edge, and my emphasis upon the advance of science here stems from this fact rather than from any implicit assumption that progress is inevitably good in itself.)

It is on these grounds, then, that I think the uninformed planning for government's relations with science could as sily injure science as encourage it.

POSSIBLE RESPONSES TO THESE THREATS

To counter this possibility, there are three points of policy which I think might well be built into the fabric of Federal science policy. They are all concerned in one way or another with protecting the incentive-system of science, and thus the values of science.

First, it should be one aim of science policy to develop enclaves within science where professional recognition retains its central importance and where research choices continue to be made solely on the basis of scientific interest. This is essentially the problem of insulating a fairly large number of scientists from the "temptations" dangled before them by the rest of society. The emergence of what Price calls "the new Invisible Colleges"—groups of between 50 and 200 men who exchange preprints and generally constitute the elite of a given field—is one way of meeting the need for such enclaves.

Certainly the establishment of many more Career Research Investigatorships, along the lines now being explored by the National Institutes of Health and a few private health foundations will be a step in the right direction. The creation of a sufficient number of these position would assure each recipient of an adequate audience for his work

Second, in order that scientists may more easily identify the audience that will be particularly interested in their work and may communicate directly with this audience, there should be no obstacles raised against the identification of new fields and the publication of new journals in these fields. Rather than having one pond so large that most fish despair of ever being noticed, it would seem better to work toward the development of more and smaller ponds, in each of which recognition will be somewhat more easily attainable. This policy might indeed lead to further difficulties in interdisciplinary communication and in information retrieval, but it would seem preferable in the long run to aggravating a situation in which the values of science themselves may be seriously weakened.

Finally, there must be a continuing search for administrative arrangements and organizational structures which will facilitate the allocation of non-scientific rewards among scientists by scientists rather than by laymen. The receipt of such rewards may then more easily be interpreted as tokens of professional recognition, and the autonomy of science may be better maintained.[4]

While this goal may seem in one sense contrary to the usual goals of planning, I think that a magic goose who runs free and lays her golden eggs in obscure places is preferable to a goose lying dead in her cage because her keepers did not understand her dietary requirements.

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1 Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Ill.: Free Press, rev. ed., 1957), pp. 560-561. "Priorities in Scientific Discovery," *American Sociological Review*, 22, 6 (Dec. 1957), pp. 635-659.

2 Bernard Barber, *Science and the Social Order* (Glencoe, Ill.: Free Press, 1952), esp. pp. 62-65, 86-94.

3 Derek J. de Solla Price, *Science Since Babylon* (New Haven, Conn.: Yale University Press, 1961), Chapter V, "Diseases of Science," pp. 92-124.

These suggestions are derived from a theoretical analysis of the social system science. I take for granted that much more research will be necessary before we can say with any certainty that this *is* the way science operates, or that *these* elements of policy are necessary. Nevertheless, policy decisions are always made on the basis of the best available knowledge.

18

A Concept of Scientists and Their Organization

from page 19.

by Alfred de Grazia

A SOCIAL scientist studying scientific behavior can readily bring to bear upon the subject certain facile propositions of his trade. None the less useful for being imprecise, are the injunctions against regarding all scientists as alike and to allow for the temporal changes in their ways of recruitment and their environmental settings. So we cannot speak of all scientists. Yet modes of behavior do exist and, in generalizing, we should perhaps imagine a biochemist on the "pure" side and a structural or electronics engineer on the "applied." Furthermore, if it is today rather than fifty years ago of which we would speak, we should conceive of a fairly administered scientist—listed on a payroll, belonging to associations, assured of a lifetime job, possessed of an M.A. degree if an engineer and a Ph.D. if "pure," using institutional rather than personal library and research facilities, spending government funds, and accorded a higher-middle-class prestige.

Whatever we would say about our model men may be cautiously extended to the remaining vast majority of scientists insofar as they are related in character, habit, and habitat. Had we time, something extra might be said of the more absolute deviants among behavioral and natural scientists; we wish we might, for it is tiresome to have scientists judged by their extremes and rather ironic when the judges are, in other spheres, experts upon sampling and restrictive inference. Surely administration, of which we have to do here, can only exist on a presumption of manageable clusters of traits and actions.

FALLACIES ABOUT SCIENTISTS AMONG PUBLIC AND SCIENTISTS

Our typical scientists are not without various conceptions that they share with the educated population and which, on the whole, do more harm than good, both in understanding the role of science and in the practice of science itself. Although an empirical validation of the extent and intensity of the attitudes is unavailable, they may be set forth hypothetically:

1) *The scientist and his educated clientele are likely to believe that the scientist is more specialized than he actually is.* Sociologists of science would do well to supply us with a variety of information: What part of the symbol-bank and logic-bank of typical scientists is a result of pre-specialized education and training, in the culture, the family, and the schools? What part of his symbol-intake is trans-disciplinary? What part of it is irrelevant, strictly speaking? What part is "mentally or operationally" employed in ways more extensive than either the intent upon intake or the *prima fácie* "scientific" and "specialized" meanings of the symbols? The sum of answers to these questions would help define how specialized the scientist

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The sciences are communication systems, writes the Editor of ABS, Professor of Social Theory at New York University. They are founded upon conventional agreements. Their administration is part of their essence. Numerous "fallacies" keep scientists from understanding themselves and their work.

is. That question would probably be answered "Not much." The typical scientist carries his specialization "on the top of his head." And a gross miscellany rides below.

(2) *A second harmful belief is that the scientific method is a* UNIQUE *behavioral set;* its procedures of hypothesis, controlled observations, findings, and relating—with all the detailed stipulations, techniques, and modes of expressing the behaviors in symbols—are thought to be the last word in human development and qualitatively distinct from other behavioral sets. Instead, the scientific method should be construed as a distinct but recognizable form of administration. That is, it may be viewed as a set of routines, historically evolved and professionally sanctioned, for arriving at a decision of a confirming or disproving sort, whose value is thereupon judged by the leaders (often co-administrators) of the system of administration. Their judgment is affected by, among other things, the relation of the decision to other decisions already made, and especially the disturbance to the system of decision-making and decisions-made of the new decision with its potentiality for heightening the efficiency (internal or practical) of the total system, if adopted.

(3) *It is further believed by the typical scientist* (from whom emerges in collectivity the general influence of science upon society) *that the real world is the hard world of the senses, that there is one world, and that science is objective in relation to this world;* that is, science "finds" the world. I suppose that scientists will go on indefinitely *en masse* "finding" the real world, rushing in to fill the gap every time that a deviant scientist, or a poet, or an Idealist cracks open reality. Yet one can still assert, no matter if pessimistically, that a number of the social problems of science would be eased if scientists themselves were to permit themselves a *hypothetical* theory of the reality that they presume to be dealing with.

An important consequence of this same recommendation, if adopted, would be that scientists and their clienteles would cease to believe that they are seeking the truth. Scientists do not seek the truth, except as a myth that is needed to inspire them. They seek an answer reflected back from the packed closets of reality in the terms of the question as they ask it.

(4) *It would also be socially and scientifically helpful, if scientists and their educated clientele would abandon the notion that there is only one way of saying things "scientifically."* A proposition may be phrased in as many ways as may prove useful with regard to the system of logic and science it is intended for or in relation to the action it is intended to guide. A single event or action sequence may be phrased in relation to several natural and human relations-sets, and in prose or mathematical language of sundry kinds.

In the view of science as administration, the difficulty referred to is one commonly experienced in administrative systems. Once devoted to a special administrative role and language, the administrator cannot adapt himself to other modes of expression; he regards them as wrong and sometimes dangerous—even when the applicability of the language is manifested in its control over behaviors and operations.

(5) *It is further erroneously believed that the natural sciences are systematic.* This condition is thought to be of immense importance to science itself and to the society it serves, as well as being a holy stigmatum that marks it off from "unsystematic social science." (We may as well put aside the last point, the nonsensical quality of which is highlighted by the general answer to the other points.) The natural sciences are not systematic, however; some elements of mathematics are, but these are forms of non-empirical logic, a world in itself. Mathematics shapes and is shaped by the empirical sciences, natural and social. Not being anchored directly to reality problems, it can sometimes unite a field or part thereof before the field has valid propositions to "really" unite; "Devinez avant de démontrer," wrote E. Kasner, is the principle of great mathematics.

However, systematic empirical science is hard to discover and is probably a myth. What we have are a few major individual propositions whose practical implications are numerous (for example, the Mendelian "laws"); a few links of large practical importance (for example, the general principle of relativity); and useful predictive classifications (for instance, Mendeléef's Periodic Table of Elements). Most laws of the individual fields of science are not tied together logically, empirically, or quantitatively. Men know them as impressive beings rising separately out of the formless stream of existence. The situation is worse when the various fields are considered. As they are written, understood, and applied, the statements of physics are as far from biology as those of anthropology. Yet "in theory and essence" they might be capable of a common formulation even while carrying on their former intradisciplinary functions. If by systematic science is meant an interlocking set of propositions, framed in the same symbol-system and moving up and down the full range of generality and across the full diameter of subject-matter, then systematic science only begins to exist. (We should add, furthermore, that not one but numerous such systems is the conceivable ideal.)

6) *The typical scientist is also likely to believe that a certain system of politics fosters the development of science.* This is usually a "welfare state," centralized, common-man

democracy. Actually the development of science has occurred richly in mixed systems, in which interstices science may house and whose inconsistencies feed it. Bureaucratic nineteenth century Germany was favorable to scientific development, but not bureaucratic Soviet Russia today. Elenents of democraticness (in the Old Liberal sense) and aristocracy played a role in the German situation; a totalitarian psychology dominates Russian public policy today. In any event, the problem is most complex, depending for formulation and solution upon a careful de-sloganized subclassification of political systems, but also upon a fine classification of scientists according to personality-structure, field, and level of problem pursued.

7) *Most scientists and their clientele still hold that social scientists are not "true" scientists and almost all of them will deny that the natural scientist is a* SOCIAL *scientist.* The first belief has been refuted elsewhere; suffice to say that no acceptable evidence demonstrates any qualitative break in the continuous susceptibility of social and natural materials to the scientific method.

It is more important here to deal with the second belief, that the natural scientist is not a social scientist. He may be a bad social scientist, but in his habits, his perceptions, and his statements he behaves as one. He would be a better scientist and a more effective personality if he acknowledged the fact. The following behaviors and conditions make him a social scientist:

a) He is a psychological product of his culture and behaves as such.
b) His work and his unconscious or conscious critical faculties are based upon the psychological preconditions of perception and cognition.
c) He uses language. He has to communicate.
d) He uses logic.
e) He operates in an administrative setting whose rules are part of his work.
f) His statements about natural events and relations are human-oriented, ultimately if not immediately, and if "applied" probably immediate.
g) Finally and most important, if least credible, any statement of natural relations (even if it be discovering a sub-atomic particle) is a statement of social science—in all of the above senses in the first place, and beyond that insofar as the "thing" described only exists as the faint echo of a set of axiomatic behaviors begun in the everyday world. Man can only know himself, and all of the finery of the artificial world is himself mirrored. Once he disdains some part of himself, that part of his image vanishes; once he fancies himself in a new guise, a new world, which may be a new science, appears. For example, today we speak of new developments in the life sciences and psychology wherein the means of psychotherapy and pharmacology are joined and where a new common language may be expected to develop. It is possible to conceive of a whole range of social and natural sciences possessing a new common language and interchangeable operations wherein social and natural are "nonexistent" as separates. This would occur, I should venture, when the major parts of critical sciences becomes "objectified" in the fundamental sense of that word, that is, independent of the "existence" of the things being talked about. At this point, scientific discourse will be constructed around problems to be solved, including perhaps some systematic ethical statement. There are indications of such a development in segments of the information sciences, in empirical-logical philosophy, in operations research theory, in non-parametric statistics, in game theory, and in model-theory in several empirical sciences.

ALL SCIENCE IS SOCIAL SCIENCE

Everything said in the previous section about the fallacies of the typical scientist's self-image, when reversed into affirmatives, helps to describe the nature of the scientific system. The scientific system is a human system in the complete sense of the phrase. It can be viewed from the perspectives of a sophisticated time and motion study, with the propositions of the science feeding into the systemic process with all other experience, with the extent of the system physically defined as the communicators to the *n*-th degree of frequency of relevant contact.

It is particularly important to reorganize affirmatively the last expressed thoughts about "all science as social science." Going directly to the last defense of a natural science as apart from human science, the question centers on the nature of a validated theory:

1) A validated theory expresses world relations according to a conventional set of perceptions, dimensions, and symbols.

2) It refers to values, understood implicitly, when couched in "pure scientific" terms, and made explicit, when in applied terms.

3) It instructs all unknown parties (this is a pretense, since the unknowns share an enormous common culture) that they will experience the equation, $x = f(y)$, as its protagonist does. It assumes that they are interested in the experience, indeed in the precise experience or one very close to it. Thus, as in (1) above, the psychological state of the unknown parties is vital to the validation and transmission of the communication.

4) As well as suggesting that in order for $x = f(y)$ to be *true* the function has to be uniform to, assimilated to the symbol system of, and lead to understandable consequences for all unknown parties, we would add that all of the factors essential to the production of the equation have to be satisfied in all succeeding experiences of the event being described. That is, it is not enough to have the equation and believe that these events occur infinitely in isolation. The total human interaction pattern has to be replicated with "sufficiently high" approximation of the original conditions of the communication. Only this can be the radical operationalist position.

If a "core" of natural science is left, it must reside in that very constricted statement of an equation that isolates and abstracts the purely "non-human" interactions of x and y. We have shown, I believe, that everything about this statement, except the presumed "existence" of two interactants, is human, not natural. Yet there can be no denying that it is precisely this de-humanizing of the natural world, the abstracting and isolating of certain "things" in it, and the making of these particular and concrete, that has given us a changed world. (This is so, even though many other historical events of a more conventionally ideological sort, Christianity for instance, have changed the world as much or more.) This *core* of science, we must say then, is vastly effective. It is so because (a) it gets credit for all the human relations that first composed and thereafter surround it; (b) its isolation is accompanied by magical instrumentation and incantations; (c) its effects are "newsworthy" in an age when, by circular definition, "news is what people want to hear" and what people believe in (thus, even though no event is as crushing as the withdrawal of love, a nuclear explosion is a new toy, unknown to other ages and the man on the street); but (d) most of all because, on the whole, the new relations of non-human being —a chemical reaction in a cell, a sub-atomic event, a new engine—produce new human relations, both psychological and real; in this sense, still quite human, the purely physical equation is a bridge between psychomotor present and human psychomotor potential.

THE ADMINISTRATION OF SCIENTISTS

The foregoing exposition of various dysfunctional perspectives of scientists and the view of science as a human system may have some utility to scientists in the process of discovery, research, and development. This is usually termed the individual creative process. It is, however, my major intent here to discuss some of their implications for the science of the administration of science. For this purpose, we shall again take an affirmative stance and talk about the ideal social setting of scientific work, the ideal scientist, and the ideal scientific organization.

First a clarification of the subject is in order. Administration is a process; the science of administration is the science that describes it; and the applied science of administration is the set of rules for conducting administration on behalf of specified goals, according to the science of administration. Administration is largely institutionalized habit with varying small introjections of hypothetical or creative behavior.

An applied science of administration perforce introduces values. You cannot act rationally without acting towards an end. The applied scientific administration of science must have goals. These goals are the same combination of elemental goals that are found in all realms of life, with an emphasis, verging upon exclusiveness, on one goal—discovery. If we use Harold D. Lasswell's classification of valuing behaviors, we say that the total of elemental base values is eight in number—power, wealth, well-being, respect, rectitude, affection, skill, and enlightement. The process of discovery is the search for enlightenment by this scheme.

Hence, in the broadest sense, that social setting, that scientist, and that scientific organization which can be termed most absolutely scientific are those that seek exclusively and successfully the goal of discovery. At the same time, the definition of the ideal in each case depends upon a set of preferences for means and ends behaviors that may produce more or less of the absolute achievement. Still, for an organization to be called scientific and a man a scientist, it must be stipulated that they have as an important high priority preference the ambition to make discoveries about natural and human relations. Given this goal, administrative and habitual conduct must be oriented toward efficiency, that is, the highest return toward the goal in exchange for the lowest resource commitment possible.

The Ideal Setting

Granted the vagueness of the value, enlightenment, and of its sub-value, scientific discovery, we cannot expect too great a precision in describing the ideal setting of science. We may list the following four event-complexes as favorable; very rough specifications are given the major terms, simply to indicate how the setting must be examined:

1. A pluralistic society, to nourish and protect differences. (Say, at least four autonomous sub-cultural groups of considerable functional and informal authority.)
2. A social orderliness and stability of at least one segment of the society that can provide a nestling place for scientists. (Say, a considerable bureaucratizing or leisure set-up somewhere, which the creative and eternal-minded can cling to and move out from.)
3. A disciplined intellectual training of a significant number (5%) of the young for intellectual pursuits. (Say, not too much "progressivism" in education, but enough drill in procedures and in the myths of intellectualism.)
4. A willingness of the elite to commit heavy resources (always relative to what is available) to discoveries. (Say, 5% of the GNP).

All of these four items are, strictly speaking, beyond the province of scientists, as such. If they occur, science is promoted, if not, then suppressed.

The Motivated Scientist

In general, keeping in mind that we are discussing a problem now of the applied science of administration, we must admit that whatever incentives produce more goal-directed behavior—with discovery as the basic aim—must be "good" ones, holding aside the surrender of certain means incentives to other citizen goals (e.g., it may be deemed socially unwise to accord too much prestige to scientists, or too much money, considering democratic or anti-materialistic ideals). Suppose for instance a scientific group has varying numbers of certain German types who are motivated to scientific discovery by the power they gain in human relations; others of Jewish type who are impelled by a search for high respect; and still other "Yankee" types who wish to "cash in" on their knowledge or to find affable surroundings. Obviously the scientific administrator had better give up any of his own prejudices as to what a scientist *should* respond to in the way of incentives. So too those libertarians who universalize the force of liberty in scientific work. Liberty is a social permission to choose without restraint ultimate goals and the means necessary to reach such goals. Here too, the scientific administrator cannot prejudge the directions of the demand for liberty nor himself demand wholesale liberty. So long as scientists and citizens make such a hash of the term liberty, of course, the administrator may often be in the position of proclaiming a desire for universal liberty on the one hand while restraining a great many of its potential manifestations on the other.

To a high degree, therefore, the administration of scientists becomes a process of giving individuals the attention they require within a framework of liberties and restraints imposed upon means-values in terms of the basic value of discovery and such basic values as envelop the larger society in which the organization operates. The sociology of science thus becomes fundamental to the administration of science.

The Changing Community of Science

At one time, perhaps from 1600 to 1920, the scientific community was fairly close-knit. Informal ties abounded. Journals were few and well-read. Dozens of the scientific fields of today had not come into being. Individual scholarship, or scholar-apprentice teams, were almost the sole mode of organization. The lone scientist and the lone tinkerer held the field. A loose, informal, but effective system, we should say.

The rapid increase in new fields, an increase in scientific activity in different countries, and an increase in the technological orientation of societies brought about the situation still prevailing. In this phase we find a great many professional associations being organized, new journals appearing in abundance, and a developing crisis of collective information procedures. Practically all of the communicative and administrative processes are bigger imitations of the former system. Huge associations use the vocabulary, machinery, and practices of old personal associations. Every journal acts as if it alone existed and sufficed. Communication through libraries and publishing is a halting step removed from 1600 A.D. Interdisciplinary projects and team research, however, are experimented with and come to be regarded as essential, but they are administered "from outside" even when the administrators are coopted from the teams. That is, administration is regarded as distinct from scientific process. One may say that there has been a failure to achieve either effective informal or effective formal community. Yet the costs of trying to maintain a community of scientists or, better, a network of communities, are mounting rapidly. Exhausting conferences and consultation, for example, are made to substitute for ample, calm flows of systematic data storage and exchange. There is a loss of creativity, too, to auxiliary occupations, such as foundations offices and research entrepreneurship. That is, there is a superfluity of expediters, because of the basic malorganization of scientists.

A new era of science appears to be in the offing. In it, a rationalization of the role of the individual scientist is occurring. Both the sources and the language of contributions to knowledge are becoming collective and anonymous. Will a peak be reached in this regard and will it be impossible to give credit where credit is due? What will then happen to the prestige motive that impels men to work as scientists? Will "Fame" be replaced by more abstract motivations such as collective honors, security, good pay, and good fellowship? The network of scientists will be very wide, covering millions of souls, highly diversified by field. But it will be tied together by a ramified system of interlingual machinery of an interscience and inter-ethnic kind, of electronic data storing and retrieval apparatus, and of improved methods of coordinating the scientists' operations with policies and decisions.

It is in this kind of general system that science as administration and the administration of science will work. It may be called a "tandem" system, for the scientific work and administrative work will go together, with each scientist aware of the communication problem as never before, seeking to observe the effects of his statements upon human action rather than their separate commentary upon an objective reality.

Strangely, this is a 2500-year-old lesson that has only been verbally learned. A naive history of science is at fault. It has often been stated that the Greeks and other ancients possessed a potential for science not much less than the present achievements of science, but lacked a sense of technique. For example, Archimedes, who was the Greek scientist most concerned with technology, reports that he did not publish some of his work because it was too mechanical and practical. Far from being an aside in the history of science, this observation is the critical statement of what brought about modern science and where lies the embryo of the new science.

It is science as procedure that created modern science. To the classical idea of the world as the real thing, Leonardo, Galileo, and especially Francis Bacon added "the scientific method". But it is the fully self-conscious recognition of science as procedure alone that would bring about the new science. Science is a hunt for all the worlds there ever might be. Hence, when we appreciate the operations of science as a communication system founded upon conventional agreements, we shall have a formula both for new scientific discovery and for organizing the discovering activities of scientists. Jean Piaget, psychologist of the origins of thought in children, once said "logic is the morality of thought, morality the logic of action." By the same token, scientific procedure is the morality of scientific thought, and the morality of science is the science of applied science.

REFERENCES and NOTES

The number of scientists of different conventional categories is reported regularly by the National Science Foundation. On the work and social conditions of scientists, little data is to be found. Rather, formal studies, full of "liberal arts" presumptions and undergraduate curricular debate, dominate the study of the creation of symbol-systems in scientists. The problems of creativity, discovery, and invention are treated, with an extensive bibliography, in a special issue of the *American Behavioral Scientist* for December 1961. Also, the summaries of studies contained in M. I. Stein and S. J. Heinze, *Creativity and the Individual* (1960) are valuable. Unusually helpful discussions are contained in J. Jewkes, D. Saivers, and R. Stillerman, *The Sources of Invention* (1958); C. Gibb, "Creative Personality," *Australian Journal of Politics and History* VII (May 1961); D. L. Giovacchini, "On Scientific Creativity," *Journal of American Psychoanalytical Association*, VIII (July 1960). On methodology, some valuable studies are N. R. Hanson, *Patterns of Discovery* (1958); John Dewey, *The Quest for Certainty* (1929); Henri Poincaré, *Science and Method*; P. W. Bridgman, *The Logic of Modern Physics* (1927); A. Eddington, *The Philosophy of Physical Science* (1939); A. Einstein, *Relativity: The Special and General Theory* (1916; trans. 1920); E. Husserl, *Ideas* (1931); H. Vaihinger, *Philosophy of 'As If'* (1924); F. Kaufmann, *Methodology of the Social Sciences* (1944); B. Malinowski, *Magic, Science, and Religion*; A. F. Whitehead, *Science and the Modern World* (1926), and Conant, *Understanding Science* are sympathetic to the historical social psychology of science.

I cannot find writings on the line I am taking here, in respect to natural science as social science, an idea that came to me some years ago while studying the theory and methodology of communications, but find that Dewey, Hansen and Vaihinger, and Poincaré send out closely related signals. I must admit to no authoritative support whatsoever to my guessing about the last critical redefinitions of a physical equation as a behavioral one. If that leap is uncertain, I feel nonetheless on firm ground once on the other side, for the connections among the science of administration, science, and the administration of science appear to be fundamental. (Cf. A. de Grazia, "The Science and Values of Administration," *Administrative Science Quarterly*, Part I, Dec. 1960, 364-98; Part II, March 1961, 557-83.) On the value-frames of social action, see H. D. Lasswell and A. Kaplan, *Power and Society* (1950). Jean Piaget's several works are pertinent, as, for instance, *The Origins of Intelligence in Children* (trans. 1952). When I speak of the new bridging sciences, I refer to the work discussed in books such as the following: E. Nagel, P. Suppes, and A. Tarski, eds., *Logic, Methodology and Philosophy of Science* (1962); Harold Borko, ed., *Computer Applications in the Behavioral Sciences* (1962); "The New Educational Technology," *American Behavioral Scientist* (November 1962); Rensis Likert, *New Patterns of Management* (1961); Paul Lazarsfeld, on reformulating qualitative situations into quantitative terms, in D. Lerner and H. D. Lasswell, eds., *The Policy Sciences* (1951); G. Schubert, *Judicial Decision-making* (forthcoming); N. Weiner, *Cybernetics* (1961); G. Tagliacozzo, "The Tree of Knowledge" in *American Behavorial Scientist* IV (November 1960), and "The Literature of Integrated Knowledge," *American Behavorial Scientist* IV (June 1961).