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Coherent Social Groups in Scientific Change

"Invisible colleges" may be consistent throughout science.

Belver C. Griffith and Nicholas C. Mullins

Recent studies have revealed a great deal about the communication and organizational patterns that underlie major advances and changes of direction in science. An important feature of these patterns is their consistency throughout a variety of disciplines, periods of time, and types of research. Biologists on Long Island in the 1940's, ethnomethodologists in southern California in the 1960's, physicists in Copenhagen in the 1920's, and mathematicians in Göttingen in the 1900's acted in very similar ways when, armed with insights radical for their times and disciplines, they faced important scientific problems. Our data on these and other such groups suggest that a single set of social mechanisms evolves in response to the challenge posed by new and major scientific problems. When challenged, some members of a scientific specialty become organized to work toward certain objectives, voluntarily and self-consciously, as a coherent and activist group. This article examines findings from surveys, individual interviews, and biographical essays, and discusses the similarities among contemporary groups that developed into small, coherent, activist groups and that subsequently had major impacts on their "home" disciplines.

Low Levels of Organization and Communication

Communication and some degree of voluntary association are intrinsic in science, and the important question therefore becomes not whether scientists organize, but rather how, why, and to what degree? As a background to understanding high degrees of communication and organization, the first section of this article examines the processes entailed in the "loose" networks, the level of communication and organization that appears normal for science. This level has been repeatedly demonstrated by different methodologies for specialties in various disciplines.

Three groups of psychological researchers studied by Griffith and Miller exhibited this effective, loose communication network (*1*). The workers in these groups had considerable knowledge of the activities of other major researchers, and it is clear that individuals sought out, and interacted very effectively with, one another on the basis of their current research interests. Mullins' data on biologists and Crane's on rural sociologists also show this kind of loose communication network. In both of these studies, respondents named more persons outside their specialty than inside as having had significant effects on their work, thereby suggesting that scientists work in and influence more than one specialty, an apparently normal condition for highly active scientific researchers (*2*). In addition, the Crane study showed that productive scientists were more frequently named as the object of contact, indicating, as expected, that communication is more intense around productive researchers. This finding was later quantified by Griffith *et al.* (*3*).

The Griffith and Miller study (*1*) focused upon persons who were comparatively productive, each of whom headed a team of students and junior colleagues. These respondents employed several special strategies to facilitate information exchange within their specialty. For example, one area, speech perception, was small enough that few communication problems developed, even though the area exhibited low levels of social organization. Of the other specialties studied, those that exhibited loose networks employed, for varying periods of time, mechanisms that are used in highly coherent groups (for example, conference series and exchanges of papers before publication). However, the adoption of a pattern of communication within these specialties was in response to a current scientific problem and to the inadequacy of formal meetings and journals for answering communications needs created by these problems. For example, psycholinguists seemed to develop different patterns of organization depending upon whether they were in the process of applying psychological theories and methodology to studies of language (as they did in the early 1950's) or modifying linguistic theories so they could be used in experimental psychology (as they did after the development of generative grammar). By contrast, research-

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ers in the effects of drugs on behavior formed a small specialty in which communication patterns reflected the activities of individual researchers, and the membership of groups in close communication changed continually in response to changes in research interests. This group was particularly well served by journals; thus informal contacts were not supplementing or replacing publications.

Crawford's sociometric analysis of data on sleep researchers revealed a network of research centers, each organized around one or more key researchers. The operation and effectiveness of such networks is illustrated by the finding that nearly 95 percent of all researchers lay within two sociometric links of such a key researcher (that is, across only two continuing, person-to-person contacts) (*4*).

These findings seem no more than would be expected because of the substantial size and efficiency of circles of acquaintanceship that exist throughout science. These circles are encouraged and developed within science by mechanisms such as meetings and conferences. Their efficiency as a basis for communication is partly a result of science's institutionalization into academic disciplines and scientific societies and partly a function of the comparatively small size of the scientific community.

To obtain a more precise idea of the effectiveness of such patterns of acquaintanceship, the active researcher might try the following experiment: first, consider how many persons he knows personally who are doing work in any way related to his own. Second, try to use organization figures to generate the total number of persons working in his discipline, then the fraction of those doing related work, and, finally, the minority who are extremely active and who contribute most to the literature. We believe most researchers, whether in the center of a highly active specialty or in a backwater, know not only their immediate competitors and collaborators, but also a vast peripheral group of other researchers (perhaps, for a mature scientist, 600 to 1000). The overlap of these many circles of acquaintanceship offers a basis for rapidly developing loose networks as temporary mechanisms for the transmission of findings and ideas (*5*).

We can regard such loose networks of researchers as resulting from "normal" scientific activities and as conforming generally to scientific norms of objectivity and emotional neutrality. A disciple of Kuhn might regard the groups that have been studied and that meet this description as working to fill out existing paradigms. If one conceptualizes the scientific community as linked by acquaintanceships, extending in all directions from individual specialties, loose networks meet researchers' needs for information and feedback by temporarily focusing communication on current research activities.

High Levels of Organization and Communication

The highest levels of communication and organization are achieved by groups that are in the process of formulating a radical conceptual reorganization within their field. Members of each of the six groups discussed in this article appeared to be convinced that they were achieving the overthrow of a major position within their discipline or making a major revision in methodology. The groups that have been studied and that exhibit such characteristics are the phage workers in molecular biology, the Skinnerian psychologists, the quantum physics group in Copenhagen, the Göttingen mathematicians, the audition researchers in psychology, and the ethnomethodologists in sociology (*6–9*). Each offered a distinctively different theory or a new or modified research methodology in opposition to a clearly established position; each maintained its beliefs over a protracted period; and each ultimately demonstrated substantial achievements (*10*). In the process, none of these groups consistently observed the attitude of disinterested objectivity that is regarded as a norm of science; indeed, these groups often ventured actively into the politics of science in order to obtain or protect appointments and research support. Most important, each of these groups operated through close and continual interaction on scientific issues; for example, when an audition researcher was asked whether he and others in his field exchanged preprints (prepublication copies of papers), he said that such exchange was usually unnecessary because they followed one another's work so closely that often a single, newly found constant sufficed to inform others of an important advance (*11*).

Table 1 shows some of the common features of these six groups. Among these features are the presence of an acknowledged intellectual and organizational leader or leaders, a geographical center, and a brief period of comparatively intense activity. As might be expected, the degree of organization was not uniform throughout these groups. The more highly organized groups were truly "revolutionary": they saw themselves as opposed to a definite outgroup that was better established than they were within universities and recognized disciplines. The three groups best described as "revolutionary" were the Skinnerians, the phage workers, and the ethnomethodologists. In operation, the three revolutionary groups differed somewhat from the remaining groups, which were recognized as being of central importance to their respective disciplines even while they were developing (usually within major universities) and while their interests and patterns were diverging from those of other research workers. The contrast between these two groupings of the six specialties is broadly analogous to that between revolutionary cabals and self-conscious elites.

Theoretical Break

An announced or evident break in a given theoretical framework can be an important social mechanism, particularly for the revolutionary groups, because it erects a barrier between those who accept it and everyone else. It provides the basis for separating groups into ingroups and outgroups and helps ingroups to develop high morale, which assists them in opposing formidable outgroups. For the phage group, biochemists were the outgroup; for operant conditioning, it was Hullian learning theorists. The histories of biochemists' references to molecular biologists, sociologists' to ethnomethodologists, and learning theorists' to operant conditioners show similar and considerable acrimony.

In addition to sounding a battle cry, a theoretical break provides personal motivation for fellow scientists. For example, Delbrück's 1938 paper "The one-step growth process in *E. coli*" (*12*) had major effects on many persons, such as Anderson, Luria, and Weigle, who later made important contributions to phage genetics. Skinner's *The Behavior of Organisms*, first published in 1938 (*13*), constituted a striking break with stimulus-response learning theory and was, for many years, a doctrinaire support for operant conditioning. A parlor game among adherents in the late 1940's was to furnish, on request, the page number of brief, recited passages (*14*). Skinner's other works both extended his approach and set forth a utopian version of his approach as applied to human society.

Leadership Roles

Highly coherent groups organize themselves consciously, usually under the leadership of a person who is actively emulated (that is, a person who serves as the scientific model). While many of the persons occupying this role have had exceptional intellectual and personal skills that seem to explain their special position within the group, the existence of such a leader may not be necessary for creating a highly coherent group. The reservoir of talent within a dedicated group of productive scientists may be great enough that leadership will appear in response to the needs of particular situations. Independently of whether a single person serves as a scientific model, two different leadership roles, intellectual and organizational, can be identified in the formation and maintenance of these groups and these roles may or may not be filled by the same individual.

The intellectual leader (or leaders) (i) lays the original conceptual foundations for work, (ii) makes public statements on theory and research, which normally result in an acknowledged theoretical break, and (iii) approves and validates others' work. Moreover, an intellectual leader frequently functions as the central communicator within the group, although this role, which is partially a social one, may be dispersed among members of the group. The phage group most clearly exemplified this pattern of leadership, although there was an identifiable intellectual leader for some period in all of the groups studied. In the phage group, Delbrück performed all leadership functions, strongly suppressing some lines of research while pushing other work forward. His opinions were widely sought within the group; he established research priorities, and he standardized techniques and quantitative models. His own original research was superior and among the first efforts in the new area (Table 1).

The organizational leader arranges times, funds, and facilities for research and means for communicating findings and ideas. He arranges appointments in such a way that specific scientists obtain jobs in specific locations, organizes research programs and obtains funds, and guides the organization of meetings (Table 1). In all cases, the organizational leader, if different from the intellectual leader, was a respected researcher in his own right.

Findings generally reveal a conscious effort to direct the group's work toward a specified series of problems, from a particular perspective, with a stated goal. Such an effort may begin after a few discoveries or ideas and may be largely triggered by individual genius or other isolated events (that is, events that would resist deliberate programming). Nevertheless, a conscious effort on the part of a leader or cadre of students seems to be required for an activist group to develop and continue for any length of time. The organizational leader largely directs and implements these efforts.

The focusing of the group's attention on a single series of phenomena and the development of a distinctive scientific style resulted, in most of the groups studied, in considerable restriction of the range of information regarded as relevant, with corresponding changes in the input of information. If we think of the highly coherent group in terms of a network, we can regard these groups as an extreme thickening in the network, with a resultant loss of some links to the remainder of the network (if we assume that individuals have some upper limit to their communication activities). Needless to say, if the highly active, coherent group is working on basic problems pursued by many others within the discipline, their general indifference to the work of other researchers can generate considerable antagonism. The revolutionary groups (the Skinnerians, phage workers, and ethnomethodologists) have, of course, been extreme in terms of isolating themselves from the remainder of their home disciplines.

Table 1. Common features of coherent activist groups. Mode of group operation describes relation to home discipline: revolutionary groups are in opposition to a better established group, while elite groups are divergent but recognized as central to the discipline.

Group	Intellectual leader	Organizational leader	Place(s)	Approximate dates (inclusive)	Field	Mode of operation
Quantum mechanics (Copenhagen)	Bohr	Bohr	Copenhagen	1920–1934	Physics	Elite
Phage	Delbrück	Delbrück	Cold Spring Harbor, California Institute of Technology	1947–1958	Biology	Revolutionary
Algebraists (Göttingen)	Hilbert, Minkowski	Klein	Göttingen	1896–1910	Mathematics	Elite
Operant conditioning (Skinnerians)	Skinner	Cadre of students and postdoctoral fellows (Harvard)	Columbia, Harvard	1947–1960	Psychology	Revolutionary
Audition research	Stevens	Stevens	Harvard	1940–1950	Psychology	Elite
Ethnomethodology	Garfinkel	Cicourel	University of California at Los Angeles, Santa Barbara	1965–1971	Sociology	Revolutionary

Geographical Center

Speculations that it is important to have a "critical mass" of research at a single location seem to be borne out by these data; a factor in the development of highly coherent specialties is a geographical center or centers—usually one, but no more than three. For every specialty studied there is a specific place or places at which its first work was done and at which the group worked exclusively for an extended period of time. For instance, the phage group showed a specific, stable migration pattern involving two locations: the California Institute of Technology was the winter location, Cold Spring Harbor, New York, the summer one.

For that part of quantum mechanics described as the Copenhagen School, the center was Bohr's institute in Copenhagen. This center has literally been immortalized in song and story as the birthplace of much of modern physics. The mathematics group's center was the Göttingen mathematics department, an imposing group assembled in 1899 around Klein, the department chairman, and including Minkowski and Hilbert. These three became the nucleus of a walking seminar that met on Thursday afternoons and in which the outlines of much of what is now regarded as modern mathematics were thrashed out. Operant conditioners developed at Harvard, Columbia, and (on the postdoctoral level) the animal behavior laboratories at Walter Reed Hospital in Washington, D.C., with orderly migration among these centers. Audition research was concentrated in Cambridge, Massachusetts—first within Harvard, then spreading to the Massachusetts Institute of Technology, industry, and other institutions; separate centers developed later around Harvard Ph.D.'s in the Midwest. Ethnomethodologists until recently were concentrated in Southern California at the University of California at Los Angeles and at Santa Barbara.

Recruitment

Recruitment of able new members is a major activity of these groups; because mature investigators outside the group have other commitments, recruitment generally proceeds among the young—mostly among graduate students in the groups studied. The more revolutionary groups emphasized the encouragement of younger researchers, easy access to leaders, and democratic procedures within the group. For example, the operant conditioning researchers confined the choice of an editor for a new journal to younger persons outside the central cadre.

The importance of these activities is clear: groups without students die. Fisher makes this clear in his analyses of the mathematical specialty of invariant theory; this group disappeared because its members had no access to students for training in the specialty (*15*). The problems did not die; they still exist, but they have been reshuffled and have become problems in other, living areas (for example, linear algebra). It is interesting to note that Hilbert, a leader at Göttingen, was once a major figure in invariant theory, but his 69 students were all trained at Göttingen after his work in invariant theory was over.

Delbrück was a noteworthy innovator at recruiting students. Most academic scientists seek settings with graduate students and teach those who come their way. In turn, their students teach other students, each new (student) generation being somewhat larger than the previous (teaching) generation, thus setting up the necessary and sufficient conditions for exponential growth. Although the length of the generation is very short in terms of years, initial growth is slow and a period of nearly 15 years is required to develop "explosive" rates of growth. [Because of limits on the population available to be drawn into any specialty, as well as other factors, the actual pattern of growth conforms generally to epidemiological models (*16*).] Delbrück short-circuited the initial slow growth period by instituting the summer phage course at Cold Spring Harbor directed at scientists who already had students. As a result, 35 people were already working on phage problems by 1950, as compared to four in 1945. Had they relied on normal student recruitment, they would have taken until 1960 to generate this level of activity.

Intellectual Processes and Dynamics

Several of the groups studied revealed the importance in the group's continued existence and development of a single, definitive program statement. This statement delineates a specific problem and a direction for the group; it defines research goals and identifies the basic steps toward that goal. Schrödinger's *What Is Life?* (1944) (*17*) was clearly phage work's first such programmatic statement. Delbrück's 1948 address to the Connecticut Academy of Sciences, "A physicist looks at biology," constituted a second (*12*). In both cases, the statements indicated that phage workers wanted to understand how biological information was transmitted from generation to generation. Mathematics found a complete, sparse, direct presentation of a program in Hilbert's 1900 speech in Paris. Hilbert stated the 23 problems in mathematics that needed to be solved and how these problems fell into groups, thereby providing a focal point not only for the Göttingen group, but also for a great deal of mathematics from that time on (*18*). Skinner's *The Behavior of Organisms* (*13*) laid down the structure of a new approach to behavior and immediately attracted wide attention. However, the development of the Skinnerian group was apparently delayed both by the war and by the absence at that time of appropriate organizational leaders.

During an initial period in the development of each group, significant communication within the group was verbal; such free, unmonitored exchanges of information about unpublished results and ideas required powerful norms to protect the individual's priority of discovery. While certain precautions are implicitly observed throughout science, highly coherent groups exhibit particularly strong controls. Production of reports and articles was controlled in ethnomethodology from 1966 to 1970, becoming virtually independent of current research activity. The research front can come to exist in the private exchanges of group members. One significant phage paper was not published for years to protect priority; among audition researchers, a creative nonwriter was pushed into being the senior author of a paper that others wrote to present his theory. With much of the group's activity consisting of the review and evaluation of research and theory, individuals may rapidly develop different public and in-group reputations as a result of the controls over research and publication and the general group involvement in evaluation. In view of the strength of the tensions within activist scientific groups and the degree of members' involvement, it is not surprising that these groups generate "tribal folklore" and customs, including distinctive lifestyles, mock ceremonies and awards, special ingroup roles, and even, on occasion, a group sport (for example, camping for the phage group and rock climbing and Ping-Pong for quantum mechanics).

Discussion

Communication and the exchange of information are intrinsic in the normal operation of science as an endeavor that produces and processes information. In our examination of highly coherent groups, we see two additional factors as basic to science: first, the radical revision of scientific theory and method (and the consequent reexamination and reinterpretation of data); second, the rarity of high levels of personal creativity. Our understanding of these factors has been extended recently through the analyses of science made by Kuhn (*19*) and Price (*20*). Kuhn has emphasized, following Butterfield (*21*) and Koyré (*22*), the conceptual reorganization required in major scientific advances (*23*). His central concept of the paradigm, the base from which "normal" science is pursued, contains elements of both scientific theory and methodology, as well as large attitudinal components—for example, untested assumptions as to the outcome of alternative lines of research. As Ziman has pointed out, even accepted scientific findings and theories rest in part on faith, given the near universal absence of logical proofs in science (*24*). Price proceeds differently, regarding science from a distance and seeking to describe the structure of science in terms of aggregates of people, monies, and results. In his analyses, elites of persons, institutions, and nations have clearly emerged.

Of these two approaches, Kuhn's is the more attractive. He is reassuring in his picture of all scientists as workers in the vineyard, filling in missing bits according to paradigm or, alternatively, finding chinks in an existing system. Price's ideas are rather harder to live with; the discrepancy he noted between the incredibly high activity level of the most creative scientist, as opposed to that of journeymen, has become, in his latest papers, a direct concern with elites and elitism and the difficulties they create in science (*25*). The bridge between these approaches may be found in the fact that coherent activist groups are, in many cases, the producers of change in science and the social location of creative scientists.

The aforementioned factors contribute in several ways to the creation of coherent groups. The difficulties of the conceptual reorganization required by major advances preclude mass conversion to any new points of view. An idea must gradually recruit adherents, such recruitment usually being expedited by active proselytizing; this process requires high degrees of communication and social organization to succeed. The work of the minority of highly active, highly creative scientists attracts other researchers; this process affords a natural basis for intellectual leadership within groups, a focus for communication, and a natural starting point for the coherent activist group. All of these factors reinforce the basic and direct role of communication of findings and ideas in science; given their presence, the conscious development of a distinctive scientific approach marks the takeoff point for the coherent group.

All groups we regard as coherent had a distinctive approach with broad theoretical implications (*26*). Thus, a key factor in determining success or failure may well be whether a group is proceeding only empirically (that is, simply moving from problem to problem within the context of the established theoretical framework) or whether its program proceeds from an innovative theoretical base. For example, an overemphasis on strict empirical procedure in biochemistry prepared an area for the innovations of molecular biology; similarly, crystallography followed empirical leads, never making a break with its past comparable to the break that originally established it as a field.

One necessity for the development and continuance of a coherent group is simply the practical one of acquiring sufficient resources to sustain a sizable group of researchers at the geographical center. Prior to the heavy governmental support of basic research, Klein in the 1900's and Stevens in the 1940's were adroit at raising research funds; the group in Copenhagen was largely supported by a special award to Bohr from the Carlsberg Breweries. Unfortunately, much of science has been done in situations that would prohibit maintaining such critical masses of researchers at single locations. Molecular biology had not only theoretical orientation and financial support, both in extreme degrees, but also other characteristics that create conditions in which a highly coherent grouping could develop in its most exaggerated form. These include the maverick nature of its leadership, the continued existence of major scientific challenges (and consequent intense competition), and the capacity to perform experiments in astonishingly brief periods of time.

There probably exist, at all times, groups within the network of science that experimentally attempt and then retreat from this role of the coherent activist group for a brief period. This retreat may be occasioned by a variety of factors, the chief of these probably being low scientific vitality or low distinctiveness of the group's interests and its work, as compared to the remainder of the discipline. However, even successful groups seem to have a comparatively limited life span; 10 to 15 years is typical, and many last for shorter periods. Some groups, as, for example, the audition researchers, largely succeed in obtaining the scientific goals they have pursued; however, even the most expansive groups must ultimately grow so large as to dilute personal ties and influence. Institutionalization (for example, creating a department of molecular biology or a departmental slot for an operant conditioner) reduces levels of antagonism and group identity and generally marks the successful group's return to the normal, loose networks of science (*27*). Thus, the penalty of success, whether success is measured in specific goals or in the conversion of a discipline to new points of view, is the death of the group as a distinct social and intellectual entity.

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25. D. J. De Solla Price, *J. Amer. Soc. Inf. Sci.* **22**, 74 (1971).
26. Skinner and his followers doggedly rejected behavioral theory; their rationale was that it offered no explanatory advantage over operational formulations of psychological laws—a rationale that, in itself, is a major theoretical stand. In general, we speculate that these groups are either, as Butterfield would contend (*21*), picking up the other end of the stick (that is, viewing findings from an entirely new perspective), or creating a methodology through which a variety of new problems can be stated and examined. Quantum mechanics and molecular biology seemed to embody both aspects clearly. The audition researchers, alone among these groups, appear to have been mainly introducing a more sophisticated methodology, and it is curious to note that, around 1940, Delbrück was teaching biologists about mathematical tools similar to those the audition researchers had recently adopted.
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28. We thank Marilyn Jahn, A. James Miller, Carolyn Mullins, and Beth Krevitt for contributing in many ways to the preparation and writing of this article. Also, we thank M. Carl Drott, William Garvey, Dorwin Cartwright, and Georgess McHargue for reviewing this article. B.C.G.'s work was supported by Public Health Service research grant 1 RO1 LM 00911-01.

Paradoxes of Science Administration

Thomas A. Cowan

In the course of an analysis of the nature of the mind, the philosopher Hegel calls attention to a deep paradox which attends all human effort. I might risk putting it somewhat as follows: By dint of superior effort, by a stroke of good fortune, or perhaps by the exercise of chicanery, one man becomes boss over another. From the point of view of the boss this looks like a happy or at any rate a superior position; the "worker" is an inferior. But then a peculiar thing happens. Time and again, the boss begins to deteriorate as a human being, and the worker gains in moral stature. Apparently, what dignifies human effort is the work itself. The loafer, the shirker, the time-server, be he boss or subordinate, pays for his dereliction in moral degeneration.

I should not like anyone to think I am here referring to the so-called Puritan ethic, the doctrine that morality is encompassed in cheerless and dogged attention to duty. What our philosopher was examining was not any specific creed dedicated to success or gain, whether spiritual or material. He was investigating the nature of the human being himself and reporting on a universal phenomenon. Some men are superior to others. This is a concrete, objective state of affairs based on discernible productivity—physical, mental, moral, and esthetic. Superiority may begin in natural endowment, but it takes sustained effort to maintain. The boss may quickly become the slave of his own workers, of his own community, even of his own image of himself. These humble truths are so well worn that it may be puzzling to imagine what new grist can be extracted from them. For one thing, a question.

Is the dialectical process, this paradoxical turn and turn about, applicable to the community of scientific workers? Or, on the contrary, is there something about the nature of scientific activity that exempts it from this human perplexity? Is science such a self-purifying activity that one need not worry about dominance-servience effects? We should hesitate to say that this is so. But are there nevertheless certain processes at work in science, such as the freedom with which scientists select or elect themselves to membership in the scientific community, that guarantee exemption from the common fate? This alternative is very tempting. For it is true that the scientist does elect himself. We do not have press-gangs shanghaiing scientists for work in the scientific salt mines.

Scientists do join up freely. How then can it be said that they are constrained by the laws that govern the exaction of slave labor? The chapter in Hegel's *Phenomenology of the Mind* to which reference has been made is entitled "Lordship and Bondage." In science, however, we are not dealing with peonage.

True. But I think the old philosopher might have something further to say. He was asking himself how men might feel constrained to work and yet feel free or, as he put it, come to know

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