DISCOVERY OF SOCIALIST OPINION*

Leonard J. Barchak
Rutgers University

INTRODUCTION AND THEORY

Since the turn of the century, there has been a continuing debate as to the well-spring of revolutionary ideas within a society. As expounded by Luxemburg, the origin of revolutionary thought is to be found in the people's intellectual reflection upon their actions of class struggle. Pursuing a different line of reasoning, Lenin and his followers have pointed to the bourgeois upbringing of Marx and Engels and have usually held that the genesis of new ideas is linked to an enlightened community of progressive intellectuals.

Taking this controversy as a starting point, Luigi Manca (1975) has argued that these vital components, Luxemburg's spontaneous element and Lenin's "external" intervention, are *not* mutually exclusive. His further thinking on this matter is that too often the socialist media have been perceived as an ideational one-way street, wending from the intellectual

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vanguard to the working masses. The masses can make a genuine contribution to the creation of new socialist ideas, he asserts, when the socialist-journalist is able to fulfill not only the role of mass communicator but also that of theorist and of researcher.

While we must disavow any particular competence as a Marxist scholar, the importance of what Manca says is crucial to any notion of a revolutionary or creative, non-utopian social communication. It is also a faithful echo of Marxian thought, as expressed in *German Ideology*, that modern universal communication can be subordinated to individuals only by subordinating it to all of them. And this surely means that neither the communication of the masses nor that of the vanguard can be slighted in the creation, maintenance, and transformation of revolutionary thought.

However, hidden within this penetrating thought of the scientific Marx--as elaborated in the Luxemburg-Lenin debate--are at least two most significant requirements: One calls for an advanced and precise logic of discovery to understand how revolutionary thought comes to the fore; the other demands a theory and methodology of ascertaining "public opinion" that will do justice to this logic of discovery while adequately equipping the tri-functioned socialist-journalist. Although the two requirements are intimately connected, neither of these, to our knowledge, has been met in any consistent manner.

Our intention therefore is to show that the logic, theory and methodology are already available and applicable to socialist, or other scientific, societies. Moreover, we shall attempt to provide a scientific (theoretical-practical) framework for the assessment and representation of the main trunks of intellectual development for a particular matter within a socialist society, including those trunks intrinsic to the working masses, the progressive intellectual vanguard, and other possible sources of revolutionary thought. It is perhaps prudent to acknowledge that this is a

novel view of assessing the bases of social communication and, as such, is opposed to the nose-counting so popular in the ubiquitous "public opinion polls" now in vogue.

We will also attempt to bring the formal model to bear on the so-called Yugoslavian "nationalities question." Although we will not venture beyond setting the problem and providing the methodology, it will hopefully become apparent how the methodology could lead directly to determining the bases of social communication and thereby provide scientific direction for the dialectical resolution of this long-standing and divisive dilemma. Implications for the socialist-journalist would then be in order.

Although it is crucial to my position, it remains almost impossible to say succinctly what a logic of discovery must entail. And this is so because the dominant philosophy of science has advocated a kind of administrative empiricism which rules out ruminations, speculations, and values of all sorts. Nevertheless, in the past 15 years a number of thinkers have once again begun to argue that science takes place within a "paradigm" of values, having concrete and sociological aspects, and is, in this way, not unlike other social institutions. Most prominent among the thinkers is Thomas Kuhn (1970), author of *The Structure of Scientific Revolutions*.

Members of a scientific group, says Kuhn, work from a common basis that directs a discipline's research. This paradigmatic research view is in contrast to descriptions of science that place it above the social system as a linear collection of facts.

Karl Popper, Imre Lakatos, and others are important advocates of this long-dominant position and have taken issue with Kuhn's view of science, claiming that the paradigm is not only mythical but an essential bulwark of irrationalism as well as scandalous comfort to new-left revolutionaries (Popper, 1970; Holton, 1974). Their rationalism, however,

amounts to a formal logician's approach to science and has little to do with the production of scientific--or revolutionary--ideas.

At this point, we would like to draw out from this historical process a point of broad agreement between Marxists and non-Marxists. This is done to emphasize that a thoroughgoing positivism is as unsuitable to the new logic of science being developed by Kuhn and others as it is to the dialectical materialism of Karl Marx.

In the Anglo-American *Encyclopedia of Philosophy*, we are told:

> Nevertheless, some efforts to incorporate Marxism into philosophy are less successful than others, for Marxism is not philosophically neutral even if it does fail to define its position in respect to the major philosophical traditions. Least successful are alliances of Marxism with materialism, from Holbach to Büchner, or with positivism, whether Mach's or Spencer's. The tendency of decades of criticism has been to show that the idealist content of Marx's thought is too dominant to allow these confusions [McInnes, 1967: 174].

Quotes to this effect from Anglo-American sources can be placed beside others more useful to Eastern Europeans. Let us turn, therefore, to the writings of academician Nikolai Semyonov, a member of the USSR Academy of Sciences Presidium. Semyonov is a pioneer in the field of chemiluminescence whose writings on scientific discovery have much in common with the thoughts of Kuhn. With an accusatory eye to Soviet philosophers and others, Semyonov considers the positivistic orientation in light of Marxist-Leninist thought:

> Some natural scientists reason as follows. Our task is to observe and describe empirical facts and to establish their interrelationships,

> formulating these in the language of mathematics. The important thing is to construct a formally non-contradictory system of equations; how that system is interpreted in respect of a world outlook is entirely immaterial and can well be left to the philosophers, who love "pseudo-problems" [Semyonev, 1972: 32].

No more succinct characterization of the anti-Marxist, anti-logic of discovery philosophy could perhaps be given than Semyonov's, whose criticism is direct and harsh: "Such positivist attitudes are sometimes the result of philosophical naivete, sometimes of lack of faith in the power of dialectical thinking and man's ability to understand the external world."

Semyonov goes on to describe the process of discovery as it occurred in his own scientific work. From his exposition it is quite clear that he is in substantial agreement with Kuhn and that something much like a paradigm of values lies like a guiding principle behind the process of discovery.

Others have variously referred to this guiding principle as "themata," "hidden structure," "abduction," and more. Our own choice is borrowed from William Stephenson, the single modern scientist who has been able to provide a theory-methodology for examining and understanding any and all "paradigms" --the locus of discovery and the fountain of revolutionary ideas.

Although Stephenson has called the guiding principle "schemata" and has time and again emphasized its subjective nature, the most fruitful--and perhaps surprising--designation he has given to this crucially vital understanding is quite simply "communication." In other words, lying behind science as behind religion, art, and every other human cultural enterprise is an organization of subjective communication that has its locus in the palaver of everyday conversation, including talking with others and with oneself. It is among this wealth of every-

day communication that one must search for the revolutionary ideas in a society.

Since profound and fundamental matters are here at issue, it is necessary to more closely examine Stephenson's theory of this organized communication and the import of its subjective methodological standpoint.

COMMUNICATION

Communication is rather much what Stephenson means by a theory of subjectivity, except that subjectivity has additional paradigmatic connotations that can lead us away from the simpler matter. One must also be cautioned that, for Stephenson, a theory of communication is tentative, an aid to getting around. This is in line with C.S. Peirce's (1956: 2) suggestions that such offerings be "not devoid of all likelihood" and "in the general line of growth of scientific ideas."

Communication theory, Stephenson (1969: 69) tells us in the most disarming manner, pre-eminently deals with the actual verbal statements a person makes or can make. To do this requires giving up the objective approach and recognizing the subjectivity of all communication, whether lovers' patter, conversation at the work bench, or gossip over the back fence.

Communication theory, he also suggests, is the reverse of information theory, ignoring facts and dealing only with meanings and values. Communicability he likens to mind as well as to consciousness and language. Yet, Stephenson holds that the vital essence of communication is not categorization or analogy, but the cultural use of language. This use can remarkably but reasonably be brought under the operations of his Q methodology.

CONCOURSE THEORY AND LANGUAGE

"Language is as old as consciousness; language is

practical consciousness...," wrote Marx is 1846 (Marx & Engels, 1959: 251), paving the way for an understanding of man and his word-thoughts as near equivalents. Since that time, Peirce, Mead, Stephenson, Bateson, and a great many others have modified this to include non-verbal symbols. Nevertheless, Stephenson (1969: 73) has adhered to the conclusion that the essence of self--or subjectivity--is roughly defined by the language one is able to use. About any controversial matter in a community setting, it will then seem that the combinations and permutations of a language would permit nearly limitless emergence of opinion statements.

Concourses, as Stephenson (1978) has named the collections of opinion statements about controversial matters, can be expected to contain large numbers of statements. And although any one concourse might range from several thousand statements to a great deal less, depending on the sophistication of the source from which it is drawn, a large concourse can usually be reduced by utilizing a Fisherian design to something below one hundred separate statements (Stephenson, 1953). However, while any particular concourse is anticipated to be large but not infinite, the number of possible concourses is itself without limit.

Concourses are an empirical matter, to be drawn from all that is spoken or written, in conversation with others or oneself, and from architecture, *objets d'art*, played works, or mime. A concourse may often be derived from a single community and is likely to differ in various respects if drawn from the farmlands of Voyvodina or the laboratories of the world.

Such concourses, ripped as they would be from integration in a master work, a recorded conversation, or a personal memoir, do not constitute the actions of a reductionist. For in due course, statements composing the concourse will be reorganized in a model of an individual's subjectivity, a model reconstituted by the individual, *not the researcher*. There is no

assumption that any statement has the same meaning for everyone. "On the contrary," writes Stephenson (1976: 143), "its meaning will depend upon the situation, and it may mean different things to different people." And it follows from this that Stephenson's concern in Q methodology is not with objective facts, but with meanings and values.

SCHEMATA

Organization of the several statements into a Q sort can be accomplished by an individual because a Q sample is a representation of the language he speaks, the verbal symbols available for his communication. Such a conclusion would mean little, however, if over a number of occasions an individual's organization of statements was chockablock with irregularities or if the organization by each individual was wholly idiosyncratic.

Neither contention has ever received sufficient empirical support to warrant its entertainment (Patterson, 1966: 49). That structure (or form) is objective, in the sense that it will emerge from any competent replication, is rather the better candidate for acceptance. As to the first contention, Stephenson has shown that the subjective structure of even those persons with the most disturbing emotional make-ups is not a phantasmagoria, but one of order and lawfulness (Parloff, Stephenson & Perlin, 1963). Where change does occur it proves to be no more unintelligible than the orderly, unitary transformations discerned for fauna and flora (Stephenson, 1974). Somewhat more intriguing than the endurance of individual subjectivity--or to use the Stephensonian alternative, schemata--must be the conclusion that subjectivity comes in a very limited number of distinct forms. Stephenson's life-long practices in Q methodology have shown time and again that with respect to any concrete situation the upper limit is not several million, or even several thousand or several hundred, but a mere seven--plus or minus two (Stephenson, 1973: 26). Modern informa-

tion theory has lent considerable support to these human limits for perception as well as for the more complex forms of cognition involving multivalued choice (cf. Miller, 1967: 14-44). This is the principle of limited independent variety, once an article of faith, but now renewed by extensive empirical support.

Concern in Q methodology is always with an expected limited number of interrelations among the organized Q sorts, that is, the schemata they entail. Such schemata might usefully be compared to their kith and kin, the intellectual paradigms of Thomas Kuhn, from which they have gained meaning. Subjective schemata, Stephenson argues, are what Kuhn has been looking for and thereby are the bases of creation in science--and outside of it.

OBJECTIVITY AND SUBJECTIVITY

Objectivity has well served the human cause of science both in theoretical and technological matters, and this remains true despite the paths taken in the present century.[1] Subjectivity, on the other hand, has scarcely been allowed a place in science at all. Stephenson proposes to alter this unjustifiable historical tradition[2] by distinguishing between the two: Objectivity, he maintains, aims to bring about change in the existing world and will settle for nothing less. For subjectivity the situation is different. What is subjective to a person--dreams, wishes, values, and so forth--retains an underlying structure that, try as try will, never can produce a silver dollar from a golden thought. The *sine qua non* of

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[1]See Bronowski (1974: 367-374) who, for one, implicates society in general, not just scientists, for perverse applications of 20th century science.

[2]Newton wrote a lengthy "fifth rule" that, in essence, rejects subjectivity in science; however, Newton suppressed the rule and went on about his science using "hypotheses" that were subjective. See Stephenson (1976: 258).

subjectivity is not change, but "form."

Distinguishing objectivity from subjectivity and impressing only the former to accomplish science's handiwork had been merely a limited burden so long as inquiry was confined to the physics of celestial and terrestial bodies. With the separation from philosophy of psychology (Schultz, 1969: 13-28), harbinger of all social sciences, thereupon began the long drive to establish it as a separate and valid science. Whether it was to have an objective base, such as mechanics or chemistry, was a moot question but one to which Dilthey suggested a solution. We quote Stephenson (1976: 383):

> Dilthey (1833-1911) was amongst the first to claim that the natural and cultural (spiritual) sciences were distinct. Thought is different in the two. The physical sciences deal with facts, and thought takes the form of explanation. The cultural sciences deal with meanings, and the mode of thought is understanding. Explanation establishes causal laws and approaches objects externally from the outside. Understanding deals with links between meanings inside the mind, on the inside, grasping connections between "meanings and meanings" by acts of intuition. The methods of the physical sciences, it was argued, could not apply to this *modus*.

Actually, as Stephenson has repeatedly emphasized, the critical difference between explanation and understanding is that understandings can be given *only* by the experiencing person; only the individual has access to his own subjectivity. Explanations can, in principle, be rendered by anyone or even by an instrument.

Most significantly, the social sciences, down to a single one, have opted for the manners and dress of the physical sciences, objectivity and explanation. It is no secret that within the conglomerate field of communication, as elsewhere, the result has been a be-

wildering proliferation of approaches that range from lip movements to satellite networks. No one person fully commands all these approaches, and the few who are conversant with the spectrum are helpless to employ them in a unified fashion (Smith, 1966; Cherry, 1957, 1971). While this does not constitute demonstrative evidence that no objective approach will be forthcoming, there is certainly a chink that should allow Dilthey to enter.

And if subjectivity comes in with Dilthey then science accompanies Stephenson through the portals. For it is Stephenson who holds, with Kuhn and Peirce, that not only is there subjectivity in science but that this subjectivity is expected to be lawful--"though little hampered by conventional logical rules." And it is Stephenson alone who offers a methodology (not simply a technique) for the systematic investigation of subjectivity.

What is so original about this is to be found in the synthesis of many elements. Others have guessed that subjectivity is at the base of the creative process in science. Popper (1970) is one from our own century, George Herbert Mead (1934: 41n) another, and Michael Polanyi (1967, 1968: 187-199) a third. All doubted that it was analyzable or analyzed it too far. Peirce (1956), Holton (1974), and Medawar (1969) grasped that it could be analyzed but produced no method. Intuitionists from the existential and critical schools saw through to the centrality of subjectivity for the social sciences but often placed it beyond the purview of methodology. Only in Q method has subjectivity been given a position that harmonizes the essences of these many views, removes the obstacles in the roadway, and lets science go forth.

Allow a little qualification to be mixed with what has just been written. Subjectivity, as Stephenson well knows, is far richer than any study possible with Q method. It is "a warmed-up dish compared with the freshly baked fare of a person's own subjectivity" (1976: 573). However, if the subjec-

tivity of (say) 30 or 40 interviews are gathered before any individual researcher, they present such a "hopeless mass of idiosyncratic comment, egregious misconceptions, and tangled ideation" as to beleaguer and fell the most worthy pursuit. Compare this with Q method that, after analyzing subjectivity into Q statements, allows it to be re-synthesized by the respondent's *own* actions, which can then be readily grasped.

In the same vein, Stephenson acknowledges that "science embraces all knowledge, objective and subjective" and that the humanities, religion, the arts, and the sciences themselves can be subject to a science comprised of both objective and subjective aspects. He proposes to bridge the two with Newton's four conventions (or rules) of reasoning and a fifth that Newton had suppressed.[3] Reformulated by Stephenson (1976: 261), the fifth rule is as follows:

> These things which neither can be demonstrated from the phenomena nor follow from them by argument of induction, I hold as subjective hypotheses. Their resolution is possible by way of operant factors relevant to these hypotheses. In

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[3]Here is Stephenson's (1976: 257) summary of Newton's first four rules: "Modern science is based on four rules, given in his *Principia* (1687):

Rule I: Nature is essentially simple; therefore, have as few hypotheses as possible.

Rule II: Similar effects must be assigned the same cause; the principle of uniformity in nature.

Rule III: Properties common to bodies on which we experiment are to be assumed (if only tentatively) to pertain to all such bodies in general; needed for universals.

Rule IV: Propositions induced from experiment should not be confuted merely by proposing contrary hypotheses. You can't confute a tested hypothesis by saying God could have done it better."

this manner new testable hypotheses arise from the subjectivity at issue.

Enough then on objectivity and subjectivity, except to note that what others have meant by soul, mind, and self is to Stephenson none other than subjectivity. Thus, subjectivity is *not* linked with consciousness of perceived states but is rather "the condition of viewing things exclusively through the medium of one's own mind or individuality" (Stephenson, 1976: v, 100).

FUNCTIONAL-INTERACTIONAL POSITION

That subjectivity can have a definite logical form is one of the two profound principles on which the scientific study of subjectivity is based, says Stephenson. Functional-interactionism is the other. For objective physical science, the functional-interactional position has been consciously the starting point since sometime after its beginnings in the writings of Peirce. Something is not "hard" according to this position but in an actual situation would rather "resist a knife-edge." Although such derived categories as "hard," "soft," "heavy," and "light" may be of service in the physical sciences, they are nonetheless tentative designations that require concrete situations for their explication.

It is the same way with subjectivity. Thinking may be going on in a mind and might be categorized as (say) nationalistic or universal, but it is only in concrete functional-interactional situations that one can go beyond such *a priori* categorizations. Q methodology and Q sorting can provide that situation.

By way of summary, we point out that although the reigning objective methodology may have its practical importance along sampling lines, it is only Stephenson's Q that allows us to deal with whole persons and not parts of persons; that avoids assuming that every individual has every imaginable characteristic to a certain degree; that invites the respondent to give

his views (within the tolerances afforded by the Q sort) and model his preferences in a multi-choice situation; that slices through the false dichotomy of subjectivity-objectivity to ask only for reliable operations; that discards minute description of trivia and bids us seek out what is likely to prove important; that offers a more sensible method of making science than the hypothetico-deductive (hypothetical-reductive?) hoax that too many modern scientists have played upon one another and upon themselves; that suggests the reasonable idea that a theory is not God's law, but an aid to finding one's way around in reality; and that operates under the most time-honored and useful assumption of atomic unity and limited independent variety (Stephenson, 1973: 25-26). Were this to exhaust the riches of Q, there would already be surfeit. Yet, Stephenson is amply capable of writing his own unique peroration:

> The writer proposes, again, that he knows of no methodology that stays closer to subjectivity, with operations sytematically involved, than Q. He knows of none in psychology, either, which is so precise in its logic-of-science respects, or so interesting in its pragmatics. It requires, in the latter connection, no normative data; it can deal with single cases; it concerns itself with pure number scales sufficient for their purposes, whose zero is the same for all Q-sorts, by all Q-sorters, for all Q-samples--provided, of course, that the rules are obeyed. And it represents what at least two great minds, Dilthey's and Stout's, regarded as fundamental in subjective psychology [Stephenson, 1963: 272].

QUALIFICATION OF OPINION

Assessment of public opinion is as old as tribal leadership, although the early intention might have been no more than to discover opinion in order to bend it to the leader's will. Assessment of public opinion to make it part of policy is only as old as democracy itself.

Marx's *qualitative* suggestions on the uncovering of public opinion--if we may so construe his concepts of false and actual consciousness--came in the nineteenth century as did the *quantitative* sampling methodology of Quetelet.

A mere three years after the death of Marx, an English lawyer named George Carslake Thompson produced a book containing a chapter on "The Evaluation of Public Opinion" in which he put forth a most interesting account of how these matters stand to one another. Although he was willing to agree along quantitative lines that such matters as *volume, intensity,* and *persistence* would be necessary to distinguish issues of public opinion from "every singular or obscure crotchet" and from "every passing flash of like or dislike," Thompson found it sufficient to say that "if a controversy was being frequently mentioned in the press, etc., then it would be enough to decide that it had these attributes in sufficient quantity to merit serious regard as a public controversy" (Stephenson, 1964: 272). That something like this attends the Yugoslav "nationalities question" is obviously the case whereas this may not be so for public opinion about the fourteen-member collective presidency, a topic which has come and gone.[4]

Quantitative assessment is hardly the whole story for Thompson, nor even the pre-eminent factor in the evaluation of public opinion. This he attributes rather to "reasonableness" by which he means preferences, wishes, beliefs, policies and the like that have been elaborated on the basis of evidence. Elaboration refers to "definiteness with regard to practical action" and "theoretical completeness" (Thompson, 1966: 8-9).

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[4]Of course, the collective presidency is still a controversial matter, but twenty-two seems to have supplanted fourteen, at least from my limited vantagepoint.

Thompson then poses the question of whether the opinions of a small number of great philosophers and statesmen might not be rightly set in balance to a larger number of uninformed persons. Answering that earnest opinions held on rational grounds outweight a greater number who hold a vague and general preference would seem to put Thompson in agreement with Manca's suggestion that *both* the contributions of the masses and those of the intellectual vanguard are to be taken into account. And like Manca he suggests a greater weight for the great philosophers.

Since the appearance of Thompson's paper in 1886 the attributes of "reasonableness" have rarely been taken into account or measured. Instead, the field has been clear for the large-sample, individual-differences doctrine made popular by Stern and developed by Thurstone (Stephenson, 1964: 268). Present day derivatives of these doctrines everywhere assume types but measure none, setting off (as they do) from the supposition that all men are more or less alike until proved otherwise. Thousands of papers resting on this doctrine must now be in existence since their analytical-reductionist methods are *the* most widely accepted technique for the social sciences.

Other than on some sort of practical grounds, the studies these papers report are useless to an extreme degree, particularly from an inductivist, logic-of-discovery point of view. Typical of this genre are any and all of the studies reported in *Opinion-Making Elites in Yugoslavia* by Barton, Denitch, and Kadushin (1973). Each contribution begins with categorical definitions (Croats, Serbs, and so forth) and cross-multiplies them to finish with a bewildering plethora of categorical types that cannot possibly be adequately interpreted. As a result their book must end without discovery or the production of new social knowledge.

Sometimes we encounter the same sort of analytical-reductionist thinking, even though it be without the numberical-quantitative framework and even though

it be the work of a careful scholar. Such is the case of a new book by Gertrude Robinson, *Tito's Maverick Media*, which almost predictably reaches the conclusion that: "To chart a course for future communication research in Yugoslavia during the twilight 1970s is difficult if not impossible" (Robinson, 1977: 229). Such exasperation is unacceptable as social science theory or as a guide to communication research. It throws a roadblock in front of science and says: go no further!

One other methodology concerned with the evaluation of public opinion is worth addressing before passing on, and interestingly enough it is one that has been offered for and utilized within Yugoslavia. Reference is made to the work of Alex Edelstein, who has, like Stephenson, proposed a methodology that would seek *operants*[5] by putting self at the center of research. All this is to the good, and it is no small achievement that Edelstein (1974: 244) pinpoints the absurdity of having the social scientist doing the thinking for his audience.

Edelstein's methodological approach is new in that sense, but in the end it too fails by settling on a large-sample, external-objective technique and by arriving at the same categories from which he began--sex, age, income, education, etc. This is proof

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[5]*Operant* is a concept of supreme importance in Stephenson's (1969: 75) Q methodology: "That is, there is no operational definition of a *status quo* or any other opinion in the *ad hoc* categorical manner of present-day attitude measurement (following the errors made initially in this respect by Thurstone, 1929). Instead, Q sorts are performed under a given condition of instruction, but only the facts count, i.e., what the individual provides as his operant view. Factors are first found in Q method, and then interpreted. In categorical testing, a test is defined (i.e., given an interpretation) and then measurements are made with it according to that prior definition."

enough that the baby has gone out with the bathwater: Edelstein never uncovers the operant types he has so painstakingly sought. Rather than the first Einsteinian in social science, Edelstein is perhaps the last of the Newtonians.

A pause might be appropriate at this juncture to ask whether empirical exploration is not in such bad repute as to be dispensed with in favor of a fundamental theoretical approach. Quite directly the answer comes back that we have already stated a fundamental theoretical approach. Moreover, our empiricism is to aid us in discovery--not to dislodge facts --and is totally in agreement with the theoretical position and practice of Marx himself. That Marx was very much an empiricist, that he learned his science deeply and unabashedly from Aristotle's most profoundly scientific works, has been given a measure of exposition by Heinz Lubasz (1977), senior lecturer in history at the University of Essex in the United Kingdom. Indeed, we have through Q method uncovered just such an Aristotelian dimension in the thinking of communication scholars in the socialist countries of Europe--prior to and independently of the appearance of Lubasz' work (Barchak, 1977).

Rejecting any "line of descent" from Plato-to-Hegel-to-Marx and firmly denying that Marxian theory is *only* Aristotle's philosophy of nature applied to society, Lubasz (1977: 17) concludes his argument on the side of scientific empiricism:

> ...the Althusser-inspired anti-empiricism trend in much of contemporary Marxist theorizing is certainly out of tune with Marx himself. Positivist empiricism is not the only kind of empiricism. There is therefore no need to ascribe to Marx a (wholly imaginary) special and original method of "constituting an object of inquiry" in order to save him from the charge of having been a (positivistic) empiricist. In fact--or so I firmly believe--Marxistic theorizing which has no solid empirical foundation is not "more sci-

entific" than "bourgeois empiricism"; it is simply vacuous.

If the importance of the above conclusions can be grasped in some of their dramatic ramifications, we should like to proceed with erecting the framework for an assessment of the main trunks of intellectual development with which to model such public opinion controversies as the nationalities question. Our choice for assessment is Q methodology, the only extant social sciences technique for reaching inductive discoveries without scientizing knowing.

Much has already been said of Thompson's chapter, especially pertaining to the issues of quantitative and qualitative evaluation of public opinion. There is, however, a nexus of other thoughts that need mentioning for the purposes of Q method. First, public opinion revolves around controversial issues, where there are nuances in possible courses of action and opinions that seem irreconcilable. Second, Thompson distinguishes between *status quo* (sovereign) and *real* opinions, which can be thought of by the Marxist scientist as *false* and *real* consciousness. Finally, there is a presumption by Thompson (1966: 9), just as there is in Q method, that genuine types are to be expected and it is not a case of the maxim, "Quot homines tot sententiae." All of this too is amenable to Q methodology.

METHODS AND PROCEDURES

As mentioned earlier, concourses are clearly an empirical matter and can be borrowed from every imaginable conversation or soliloquy, work of art, or scientific treatise. Likewise, concourses can be culled from news-gathering agencies, political speeches, books, the remembrances of tourists, and so on.

In approaching the formal representation of Q method to Yugoslavs, one naturally has in mind that the country is certainly the most important among Balkan states, the most interesting example of so-

cialist democracy in action, and perhaps more in need of fresh thoughts on the nationalities question than on any other issue. Especially our thoughts turn on the words of Vladimir Bakaric, who once again has highlighted the thought that has been repeated many times since the inception of modern Yugoslavia--and before, too: "Yugoslavia has two main problems--the economic reform and the nationalities question--and if the first cannot be solved, the second will immediately move to the fore as problem number one" (Robinson, 1977: 184).

(1) With this in mind, a rich concourse of statements could be drawn from the press and broadcast media, from scholarly journals and political treatises, and from diverse literature of the many republics, religions, and cultures. Our lack of skill with the Yugoslav languages has precluded this for the present, but since our attempt is heuristic, nothing has been lost.

A concourse has instead been drawn together from those writings appearing in U.S. journals, periodicals, magazines, and books during the past eight years. Typical of those writings is Claude Bourdet's "Yugoslavia: Experiments with Liberty" in the *Nation*, September 20, 1971. *The New York Times Magazine* yielded "After Tito--Who Can Keep Together the Serbs, Croats, Slovenes, Macedonians, Bosnian Moslems, Albanians, Hungarians and Montenegrins?" by Anatole Shub (1972), European editor for *Harper's* magazine, who has been visiting Yugoslavia regularly since 1962. Other contributions came from the books by Robinson (1977), Barton, Denitch, and Kadushin (1973) and Edelstein (1974). Even though the concourse is merely formal at this point, we have given our best effort to take south slavian sensibilities into account; as many statements as possible have come by one route or another from the Yugoslav press, Yugoslavian political and cultural figures, and the Yugoslav working people.

Taking all this into account, I nevertheless wish

to re-stress the obvious: These statements perhaps comprise a uniquely American concourse for the nationalities question; a concourse most suitable to Yugoslavians should come from *their* media.

(2) Under the described circumstances of collection, there will be (and, indeed, has been) a large number of opinion statements from the various sources. From these can be drawn a representative sample either at random or balanced to fit a Fisherian design. Stress is placed on the word *opinion* since the issue is not one of facts, but of subjective opinions. A statement such as "The web of union remains tough and resilient in young and kaleidoscopic Yugoslavia" is acceptable; "Today Yugoslavia has nine cities with over 100,000 population" is factual and cannot enter into Q method.

For the nationalities question we have easily parsed out more than 100 statements in a matter of hours and could have had many more. Examination of these led us after some cogitation to suspect that the opinion statements could *pro tem* be assigned to the categories in the table below. Design for the table was suggested by Robinson's (1977: 229) categorical observation on Yugoslavia, that "It is known that the country's information flow contains at least three different streams, political, economic, and cultural. But it is not known exactly what these flows contain or how they differ from republic to republic." Additional impetus was given by Matko Mestrovic's concept paper (for the journal *Prilozi*) which cited the vital importance of developing a fundamental theoretical approach for dealing with worldwide socio-economic, conceptual, political and cul-

Effects	Levels	
A. Information flow	(a) communication	(b) cultural
	(c) economic	(d) political
B. Weltanschauung	(e) federalism	(f) nationalism

tural turmoils. All this is represented in our design under "Information flow."

Although the first investigation gave us only a few statements ostensibly about the media role in the nationalities question, we accept this as a kind of poor vision that individuals and institutions exhibit in viewing themselves. Our recourse is to represent this possibility with statements from critical texts. This too is subsumed under "Information flow."

Finally, the world-view, or *Weltanschauung*, of prospective respondents is taken into account since statements could be either from a "nationalistic" or from a "federal" point of view. Thus, the logic of the design.

(3) Eight combinations of these effects are possible: ae, be, ce, de, af, bf, cf, and df. Every statement about the nationalities question can be placed into one of these eight categories on "common sense, presumptive, theoretical, or hypothetical grounds" (Stephenson, 1964: 269). "Political" (d), for example, is a *pro tem* categorization which would ostensibly refer to the political workings of the Yugoslav state. One might come down on either side of the issue, favoring "federalism" (e) or "nationalism" (f). In the same manner, each of the other "Information flow" effects--"communication," "cultural," "economic"--could lean toward "federalism" or "nationalism."

A characteristic statement to represent *df* (political-nationalism) might be: "Every people has the right of self-determination, including the right to succession"; for *be* (cultural-federalism), "Yugoslavia means 'Land of the South Slavs.' The South Slavs always conquer their conqueror. They love their homeland beyond price, having paid dearly for it"; and so on for each of the 4 X 2 combinations.

If we replicate the design *m* times--i.e., if we select statements to represent ae, af, be, bf, ce,

cf, de, and df each (say) $m = 6$ times--the Q-sample size is $8m = 48$. Were the objective framework our chosen guide to investigation, the assumption would have to be made that the statements are of general significance. This would usually be followed by an effort to study the "effects," or individual differences, of the various combinations, all of which is a matter of deductivism, i.e., using variance analysis. However, with the inductivism of Q (employing factor analysis), the Fisherian design is merely a reasonable aid in making a beginning. All categories of a design must later yield to the operant factor structure derived through empirical investigation.

(4) Whatever the method chosen to determine the statements entering a Q sort, the selected statements need to be typed or written on cards, shuffled or otherwise randomized, and assigned a nominal designation--a letter or number for each will suffice. The next step is to have appropriate respondents provide a Q sort of the opinion statements according to a "condition of instruction," which is a surrogate for a hypothesis (Stephenson, 1976: 262-263).

Conditions of instruction for the present study are drawn from Thompson's hypothesis that there are both *status quo* and *real* opinions, or, to use the appropriate Marxian terminology, *false* and *real* consciousness. To bring forth a *status quo* opinion, it will usually be enough to provide the following condition of instruction:

> (a) Give an account of *your* understanding about .... (e.g., the nationalities question).

To measure *real* opinion requires something different, one such way being to request a Q sort with the following condition of instruction (borrowed from Stephenson, 1964: 270-271):

> (b) What, in your view, would be the *ideal* situation *vis-a-vis* the controversy about.... (e.g., the nationalities question).

Whether the two Q sorts, or two sets of Q sorts, correspond is an empirical matter to be brought out by factor analysis.

(5) Wherever R method and the objective have taken precedence over Q and the subjective, sampling of a population of persons has generally been arrived at through random or representative techniques. In Q, Stephenson (1953: 62-85) has followed the small-sample line of reasoning employed by experimental psychology, except that he selects persons according to the *interests* he wants to represent. This allows him to use no more than a few score respondents when others would need many thousands (Stephenson, 1965). Since our present study is concerned with evaluation of Yugoslav public opinion as regards the nationalities question, we will follow the five main segments of interest that Thompson (and Stephenson more exactly) distinguished in relation to a controversy:

A. People with a *special interest* in the controversy (e.g., Yugoslav business managers who might stand to lose money and power in an upheaval, or leaders of the Catholic, Moslem, or Orthodox Churches).

B. *Experts*, persons of maturity, broad education and experience, who, given all the pertinent facts on the controversy, are asked to form a dispassionate judgment. President Tito might fit this category as might certain other statesmen and the Praxis-Marxist philosophers.

C. *Existing authorities*, those who would take it upon themselves to speak *for* one side or the other of a controversy. They speak as though they know for certain what the truth of the matter is. Theoreticians and politicians from each of the republics and provinces might speak on either side of the issue.

D. *Class interests*: This can be construed in something closer to the Marxist sense to include

working people (proletariat), peasants, and perhaps technocrats.

E. The *uninformed*: This might be considered a control for the other interests. Children, the very poor, or shut-ins may be of this category. Edelstein (1974) has, of course, pointed out that many surveys turn up opinions when respondents do not have any. These persons would also be suitable.

A set of persons (P set) to represent the various interests to deal with the question at hand is suggested:

Interest	*No.*
Business managers: two from each republic and autonomous province	16
Church officials: one from each, for each of the three religions	24
President Tito: if impossible, someone of stature who is believed to hold similar views	1
Praxis Marxists: one from each republic and autonomous province	8
Authorities: two from each	16
Proletarians: one from each	8
Peasants: one from each	8
Technocrats: one from each	8
Uninformed: two from each, either children, poor, shut-ins, or others	16
	105

Like statement categories, "interests" are accepted *pro tem*. If it should turn out that these categorical interests--featured as they are in the works of Hondius (1968), Shoup (1968), Fisher (1966), Farkus (1975), Rusinow (1977), Dedijer et al. (1974), Barton et al. (1973), Edelstein (1974), and Robinson (1977) --fail to hang together, this is all to the scientific good. Our concern is with genuine trunks of opinion, not *a priori* categorical types. It matters

not a whit if none of the *a priori* interests are cohesive, nor is it to be expected.

(6) Stephenson, in an attempt to assist those studying opinions, beliefs, attitudes, and the like has suggested the mnemonic: "Opinions are used as items of a Q-sample; attitudes are modelled by Q-sorts and factors; beliefs are explanations of the latter" (1965: 281). Remembering that the issue at hand in Q is organized subjectivity, not *a priori* categorization, these designations can be useful in communicating with respondents whether lay or expert. One can also relate them, for similar practical reasons, to paradigms, themata, schemata, hidden structure, or the like, as long as the categorical nature of the designations is not banished from memory. But basically, the focus remains on the main trunks of Yugoslav opinion as regards the nationalities question.

Perhaps a by-the-by observation on the relative occurrence of opinions, attitudes, and beliefs would be of use. Stephenson (1965: 286) provides a most laconic one for us:

> Thus, *opinions* by our definition are as numerour as the waves of the sea, *attitudes* of mind are considerably fewer, and *beliefs* are few indeed.

In any event, each respondent is requested to represent his "attitude" (or feelings) about the particular Q sample according to the selected conditions of instruction.[6] Q sorts performed under each condi-

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[6]Sorting might be accomplished according to the following forced frequency distribution:

	disagree					*neutral*				*agree*	
Score	-5	-4	-3	-2	-1	0	+1	+2	+3	+4	+5
Frequency	3	4	4	5	5	6	5	5	4	4	3

Many other possibilities will work as well. Instruction sheets and further information, including the

tion can then be correlated and factor analyzed, yielding a number of common "attitudes" or schemata held by several clusters of persons.

Factoring 105 Q sorts (and this could be as few as 30-50) can be expected, by both theory and practice, to produce only a handful of uncorrelated "attitudes." No more than a few idiosyncratic Q sorts are to be expected, and the factors will usually be *invariant* (Stephenson, 1964: 271).

There is one additional step that can--and perhaps should--be taken before the Q sorts are factor analyzed. To the 105 Yugoslavian Q sorts, we would recommend the inclusion of one more. It is quite a usual matter in Q method to have the researcher represent his personal position vis-a-vis the Q sample for reasons hitherto excluded from sampling methodology. Let us explain this purpose with a quote from Stephenson (1976: 392):

> [Providing an author's Q sort] is done in no sense to pit oneself against the authorities whose views have been abstracted, but for a technical reason. It is important in factor analysis to use James' Law to separate factors which are "me" from those which are "mine": The primary interest...is in those factors shown not to involve one's own understanding of one's position. Ordinarily, any factor involving the latter would be suspect, unless, of course one happened to have grasped a position missed by the authorities that appears (with some support from one or another of them) to be innovating and a new ideal type.

(7) Analysis of the data requires many complex steps for the beginner, but these can be likened to the months of necessary training needed to operate an electron microscope or an observatory telescope.

48-statement Q sample on the Yugoslav nationalities question, can be obtained from the author.

Training is much facilitated by new computer technology and programming, which must remain subordinate to the grasping of genuine operant forms. More detail of this process is given elsewhere (Stephenson, 1976).

(8) Interpretation of the factors is still to come and requires combining the biographical information we have turned up with pertinent statements and observations from published writings. Interpretation is also broadly advanced by making note of statements which differentiate factors and of those scored most highly by a factor.

It is quite unnecessary and perhaps impossible for anyone to explain all that comprises factor interpretation, yet the fits and starts demanded by such a method must be carried through until one grasps the "form" of the factor. Much depends, it is clear, on the knowledge and skill of the researcher. Nothing exceptional as to biography, writings, or statement scores should be passed over in coming to such understandings; analysis is a lowly serf and allowed no commutation from the demesne of synthesis. One must seize the meaning of trunks (organized forms) of socialist opinion in order to open oneself to possible lines of intellectual growth and significant discoveries.

A DRAWING TOGETHER

In erecting our formal model around the long-standing nationalities question, we have attempted to lay the basis for making communication, public opinion, and all other forms of social science come into harmony with socialist-journalism by bringing them under the "paradigm" of subjectivity and by providing an empirical methodology. Our agreement with Lubasz is evident; empirical Marxism is not only acceptable, but actually demanded.

In a larger sense, we have merely arrived anew at the commencement point in the gyre of dialectical research, a gyre that seeks a fertile alliance between

technical scientific procedure and an open-ended speculative inductivism. Similar seeking has long been evident in the field of "communication" although more often than not the gyre has been superseded by an open-ring or a closed-circle. Criticism along these same lines goes back at least as far as Berelson's (1959) "withering away" remarks and MacLean's (1966) "Frontiers of Communication Research" paper.

Among the best and the brightest of the newest generation of communication researchers, Nordenstreng (1968)--a socialist-journalist in his own right--has wielded a far more fire-tinged sword against the hyperscientific, theoretically imprecise, and conceptually barren state of affairs within American communication research. He specifically points up the poverty of the objective behavioristic tradition, the overgrowth of technicians and technique, and the widespread disregard for any sort of conceptual thinking or speculation. Nordenstreng's parrying is extremely spry and accurate, cutting and burning to the heart of the matter, but perhaps too often for the sake of one's own skin. At any rate, there is but little doubt that his observations are well-founded and in the right direction. Unfortunately, Nordenstreng's criticism ceases at this point with a comment on the "pathological" tendency of the American communication research tradition, which to his mind also includes Europeans following the American pattern.

It perhaps did not occur to Nordenstreng that a scientist must do more than provide an insightful analysis of a problem, that he must offer a methodological-theoretical framework that will allow a scientific approach to the subjective, normative, ethical, ideological, speculative, and philosophical thinking which he so highly and rightly regards.

Just such an attempt was made by Lazarsfeld with his theory of complementarity and his strivings to marry administrative communication research to the critical theory of the Frankfurt School (McLuskie,

1975). Early on, Lazarsfeld had recognized that analytical reflection is incomplete social theory while co-ordinately proposing that there should be administrative restraints to pure speculation. Like his colleagues, Berelson and MacLean, Lazarsfeld thought that each form of social theory--the objective and the speculative--should aid the other. Methodology was to be the bridgebuilder between them and would be employed to study the social scientist--more properly, the sociologist--at work. All of this was part of Lazarsfeld's career-long desire to bring about a unification of science through an interpenetration of objective facts and human values (Lazarsfeld, 1972).

Our best account to date indicates that Lazarsfeld's attempt came to naught and that he failed because his empirical methodology ignored the dialectical gyre between theory and practice, rendering the speculative (or hermeneutic) aspect into a technical closed ring. In effect, Lazarsfeld's hope to enrich empirical research with critical theory culminated in the swallowing up of interpretation inside the belly of categorical analysis.

It is a profoundly sad irony that Lazarsfeld could long ago have had what he wanted by turning to Q, an empirical methodology that models but does not scientize knowing, that reaches understandings not explanations, that for the first time joins knowing and doing together in a social science research "paradigm." So it is that Berelson can come back once more to "communication" research and Nordenstreng can assist in healing the pathology of his colleagues with the rich concourses derived from subjective, normative, ethical, ideological, speculative, and philosophical musings.

For the Yugoslavian socialist-journalist, the implications are quite clear. He must go beyond his patrimony of mass communicator to become a theorist and "communication" researcher as well. The tools have been provided and the way is open.

However, it is only fair to note that underlying Q methodology is a value theory, *not* the general-systems-efficiency-information theory that now pervades social science. Human beings--whether laymen or scientists, businessmen or proletariat--are at the center of such a theory, and there are no experts to pass judgment on *a priori* "scientific" grounds as to what is logically right, good, proper, and truthful.

It is our understanding that the form of the present conglomerate of social sciences will hold no further weight or growth for it has reached an evolutionary deadend. A return must be made to the last level of stability so that the main branch of growth may be joined. At the highest levels of physical science, human mind and human values have been brought into the process of knowing. No less an accomplishment should be accepted as the standard for the present generation of socialist-journalists.

Leonard J. Barchak, Department of Human Communication, Van Dyck Hall, 101E, Rutgers University, New Brunswick, NJ 08903

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The psychologist who is accustomed to dealing with fifty or a hundred or a thousand organisms may be disturbed by groups limited to four or eight. But large numbers of cases are required, if they are required at all, in order to obtain smooth and reproducible curves. The recourse to statistics is not a privilege, it is a necessity arising from the nature of many data. Where a reasonable degree of smoothness and reproducibility can be obtained with a few cases or with single cases, there is little reason, aside from habit or affectation, to consider large numbers. (B.F. Skinner)