Thematic Analysis in Science: Notes on Holton's Concept

Robert K. Merton

The generation now at work in the philosophy, history, and sociology of science has witnessed the resurgence, convergence, and often the collision of ideas about the cognitive and social structure of the scientific enterprise. Some of these ideas have spread rapidly beyond their original disciplinary boundaries, at times in that extravagant form which leads masters to disown disciples. Polanyi's "scientific community" and "tacit knowledge," Popper's "falsificationism" and "third world," Kuhn's "paradigms, disciplinary matrices, and normal science," Lakatos's "research programmes," Campbell's "evolutionary epistemology," Elkana's "images of science," and even Merton's "normative structure of science" and "reward-system of science" are a few of the familiar tags for these ideas (which threaten to deteriorate into little more than vogue words and vogue concepts) (*1*, *2*).

While these ideas were being hotly debated, Gerald Holton was quietly developing his concept of "thematic analysis" as both perspective and tool for the historiography of science, a concept that has begun to attract wide notice only since the appearance of his recent book, *Thematic Origins of Scientific Thought: Kepler to Einstein* (*3*). In the article that precedes this one he has gone on to work out a problematics for the understanding of science and scientists. Naturally, that requires him to compress much into little. In proposing the eightfold way to such understanding, he adopts a to me altogether congenial attitude of what can be described as disciplined eclecticism (as distinct from "mere" or motley eclecticism). From that attitude, particularly appropriate for composite humanistic-and-scientific disciplines such as the history and sociology of science, the various perspectives appear supplementary rather than antithetical, with each perspective having its own problematics, its own set of basic questions and derivative puzzles.

But it is the ninth way, the way of thematic analysis, that concerns us here. In Holton's implied definition, this way of interpretation assumes underlying elements in the concepts, methods, propositions, and hypotheses advanced in scientific work. These elements function as themes that motivate or constrain the individual scientist in his cognitive formulations and consolidate or polarize the cognitive judgments appearing in the community of scientists. Although public expositions of scientific work rightly focus on reproducible phenomena and analytical propositions, it is the themata that help shape their form and content.

The ninth way, then, is Holton's distinctive effort to deal with tacit knowledge (partly in the insufficiently appreciated sense given that concept by Polanyi). The themata of scientific knowledge are tacit cognitive imageries and preferences for or commitments to certain kinds of concepts, certain kinds of methods, certain kinds of evidence, and certain forms of solutions to deep questions and engaging puzzles. Implicit in Holton's own investigations of themata is the notion that they are unevenly accessible to observation. So to say, not all tacit knowledge is equally tacit. As Holton puts it, in one symptomatic footnote, "Not all themata appear in so many words."

After some years of inquiry, Holton has come to a provisional conclusion about the distributions of themata in scientific knowledge. Some scientists, especially the pathbreakers, have their distinctive configurations of themes. These configurations, I take it, make up much of the styles of thought that characterize many scientists and uniquely identify scientists of utmost consequence (as in the celebrated episode of Johann Bernoulli's instantly recognizing the source of Newton's anonymous solutions to two bruited mathematical problems as "ex ungue leonem"). In Holton's account of the persistence or recurrence of themata, we also note that some of them are shared by sets of scientists, both contemporaneous and successive. In suggesting that many themata are formed by scientists even before they have decided to become scientists (*4*), Holton advances *his* most distinctive and daring idea—one that seems to require new procedures of investigation if it is to be looked into systematically.

Although individual scientists have their distinctive configurations of themata, they nevertheless share some themes with other scientists. Such composites of individuality and communality are not at all peculiar to the domain of science; they are found in patterns of human behavior generally. Both in the special case of science and in the more general case, similarities and differences in themata may help explain the sense of congeniality or incommensurability of ideas experienced by people in interaction.

Having proposed thematic analysis as the ninth way to an understanding of the scientific enterprise, Holton confronts the question of how one goes about discovering themata in what he calls the "events," the phenomena that make up the changing substance of the sciences.

The Method of Thematic Analysis

In dealing with that question, Holton never lapses into the high-sounding phrase "the methodology of thematic analysis." Nor shall I replace his plain words with showy ones. Instead, the question of method puts me in mind of a maxim prized by the first president of the History of Science Society (who happened to be one of my teachers). L. J. Henderson liked to remind his more attentive students that "it's a good thing to know what you are doing."

The author is University Professor at Columbia University, New York 10027. The article is adapted from an invited commentary on an address by Gerald Holton delivered at a meeting of the History of Science Society (see page 328).

For some decades now, I have transmitted this bit of earthy wisdom to other generations of students in slightly modified form: "*In general*, it's a good thing to know what you are doing—and why you are doing it." The qualifier "in general" is designed, of course, to warn against the danger of that premature faultfinding which stifles ideas that need to be played with before being subjected to systematic and rigorous examination. There is a place, as Max Delbrück and Dickinson Richards have severally reminded us, for "the principle of limited sloppiness."

What, then, does Holton do when he does thematic analysis? How does he identify themata so that we, the beneficiaries of his concept, can in turn proceed to discover and understand other themata?

Having studied his book, *Thematic Origins of Scientific Thought*, and his paper on the role of preconceptions in the work of Millikan and Ehrenhaft (*5*), I must report my strong impression that Holton identifies themata *inductively*. In saying this, I realize that in some quarters these days the very term "induction" is in ill repute, as in other quarters is the term "deduction" and even, one gathers, the Peirce-Hanson notion of "abduction." Nevertheless, it appears that this early phase in exploring the potentialities of thematic analysis (just as with current explorations of its distant conceptual cousins, Kuhn's "paradigms and disciplinary matrices," Lakatos's "research programmes," and, to go no further, Elkana's "images of science") requires case-by-case analysis in order to obtain a working list of themata: of thematic concepts, thematic methods, and thematic hypotheses or propositions.

To the best of my knowledge the list of themata in the physical sciences has not yet been assembled in any one place, but Holton estimates them to number fewer than 100, including doublets and occasional triplets. A next step would therefore be to convert a list of themata into a classification; for whatever its limitations, the ancient device of classification serves to convert the tacit empiricism of lists into the analytical rationalism of categories. Inductively assembled lists of themata in scientific thought seem ready to be transformed into classifications and related propositions designed to help us understand what Holton has dealt with as event-structures and event-sequences in the development of science.

Parallels in the History and the Sociology of Science

Much more than specialized (and often learned and provincial) historians of science have yet acknowledged in print and much more than we specialized (and often unlearned and provincial) sociologists of science have apparently considered, parallel lines of inquiry are being pursued in the two disciplines. The practice of thematic analysis provides a case in point.

For several decades, sociologists and political scientists have engaged in systematic "content analysis," as Lasswell called it, of communications in general and of propaganda in particular (*6*). A procedure of thematic analysis was developed to identify implicit as well as explicit themes in order to infer states of mind of the communicator and to interpret responses to the communication (*7*). A mode of structural analysis served to investigate "the interrelations of various themes."

Plainly, Holton was aware of this sort of parallelism when he proposed "a discipline that may be called thematic analysis of science, by analogy with thematic analyses that have for so long been used to great advantage in scholarship outside science" (*3*, p. 57; *8*). By advancing the thematic analysis of far more enduring and, one would like to think, more consequential cognitive materials than short-run propaganda, Holton has been bringing alive in the history of science what has become almost dormant in the sociology of public opinion: the content analysis of documents to identify tacit themes and thematic structures.

This case only illustrates the growing need for a special breed of scholar brought about by increasing specialization: the hybrid who, though more deeply committed to one discipline than to others, also manages to become thoroughly schooled in neighboring disciplines and to keep in reasonable touch with what is going on there. I have no doubt, for example, that my colleagues in sociology have much to learn from the kind of thematic analysis being advanced by Holton.

I can here only touch upon a few other parallels in the problematics identified by Holton and also found in the sociology of science, that domain once infinitely remote from the history of science and now, plainly, within hailing distance.

There is the problem, included in Holton's inventory of the eightfold way, of the time-trajectory of shareable scientific knowledge, involving "antecedents, parallels, continuities, and discontinuities." In this connection, I refer only to a case study of "premature" and "postmature" discovery being conducted jointly by a biologist (Joshua Lederberg), a historian (Yehuda Elkana), and two sociologists of science (Harriet Zuckerman and R. K. Merton) which is designed to identify social and cognitive processes underlying continuities and discontinuities in scientific knowledge (*9*).

There is the related problem of understanding the role of "failure" as well as "success" in science. Holton's proposed line of inquiry intersects rather than parallels sociological investigations by Glaser and Rubin (*10*) of types of failure experienced by scientists at various stages of their careers. It also intersects philosophical investigations of "imperfect rationality"; for example, by the philosopher of science Watkins, who aptly begins an essay on the problem by remarking that "historians are not much concerned with also-rans and drop-outs. They have a bias toward success" (*11*). But historians and sociologists alike increasingly recognize that the unwitting preoccupation with success must be offset by a focus on the phenomenon of failure. That is just another shift in foci of inquiry needed to advance our understanding of the complex interactions between the behavior of scientists and the development of scientific knowledge in the context of the historically changing normative structure and social organization of science and the environing society and culture. Historians and sociologists must both examine the various sorts of "failure": intelligent errors and unintelligent ones, noetically induced and organizationally induced foci of interest and blind spots in inquiry, promising leads abandoned and garden-paths long explored, scientific contributions ignored or neglected by contemporaries and, to draw the sampling to a close, they must examine not only cases of serendipity gained but of serendipity lost (*12*) (as with the many instances of the antibiotic effects of penicillin having been witnessed but not discovered).

When Holton distinguishes "private science" from "public science," the terminology suggests a distinction parallel to one drawn in the sociology of science. But the similarity turns out to be merely homonymous, with the terms

alike only in appearance, not in meaning. Nevertheless, the seeming parallel is instructive, signaling current efforts by both historians and sociologists to identify types of scientific work and of scientists rather than implying, by their silence on the matter, that these are all much of a kind. For Holton "private science" refers to the deeply personal aspects of science-in-the-making, to those aspects of the "nascent moment" of discovery which, by convention, ordinarily remain unreported in the "public science" recorded in scientific journals and monographs. For the sociologists Cotgrove and Box, empirically investigating types of scientific identities, "private scientists" are those who "attach importance to [the norms] of disinterestedness and organized scepticism [but] do not seek [though they sometimes obtain] recognition and confirmation from the scientific community" (*13*). Principally at work in industrial research laboratories, they typically set little store by publication, as Derek Price (*14*) has long noted in distinguishing the ways of science from those of technology. In contrast, "public scientists," found chiefly in academia, act in accord with the norm that calls for them to communicate the results of their research beyond the immediate organization or locale, this practice being reinforced by their finding major reward in the recognition and use of their published work by peers in the larger scientific community.

It is not difficult to compile a short list of other subjects and problems that have been turning up in both the history and the sociology of science: models of the growth of scientific knowledge; taboos on certain forms of knowledge dubbed dangerous (by laymen or by scientists themselves); bases of problem-finding and problem-selection; the dynamics and cognitive consequences of conflict in science (with special reference to orthodoxy, heterodoxy, and, if it be allowed, polydoxy); the reciprocal transfer of analogies and models from one to another domain of scientific inquiry [as exemplified in Keynes's remark (*15*) that "the Principle of the Survival of the Fittest could be regarded as one vast generalization of the Ricardian economics"]; independent multiple discovery and scientists' efforts to establish their priority as phenomena providing strategic research sites for investigating a variety of problems in the development of scientific knowledge (*16*).

Along with such parallels in problematics are parallels in what qualify as themes in the two disciplines. Two cases in point must serve. The first is the historical and sociological theme that the stock of scientific knowledge accumulates selectively. In emphasizing the selective character of scientific growth, sociologists (*17*) long since rejected the earlier image of a royal road to knowledge along which science inexorably advances in unilinear fashion. More recently, even this moderate image of progress in science has been declared unacceptable. Evidently stimulated by idiosyncratic readings of the work of Kuhn, Popper, and Lakatos, some sociologists have adopted an acutely relativistic position. From that standpoint, it is enough to adopt the model in which scientific knowledge accumulates selectively to qualify one as a vulgar positivist, committed to a belief in the growth of that knowledge as a "cumulative and one-dimensional process" (*18*). Relativists of this sort judge historians and sociologists guilty of perpetrating an ethnocentric and tempocentric "Whig interpretation of history" (*19*) when they so much as hint that, despite many vicissitudes, scientific knowledge does accumulate: that today's astronomers may actually have a more solid, more sweeping, and more exacting knowledge of the sun, moon, planets, and stars than did Aristarchos of Samos or even Ptolemy, or that today's demographers just might have a deeper and broader understanding of the dynamics of population change than, say, the 17th-century William Petty or even the early-19th-century Thomas Malthus.

Donald Campbell has noted that some recent work in the philosophy of science "portrays science as a self-deceiving system incapable of distinguishing truth from tribal myths" (*20*). When historical relativism reaches this point, perhaps we should come full circle. Perhaps the half-century-old taboo on Whiggery in historiography has moved too far beyond the original purpose of countering the celebrative presentism which regards the historical past only in terms of how it led to the historical present. Perhaps the time has come for an *anti*-anti-Whig orientation to history.

At any rate, it is comforting to have Holton's mode of thematic analysis reject the untenable relativism that is currently being substituted for an untenable progressivism. As he observes, the themata in a scientific work are not, after all, "its chief reality." New themata may expand the scientific imagination or contract it. But they do not erase the knowledge that went before to provide a wholly clean slate on which scientists then proceed to write their new stories. Old themata are occasionally abandoned. But, Holton notes, "there undoubtedly has been on the whole a progressive change to a more inclusive, more powerful grasp on natural phenomena."

Finally, Holton observes in the history of science a characteristic that some of us have observed in the sociology of science: the discipline exhibits a self-exemplifying character (*2*, pp. ix, 352–356, 554). The behavior of the discipline and of its practitioners exemplifies ideas and findings about the behavior of sciences and scientists that have been developed in the discipline itself. Thus, Holton notes that the "search for answers in the history of science is itself imbued with themata." Belief in the value of thematic analysis can itself be interpreted as a case of thematic predilection. Holton is saying in effect that if we do not perceive the basic themata in a scientific work, we cannot understand well enough what makes it important, the reasons for its distinctive reception, and, not least, what is "sacred" enough in it to withstand disappointing delays in confirmation or to survive seeming disconfirmation. One could scarcely ask for a more apt instance of a self-exemplifying cognitive theme.

References and Notes

1. M. Polanyi, *The Logic of Liberty: Reflections and Rejoinders* (Univ. of Chicago Press, Chicago, 1951), chap. 4 (first published 1942); *The Tacit Dimension* (Routledge & Kegan Paul, London, 1967); K. R. Popper, *The Logic of Scientific Discovery* (Basic Books, New York, 1959) [first published 1935]; *Objective Knowledge: An Evolutionary Approach* (Clarendon, Oxford, 1972); T. S. Kuhn, *The Structure of Scientific Revolutions* (Univ. of Chicago Press, Chicago, enlarged ed. 2, 1970); in *Criticisms and the Growth of Knowledge*, I. Lakatos and A. Musgrave, Eds. (Cambridge Univ. Press, New York, 1970), pp. 231–278; in *The Structure of Scientific Theories*, F. Suppe, Ed. (Univ. of Illinois Press, Urbana, 1974), pp. 457–482; I. Lakatos, *Proc. Aristotelian Soc.* **69**, 149 (1968–1969); in *Criticism and the Growth of Knowledge*, I. Lakatos and A. Musgrave, Eds. (Cambridge Univ. Press, New York, 1970), pp. 91–195; in *Boston Studies in the Philosophy of Science*, R. Buck and R. S. Cohen, Eds. (Reidel, Dordrecht, Netherlands, 1971), pp. 91–136; D. T. Campbell, in *A Handbook of Method in Cultural Anthropology*, R. Naroll and R. Cohen, Eds. (Natural History Press, Garden City, N.Y., 1970), pp. 51–85; in *The Philosophy of Karl Popper*, P. A. Schilpp, Ed. (Open Court, LaSalle, Ill., 1974), pp. 413–463; *Inquiry* **2**, 152 (1959); Y. Elkana, *The Discovery of the Conservation of Energy* (Hutchinson, London, 1973); in *The Interaction*

between Science and Philosophy, Y. Elkana, Ed. (Humanities Press, Atlantic Highlands, N.J., 1974), pp. 243–279; in *Methodological and Historical Essays in the Natural and Social Sciences*, R. S. Cohen and M. W. Wartofsky, Eds. (Reidel, Dordrecht, Netherlands, 1974), pp. 277–305.
2. R. K. Merton, *The Sociology of Science: Theoretical and Empirical Investigations* (Univ. of Chicago Press, Chicago, 1973).
3. G. Holton, *Thematic Origins of Scientific Thought: Kepler to Einstein* (Harvard Univ. Press, Cambridge, Mass., 1973).
→ Holton puts his suggestion in these words: "Elementary particle physics is . . . shot through and through with themes that may well have, as many themes seem to me to have, their origins in a part of the imagination that was formed prior to the conscious decision of the researcher to become a scientist" [*Science* **188**, 328 (1975)].
5. G. Holton, in *Proc. Varenna Course on The History of Recent Physics*, C. Weiner, Ed. (Il Nuovo Cimento, Bologna, Italy, in press).
6. H. D. Lasswell, *Psychiatry* **1**, 197 (1938); ——— and N. Leites, *Language of Politics: Studies in Quantitative Semantics* (Stewart, New York, 1949). See also B. Berelson, *Content Analysis in Communication Research* (Free Press, New York, 1952); M. W. Riley and C. S. Stoll, in *International Encyclopedia of the Social Sciences*, D. L. Sills, Ed. (Macmillan and Free Press, New York, 1968), vol. 3, pp. 371–377.
7. P. F. Lazarsfeld and R. K. Merton, *Trans. N.Y. Acad. Sci. Ser. 2* **6**, 58 (1943); for a specific thematic analysis as preliminary to the investigation of response to propaganda, see R. K. Merton, with assistance of M. Fiske and A. Curtis, *Mass Persuasion* (Greenwood, Stamford, Conn., 1971, reprint of 1946 ed.), chap. 3.
8. Holton goes on to specify parallel usage of the term "thematic analysis" in the preceding article (p. 328); a term "familiar from somewhat related uses in anthropology, art criticism, musicology, and other fields."
9. H. A. Zuckerman, "Cognitive and social processes in scientific discovery: recombination in bacteria as a prototypical case," paper presented at the annual meeting of the American Sociological Association, August 1974.
10. B. G. Glaser, *Science* **143**, 1012 (1964); *Organizational Scientists: Their Professional Careers* (Bobbs-Merrill, Indianapolis, 1964); L. Rubin, "Reactions to negative tenure decisions in chemistry and sociology," paper presented at the annual meeting of the American Sociological Association, August 1974.
11. J. Watkins, in *Explanation in the Behavioural Sciences*, R. Borger and F. Cioffi, Eds. (Cambridge Univ. Press, New York, 1970), pp. 167–217.
12. B. Barber and R. C. Fox, *Am. J. Soc.* **64**, 128 (1958).
13. S. Cotgrove and S. Box, *Science, Industry and Society: Studies in the Sociology of Science* (Allen & Unwin, London, 1970), p. 26.
14. D. J. de S. Price, *Technol. Cult.* **6**, 553 (1965); in *Factors in the Transfer of Technology*, W. H. Gruber and D. R. Marquis, Eds. (MIT Press, Cambridge, Mass. 1969), pp. 91–104.
15. J. M. Keynes, *Essays in Persuasion*, vol. 9 of *Collected Writings* (St. Martin's, London, 1972), p. 276.
16. A number of these problems are examined in (*2*).
17. Some sociologists—P. A. Sorokin, for example—acquired a certain notoriety for questioning the doctrine of unfailing, unilinear progress at a time when it was a doctrine widely held. For an effort to identify quantitative variations in the development of science, see P. A. Sorokin and R. K. Merton, in P. A. Sorokin, *Social and Cultural Dynamics* (American Book, New York, 1937), vol. 2, pp. 125–180, 439–474; for an archeologist's penetrating analysis of "knowledge as a social construction" and the selective accumulation of scientific knowledge, see V. G. Childe, *Society and Knowledge* (Allen & Unwin, London, 1956).
18. P. Weingart, in *Social Processes of Scientific Development*, R. Whitley, Ed. (Routledge & Kegan Paul, London, 1974), pp. 45–68.
19. The classical diagnosis of the "whig fallacy" was provided by H. Butterfield, *The Whig Interpretation of History* (G. Bell, London, 1931). Thoroughly in control of his idea, Butterfield does not allow it to deteriorate into a dedicated relativism that denies the selective accumulation of transcultural "objective knowledge"; see his course of lectures, *The Origins of Modern Science, 1300–1800* (G. Bell, London, 1949). J. Agassi may have provided a prophylaxis against the whig fallacy in his "principle of historical reconstruction: apply no hindsight" [*Inquiry* **14**, 154 (1971)].
20. D. T. Campbell, "Objectivity and the social locus of scientific knowledge," presidential address to the Division of Social and Personality Psychology of the American Psychological Association, 2 September 1969.
21. This work was supported by NSF grant GS-33359X to the Program in the Sociology of Science at Columbia University.

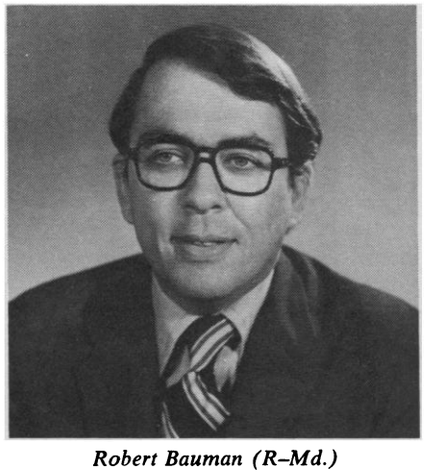
NEWS AND COMMENT

Congress: House Votes Veto Power On All NSF Research Grants

In a move that has dumbfounded officials at the National Science Foundation (NSF), the House of Representatives on 9 April voted that Congress should have a veto power over all of the 14,000 grants which NSF awards every year. To accomplish this, NSF would have to submit a list of all proposed grant awards to Congress every 30 days as well as justifications for them. Either house could veto the award of any grant, but if no action were taken inside of 30 days, the grant award would be made.

The provision would put Congress in the position of effectively approving research grants in every area of NSF support, from education to basic science. Needless to say it would revolutionize—some would say jeopardize—NSF's method of research support, which hitherto has exclusively involved NSF bureaucrats, grant applicants and their institutions, and the 40,000-odd scientists whom NSF invites to make peer review judgments on proposed projects.

The amendment was sponsored by Robert Bauman of Maryland, a second-term Republican, and passed by a vote of 212 to 199 just before the house overwhelmingly approved the entire NSF authorization of $755.4 million by a vote of 390 to 22. The amendment came up after a long debate which focused on another controversy concerning an NSF-sponsored introductory anthropology course titled "Man: A Course of Study" (MACOS).



Robert Bauman (R–Md.)

Conservative House members have attacked MACOS for an array of reasons, ranging from its course materials and films aimed at 10-year olds—which allegedly deal with "adultery, cannibalism, killing female babies and old people, trial marriage and wife-swapping, violent murder and other abhorrent behavior"—to the question it raises of the role of the federal government in shaping local school curriculums. And, at the end of an emotional, 3-hour debate, during which several proposals to control NSF in various ways were narrowly voted down, Bauman rose to propose his amendment to the surprise of many house members and staffers. As one staffer said later, "They passed it because they were ready to pass something."

The Bauman amendment is not yet law since the Senate has not completed action on its version of the NSF authorization bill. Senator Edward M. Kennedy (D–Mass.), who will have some influence as to whether the Senate passes a parallel measure since he is chairman of the NSF subcommittee of the Committee on Labor and Public Welfare, has stated that he is "shocked" by the House action and will "lead the