behavioral and post-behavioral methodologies in communist studies*

Slightly over a decade ago, Frederic Fleron edited a volume of critical essays and research breaking with the established tradition in communist studies. Two years later, another book on the behavioral revolution in communist studies appeared in print, edited by Roger Kanet. These two publications signified a turning point when scattered dissent grew into an open revolt against traditional modes of inquiry practiced by students of communism.

Ten years are perhaps too short a time span to judge the fruits of the behavioral revolution. As Triska and Cocks have recently observed, "The behavioral revolution in Communist studies is still an unfinished revolution." It seems justified, however, to pause and reflect on what has already been done without waiting for the behavioralist promise to be kept in full. I cannot undertake here a comprehensive review of numerous works based on behavioralist methodology. What I attempt to do is to isolate some critical problems raised by behavioralist research and to contrast behavioralist and nonbehavioralist methodologies in communist studies. The major concern of this article is methodological. Substantive issues are touched insofar as they help to shed light on procedural questions. I begin with a short review of the current split between communist area studies and behavioralism. Then I discuss the results of some behavioralist research and examine records of predictions in traditional and behavioralist research on communist society. Next, I contrast theoretical assumptions underlying behavioralist and interpretative methodologies in social research, and finally, I discuss the prospects for hermeneutical study of communist societies.

The following analysis draws primarily on the contributions made by political scientists and sociologists scholars most receptive to the behavioralist appeal. It is hoped that this discussion may be useful to behavioralist and interpretative researchers working in other branches of Western scholarship on communism.

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Communist Area Studies and the Behavioral Revolution

Communist area studies emerged as a field of graduate training and scientific inquiry after World War II. Among the factors contributing to its growth were the expanding diplomatic and economic relations between the USA and the USSR. The need for expert knowledge of Soviet society, the improved prospects for employment, along with the expanding governmental funding have improved the image of the field and secured its steady growth until the mid-sixties. Nevertheless, the position of communist studies in academia was somewhat precarious.

The new programs established in the leading universities could not compete in academic status with social sciences more conducive to the behavioral revolution. The field was dominated by specialists with a general background in the social sciences, whose methods were qualitative rather than quantitative, and who did not always keep track of the methodological and theoretical advances in social science as a whole. According to a survey conducted in 1953, early specialists in Soviet and East European studies were recruited primarily from among historians, economists, and political scientists.

The situation began to change in the second half of the sixties. With the market for communist area specialists nearing the point of saturation and the prospects for funding less certain, voices began to be heard calling for a methodological and theoretical reorientation of the field. The new approach to communism was greatly facilitated by the liberalization process in East European countries. The proponents of the behavioralist and comparative approach in communist studies insisted that the students of communism must enrich their conceptual schemes by borrowing theoretical models from modern social science; that students of communism must incorporate quantitative methods accepted in the behavioral sciences; and that area specialists must abandon the traditional vision of communist society as phenomenon sui generis and analyze it in comparative perspective. The proposed changes were consistent with the trend in political science initiated by the pioneering works of Almond, Powell, and Easton.

The first appeal to embark upon the new course was made in 1960 by Skillling. 4 But the new mood began to take the form of a trend in the late

sixties, when Tucker proclaimed comparative studies of communism "the most hopeful direction for scholarship in Communist studies."5 A symposium on comparative politics and communist systems was organized by the editors of Slavic Review in 1967. The participants emphasized the growing polycentrism in the communist world, stressed the need to break with the ethnocentrism of the earlier studies, and advocated new methods for studying communism.6

The movement was given impetus with the publication in 1969 of a collection of essays edited by Fleer. The editor took mainstream Sovietologists to task for their "carelessness in formation and use of concepts, preoccupations with clever techniques, inappropriate use of models."7 He made a strong plea for raising methodological awareness among students of Communism. His arguments were further expounded by papers presented in Kanet's volume on the behavioral revolution in communist studies.8 At about the same time, Starlet advocated a shift from "metaphoric" to "scientific concept formation" and from "tendency" to "probability statements" on communist behavior.9 Students of communism with a behavioralist background strove to live up to the standards accepted in more developed behavioral sciences. Their works often appeared in political science and sociological journals. Thus all previously published articles reprinted in Kanet's volume had earlier appeared in general social science journals, and not in specialized periodicals devoted to communist studies.

Proponents of behavioralist and comparative research leveled a strong attack on the totalitarian model of communist society. This theory was criticized for its speculative nature and for an unduly rigid borderline drawn between communist and Western politics. In the words of Korbonski, "the demise of the totalitarian model, accompanied by a growing availability of political, economic and social data, made the traditional distinction between 'communist' and 'noncommunist' systems obsolete and redundant."10


The split between the traditional and modern modes of inquiry in communist studies very much seemed the transition from an old to a new "scientific paradigm," to use the popular Kuhnian expression, with the younger generation leaning toward new methods and the older generation of Sovietologists finding themselves on the defensive. In Gati's words, "The methodologically old-fashioned—however well-grounded he may be substantially—is tolerated only grudgingly these days, and his place is assured only if he has tenure or is otherwise a 'nice guy!'"11 Similar complaints could be heard from other scholars. Understandably, many practitioners in the field reacted less then enthusiastically to the call to forgo old ways and to embrace behavioralist methods. As early as 1967, at the time of the first extensive discussion in the pages of the Slavic Review, Jacobs warned against uncritical borrowing of quantitative methods because "too often they serve as an excuse for doing the self-evident."12 Answering some of the criticisms of the traditional approach, Hollander pointed out that the charge of ethnocentrism should rather be addressed to comparativists who analyze communist countries in terms of theories modelled after Western societies.13 In 1970, Tokes ventured that the introduction of new techniques and rigorous model-building has "thus far produced neither the kind of new evidence, nor the original insight the proponents have claimed."14 A few years later, Tucker, one of the early supporters of comparative communism, admitted that, although the results achieved so far have by no means been negligible and are not necessarily discouraging...we have found, as would-be pioneers have so often found in the past, the path ahead to be longer and harder than we thought when we started." A similar sentiment was expressed by Hough.15

The opponents of behaviorism became more vocal in the mid-seventies. Half of the papers contributed to a symposium on comparative communism and international relations expressed reservations about the high priority given to comparative studies and questioned the virtues of behaviorism. The symposium organizer, Charles Gati, took pains to emphasize that "without

oft-criticized "idiographic" studies there would be no comparison, model-
building, or theorizing.\textsuperscript{16} A plea was made by Rubinstein "to demystify comparative study, theory-building, statistical computations and the like."\textsuperscript{17} Another author expressed doubt as to the high priority assigned to the comparative approach in view of the limited sources of hard data on communist societies and stressed the need for further accumulation of knowledge about individual countries.\textsuperscript{18}

Few critics of behavioralism would go so far as to advocate a methodological counterrevolution. Most would settle for peaceful coexistence. Denouncing "the behavioralist-area studies confrontation,"\textsuperscript{19} the critics of behavioralism could hardly offer anything approaching the methodological sophistication of their opponents. Part of the reason why students of communism favoring non-behavioralist, idiographic inquiry find themselves on the defensive is that they lag behind their behavioralist counterparts in questions of methodology. Indeed, behavioralists' primary contribution so far seems to have been that they have raised methodological self-consciousness among students of communism. The adherents of behavioralism, however, have been highly selective in their borrowing from modern social science theory and methodology. They virtually ignored the methodological and theoretical advances made by non-behavioralist researchers who continue the tradition established by Max Weber and Georg Simmel. The post-behavioralist orientation in the social sciences attempts to codify this interpretative or, as it is now often called, "hermeneutical" tradition in social research. Whether or not students of communism find this approach congenial, they are likely to benefit from the methodological arguments advanced by the opponents of behavioralism. Before I turn to these arguments, let me examine some research projects conducted by behaviorally oriented students of communism.

Operationalization and Validity in Behavioralist Research on Communism

Despite numerous attempts to define behavioralism, one cannot draw a sharp line dividing behavioralist and non-behavioralist research. If we take Kanet's definition of "the behavioral revolution, by which is meant a concentration on both the observable political actions of individuals and groups and the psychological processes which influence these actions—e.g., perception, motives, and attitudes,"\textsuperscript{20} we have to admit that many traditional works fall under the heading "behavioralist." Brzezinski, Hazard, and Moore are as much preoccupied with observable actions as Lodge, Fleron, or Hopmann. A painstaking empirical analysis is a mark of the work done by all these authors. To distinguish their studies, one has to consider the ways theoretical generalizations are derived from and justified by empirical data. The adjectives "empirical" and "speculative" can hardly differentiate the two approaches.

To avoid the confusion, one can stipulate that the behavioral researcher deals with quantifiable variables and avoids those statements which cannot be operationalized and supported by mathematical calculations. This restrictive definition runs into trouble when matched with reality. Only four of twenty-four articles assembled in Fleron's volume and six of thirteen papers collected by Kanet appear to meet these criteria. Moreover, even those authors who employ quantitative techniques routinely resort to ad hoc interpretations and qualitative evidence to explain anomalous data and inconclusive calculations.

A less restrictive approach equates behavioralism with a general trend to experiment with models and methods currently used in the social sciences. This definition, favored by Fleron and Sharlet, is unacceptable because it equates modern social science with behavioralism and thereby discards non-behaviorist—interpretative—schools of social research, such as symbolic interactionism, ethnomethodology, phenomenological sociology, and dramaturgical analysis.

A comprehensive definition is offered by D. R. Little. He renders behavioralism as a "commitment to the development of empirical theories with explanatory and predictive value, quantification of empirical data, value-free sciences."\textsuperscript{21} Exhaustive as it is, this definition contains premises that may be questioned by behavioralists and supported by some non-behavioralists.

I shall not attempt to give my own definition of behavioralism. Instead, I will discuss several works employing behavioralist methods. I start with the works reprinted in Kanet's book \textit{The Behavioral Revolution and Communist Studies} and then examine some more recent publications consistent with the behavioralist trend.

The first work to be dealt with is Milton Lodge's oft-quoted research on Soviet elites. Its central thesis is that Soviet political life is characterized by the increasing participation of various "specialist elites" in the decision-making and policy-making process. To prove this thesis, the author undertakes content-analysis of verbal statements appearing in Soviet periodicals.

\textsuperscript{17} A. Z. Rubinstein, "Comparison or Confusion?" \textit{Studies in Comparative Communism}, 8 No. 1-2 (Spring/Summer 1975): 45.
\textsuperscript{18} A. L. Hecok, "Does the Comparative Approach Merit High Priority?" \textit{ibid.}, pp. 36-41.
\textsuperscript{19} R. L. Tokes, "Comparative Communism: The Elusive Target," \textit{ibid.}, 8, No. 3 (Autumn 1975), 214.
\textsuperscript{20} Kanet, ed., p. 2.
\textsuperscript{21} D. R. Little, "Communist Studies in a Comparative Framework: Some Unauthor-
The selected journals and newspapers serve as a medium for articulating the interests of four specialized elites (military, economic, legal and literary) and one non-specialized elite (Party apparatus.)

The definition of the main theoretical variables is the first difficulty that the reader encounters in this paper. We learn that the level of elite participation in policy articulation may be high or low, that some political systems are "ideological" ("all elites are participating within the ideological parameters, ... of the political arena") and some are "instrumental" ("instrumentality [is] operationally defined as specialist elite participation in the policy process"). These circular statements do not clarify what the author has in mind when he refers to political participation. The latter is never explicitly defined in the text. The question is not a minor one if one takes into account the diverse nature of the elites under study. It is not self-evident that the author deals with the same phenomenon when he compares the military and literary elites' participation in the decision-making process. To clarify the latter, Lodge should have started with a substantive theory of the examined phenomenon. The lack of such a theory tends to blur the central concept. It also casts a shadow on the appropriateness of the theoretico-deductive apparatus used in this study.

Statements like "Hypothesis I: Over time the specialist elites increasingly describe themselves as participants in the policy making process" or "Hypothesis II: The specialist elites are pressing the party for greater influence in the policy making arena" are empirical generalizations rather than hypotheses. The fact that they have been guessed in advance does not change the matter, if we understand by hypotheses conditional statements which are derived from a substantive theory and which, being corroborated, lend partial support to a given theory. The author does not test any theoretical construct. Without it, he is hardly justified in employing an elaborate theoretico-deductive analytical framework.

The operational procedures and empirical indicators employed by Lodge raise additional questions. Behavioralist methodology stipulates that operational procedures must permit unambiguous identification of an item as an instance (or non-instance) of a given concept. The examples of operational definitions and indicators chosen by Lodge point to the common difficulties that behaviorists experience in following this principle.

Several commentators have indicated that Lodge made the unwarranted assumption that Soviet journals function as media for articulating elite interests and attitudes. The fact that certain opinions appear in print is not proof that these opinions are representative of elite attitudes as a whole. Such an operationalization of elite attitudes is particularly questionable for a country with a state-controlled press.

Similarly suspect is the operational distinction between policy-making and decision-making levels of elite participation. The former is defined as a process that involves decisions affecting the whole country, and the latter as a process of decision-making affecting any one administrative unit below the Republic level. As examples of empirical indicators delineating this distinction the author chose Khrushchev's statements at a writers' meeting and at a meeting of agronomists. In both cases, Khrushchev insists that he lacks professional competence and exhorts experts to take responsibility for their professional advice. Khrushchev's appeal to agricultural workers is classified by Lodge as a policy-making statement and his address to Soviet writers is seen as a decision-making statement. The rationale for this differential class-attribute is not clear.

Also blurred is the distinction between elite values and elite beliefs. The former indicates the desirable level of elite participation, and the latter—the actual responsibility for policy-making as perceived by elite members. To distinguish between the two, coders were instructed to sort out "is" statements and "should" statements indicating the actual and desirable state of affairs. But if one examines a model statement offered by the author as an example of "is" (belief) statement, one is likely to be puzzled by the following sentence: "Indeed, if the chief means to be used in war is nuclear missiles, this means that we are obligated to construct both the theory of the art of warfare, the operational tactical training of troops and their indoctrinating with regard for the use, above all, of these weapons. This means that each officer, master sergeant, soldier and sailor must learn to act, to carry out his duties and battle orders, as required by the conditions of nuclear-missle war." The expressions "we are obligated to act" and "sailors must learn to act" could be classified as indicators of the desirable state of affairs. The author interprets this as a typical statement expressing elite beliefs about the actual participation by the elite as perceived by its members.

It should be stressed that examples of empirical indicators provided in the text are intended to be the most "typical" and "pure" instances of the phen-
omenon in question. The uncertainty in the model sentences indicates that it must be a fortiiori present in the bulk of the processed material. The fact that a second coder, who independently re-coded 25 percent of the original material, made only 11 percent mistakes, does not eliminate the problem. As Garfinkel's suggestions, coders' consensus is achieved not so much through strict adherence to the instructions as through common tacit assumptions and persistent violation of the coding rules in ambiguous situations. It is through this process that the grammatical structure of sentences, particularly those in Soviet editorials, is a good indicators of elite values as distinct from elite beliefs. Given the tenuous evidence bearing on this question, one has to doubt all major conclusions reached by the author regarding the "party-elite conflict".

The methodologically most vulnerable aspect of Lodge's study is the validity of his findings. The data gathered by the author apparently refer to the "perceived" participation and not to the actual one. Lodge is aware of this limitation. Nevertheless, when he formulates his hypotheses and states his conclusions, he disregards it. His conclusions that "the Soviet system is competitive" and that "conflict characterizes Party-specialist elite relations in the post-Stalin period" constitute a jump from dispositional language to the language of behavior. There are no direct links between attitudinal and behavioral manifestations. The transition from one level of analysis to the other requires interpretative work which the author does not undertake. Without such work, his conclusions about "Party-elite conflict," "competitiveness," and "increased elite participation" in the USSR have no validity.

The question of validity is a stumbling block of behavioral research not only in communist studies. Deutscher argues that behavioralists devote too much attention to operationalization and reliability and tend to sidestep the issue of validity. "The idea of operational definition is," in his words, "a device precisely designed to eliminate the problem of validity." This observation is supported by the fact that only one paper published in Kalet's book discusses the issue of validity. Even this theoretically well-grounded and methodologically sophisticated study fails to resolve it satisfactorily. I have in mind the research on cohesion in the communist system conducted by Hopmann. The author operationally defined cohesiveness among communist nations as a consensus in their first verbal reaction to certain international events, such as the American bombing mission over North Vietnam and the opening of the Geneva Summit Conference in 1963. If we take the author's calculations at face-value, we have to conclude that the degree of cohesiveness among communist nations in 1950 and in 1965 was practically identical, as indicated by the scores of 0.50 and 0.40 respectively. Much substantive evidence militates against such a conclusion. The Stalin-dominated communist block of 1950 was far more cohesive than the communist alliance in the mid-sixties, when several communist countries followed China in their bid for greater independence from the Soviet Union. Hopmann is very much aware of this inconsistency, though he is reluctant to admit that it invalidates his operational definition of cohesiveness in terms of an attitudinal response to one particular event. His recourse to substantive evidence aimed at explaining anomalous results is an example of ad hoc reasoning, which is, in principle, eschewed by behavioral methodology.

Similar problems beset studies carried out by Fleron, Gehlen-McBride, and Schwartz-Keech. The first two projects are devoted to Soviet elite participation and the process of cooptation of professionals to the party Central Committee. The authors document the steady growth in the number of Central Committee members with training and working experience in various professional spheres. This growth in the number of coopted CC members is interpreted as evidence that "...in the Soviet Union points of access exist in the political system for the articulation of interests through elite representation and providing expert judgments to the decision makers." This conclusion is predicated on the assumption that coopted elite members (those who spent seven years or more in a non-political sphere prior to entering a party career) differ substantially from recruited CC members (those entering party careers prior to spending seven years in non-party careers). The former represent professional elite interests; the latter, the interests of the party apparatus.

This operational definition is vulnerable to criticism. For one thing, some coopted CC members take responsibility for sectors unrelated to their professional competence. On the other hand, one cannot assume a priori that the coopted CC member remains responsive to the group of his primary affiliation, even if he continues to function as an expert in his area. It would be preposterous to infer that, say, Sholokhov and Chakowski represent the interests of Soviet writers just because both of them are professional writers and members of the CPSU Central Committee. What is a reference group for a given member of the Central Committee cannot be determined off-hand on the basis of his previous affiliations.

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23. At one point, Lodge cites the Brezinski-Huntington view that the Soviet Communist Party may be forced in the future to tolerate a greater elite participation. But he does not refer to their ideas anywhere else in the text, after mentioning them in the beginning of the paper.
26. Ibid., p. 89.
Schwartz and Keech make an interesting attempt to reconstruct Soviet policy debates on the basis of statements made by various groups in which they expressed their attitude to the 1958 School Reform. This research illuminates how behavioralists go about subsuming an observable particular under the nomothetically generated category. The authors’ objective is to generalize about a class of individuals on the basis of opinions expressed by particular individuals and on the basis of other indirect evidence. Thus the statement issued by the Soviet Minister of Education is treated as an indicator of Soviet sentiments. It is supported by the fact that some parents were criticized for their wish to secure the best possible educational opportunities for their children. The authors interpret these observable occurrences as a sign that various social groups in the Soviet Union had opposed the educational reforms initiated by the party in 1958. And since the final draft of the reform differed from the original one proposed by Khru- shchev, Schwartz and Keech conclude that social groups in the Soviet Union are capable of exerting pressure on policy-makers.

This causal attribution of the changes in the final draft of the new educational law to group pressure is not sufficiently validated. Soviet factory managers are periodically reprimanded for lack of attention to young workers. To infer from this that managers, as a group, had opposed the reform is preposterous. The authors’ comment that the inflow of juvenile workers following the reform would “divert managers from their own duties of production and plan fulfillment” is an ad hoc interpretation. It may or may not be true (one could also argue that the abundance of cheap labor would outweigh any other consideration in the eyes of managers), but it suggests that behavioralists research is not free of speculations and guesswork, as behavioralist statements may imply.

33. The problem with many generalizations about Soviet elites and interest groups is that they are modelled after Western political experience. Behavioralist studies of Soviet elites rely on the input-output model of political process that underestimates multiplegroup affiliations and overestimates the stability and cohesion of interest groups. It leaves relatively unattended a parallel process in which politicians inform social groups—a flaw especially noticeable in the case of communist societies, where professional elite members are also members of the Party elite with a huge stake in the perpetuation of Party dominance. Soviet writers and trade unionists who reported to the Central Committee are as much devoted to representing Party interests in writers’ and workers’ organizations as to articulating and defending the interests of their constituencies in the highest decision-making bodies. This point is nicely illustrated by a remark of the Program Director of Soviet TV: “We are, many of us, Party members. We try to fulfill the tasks given to us by the Party and the government. But if you mean, do we have to run, to the Central Committee and ask, ‘May we put this on, or that?’ the answer is: Of course, not. No one could work that way.” A. Austin, “Soviet TV—Better Packaging for the Party Line,” The New York Times, Sunday, 21 Oct. 1979.
35. Ibid., p. 1283.
36. Ibid., p. 1274.
sumer production.” In the second period, the rational-technical variables did not significantly influence political mobility, while the patronage variables showed the negative effect on elite mobility. These findings are linked to the creation of the regional economic councils and Khrushchev’s efforts “to bring into the top leadership those secretaries from the Russian republic who had established few career contacts with either their fellow secretaries or with Politburo or with Secretariat officials.” The anomalous results for 1964 are tied to the perturbations caused by the elimination of Khrushchev’s bifurcation of the Communist Party. And finally, the marginal effect of both patronage and rational-technical variables in the early Brezhnev-Kosygin era are interpreted as a result of “a delicate balance of contending groups” in this period and “the need to distribute rewards, such as promotions, in some relationship to the strength of groups within the ruling coalition.”

Had the authors elucidated in advance the policy imperatives of each regime, their subsequent interpretations would be impressive indeed, even though the questions regarding their empirical indicators would still persist. But the sequence of the steps in the project is reversed: after the findings become known, the authors proceed to elucidate the policy decisions that best account for the observed mobility patterns in a given period. The salience of the patronage variables during Khrushchev’s rise is linked to his determination to pack the Party bodies with his own people, but the negative effect of the patronage variables during Khrushchev’s ascendency is explained by his efforts to bring into power positions those unburdened by old allegiances. Should we infer therefrom that Khrushchev lost interest in packing the Party bodies with loyalists between 1957 and 1959? The alleged impact of agricultural success and consumer goods performance on political mobility between 1955 and 1957 is predicated on the priority assigned to these areas of the Soviet economy during this period; but when the same factors failed to make a difference in the succeeding period, the authors resort to the Party’s reorganization as an explanation. Does this mean that agriculture and food-clothing ceased to be the areas of priority for the Party? The delicate balance of power is cited as a reason for the lack of correlation between the dependent and independent variables in the Brezhnev-Kosygin era, but similar results in the 1957-59 period are attributed to administrative-economic reconstruction. Again, how is this shift in the logic of explanation justified?

I am not saying that the proposed explanations are wrong. Nor do I imply that post hoc interpretations are unacceptable in principle. The point is that the authors fail to live up to their self-imposed ban on such reasoning. Their inferential procedures allow them to emphasize those aspects of the regime’s policy that are consistent with the findings, while disregarding concurrent policies incongruent with the results. Any set of statistical data can be rationalized in this manner, one way or another. Should the rational-technical variables come into prominence in the Brezhnev-Kosygin era, the authors could reasonably quote the regime’s philippics against Khrushchev’s voluntarism and the then common exhortation to raise the productivity and quality of economic performance. The preeminence of the patronage variables in the same period could be taken to indicate the rulers’ concern with the consolidation of power. The positive correlation of industrial performance and political mobility during Khrushchev’s ascendancy might have been a testimony to the party secretaries’ enhanced responsibility for the industrial sector. The absence of such an impact could as well be blamed on the political conflict resulting from the reorganization of the party. The same procedure enables the authors to gloss over the data inconsistent with the proposed explanation. The trends in Khrushchev’s time of trouble are briefly referred to in the text as being similar to the trends established during Khrushchev’s rise. This conclusion seems to be at odds with the relationship between political mobility and career group links in the 1955-57 period (beta .45) and in 1961-63 (beta -.16). As speculations and hypotheses for future investigations, the authors’ interpretations would be welcome. To feature them as empirically grounded conclusions is hardly appropriate, given the authors’ stated commitment to “rigorous, systematic testing of models.”

The decision to resort to quantitative data and the failure to judge them against the available qualitative evidence have undermined the results of many interesting projects conducted by students of communism during the past decade. Brian Silver’s study of the Russification of Soviet nationalities is a telling example. The author is one of the most sophisticated behavioral researchers now working in the field. His intricate research design, however, is compromised by a lack of interpretative insight. Silver defines national or ethnic identity as “an individual’s emotional attachment to certain core symbols of his ethnic group.” He derives the data on nationality from the 1959 USSR Census, taking an individual’s answers to the questions about his nationality and native tongue as the indicators of his ethnic consciousness. The measure of Russification proposed by the author is the observed discrepancy between an individual’s adherence to Russian as native language and his identification with a non-Russian nationality. Silver formulates a number of hypotheses concerning the extent of Russification as dependent on the level of contacts with Russians and on the intensity of ethnic ideologies.

Silver has his own doubts regarding the information on national self-identification provided by the Soviet Census. He acknowledges that the available

37. Ibid., pp. 1277-79.
38. Ibid., p. 1285.
data leave uncertain the intensity of the individual's ethnic feelings and his actual adherence to an ethnic way of life. Nevertheless, he insists on using Soviet census data, arguing that "despite certain limitations in presently available measures of ethnic identity in the USSR, these measures do have an important virtue: they have been used in three Soviet censuses. . . ." 40 But this reasoning is faulty. What may be a virtue from the standpoint of a Soviet census taker is a vice in view of Silver's emphasis on the subjective dimension of national identity and his stated commitment to dealing with "an individual's emotional attachment" to ethnic symbols. The Soviet census taker is not likely to ask the respondent to produce his internal passport as a proof of his nationality. But the written instructions issued to Soviet census takers by the USSR Central Statistical Agency stipulate in no uncertain terms that the question "Vasha natsional'nost'?" refers to the respondent's passport nationality, and not to his "emotional attachment." That is the sense in which this question is understood by an adult person in the USSR, at least when raised during the census interview. In what way the individual's passport nationality is related to his ethnic consciousness is an extremely complex issue, particularly in a multi-national state like the USSR. To assume without "taking the role of the native" that the former is an indicator of the latter is to cast a shadow on all the substantive findings reported in the paper.

Jerry Hough's study of the decentralization process in the Soviet Union is one more example of the pitfalls awaiting those who give precedence to quantitative over qualitative data. His research is based on the explicit assumption that any discrepancy between the centrally established policy and its implementation in various regions of the country indicates the degree of centralization in the administrative system. His study, Hough explains, "will focus on a single indicator--the number of hospital beds per 10,000 population--but the indicator chosen is a crucial one. . . . In a totally centralized system with very rigid norms, each oblast presumably would have almost precisely the number of beds appropriate for its percentage of urban population. . . . In a more decentralized system in which the horizontal supervision is stronger, there presumably would be much more variation from the norm. . . ." 41 The calculations performed by the author confirmed his expectation that "there is variation in per capita hospital beds from region to region, even controlling for the proportion of urban population. . . . The regional variation is so great and so 'random,'" Hough stressed, "that it absolutely must be produced by some local factors either in the present or the past. There is no other explanation that is reasonable." 42 Granted this, on what grounds does the author conclude that this "random variation" is due to a local policy initiative and is a testimony to the growing decentralization in the Soviet administrative system? The variation may be due to the differential rates of disease requiring hospitalization, to the lack of economic resources, or to sheer negligence on the part of the local leader. To substantiate his conclusion, Hough needed qualitative evidence combined with interpretative insight into the process of decision making in the Soviet Union. The fact that the growth in the number of beds in a region is somewhat more likely to follow a discussion of health problems in a regional Soviet is no substitute for such an insight. Besides the statistically tenuous nature of the relationship reported by Hough, that fact says nothing of the substantive arguments raised during the session of the Soviet (which explains why the author downplays the importance of this piece of evidence in the afternote).

I wish to draw attention to the manner in which behavioralists assign meaning to individual behavior. They definitely prefer the standpoint of the observer to that of the actor. The subject's interpretations of his own actions are treated (if at all) separately from the objective significance that his actions have in the eyes of the researcher. Brian Silver shows little concern with the meaning of being a non-Russian in Soviet society. Milton Lodge does not inquire into the conflicting allegiances of a Soviet professional coopted into the Central Committee. Jerry Hough does not pause to ask what it means to be a participant in the Soviet political process. What strikes Hough as important is that Soviet citizens form voluntary associations and write letters to the editors, much as their Western counterparts do: "Letters to the editor, the publication of articles in journals, personal appeals to governmental or political officials, membership in many types of committees . . . so far as can be observed by an outsider, these activities seem little different on the surface from their counterparts in the West." 43 The key words here are "as can be observed by an outsider." As an outsider, Hough may well perceive "the Soviet system as a very participatory one." 44 As a participant observer, he could well have concluded that "being a participant" means a different thing in the Soviet Union than in the United States.

How much do we learn from the conclusion that the Soviet and American societies are participatory ones, that the political process in the USA and the USSR is influenced by interest groups, that Soviet and American trade unionists have an input into policy-making in their respective countries? These conclusions may obscure as much as they reveal. What they seem to reveal clearly enough is that students of comparative communism should.

40. Ibid., p. 50.
42. Ibid., pp. 165, 169.
43. Ibid., p. 120.
44. Ibid., p. viii.
reexamine the distinction between phenomenal identity and conceptual equivalence.\textsuperscript{45}

It would be wrong to construe the above criticism as unqualified opposition to operational procedures. The message is rather that behavioral data are as hard and sound as the substantive interpretation supporting them. When \textit{a priori} assumptions underlying behavioralist research come under close scrutiny, the hardness of equivalence?

as hard reexamine the distinction between phenomenal identity and conceptual

same is true of operational procedures. The latter are based on an unambiguous

mentioning to empirical indicators which are inherently ambiguous, which owe their sense to our assumptions, and which change their meaning with changing human activities. The number of political trials and purges is not directly related to the degree of alienation among intellectuals and youth in the country, as Korbonski's research explicitly assumes.\textsuperscript{46} The number of foreign books translated in a communist country may or may not correlate with the degree of its internationalization, as Triska and Finley have come to discover.\textsuperscript{47} The recruitment of younger individuals to the Soviet leadership does not necessarily indicate its greater flexibility, as Bunce and Echols rightly point out.\textsuperscript{48} The promotion of the Soviet defense minister to the Politburo is not sufficient grounds for the inference that the Soviet army has a greater role in political affairs, as some analysts imply. As Gati has pointed out, a vote in the UN does not automatically signal a pro- or anti-Soviet stance of a UN member state.\textsuperscript{49}

It would be equally wrong to assume that the problems outlined are temporary and that they will be gradually eliminated with the progress of behavioral research in communist studies. Whatever progress we can expect in the future, we should keep in mind that the problems besetting behavioralist studies of communism are by no means peculiar to them. Such problems plague the most sophisticated projects conducted in the social sciences - even those most conducive to the behavioral revolution. The root of the problem is in the limits of the behavioralist methodology itself. I will examine these limits below. First, I should like to discuss the patterns of prediction in traditional and behavioral approaches to communist societies.

\textbf{Patterns of Prediction in Communist Studies}

Behavioralists like to stress predictive power as an important advantage of their approach. What is usually meant by predictive power is the ability to describe in advance a forthcoming state of affairs as it evolves according to a formally defined scientific rationality. This definition does not apply to formulations that purport to explain how past or present events must have evolved according to a certain logic. An \textit{ex post facto} explanation of why the de-Stalinization campaign must have been launched in the Soviet Union will not qualify as a prediction. Neither would Welsh's "prediction" made in 1968 and aimed at explaining the American response to the 1956 Hungarian uprising.\textsuperscript{50} I call such past-oriented inferences "postdictions" and reserve the term "prediction" for future-bound statements.

The surest way to predict an event is to influence its course, or, literally, to cause it. That is what the experimental scientist does when he deliberately abstracts variables from things themselves and equalizes "other factors" in laboratory conditions. Specialists on communist affairs do not have much opportunity to experiment with communist behavior. They have to rely on the power of theoretical abstraction. Experience suggests that this is an unreliable guide to the future.

One early attempt to examine the record of prediction in communist studies was a monograph by Reshetar, \textit{Problems of Analyzing and Predicting Soviet Behavior}. The author assembled ample evidence testifying to the difficulties in predicting and explaining the Soviet Union's political decisions on the basis of its alleged ideological nature. Observable developments often diverged from theoretical speculations.\textsuperscript{51}

Given the hazardous nature of forecasting, one can understand why experts on communist behavior have adopted a cautious attitude in forecasting the evolution of Soviet and East European states. Three representative collections of essays dealing with the Soviet future are particularly valuable for understanding the strategies used by traditional area specialists.\textsuperscript{52} A sample of

\begin{itemize}
  \item \textsuperscript{45} Phenomenal identity is the term designating the formal similarity of empirical indicators and methods employed in different cultural settings. Conceptual equivalence refers to qualitative evidence showing that the empirical indicators - regardless of their formal identity - designate the same concept. Thus phenomenally identical indicators of participation, utilized by Hough, may stand for different phenomena within the framework of the Soviet and American political systems. For further discussion, see M. Armer, "Methodological Problems and Possibilities in Comparative Research," in M. Armer and A. D. Grunshaw eds., \textit{Comparative Social Research} (New York: John Wiley, 1973), pp. 49-79.
  \item \textsuperscript{46} Korbonski, "Liberalization Process," p. 207.
  \item \textsuperscript{49} See Gati, "Area Studies."
  \item \textsuperscript{50} W. A. Welsh, "A Game-theoretic Conceptualization of the Hungarian Revolt: Toward an Inductive Theory of Games," in Heron, ed., pp. 420-65.
\end{itemize}
assessments and forecasts from these volumes may help to clarify their strategies of prediction:

It seems safe to predict that the general level of education in Soviet society twenty years from now will be higher than it is today, and that the Soviet melting pot will have done much to obliterate status differences among the various nationalities, perhaps even the hard edges of national consciousness, although it is likely that, all efforts at indoctrination notwithstanding, feelings of antagonism . . . may linger on for decades . . . It is conceivable that the party might come to realize that differences in taste or disagreements in philosophy are not necessarily subversive, and that even some of the taboos now protecting social institutions and the historical past from scrutiny may be lifted with safety (Meyer).

In these circumstances, both Moscow and Peking will be under strong compulsion to sacrifice the present uneasy status quo if the resulting open split appears to offer sufficient advantages. In any event, a situation of such intense antagonism is inherently unlikely to remain stable over a long period of time (Duevel).

In the realm of domestic politics, the prospects for structural change do not appear to be significant . . . As a new generation of officials rises through the ranks, the top leaders are likely to be increasingly less erratic and more concerned with the smooth operation of the administrative apparatus . . . The Central Committee will probably play a larger role in party affairs, and may even reach the point of electing the members of the Politburo on the basis of competition among candidates (Black).

A long uphill struggle will be necessary before the forces of adaption within the system can be credited with a conclusive victory. Given the underlying logic of socio-economic maturation and the increasingly numerous manifestations of this logic in recent Soviet practice, it may be that these forces will ultimately prevail. But their progress will be interrupted by major reversals (Azrael).

What can be dimly foreseen cannot be confidently predicted, not only because we see through a glass darkly and lack critical data on ferment in the USSR today, but also because the Russians of the new “fourth generation” seem surer of what they oppose than of what they favor . . . There will probably be no return to private ownership of the basic means of industrial production, and little inclination to imitate the forms of traditional liberal democracy. But remarkable changes may well be in store, and could come with the unpredicted suddenness that is so characteristic of

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Russian history and is invariably later seen to have been predictable (Billington).53

What is typical of these predictions is that they are hard to disprove. The Popperian falsificationist could say that they are designed to be as irrefutable as possible. Indeed, these forecasts are couched in general terms, the statements are accompanied by numerous qualifications, the time span is left deliberately uncertain. Area specialists prefer to leave the door open to contingencies, “major reversals” and changes coming with “unpredicted suddenness.” They avoid a commitment to any one rigidly defined rationale for calculating the future. The strategy underlying these forecasts may be called “plausibility assessment.” The accent is on diverse trends, multiple logics, alternative scenarios for the future, accompanied by a guess as to their plausibility.

Brzezinski’s analysis of the prospects for Soviet evolution in the 1970s is a good example of this strategy. He presents five scenarios for the future: “oligarchic petrification,” “pluralistic evolution,” “technological adaptation,” “militant fundamentalism,” and “political disintegration.” After outlining the rationale for each script, Brzezinski makes a bet on a combination of the first and the third one as a most likely prospect for the next decade.54 I must admit that my efforts to find behavioralist and behaviorally oriented comparative works on communism that would lend themselves to comparison with the area studies approach have been largely unsuccessful. There is no single book featuring behavioralist forecasts. None of the papers included in Kanet’s collection offers a single predictive statement, despite the fact that some authors refer to the predictive power of the behavioral approach. Besides a few general allusions to the communist future, the works comprising Fleron’s collection also avoid predictions. One piece of behavioral research that I was able to find stands clearly apart from other, similar studies. This is an article by Sam M. Lowry titled “A Model for Predicting Succession in Communist Political Systems: The Case Study of the Soviet Union.” The variables discussed in this paper, according to this author, “enable a student of the politics of succession . . . to reduce significantly the guesswork involved in identifying the most probable single ‘liner’ to emerge


from a succession situation."55 The major thesis of this paper is that "the length of career service of persons in certain positions [such as party secretariat and Politburo] or . . . certain types of expertise [such as party vs. state functions] are determinants of the authority ranking attainable within the top reaches of the political system of the Soviet Union."56 The author does not explain how he operationalized the dependent variable—the authority ranking among Politburo members (after Stalin's death, Soviet propaganda generally avoided listing Politburo members according to their rank in the hierarchy). Information on the independent variable is more readily available. By a retrospective analysis, the author had found support for some of his hypotheses. When he made a prospective assessment, he predicted that K. T. Mazurov had the best chance to succeed in the future struggle for power in the Politburo. The next in line would be F. D. Kulakov, followed in turn by V. V. Grishin.

As we know, Mazurov was expelled from the Soviet Politburo (for health reasons) soon after Lowry's article appeared in print. Kulakov, the second candidate, came under strong attack by Brezhnev for the repeated failures of Soviet Agriculture—a sector of the Soviet economy for which Kulakov was responsible—and died soon after (under still not fully clarified circumstances.) It remains to be seen if the third most likely candidate is going to survive the forthcoming succession struggle. Meanwhile, Lowry's paper serves as a useful reminder that even mathematical calculations are unlikely to obliterate completely the old art of guess-work and future-telling.

One would be equally hard pressed to find clear-cut, verifiable predictions in recent comparative works on communism. Most of the comparative studies, published in Studies in Comparative Communism, do not offer any forecasts, and practically all the authors offering predictions prefer the same equivocal, non-probabilistic statements on the plausibility of certain future developments. Thus Korbanski's opposition to "descriptive-factual history and speculative prophecy—i.e., 'Kremlinology' and 'crystal-ball gazing'"57 does not accord well with his own mode of forecasting. His prediction that "the notion of 'succession crises' [in communist countries] may be a thing of the past"58 is yet to be checked against the future. His further assertion that the transition from individual to collective leadership is becoming irreversible can hardly be sustained in view of the recent developments in the USSR, Romania, Poland, and Bulgaria. Another prediction made by the same author follows the familiar strategy of plausibility assessments: "My feeling is that the ruling elites in a number of East European countries will in fact permit a somewhat higher level of criticism and dissent than in the past."59 Beside the fact that this statement is hardly an improvement on "Kremlinology and crystal-ball gazing," it seems to be at odds with the observable facts.

I am hesitant to generalize about the predictive capacity of behavioral research on the basis of such scanty evidence. One thing I am certain about is that clear-cut statements about the future are as rare in behavioralistically oriented studies as in nonbehavioral ones. There does not seem to be much evidence to support the claim that behavioralism offers a more accurate insight into the future. Where possible, scholars extrapolate current trends into the future and thereby hope to arrive at reasonable predictions. "If probable behavior of communist states in future situations is to be estimated," write Triska and Finley, "it is essential to assess the nature and rate of this [current] change and to project it as accurately as possible."60 This method, however, rarely yields satisfactory results. Today's rates of change may or may not be sustained in the future; hence the need to switch to forecasts.

Plausibility assessment as a forecast strategy is not very appealing in the age of behavioral revolution. It leaves too great a margin of uncertainty. Emphasizing the weaknesses of this strategy, we should not forget that putting forecasts in probabilistic terms does not necessarily alleviate uncertainty. When meteorologists issue a forecast such as "Chances of rain tomorrow are 50 per-cent," all we know is that tomorrow it may or may not rain. Even if the 90 percent-chance-of-rain-tomorrow prediction is not followed by rain, the forecast is technically, blameless.

Placing prediction in a more sharply defined context is not always possible, and where possible, it is often judged trivial. One could safely predict how the Supreme Soviet will vote on a SALT II Treaty and fail with a similar prediction about the American Senate. It would be desirable to foresee how specific segments of the Soviet population would behave under various circumstances, or to predict whether or not Soviet sociology would sustain its trend toward greater autonomy. Yet, when Brežněv makes a more definite statement—"it is unlikely that the Soviet Union will altogether evade the experience of student unrest"61—he finds his forecast unfulfilled. Similarly, when Simirenko predicts the growth of "ideological tolerance"62 for the next ten

56. Ibid., p. 145.
58. Ibid., p. 19.
60. See Triska and Finley, p. 1.
to twenty years of Soviet sociology, he is almost immediately forced to revise his prediction.

This situation is not peculiar to communist studies. Contrary to Kanet’s insistence on “the considerable progress made to date in permitting the political scientist to make predictions about the future political behavior based on statistical generalizations,” progress to date has been anything but impressive. Here is the opinion of one of the chief architects of the behavioral revolution in political science, G. Almond: “We are not the first generation of political theorists to discover that the problem of prediction comes close to insoluble.” Similar opinions can now be heard from demographers and economists—by far the most advanced students of social affairs. No less an authority than J. K. Galbraith has recently observed, after citing “an unparalleled record of error in political forecasting,” that “people who predict don’t know or don’t know they don’t know.” This startling conclusion is fully borne out by the record of prediction in psychology, where more than 10 percent of the variance in human behavior can be explained in terms of structural-dispositional variables.

Perhaps social science is a postdictive and not a predictive enterprise. If we can blame experts who predicted stability in Iran for the next twenty years at the time when the country was about to be swept by the revolution, then it is for their adherence to a far too rigid scientific rationality. One can understand the ironic statement by Triska and Finley in which they compare the traditional student of communism with the Delphic Oracle, whose pronouncements are “amenable to a great variety of sometimes contradictory attributions of meaning.” But given the “harder” social sciences’ failure to forecast human affairs, we may, upon closer examination, find the practice of the Delphic Oracle worthy of greater scientific attention. One thing it teaches us is that the future is always in the making, that our forecasts are a constituent part of the environment against which the future is formed, and that the prophet casts the future at the same time as he forecasts and broadcasts it. The lesson to be learned from the Delphic Oracle is that the indeterminacy and uncertainty of social reality are a phenomenon that defies reliable forecasting. Or, as Billington put it, “remarkable changes may well be in store, and could come with the unpredicted suddenness that is . . . invariably later seen to have been predictable.”

**Taken-for-Granted Assumptions of Behavioralist and Interpretative Methodologies**

In retrospect, it seems ironic that Fleron made his appeal for the behavioralization of communist studies in the same year that Easton, one of the early proponents of behaviorism in political science, announced the beginning of a “post-behavioralist” era. In his presidential address to the Annual Meeting of the American Political Science Association, titled “The New Revolution in Political Science,” he declared that “the heart of behavioral inquiry is abstraction and analysis and it serves to conceal the brute realities of politics.”

The term “post-behavioralist methodology” is somewhat misleading. It suggests that post-behavioral research is intended as a substitute for behavioral ones. Non-behavioralist or interpretative tradition in social research dates back to the works of Dilthey, Weber, and Simmel. What is now called post-behavioralist methodology began to take shape at about the same time that Durkheim issued his behavioralist manifesto, *The Rules of Sociological Method.* Nevertheless, there is some reason for viewing post-behavioralist methodology as a new phenomenon. For a long time, the interpretative critique of behavioralism has been scattered and unsystematic. It is only in recent times, after decades of behavioralist predominance, that the opponents of behavioralism began to codify a research methodology of hermeneutical inquiry. One of the early attempts to reappraise behavioralism in the social sciences was an article by Herbert Blumer, “What is wrong with social theory?” This paper was followed by works of Cicourel, Garfinkel, Glaser and Strauss, Douglas, Lofland, Mehan and Wood, Giddens, and other authors who undertook to elaborate an alternative to the traditional variable analysis.

68. See Billington, "The Intellectuals," p. 270.


I cannot analyze here in much detail the contributions made by these scholars to the development of social hermeneutics. What I will try to do is to summarize briefly the main points of criticism levelled against classical research methodology and relate it to the current debates on behavioralism in communist studies.

One of the cornerstones on which behavioralists erected their methodological castle is a radical distinction between the subject and the object of social research. For behavioralists, things are determined in themselves and must be apprehended as they exist in reality apart from the knowers' accounting practices. The behavioralist researcher is aware that his presence can influence his subject. He views this influence as "noise" that must be eliminated through a system of careful controls. The purpose of social inquiry, as seen in this perspective, is to grasp things' inner determination as facts and to render them into a neutral scientific language, avoiding idiosyncrasies of everyday language. "The words themselves are unimportant," writes Fieron, "what are important are things for which they stand."73

For the hermeneutical thinker, the world is not determined in itself. Things' mode of being in the world is that of indeterminacy, which must be terminated by the subject before things reveal themselves as identifiable objects. Objectivity does not reside in things. It is bound to the subject, who interferes with the flux of things themselves through his accounting practices. In a dramatic form this principle has been demonstrated in quantum mechanics, where, Bohr contended, "the usual idealization of a sharp distinction between phenomena and their observation breaks down."74 In a similar fashion, hermeneutical scientists maintain that an objective state of affairs in the social domain is inseparable from the ways it is accounted for. Every set of accounting frames carries with it its own "map of social being" that predetermines the objective forms in which things are apprehended. Our conceptual nets are so designed that they can "catch the fish" of a strictly defined denomination. By confronting human beings with questionnaires, tests, and experimental devices, the researcher forces individuals to take a stand and thereby affects the future course of action. The resulting "complementarity effect" cannot be eliminated in principle.

Another assumption underlying behavioral methodology concerns concept-formation and operationalization in the social sciences. Behavioristic scienti

73. See Fieron, ed., p. 12.
75. See Durkheim, p. 14.
76. See Sharlet, "Comparative Communism."
perception so that we can disregard some of the things' properties and treat as "functionally identical" their other characteristics. Thanks to this device, we can treat things rationally despite their situational inconstancy and variation.

The ambiguity of social concepts—a subject of typical complaint by behaviorists—is given new interpretation in the hermeneutic perspective. Social categories are "fuzzy at the fringes," to use Lazarsfeld's expression, not because social scientists fail to be rigorous, not because they fail to observe "the logic of either-or," as Sartori complains, but because they deal with fuzzy things, and yet are determined to break perception so that we can disregard some of the things' properties and treat as administrative taxonomical boxes. (An example of conceptual fuzziness is the above-mentioned failure to give an unambiguous definition of behavioralism).

One can agree with Kanet that certain quantifiable materials on Soviet politics are readily available and can be easily quantified. The crucial question, however, is what do these materials stand for, which alternative means can be imputed to them. This question is often sidestepped by behavioralists. But "that which can be counted does not always count." Before they are counted, things must be made accountable, i.e., subsumed under qualitative categories. Measuring and counting convert things themselves into identifiable objects. In Cicourel's words, "The very process of measurement imposes the comparability which allows each frequency in a given cell to be treated as identical and, hence, subject to statistical manipulation. The measurement process imposes the reification as a necessary condition for extracting the information required for comparative or statistical analysis." Only if one forgets about this reification can one reach the conclusion that "the traditional distinction between 'communist' and 'noncommunist' systems is obsolete and redundant." For a hermeneutical researcher, the comparability of the phenomena under study is an artifact, albeit an inevitable one, of our accounting practices. Mathematical manipulations with interpreted things can reinforce or challenge a chosen nomenclature. But the computed picture of reality

doned the assumption of classical thermodynamics regarding a particle's objective state. If our instruments fail to reidentify a previous state as "the same," it is not necessarily due to the inadequacy of the instrument. "The notion of test-retest or any other 'reliability' measure is antithetical to social science since it must make the incorrect assumption that human thought and behavior is static and, therefore, any change in response is a reflection of either instrument error or deception." Reliable, consistent responses are normally secured in vitro, under the ceteris paribus clause, after the influence of "other factors" is artificially eliminated. Yet the external factors are never equal in vivo, in everyday life, where social things continuously transcend the classificatory borders and change their denominations. The more successfully we strip a thing of its numerous determinations and secure its reliable behavior in a laboratory situation, the less accurately we will be able to predict its behavior in real life. The relationship between life in vivo and life in vitro, between validity and reliability, is that of uncertainty. Another way of putting it is that validity and reliability of social data cannot be maximized simultaneously with an arbitrary precision.

One can agree with Kanet that certain quantifiable materials on Soviet policy are readily available and can be easily quantified. The crucial question, however, is what do these materials stand for, which alternative means can be imputed to them. This question is often sidestepped by behavioralists. But "that which can be counted does not always count." Before they are counted, things must be made accountable, i.e., subsumed under qualitative categories. Measuring and counting convert things themselves into identifiable objects. In Cicourel's words, "The very process of measurement imposes the comparability which allows each frequency in a given cell to be treated as identical and, hence, subject to statistical manipulation. The measurement process imposes the reification as a necessary condition for extracting the information required for comparative or statistical analysis." Only if one forgets about this reification can one reach the conclusion that "the traditional distinction between 'communist' and 'noncommunist' systems is obsolete and redundant." For a hermeneutical researcher, the comparability of the phenomena under study is an artifact, albeit an inevitable one, of our accounting practices. Mathematical manipulations with interpreted things can reinforce or challenge a chosen nomenclature. But the computed picture of reality

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80. See Blumer, op. cit.
82. R. G. Kanet, "Is Comparison Useful or Possible?" Studies in Comparative Communism, 8, No. 1-2 (Spring/Summer 1975), 25.
83. A. Z. Rubinstein, "Comparison or Confusion?" ibid., p. 44.
is ultimately contingent on our assumptions. Says Gadamer, "What is established by statistics seems to be language of facts, but which questions these facts answer and which facts would begin to speak if other questions were asked are hermeneutical questions." 86

One more behavioralist tenet deserves to be mentioned here. It concerns the familiar premise that scientific practice is radically different from the value-laden concerns of everyday life. Scientific accounting frames are held to be a privileged system of coordinates that represent things as they really are, rather than as they are reflected in a commonsense consciousness through the prism of various ideological veils. To verify scientific theory means here to make it conform to praxis without interfering with its natural course. The scientist’s role as a student of reality is radically separated from his role as a man in the street with all his mundane commitments.

Interpretative sociologists emphasize that the social scientist, whether or not he is aware of this, constantly relies on his everyday experiences. His scientific activities are part of social practice. His accounts influence reality at the same time that they describe it. Theoretically to ascribe common interests to a given class is an act that helps to make these interests objectively real. The root of the problem is that human beings classify each other as constructs of the second degree, namely constructs of the constructs made by the actors on the social scene. . . ."87 This tenet suggests a new approach to verification. The latter is defined as a process of mutual adjustment of theory and praxis, of facts and values. Objective social reality is forced to meet theoretical requirements as much as scientific theories are adjusted to objective reality.

Hermeneutical researchers accept Weber’s position that “the objective validity of all empirical knowledge rests exclusively upon the ordering of the given reality according to categories which are subjective in a specific sense, namely, in that they present the presuppositions of our knowledge. . . .”88 This assumption sheds new light on Scharf’s claim that the behavioralist “avoids conscious value bias.”89 Behavioralist criticism of the traditional communist studies for their “failure to make the fact-value distinction”90 overlooks that our data function as facts precisely because they are embedded in our taken-for-granted, value-bound presuppositions. What the hermeneutical researcher strives to do is not to avoid “conscious value bias,” but to re-
In retrospect, we can say that behavioralists did an important job by introducing new methods and raising methodological awareness among students of communism. Methodological reflection, however, must be an ongoing concern. If we consider the behavioral revolution to be part of the methodological revolution, a strong case can be made that it is bound to be unfinished, permanent revolution. Quantitative methods will always be a part of scientific inquiry, though they are likely to be more valued for their heuristic insight than for the ultimate truth they confer on our theories. Things do not present themselves to the researcher with concept-tags and number-badges attached. We assign sense and number to things and thereby convert them into objects. Mathematical calculations are a powerful analytical tool, but hard methods are no substitute for hard interpretative work.

The transition from Kremlinology to behavioralism in communist studies somewhat resembles the shift from sorcery to astrology. As a formalized technique for assigning meaning to events, generating reliable data, and shaping one's conduct rationally, the latter was a significant improvement on the former. But the enhanced rationality was achieved at the cost of flexibility in dealing with ever-changing social contingencies. Mathematization of social inquiry is another example of the Weberian “bureaucratic rationalization.” To the extent that human beings succeed in fitting their actions into rationally calculated schemes and remodelling their world according to scientific rationalities, reality begins to resemble its “objective image.” The same process signifies a progressive reification of social reality, which is no longer seen as a man-made and man-sustained abstraction, but as a thing out there, governed by impersonal logic and natural law. Insight into the ongoing reification of society by its lay and professional members is an important contribution of hermeneutics to social studies.

Toward a Hermeneutics of Communist Life

Among students of communism, historians are perhaps the most sympathetic to interpretative inquiry. Their skepticism about the value of generalizations in historical research and distrust of historical determinism make them particularly sensitive to “what the Germans call Einfühlung, or ‘in-feeling,’ and Russians themselves prom'niknovenie—meaning penetration, or permeation... Only some such sense of involvement,” Billington maintains, “can take the external observers beyond casual impressions, redeem unavoidable generalizations...” 99 Valuable as the works of Billington, Florovsky, or Berdiaev may be, they are hardly representative of modern hermeneutical analysis. The student of communism seeking up-to-date information on the subject would have to turn to neighboring scientific domains.

Clifford Geertz is an anthropologist. His primary scientific concern is with traditional cultures and developing nations. But the importance of his work extends far beyond the confines of anthropology proper. Geertz’s notes on the Balinese cockfight, coupled with his theoretical essay on the interpretative methodology, may be the best starting point for those unfamiliar with the hermeneutical tradition. 100 The study that I have in mind was conducted in a remote village, where the author spent many months immersing himself into the intricacies of Balinese life. As a participant observer, Geertz is able to reconstruct the meaning of mundane activities that normally escape the notice of researchers dealing with kinship, political structure, and other macroscopic institutions. His attention is drawn to what seems to be an inconsequential pastime: the cockfight. He watches people talking about the cocks, making jokes, striking poses, placing bets, and he comes to see it as a “sociological entity.” Geertz perceives this cultural form as a “collective text,” “a collectively sustained symbolic structure,” “a means of saying something about something.” The author is not looking for an explanatory scheme that would account for the observed behavior. He is trying to grasp the schemes of understanding that the Balinese use to make sense of their own lives. The focus of inquiry thereby shifts from disinterested observation to sympathetic participation, from explanation to description, from prediction to diagnosis. The hermeneutical researcher “is faced with a problem not in social mechanics but in social semantics,” his objective “is not to codify abstract regularities but to make thick description possible,” his “conceptualization is directed toward the task of generating interpretations of matters already in hand, not toward projecting outcomes of experimental manipulations or deducing future states of a determined system.” 101 Step by step, Geertz reconstructs the message that the Balinese are sending to the world about the meaning of being a Balinese, of doing the Balinese life. The cockfight is decoded as “a dramatization of status concern,” as “a metasocial commentary upon the whole matter of ascertaining human beings into fixed hierarchical ranks and then organizing the major part of collective existence around the


101. Ibid., pp. 26, 449, 448.
assortment. Its function... is interpretative: it is a Balinese reading of Balinese experience, a story they tell about themselves. 102

Geertz’s study is only one example of a fast-growing body of interpretative research. Erving Goffman, a founder of dramaturgical sociology, applies the same method to modern urban life. His analysis is centered on the facework that individuals and groups of individuals undertake in order to impress upon others socially approved appearances. Goffman studies a team of employees in a small Irish hotel trying to maintain an appropriate image in front of their customers, the framing schemes used by the managers of a big Manhattan department store to induce customers to buy, the techniques developed by prison and mental hospital inmates, seeking to protect their self-identities. 103

Each study is an exercise in interpretation that describes the social world from the standpoint of actors and in terms of their own frames of meaning.

Ethnomethodology is another evolving domain of hermeneutical analysis. Harold Garfinkel, the founder of ethnomethodology, has outlined a program of research that focuses on the accounting practices by means of which individuals gain a sense of orderliness and induce the appearance of rationality in routine activities. 104 Following his lead, ethnomethodologists examine the work of modern bureaucracies where the raw material of unclassified case numbers is organizationally processed into facts and data. The idea is to show how social structures and institutions are perpetuated by individuals, who continuously subsume ambiguous occurrences under standardized and a priori established nomenclatures. 105

What can the student of communism learn from these examples? How can the paradigm of hermeneutical research be applied to communist countries? What is an equivalent of the Balinese cockfight, in, say, Soviet society? Since the interpretative methodology is yet to be tried in communist studies, the answers to these questions can be only tentative. What can be definitely asserted at this point is that there is an urgent need to reconsider traditional attitudes toward the living carriers of the communist culture and native informers.

A common attitude toward native accounts is one of suspicion, especially if the informer happens to be transplanted into a new cultural setting. Isaac Deutscher expresses this attitude well when he says that “of all people, the ex-Communists know least what it is all about.” He compares communist renegades and “fellow-travelers” to the patient undergoing surgery who, after having recovered “from a traumatic shock might claim that he is the only one who really understands wounds and surgery.” 106 This apt, even if indiscriminate, statement (does it apply to Djilas, Kolakowski, or Baumann?), accords well with the traditional distinction between things and facts, on the one hand, and their reflection in human mind and soft facts, on the other. Classical research methodology hypostatizes this distinction into an absolute. It transforms specific socio-historic standards of objectivity into a privileged reference frame in relation to which every account could be judged true or false depending on how closely it conforms to these standards. In his search for objectivity, the behavioral researcher tends to forget that social realities are constructed by individuals, that individuals’ accounts are part and parcel of social reality, that social reality persists as long as it is translated into meaningful actions of individuals and breaks down when individuals fail to subsume things themselves under a priori established and socially enforced categories.

The contribution of native informers to an understanding of communist society is not exhausted by the facts they unveil. In most cases, these facts are not new, and often enough they would not pass the test as hard data. These accounts perform a quite different service: they help one understand how these facts are routinely manufactured and made to happen by real people who are engaged in the daily construction of communist reality. They reveal how the communist universe of discourse is reproduced on the micro-level in practical situational encounters. The communist reality is portrayed here from within by those who lived it through. The native informer knows first-hand how to do hard facework in order to pass for an ordinary citizen, how to dramatize the official reality as objective and meaningful. When we try to understand such an account, “we do not try to recapture the author’s attitude of mind,” says Gadamer, “we try to recapture the perspective within which he has formed his views. But this means simply that we try to accept the objective validity of what he is saying. . . . In our hermeneutical attitude . . . we share fundamental prejudices with tradition.” 107 The ultimate aim here, in Geertz’s words, “is the enlargement of the universe of human discourse.” 108 This attitude presupposes a reevaluation of what is normally dismissed as soft data. What is soft data for the behavioralist scholar striving to explain and control the behavior of the objects under study, is hard data for a hermeneutical student of social reality, whose purpose is to understand the meaningful world of the subjects with whom he comes into direct or indirect contact.

102. Ibid., pp. 437, 448.
In some respects, the hermeneutical orientation in social sciences resembles quantum mechanics insofar as the latter analyzes the "solidity" of macro-bodies through the "liquid" processes of micro-interactions. Similarly, hermeneutical analysis attempts to explain how social structures are held together by individual interpretative practices. In both cases, the researcher confronts the problem of transition from the micro- to the macro-level of analysis. This problem is not unfamiliar to students of communism. The "Russian character" theory, designed to explain the continuities between Russian and Soviet cultures, dealt with it explicitly. The research inspired by this tradition has failed largely because national character was perceived as a psychological structure. Even readers sympathetic to this approach could not accept the leap from psychological to sociological explanation. Hermeneutical ideas of phenomenological and dramaturgical sociology may help to overcome psychological reductionism and to reconceptualize the problem of national character.

Social phenomenology conceptualizes social universe as a uni-verse or universe of discourse-literally, one word, one deed, or one verse. Social verses are terminological frames in terms of which humans apprehend their reality. Individuals terminate indeterminacy in terms of socially defined frames and thereby continuously reproduce their world in macro as a meaningfully objective whole. The social universe is a product of an ongoing reification, or, if you prefer, a "Potemkin portable village." It is erected by individuals doing their facework and it persists as long as individuals lend their faces to dramatizing the official reality as objective and meaningful. Objective social reality loses its appearance of external social fact when individuals—the living carriers of their culture—fall, in Simmel's words, to "perform the synthesis of 'social being'." What earlier theories meant by national character can be now conceptualized as a mode of uni-versalization and dramatization of things themselves in a given culture, as a mode of production of objective social reality. Macrostructures could then be analyzed from the standpoint of their symbolic codes and dramaturgical practices through which these codes are decoded in everyday interactions. This approach differs from the traditional formulation in that it does not ascribe to carriers of culture a set of invariable properties. It holds national character to be semiotic, rather than a psychological structure. The expectation is that different cultural wholes are characterized by different "hermeneutical styles" or modes of defining the situation, of terminating indeterminacy, of maintaining and breaking established frames.

I cannot analyze here the problem of national character. Such an analysis requires a substantive discussion that would lead me too far from the methodological focus of this article. From the methodological standpoint it may be interesting to note that the main principles of the outlined approach can be found in the works of Max Weber, familiar to all students of society. It was Weber who first realized that uncertainty and fuzziness are strategic properties of social reality and that concepts with sharply defined boundaries and lawlike statements cannot do justice to observable behavior because human behavior is itself a boundary-making and boundary-breaking activity. Standard generalizing procedures do not suffice in the "cultural sciences" where, according to Weber, "the knowledge of the universal or general is never valuable in itself."\(^{111}\) Hence his quest for idiographic and interpretative methods that resulted in what is in effect the first full-scale hermeneutical study in sociology: The Protestant Ethic and the Spirit of Capitalism. It was also Weber who emphasized that interpretative inquiry does not imply that one gives up on rational patterns. Interpretative analysis employs generalizations as much as behavioral research relies on interpretative insights. Weber sensed this very clearly, as his search for a synthesis of nomothetic and idiographic methods suggests. Despite Weber's own contributions and continuous efforts by his followers, this search is still far from ended.

The trouble with interpretative inquiry is that it is not susceptible to formalization and paradigmization. "The besetting sin of interpretative approaches to anything-literature, dreams, symptoms, culture—is that they tend to resist, or to be permitted to resist, conceptual articulation and thus to escape systematic modes of assessment. You either grasp an interpretation or you do not, see the point of it or you do not, accept it or you do not."\(^{112}\) Unlike behavioral methodology, hermeneutical research cannot be easily presented through a set of formalized steps (though some attempts to this effect have already been made).\(^{113}\) It is not a "puzzle solving" enterprise, in a Kuhnian sense of the word, and therefore it does not quite qualify as a "non-

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110. If we consider Soviet culture in this perspective, it may be interesting to note that Bolsheviki did not confine their reforms to the rationalization of the means of industrial production. They also established monopoly on the human face—principal means of production of objective social reality. They turned the human face into a state property and face-work into face-labor. The phenomenon of totalitarianism can be seen in this perspective as a drive to maximize the universality of terminological frames, to eradicate indeterminacy from everyday situations. A forced universality, intolerance to indeterminacy, and fake authenticity are among the salient features of Soviet hermeneutical style.
mal science." 114 The behavioralist methodologist could charge that hermeneutical inquiry is bound to turn up soft data and that this approach encourages the proliferation of conceptual schemes, none of which can be proved to be sounder than any other. To this the hermeneutical researcher could say that hard data are of little value if they are not sound and that the elevation of a scheme to the status of a true and objective account entails criteria that are arbitrary and value-bound. In the end, we may face an uneasy choice between the data which are reliable but not valid and the findings which are valid but not reliable. This dispute is likely to go on, as social scientists continue to search for a synthesis of nomothetic and idiographic methodologies. Whether or not the two approaches are fully reconcilable may not be known for quite some time. Meanwhile, the hermeneutical researchers will continue their efforts to understand how "society is created and recreated afresh, if not ex nihilo, by the participants in every social encounter." 115

To paraphrase Weber, one does not have to be a communist in order to understand communists. Yet there is no substitute for the first-hand encounter with the living producers of communist culture. In this respect, the hermeneutical approach to Marxist-Leninist countries comes close to anthropologists' studies in remote cultures. Ideally, the hermeneutical researcher would have to immerse himself in communist life the way Carlos Castaneda did while studying witchcraft in a Mexican tribe. 116 In the absence of a more direct approach to everyday communist life, students of communism may capitalize on the current exodus from the Soviet and East European societies. Communist or ex-communist, humans in flesh and blood are being approached and studied in situ. Talking to them face to face, observing them in their everyday lives, trying to understand the ways they go about understanding things are indispensable for the researcher who "wishes to study actual developments, and to do so not scientifically but with the gift of normative and aesthetic empathy-a sense for values and for style." 117 With these words of a well-known student of communism, I close these notes on the methodological currents in communist studies.

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116. A comparison of Marxism-Leninism with witchcraft may seem an odd enterprise. As a set of routine practices, taken-for-granted presuppositions, and sacred symbolic representations, Marxism-Leninism does not preclude such a treatment.