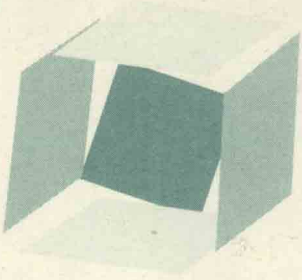
A Theory About Control



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Foreword

Edward Shils (1985:810) argued that "the original programme of sociology"—legitimacy in the academy and on the part of the educated public—"has been achieved" in large part, though the intellectual task of "discovery of the fundamental laws of social life" remains. Today, neither the legitimacy of sociology nor its fundamental intellectual task seem secure.

There is no shortage of diagnoses as to why this should be the case. Most focus on such matters as the neglect of undergraduate teaching among academic departments of sociology, the balance between "basic" and "applied" sociology, or on other internal divisions within the discipline. But something more fundamental may be involved.

Sociology is a congeries of theoretical and methodological perspectives and substantive interests. Sociologists are contentious about such differences, and they worry about too much diversity. With rare exceptions, however, worrying has not translated into serious scholarly attention to explication of key concepts or debate as to whether sociology requires a "central notion" or what that notion might be.

Jack Gibbs is one of those rare exceptions. In *Control: Sociology's Central Notion*, Gibbs (1989) argued not only that control should be the discipline's central notion but also that failure to agree on a central notion is debilitating to sociology, retarding its theoretical power and development, and contributing to fragmentation and the lack of cumulative knowledge. Others, including the editor of the most recent (1988) *Handbook of Sociology* and co-editor of a National Academy of Sciences/National Science Foundation–sponsored assessment of "Achievements and Opportunities" of all the behavioral and social sciences (Gerstein, *et al.*, 1988), seem to forecast the demise of sociology as an "identifiable field" in the future (Smelser, 1988:13).

A reviewer of the aforementioned *Handbook* expressed his unease that the volume pays scant attention to the work of several important scholars "*because their work cannot be fitted neatly into the boxes of disciplinary subfields.* Instead, these sociologists speak to our discipline as a whole" (Powell, 1989: 493; emphasis added).

The implications of this assessment are troubling, suggesting not only that sociology lacks an agreed-upon core, but that important work of relevance to

sociology *qua* sociology is less likely to influence the discipline than are more narrowly specialized inquiries that may have less relevance to the discipline as a whole. Here, again, is where Jack Gibbs weighs in.

Gibbs here returns to the central notion issue and to his candidate: control. He returns, also, to his insistence on predictive power of theories as "the issue in sociology," an argument first advanced in his presidential address before the Pacific Sociological Association (Gibbs, 1968). Here Gibbs delineates seven dimensions of predictive power: testability, range, parsimony, accuracy, scope, intensity, and discriminatory power.

The theory about control here advanced cannot be easily summarized (e.g., as Gibbs remarks, "something comparable to Marx's famous reduction of human history to class struggle"). Suffice to say, in Gibbs' words, that it identifies "several purported effects at the international level of two reciprocally related *construct* variables: (1) the extent and efficacy of control attempts...and (2) perceived control capacity." The asserted effects, "listed in the form of constructs or concepts," are the following: "the extent and efficacy" of three types of control attempts (inanimate, control of human behavior, and biotic), the amount of supernaturalism, the amount of scientific activity, concentration of biotic control, degree of division of labor, technological complexity, technological efficiency, intranational variation in death ages, the level of educational attainment, predominance of animate industries, degree of industry differentiation, degree of occupational differentiation, and amount of inanimate energy use.

After setting forth the theory in formal terms Gibbs tests 15 hypotheses derived from the theory's theorems. The result is generally positive but predictive accuracy is modest. His discussion of possible interpretations of this finding and of future work on the theory is candid and rigorous. A lengthy appendix discusses in more detail a formal mode of theory construction for sociology.

The model of theory construction developed and illustrated in this book is foreign to most of sociology and to most sociologists. Few have even begun to attempt, in their own theorizing, what Gibbs has undertaken with rigor and at great length. Indeed, the lengths to which Gibbs explains his model and its application will seem to many to demand prohibitively long exposition for that most common medium of scholarly publication, the professional journal article. And so it is. But formal theorizing need not require such lengthy presentation if its conventions are agreed upon and followed. Until that day, Gibbs' exposition is likely to be viewed as informative and illustrative, but too radical to be seriously entertained.

Skepticism concerning the validity and reliability of data, research methods, and theoretical interpretation are marks of a healthy discipline. But sociologists

seem prone to excess, perhaps too willing to entertain doubts concerning attempts at rigor and quick to embrace nihilistic thinking regarding the possibility of a science of human behavior. Whether one agrees with the position adopted in this book, and in Jack Gibbs' other expositions of his position, one must admire his attempt to present an alternative based firmly on science and positivism.

In the final analysis the importance of this book is neither the power nor the substance of the theory here advanced; it lies rather in the challenge to the discipline, the boldness of the alternative it proposes, and the rigor and candor with which that alternative is pursued.

Jack Gibbs has done a great service, not only to sociology, but to all the social and behavioral sciences, with his candor, his rigor, and his persistence. We will do ourselves a great disservice if we do not take his multiple messages seriously.

James F. Short, Jr.

Preface

My critics (alas, numerous) will be delighted to learn that this book may well be my last, but they are likely to be dismayed on recognizing that I have not changed my opinions about any fundamental matter. I persist in viewing sociology as fragmented beyond description, and the fragmentation is virtually certain to increase as more and more sociologists flock to postmodernism, postempiricism, postpositivism, post *ad nauseam*. Sociology always has been a fertile ground for any intellectual movement that belittles even the idea of objectivity; and long before liberating sociology, critical sociology, resurgent Marxist sociology, hermeneutics, deconstructionism, and all of the "posts," there was no effective consensus in the field as to appropriate criteria for assessing theories. Worse, in recent decades on the whole sociologists have become less inclined to recognize that such consensus is essential for progress in a field. Indeed, the notion of progress is alien to "post" thinking, and nothing is gained by appealing to science. Numerous sociologists eagerly embraced Kuhn, and currently they can cite various philosophers and historians as evidence that science has no distinctive unity.

I have not come to the foregoing conclusions only recently (see Gibbs, 1968). The only thing I have learned over past decades is that the *new* "received view" in the philosophy of science and antipositivism in sociology promote epistemological nihilism. Mathematics and the advanced sciences may survive it, but I seriously doubt if sociology will survive should current trends continue; and, again, my pessimism antedates the fashionable (Gibbs, 1979).

Unfortunately, the problems that plague sociology transcend the epistemological. From the outset the field has been badly fragmented, in part from an incredibly diverse subject matter, but even more so in recent decades by the proliferation of contending perspectives. So the first of two principal arguments pursued in my previous book (1989)--maximum coherence in a field requires a central notion--has special significance for sociology. By a central notion I mean a notion that those trained in a field can use to describe and think about all or virtually all of the field's subject matter. I have not changed my mind on that subject, nor about the second principal argument: control could be sociology's central notion.

My previous book did not go far beyond the two principal arguments. I had to be content with an elaborate conceptualization of control and an examination of contending sociological perspectives in connection with the argument

that control could be the field's central notion. However, describing and thinking about a field's subject matter is not an end in itself. If a central notion does not promote impressive theories, it fails for that reason alone. So this book's primary purpose is to state a theory about control (Chapters 3-7) and report a series of tests (Chapter 8).

The tests are important if only because they are consistent with still another argument that I have pursued for decades (see, especially, Gibbs, 1968, 1972). Briefly, effective consensus in assessments of particular theories cannot be realized unless predictive power is taken as the supreme criterion in those assessments. The tests reported in Chapter 8 bear on the theory's predictive accuracy, but there are six other dimensions of predictive power. Although all seven dimensions are recognized in this book, it is not presumed that the recognition of diverse dimensions will convert avowed antipositivists in sociology. They appear reluctant to go beyond the use of the term "positivism" as a pejorative label to a clarification of its meaning; even so, that point is secondary to the most fundamental reason why so many sociologists reject predictive power as the criterion for assessing theories. The rejection gives them a license to criticize theories solely on the basis of personal opinion or some ideology, though not making that basis explicit. If a theory is contrary to a sociologist's preconceptions (including ideologies) of or presumptions about human nature and social life, then so much the worse for the theory. To be sure, many sociologists write or speak as though tests of theories are desirable, but it is an old trick to recognize and emphasize test findings only when the theory is consistent with one's preconceptions or presuppositions.

Unfortunately, there are several possible reasons for rejecting predictive power as the appropriate criterion when assessing theories, and one is far from obvious. It is difficult to apply the predictive power criterion when assessing a sociological theory stated in accordance with the traditional discursive mode of theory construction, which is nothing more than the conventions of some natural language (e.g., English, German); and the outcome of an attempted application of the criterion is virtually certain to be debatable. The difficulty and the debatable outcome are inevitable because the logical structure of a discursively stated theory is so obscure that divergent interpretations cannot be avoided (for elaboration, see Gibbs, 1972). So exegetical sociology is a cottage industry, and divergent interpretations are viewed sanguinely because personal opinion in the assessment of theories has become sacrosanct.

And why do many sociologists cling to the discursive mode of theory construction? One obvious reason is their familiarity with the natural language in which they lecture and write. Because the rules of a formal mode transcend natural language conventions, adopting a formal mode is akin to learning a new language; hence, it is likely to be perceived by many sociologists as a

burden. Less obvious, no formal mode can match a natural language's flexibility, especially when it comes to rhetoric, the staple of conventional sociological theory. Still less obvious: because a discursive theory's logical structure need not be truly clear, the theory may run for hundreds of pages without taxing the audience's patience. Members of that audience are not expected to see whether and how the components are logically interrelated, and the theorist himself or herself may not know.

Despite recognition of the seeming permanence of the discursive mode of theory construction, the control theory in Chapters 3-7 is stated in accordance with a formal mode of theory construction, one set forth in a lengthy appendix. Why steer such a course of intractability? To do otherwise would be a tacit denial of previous arguments about formal theory construction.

Far from placating critics, the foregoing is likely to make them all the more hostile. So why make the arguments explicit and try to adhere to them throughout the book? Because in science certainty is an illusion and only candor can be the supreme value. Unfortunately, candor is not enough, but in this case I hope it will prompt critics to confront the arguments that gave rise to this book and articulate alternatives.

Most of the initial draft was completed while the author was a Fellow at the Center for Advanced Study in the Behavioral Sciences. My gratitude for support extends also to Vanderbilt University's College of Arts and Science and the College's Department of Sociology. I am grateful for three commentaries: Glenn Firebaugh's on the manuscript, Larry Griffin's on the Appendix, and Walter Wallace's on the Appendix; but all three are free of any liability. Jim Short deserves my thanks for his candid and insightful Foreword. The typing of Ms. Linda Norfleet (Vanderbilt's Department of Sociology) was an enormous contribution, and my wife Sylvia helped me in myriad ways.

Jack P. Gibbs

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PART ONE

Preliminary Considerations

If a scientific field's progress must be judged by only one criterion, the most conspicuous possibility is an increase in the number, range, and/or scope of *accepted* theories. However, when a scientific field is plagued with issues and problems, that condition precludes progress. It is not just a matter of the issues and problems being such that they cannot be overcome by any theory; worse, they may be such as to eliminate the possibility of effective consensus in assessments of theories and thereby preclude progress.

Unfortunately, two particular problems and a particular issue in sociology make it extremely unlikely that any theory will be accepted by even a majority of sociologists. So this part of the book, comprising two chapters, largely pertains to those problems and that issue.

The first problem is the extreme diversity of sociology's subject matter (see Boudon, 1980:3, and Gibbs, 1989:3). That diversity makes it inevitable that each theory is limited to only a minute fraction of sociology's subject matter; consequently, numerous sociologists are not likely to be impressed by any theory, viewing it (rightly) as alien to their specialty or interests. Such judgments are not constructive, but all a theorist can do is make the limits of his/her theory explicit rather than follow the tradition of "grand" theory and create the impression of an all-inclusive theory. That disclaimer is set forth early in Chapter 1, and it is all the more needed because of the argument that control could be sociology's central notion, meaning that sociologists can describe and think about all or virtually all of sociology's subject matter in terms of control. Although the theory set forth in Chapters 3–7 pertains to control, it does not follow from the central notion argument, not even if valid, that the theory is all-inclusive. However, if the argument is valid, the theory can be expanded much more readily than can a theory not focused on the field's central notion, control or otherwise.

The second disclaimer in Chapter 1 relates to an issue rather than a problem. Sociologists have never realized effective consensus as to appropriate criteria for assessing the merits of a sociological theory, and no theory can resolve that issue. All that the theorist can do is identify what he/she takes to be

appropriate criteria, and in Chapter 1 one particular criterion is described at length. That criterion is predictive power, and a large part of the chapter is devoted to a description of predictive power's seven dimensions. Such treatment is justified because many sociologists ostensibly have a narrow conception of that criterion, commonly equating it with predictive accuracy, which is actually only one of predictive power's seven dimensions.

The suggestion is not that numerous sociologists reject predictive power as the appropriate criterion for assessing a theory merely because of confusion. Unfortunately, there is a plethora of reservations about the criterion, some of which are truly understandable. Nevertheless, sociology always has been fertile ground for epistemological nihilism; hence, sooner or later many sociologists will be attracted to "postmodernism" (a term used here as a generic label for all "posts"—postempiricism, postpositivism, etc.—and hermeneutics and deconstructionism). But if there is anything that sociology does not need, it is more epistemological nihilism; and its escalation can be checked only by promoting some particular criterion for assessing theories. So reduction of epistemological nihilism is the principal justification for the concern with predictive power, even though that concern diverts attention from the theory and may alienate some readers.

The second and final problem stems from recognition that a simple identification of a candidate for sociology's central notion will not do. Whether control or something else, an elaborate conceptualization is virtually certain to be needed. In that connection, contemporary sociology suffers from far too little conceptual work, perhaps in part because Weber and Parsons tarnished that line of work by confusing conceptualizations with substantive theories. Be that as it may, Chapter 2 offers an elaborate conceptualization of attempted control, including a fairly extensive typology. But the conceptualization is not offered in the naive belief that even the majority of sociologists will see the need for it; to the contrary, some sociologists appear indifferent if not hostile to conceptualizations, even to the point of believing that constructive research and impressive theories are possible without confronting conceptual issues and problems.

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Claims and Disclaimers

As argued at length elsewhere (Gibbs, 1989), control (more precisely "attempted control") could be the central notion for the social and behavioral sciences, meaning that professionals trained in one of those fields can describe and think about all or virtually all of the field's subject matter in terms of control. If the argument comes to be accepted, it will further the integration of the social and behavioral sciences, an advance if one grants that those disciplinary boundaries are either extremely vague or arbitrary (unconstructive in either case). However, even if it could be demonstrated that the subject matter of the social and behavioral sciences can be thought of in terms of control, that demonstration would not warrant unqualified acceptance of control as the central notion.

Whatever the field or the central notion candidate—control or something else—acceptance of it requires considerable evidence that describing and thinking of phenomena in terms of the notion furthers the field's coherence. That point is consistent with a corollary argument—no field can realize maximum coherence without a central notion. Yet a central notion must do more than further conceptual integration; it must also promote the formulation of impressive theories.

The extent to which a central notion candidate meets the requirement cannot be known without using it extensively, all the more because there are at least two major ways that the use of a central notion can promote the formulation of theories. To the extent that the notion enters (directly or indirectly) into definitions of a field's terms, it promotes conceptual integration. Granted that scientists discover order, they also *create it* through conceptual innovations; and conceptual integration facilitates the formulation of theories. Indeed, what could be taken as the ultimate goal—a synthesis of all theories in a field—virtually requires conceptual integration. The second way by which a central notion facilitates the formulation of theories cannot be described without making still another disputable claim. Describing or thinking of phenomena in terms of a particular notion prompts recognition of *possible* empirical