains problemical (Sellin;1928: 64). One point of view holds out the prospect that under comparable circumstances the white and negro crime rates would not differ substantially (Wolfgang;1964:61). . . a less sanguine view holds that the circumstances of whites and negroes are not fully comparable, that the experience of the negro in America differs not only in degree but in kind from that of lower class white ethnic minorities (Johnson 1941:94; Moses, 1947:420) (quoted from Green, 1970:476-477).

The corollary to this is that, should an empirical subject be presented without such reference to the conceptual sphere, it would be rejected as: 1) "not interesting," and 2) not "social science." The reader who doubts this is invited to search the journalistic literature to find an empirical presentation that does not make reference to the conceptual sphere.[4]

THE PROBLEM OF ASSIMILATION

The problem of assimilation is posed by the question: "What does this mean?" and, once again, is answered with reference to the conceptual spheres, because only in this way can it be made *intelligible*. This translation is necessary, because social scientists think in the language of their conceptual spheres.

In this connection the use of so-called "jargon" in the social sciences can be correctly understood. The complaint of the "layman" is that social scientists use a specialized language to obfuscate the obvious. But this is not at all the case. The jargon is in fact the *language of the conceptual sphere,* and it is used because social scientists think in this language. Therefore, if one wishes to be understood one must use the language of the audience that is being addressed. The complexity, and perhaps cumbersomeness of this specialized language, is intrinsic to the cognitive structure of the conceptual sphere, and is no more a device of deliberate mystification than is the use of any language that is opaque to those who do not speak it.

Another device for solving the problem of assimilation is the use of "translating citations." These, as the designation suggests, translate the arguments into the language of the conceptual sphere by referring to the familiar ideas of writers whose work constitutes the corpus of the sphere. Here are some examples from Berger and Luckmann (1967) who, writing within the conceptual sphere of Schutzian "phenomenological sociology," deploy such translating citations in order to assimilate their argument into other conceptual spheres in sociology:

> 54. Weber repeatedly refers to various collectivites as "carriers" (Trager) of what we have called here sub-universes of meaning, especially in his comparative sociology of religion. The analysis of this phenomenon is, or course, related to Marx's unterbau/ ueberbau scheme.

> 57. This is the phonomenon commonly called "cultural lag" in American sociology since Ogburn. We have avoided this term because of its evolutionistic and implicitly evaluative connotation.

> 60. Compare here Sartre's concept of the "practico-inert," in *Critique de la raison dialectique.*

> 68. Both Marx and Pareto were aware of the possible autonomy of what we have called legitimations ("ideology" in Marx, "derivations" in Pareto) (Berger and Luckmann, 1967:200-201).

What is particularly interesting and revealing about these "translating citations," as concerns the dynamics of the conceptual sphere is that *they are entirely superfluous.* They add nothing to the argument, which stands quite nicely without them. But these citations are essential for the resolution of the problem of assimilation: were they not there the reader would be inclined to say: "This is not social science (as *I* know it)."

It follows from what has already been said about the types of arguments in social science that the resolution of the problem of assimilation poses different problems for each of them. Scholarly arguments of course face no problem in this regard, since they move entirely in the precincts of the conceptual sphere. "Applications" must translate the empirical area to which the theory is being applied into the theoretical language. Synthetic arguments must translate what is new into the language of established conceptual spheres, as the above examples illustrate.

THE PROBLEM OF CREDIBILITY

The problem of credibility is raised by the question: "Why should I believe this?" An important consideration in dealing with this challenge is the "display of erudition." The writer must demonstrate mastery of the conceptual sphere and, if possible, much more. To this end, familiarity with the names associated with the conceptual sphere *must* be demonstrated. An adequate array of documentary and translating citations will establish confidence in the writer as a person who is thoroughly grounded in the subject matter. The failure to pass these essential tests, i.e., the omission of obvious citations will be greeted, implicitly or explicitly, with the fatal riposte: "Why doesn't he mention so-and-so?"

But the author can do more. He can overwhelm the reader with *learned citations* that serve *essentially* to "display erudition," for example:

> 8. Schutz's "phenomenological" sociology is little more than a development of Weber's methodological views and of the individualist idealism of the German *Geisteswissenschaften* tradition. Schutz's philosophical borrowings (from Husserl, Bergson, and later from pragmatism) are different from those of Weber (Heidelberg Neo-Kantianism), but they serve precisely the same function. Schutz's work combines the irrationalism and personalism of the German idealism with the conventionalist positivism of speculative empiricists such as Duhem and Lazarsfeld. Weber, of course, is the classic point of union of these two traditions" (Hindess, 1977:234n).

Impressive! And no doubt the reader will be more readily inclined to accept the arguments of a writer who brandishes with such apparent ease and self-confidence terms such as "individualist idealism," "conventionalist positivism," *"Geisteswissenchaften,"* and the like.

Canons of Evidence. But we seem here to have overlooked the major consideration of the issue of credibility—the "facts." If we are speaking of "empirical" social science, that is, virtually all social science offering "non-scholarly" arguments, then surely we must concede that the acceptance or rejection of an argument ultimately depends on the "facts" that are cited in support of it, i.e., the "primary citations" that point to the "evidence." This, I submit, is another specimen of naiveté concerning the actual workings of social science. My position is that the "facts" do not "speak for themselves," *but that the conventional canons of the conceptual sphere determine what are "facts."*

Consider, for example, that a perusal of certain major American journals in sociology (e.g., *The American Sociological Review, The American Journal of Sociology*) reveals a plethora of statistical "data" in comparison to certain "theoretically" oriented American and European journals (e.g., *Theory and Society, The British Journal of Sociology, The European Journal of Sociology*).

Does this mean that the latter dispense with "evidence" in comparison with the former?

A closer examination will indicate that this is not at all the case. "Theoretical" sociology, insofar as it is not "scholarly" is as "empirically" oriented as is so-called "empirical" sociology. What is different is the nature of the "evidence" that it points to. Thus, writings in ethnomethodology utilize what can be called "observational" or "ethnographic" data. Historical sociology relies on the "evidence" contained in historical writings (e.g., Moore, 1967; Wallerstein, 1974) although it may employ statistical data as well (e.g., Paige, 1975). Phenomenological sociology (Schutz, 1967) relies on "data" that may be called "experiental," in that it appeals to the reader's experience of the "taken-for-granted" aspects of the social world. The difference, therefore, is not in the presence or absence of an orientation to "social reality," but in the conventional definitions of each conceptual sphere as to what is "evidence" of "social reality."

These conventional canons of evidence will then be decisive for the resolution of the problem of credibility. The social scientist will implicitly or explicitly assess the credibility of a work in terms of the conceptual sphere *within which he thinks.* Thus, the experimental psychologist will rule out of court the non-quantifiable, "anecdotal" data of clinical investigation (a factor that delayed the acceptance of Piaget's work in the U.S.). The positivistic, survey research oriented sociologist will summarily dismiss the work of ethnomethodologists and phenomenological sociologists ("how do they know these things...?"), and the latter will return the compliment by characterising positivistic data as "specious quantification."[4] The economic historian will have nothing to do with the non-quantifiable works of political and diplomatic "narrative" history, and the behavioral political scientist will dispatch the writings of the political philosophers as "impressionistic."

I have now argued above that the three "implicit questions" posed by social scientists when reading a work in social science are more-or-less implicitly answered by the author with reference to the "conceptual sphere," and that the use of citations, both "primary" and "secondary" can be explained in this connection.

The question remains: in what sense is this "pathological"? Let us turn then to a consideration of how these practices constitute an "epistemological pathology" of the social sciences.

THE "EPISTEMOLOGICAL PATHOLOGY" OF THE SOCIAL SCIENCES

The key to an understanding of this phenomenon is presented in the justly celebrated work of Thomas Kuhn (1970), who points out the consequences of the pathological development of the conceptual sphere ("paradigms") in the natural sciences. Kuhn argues that the "normal" practice of the natural sciences is not the textbook formulation and testing of "hypotheses," but the working out of the implications of "paradigms" that are treated as absolute truths: In effect the "conceptual sphere" is *reified*. This is precisely the situation that I have described above in the case of the social sciences. The "conceptual sphere" is the *reality* to which social scientists are oriented. We have then a classic instance of the "fallacy of misplaced concreteness."

We have seen, for example, how the conceptual sphere defines the parameters of "relevance." What is of interest is not "social reality" as such, but the reality insofar as it is an exemplification of application of the conceptual sphere. The social scientist, who is shall we say *lodged* in the conceptual sphere, will not read three pages of a manuscript that does not *point* to a conceptual sphere of social science without expostulating in the "inner voice," " . . . this is not social science. . . ." The extent to which this is true is most clearly demonstrated in the "scholarly arguments" of social science. These arguments, which move entirely within the conceptual sphere, represent the ultimate withdrawal of social science into its own private corpus of thought. The "world out there" is lost entirely from view within these arguments that compare Durkheim with Weber, or the "influence" of Mead on Goffman, or that deliver a learned critique of the fate of Weberian *verstehende* sociology in the hands of Schutz, etc. The perennial fascination of social science with this narcissistic terrain illustrates, as perhaps nothing can, what is *really interesting*.

This reification of the conceptual sphere is also demonstrated, and most critically, with respect to "evidence." The canons of evidence of the conceptual sphere define what is "social reality" in a way that is, as we shall see further below, removed from critical scrutiny. The social scientist faces social reality in his own *conventionally defined terms*. What this means concretely is that social science cannot confront the "evidence" of social reality as this is presented in the world of ordinary experience. Thus, the survey research oriented social scientist cannot deal with the experience of marriage, work, literature, film, and theater unless and until these spheres of social reality are "translated" into the conventionally defined quantitative evidence of the positivistic "conceptual sphere." This applies as well to ethnomethodology and phenomenological sociology, which, although they consider themselves superior in this regard, cannot cope, within the framework of their own canons of evidence, with the "macro-" reality of economic life and politics.

This phenomenon of conservatism, or intellectual petrification in a conceptual sphere is not exceptional or accidental. It is the consequence of a "trained incapacity" to surmount the horizons of the conceptual sphere that is part of the *ordinary process* of professional training in the academic disciplines. Graduate training consists of a system of rewards and punishments that effectively disciplines the student in the ways of thought of a conceptual sphere. The student is required to master the language of the conceptual sphere of the graduate faculty, and proficiency and dexterity in the use of the established thought-modes are the criterion for "good grades." The dissertation is the culminating exercise in this solidification of the student's thought in the model of that of the ruling professoriate: the "great men" of the graduate faculty. The student is here required to think "creatively" within the confines of the conceptual sphere, which means to develop applications, and synthetic and scholarly arguments, but explicitly not "new arguments" that might shake the foundations of the thought-ways in which the faculty has invested its intellectual capital.

And, once the student has graduated from this array of institutional sanctions, and seeks to fulfill the imperatives of

"publish or perish," a new set of sanctions awaits in the arena of professional publication. The fledgling social scientist learns, in the process of submitting books and articles for publication, that the failure to adequately root the argument in established conceptual spheres will be greeted with the fatal critique, "why doesn't he mention so-and-so?" ("This person does not know *the literature*"). Enough experience with this form of academic wrist-slapping will eventually produce an "internalization" of the critique, such that the "finished" social scientist will be incapable of thinking in any terms, save that which is in "the literature." The product of this training is a mind firmly demarcated by the conventional parameters of thought in which it was trained. The consequence for social science as a whole is an intellectual culture of surpassing conservatism that is stolidly hostile to "new arguments."

This conservatism acts as an effective barrier to the diffusion of "new arguments," because social scientists, disciplined into a reflexive adherence to the conceptual spheres in which they were trained, are in no position to surmount the "paradigms" of those spheres. If "new arguments" are to make their way they must, in effect, by-pass the devotees of the old schools of thought, who stand in an inflexible, life-long embrace with the ideas which they received early in their careers. The mature social scientist (and I fully recognize the presumptuous harshness of this statement) has thus *lost the capacity to think in new terms!* Kuhn vividly described the consequences of this process for the diffusion of "new arguments" in the natural sciences.[5]

> Copernicanism made few converts for almost a century after Copernicus' death. Newton's work was not generally accepted, particularly on the continent, for more than half a century after the *Principia* appeared. Priestly never accepted the oxygen theory, nor Lord Kelvin the electomagnetic theory, and so on. The difficulties of conversion have often been noted by scientists themselves. Darwin, in a particularly perceptive passage at the end of his *Origin of Species*, wrote: 'Although I am fully convinced of the truth of the views given in this volume . . . I by no means expect to convince experienced naturalists whose minds

are stocked with a multitude of facts all viewed, during a long course of years, from a point of view exactly opposite to mine. . . . [B]ut I look with confidence to the future—to young and rising naturalists, who will be able to view both sides of the question with impartiality.' And Max Planck, surveying his own career in his *Scientific Autobiography,* sadly remarked that "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it (Kuhn, 1970:150-151).

From all of this we derive a vision of the structure of the social sciences at any moment in time as a field of mutually exclusive conceptual spheres, or "schools of thought." Each of these spheres consists of a private language that expresses its domain of ideas, and each is superintended by a core of adherents who dispense the perquisites of academic posts and publishing possibilities, by which they are able to attract and discipline a cohort of followers. Each such domain has its sphere of relevance established by its ruling ideas and faces a "social reality" that is also defined by those ideas, in terms of "canons of evidence." And dispite all the rhetoric of "interdisciplinary" endeavor, these schools of thought rarely encounter one another, and are, indeed, hardly disposed to do so. Thus psychologists, historians, political scientists, economists, and anthropologists proceed with their academic business, more-or-less ignorant of, and indifferent to, developments in other disciplines. Thus it is that within the disciplinary domains, e.g., sociology, the "structural-functionalists," "neo-Marxists," "phenomenologists," "ethno-methodologists," "organizational theorists," "symbolic interactionists," etc., meet, discuss, read one another's work, and dismiss the work of others as erroneous, irrelevant, or simply, *uninteresting.*[6]

How then, given this state of affairs, does social science "progress"? I cannot, within the framework of this discussion, deal with that weighty problem. But it is possible to observe here that individual schools of thought *cannot progress*, because they are locked into their conceptual spheres. Work within these conceptual arenas consists of applications and synthetic arguments

that explore the potentialities of the "paradigms" (see Kuhn, 1971, on "normal science"). And there comes a point, where, having exhausted their basic ideas, the schools of thought slide into a terminal state of senescence, in which they develop principally "scholarly arguments" that chew on the dry bones of earlier arguments (e.g., "orthodox" Marxism, classical psycholanalysis, and phenomenological sociology).

"New arguments" can only arise outside of the established conceptual spheres (e.g., ethnomethodology, "conversational analysis," and "attribution theory"), and will, in due course, also exhaust the logic of their ruling ideas and eventually be displaced by further "new arguments" that will generate new conceptual spheres. The problem in the social sciences, which again raises issues beyond our present purview, is that the new conceptual spheres do not absorb the insights of the old, but pass them by, unlike the situation in the natural sciences, where the new paradigms progressively enlarge the scope of the "facts" that they can comprehend (Kuhn, 1971).

If we return now to our starting point, we can, from this perspective, better appreciate the significance of the ubiquitous academic challenge, "why doesn't he mention so-and-so." This is not, as it may appear to the person who utters the cry, a confrontation of erudition with poor scholarship. It is rather, I suggest, an important moment in the conservative impulse of social science that disciplines the neglect of prevailing conceptual spheres, and that thereby helps to throttle deviations from established modes of thought. A social science that unwittingly deploys such devices is fated to defeat its own purposes.

NOTES

1. Those who will ask "How does he know this?", i.e., "where are the "data?", are referred to the discussion below on "canons of evidence."

2. On this point see Tiryakian (1966). I am indebted to my colleague, Prof. N. ch. Tatsis for this citation, as well as for the constant provocations concerning my bibliographic shortcomings that have stimulated the writing of this essay.

3. Some readers will perhaps ask at this point: "Why doesn't he mention...?", but, considering the argument of this essay I shall, with justification, forbear from doing so. See the concluding sections of the argument, which point out the *pathological consequences* of indulging this demand.

4. I limit this challenge to the "journalistic literature" because books can obviously be directed towards a non-academic audience that is quite innocent of any intellectual bondage to the conceptual sphere. This was the route followed by C. Wright Mills and John K. Gailbraith, when they decided that they could not effectively present their ideas within the structures imposed by the conceptual spheres of the academic world.

5. Along similar lines, Toulmin and Goodfield (1977) note what it "cost" Charles Lyell to achieve the "conversion" to Darwinian theory:

    > Speaking in old age about his own first reaction to the *Origin of Species*, he still regarded his eventual conversion to Darwin's theory, not as the fulfillment of his earlier vision, but its abandonment: 'it cost me a struggle to renounce my old creed' (1977:189).

6. Is there a reader perhaps still inclined to confront me at this point with a "why doesn't he mention so-and-so?", i.e., the "literature" on the "sociology of sociology," e.g., Gouldner, Friedrichs, Mullins, Ritzer, etc. But what would be the point of such a citation, except to serve as a "display of erudition," assuring the reader that *I have read what he has read!* It will add not one iota to my argument.

REFERENCES

Anderson, Perry
1976   Considerations on Western Marxism. London: NLB.
Aron, Raymond
1964   German Sociology. Glencoe, Ill.: Free Press.
Bellah, R.N.
1957   Tokugawa Religion. Glencoe, Ill.: Free Press.
Berger, Peter L. and Thomas Luckmann
1967   The Social Construction of Reality. Garden City, N.Y.: Doubleday.
Bredemeier, Harry C.
1979   Review of I. Blauberg, V.N. Saduvsky, E.G. Yudin, Systems Theory: Philosophical and Methodological Problems (transl. from the Russian). Contemporary Sociology 9:315-317.

Cassirer, Ernst
1971   Das Erkenntnisproblem in der Philosophie und Wissenschaft der Neveren Zeit (three vols.). Hildesheim: Georg Olms.
Collingwood, R.G.
1946   The Idea of History. New York: Oxford University Press.
Erikson, Erik
1950   Childhood and Society. New York: W.W. Norton.
Foucault, Michel
1970   The Order of Things. London: Tavistock.
1976   The Archeology of Knowledge. New York: Harper & Row.
Gove, Walter R.
1970   "Societal reaction as an explanation of mental illness: an evaluation." American Sociological Review 35:873-883.
Green, Edward
1970   "Race, social status, and criminal arrest." American Sociological Review 35:476-490.
Hindess, Barry
1977   Philosophy and Methodology in the Social Sciences. Atlantic Highlands, N.J.: Humanities Press.
Kuhn, Thomas
1970   The Structure of Scientific Revolutions. Chicago: University of Chicago Press.
Lichtheim, George
1967   The Concept of Ideology. New York: Random House.
1976   Marxism. New York: Praeger.
Lofland, Lyn H.
1979   Review of R.G. Benton, Death and Dying. Contemporary Sociology 8:276.
Lovejoy, Arthur O.
1936   The Great Chain of Being. Cambridge, Mass.: Harvard University Press.
McIntosh, Donald
1970   "Weber and Freud: on the nature and sources of authority." American Sociological Review 35:901-911.
Meinecke, Friedrich
1965   Machiavellism: The Doctrine of Raison d'Etat and its Place in Modern History. New York: Praeger.
Moore, Barrington
1967   The Social Origins of Dictatorship and Democracy. Boston: Beacon Press.
Paige, Jeffery M.
1975   Agrarian Revolution. New York: Free Press.
Parsons, Talcott
1968   The Structure of Social Action. Glencoe, Ill.: Free Press.
1971   The System of Modern Societies. Englewood Cliffs, N.J.: Prentice-Hall.

Parsons, Talcott and N.J. Smelser
1956   Economy and Society. Glencoe, Ill.: Free Press.
Schutz, Alfred
1967   The Phenomenology of the Social World. Evanston, Ill.: Northwestern University Press.
Smelser, N.J.
1959   Social Change in the Industrial Revolution. Chicago: University of Chicago Press.
Smith, Thomas E.
1970   "Foundations of parental influence on adolescents: an application of social power theory." American Sociological Review 35: 860-872.
Talmon, J.L.
1970   The Origins of Totalitarian Democracy. New York: W.W. Norton.
Tiryakian, Edward
1966   "A problem for the sociology of knowledge: the mutual unawareness of Emile Durkheim and Max Weber." Archives Européenes de Sociologie 7:330-336.
Toulmin, Stephen and June Goodfield
1977   The Discovery of Time. Chicago: University of Chicago Press.
Wallerstein, Immanuel
1974   The Modern World-System. New York: Academic Press.