urban living. Of greater pertinence to an analysis of urban life are the *multiple* levels of community organization in which the resident participates.

The smallest of these units is the "face block." For children it is the prescribed social world carved out by parents. It is here that face-to-face relations are most likely, and the resulting institutional form is the block association. Next, in Suttles's typology, is the "defended neighborhood," which is the smallest segment of the city recognized by both residents and outsiders as having some corporate identity, and possessing many of the facilities needed to carry out the daily routine of life. The defended neighborhood frequently lacks official recognition, and its boundaries, because they have no legal status, are often precarious. Street gangs arise which protect it from unwanted incursions by outsiders.

The urban resident also participates in the "community of limited liability," a larger realm possessing an institutionally secure name and boundaries. The concept, originally developed by Morris Janowitz, emphasizes the "intentional, voluntary, and especially the partial and differential involvement of residents in their local communities." Frequently an external agent, such as a community newspaper, is the most important guardian of a community's sense of boundaries, purposes, and integrity. A single individual may be defined as living in several such communities. The multiple claims on the person may limit and even paralyze active involvement in any of them.

Even larger segments of the city, such as an entire East Side area, may also take shape in response to environmental pressures, creating an "expanded community of limited liability." Thus an individual may find himself picketing to keep a highway not just out of his neighborhood, but out of the entire South Side.

Thus what Suttles teaches us is that the concept of neighborhood is not adequate to handle the multiple levels of urban organization in which the individual participates. Varied levels of community organization are created as responses to the larger social environment. Neighborhoods cannot be seen as a society in microcosm. They never were, and never can be. The urban community is a form of social differentiation within a total society.

Does Suttles's analysis have a bearing on the contemporary issue of "community control"? It suggests, first, that the fully self-contained community within the city is a fiction. The urban community can be a differentiated but never a fully autonomous unit within the larger urban context. Second, Suttles points out that the idea of a centralized government is not incompatible with a well-served local community. "One of the sources of community weakness in most American cities is that many mayors are responsible to local communities but have little direct recourse to the federal levels at which major power and resources are located." In Sweden, in contrast, the mayors of certain local communities are appointed by the central government but this strengthens rather than weakens the resources available to the community.

It is a central theme of Suttles's analysis that "total societies are not made up from a series of communities, but communities are areas which come into being through their recognition by the wider society." Suttles overstates the case. Sometimes cities do develop through the coalescing of smaller communities, which continue to maintain their identity. London is a good example. To some extent it depends on the phase of a city's development under discussion. In later stages of development, when a city's origins are no longer relevant to its functioning, the social-constructive approach may well constitute the dominant mode of defining neighborhoods. More important, is the point really worth a great deal of theoretical fuss?

The book has other faults: it is repetitious and disjointed, with a number of essays only tangentially related to the main theme. Yet these flaws are unimportant alongside the book's considerable achievements. First, it helps break away from the limiting view of Park, Burgess, and others that "a city consists of a mosaic of little worlds which touch but do not interpenetrate." The urban community is a form of social differentiation within a total society. Second, Suttles teaches us that the concept of neighborhood is not adequate to handle the multiple levels of urban organization in which the individual participates. Participation ranges from the face block to larger segments of the city. Third, Suttles shows that there is no necessary discontinuity between how we experience neighborhoods, communities, cities, and so on and the sociological concepts needed to describe them. Neighborhoods are not primarily segments of real estate but collective representations existing in the minds of inhabitants, and attaining reality through social consensus. This is a stimulating viewpoint of great heuristic value. Fourth, he demonstrates that the phenomenon of mental maps, developed by Kevin Lynch and others, is not a disembodied esthetic or cognitive phenomenon but is part of the ongoing life of individuals, with practical meaning and significance. Fifth, Suttles translates the concept of territoriality, so foolishly caricatured in the work of Ardrey, Morris, and others, into its proper human context. He recognizes the importance of territoriality in human life, without equating it with its animal expression. Finally, his book is a work of considerable originality and insight; the author is a keen observer, bringing the same order of sensitivity to urban analysis that Erving Goffman has applied to the study of small-scale social interaction. And in both cases, we emerge with a sense of clarified perception.

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Behavioral Science of Science

Politics in Science. MARLAN BLISSETT. Little, Brown, Boston, 1972. xvi, 230 pp., illus. Paper, $4.25. Basic Studies in Politics.

Why do scientists believe this or that? Because reason, say the scientists, working on the available evidence, requires this or that belief. Because the scientific community, say the sociologists of science, working on its members, requires this or that belief. The two responses can be reconciled, but only, it often seems, at the expense of making each largely irrelevant to the other. The scientists may concede that their community is not absolutely rational, which is simply another way of saying that present knowledge is imperfect—so back to the preoccupation with reason working on evidence to improve knowledge. Sociologists of science may concede that the product generated by a scientific community is truth, but their preoccupation is with the process of generation rather than the thing generated, which shrinks to the significance that mudpies have for child psychologists, a clue to the thing sought rather than the thing itself.

Marlan Blissett has a point of view that would overcome this mutual irrelevance, if he were as bold in applying it as he is in announcing it. He declares that the content of science is shaped by political relationships within the scientific community. He proposes to go beyond previous sociologists of science, such as Warren Hagstrom, whose *The Scientific Community* "does not explicitly describe the tactics of pressure and manipulation (i.e., the politics) responsible for certain methodologies and research problems in science" (p. 171). Now there is an argument that scientists could hardly shrug off, if it were seriously defended by substantial analysis of the "tactics of pressure and manipulation" that have shaped specific chunks of scientific knowledge. Unfortunately—or fortunately, some may feel—Blissett offers very little of such substantial analysis. He offers a small survey of scientists' attitudes, imbedded in a great deal of abstract theorizing.

It takes a lot of labor to get through Blissett's theorizing. He has relatively little to say, but he says it very pretentiously. For example (p. 63):

> Pure science is differentiated from technology or applied research in order to preserve the spatial dimensions of the autonomous system. The majority of pure scientists . . . feel the autonomous system can provide a more reliable articulation of the purposes of science and its epistemic commitments.

What this passage says—if it says anything—is that pure scientists feel they can learn more by picking their own problems than by taking on the problems of engineers. "Spatial dimensions" or "space" is a modish term in political science—Blissett's discipline—for the pattern of relations within a portion of society, or maybe a whole society. At times it seems to be a synonym for institution, or community, or group. Precision is hardly gained by borrowing terms from the exact sciences and using them as metaphors. Nor do we achieve "laws of social thermodynamics" by invoking the phrase, or by borrowing "entropy" to indicate a tendency of "space" to become disorganized, or maybe to lose autonomy, or maybe just to change. (I urge my friends in political science to study George Orwell's essay "Politics and the English Language.")

Blissett agrees that scientists are members of a special kind of social system, designed to achieve, in John Ziman's phrase, "a consensus of rational opinion." The problem for the social scientist is to discover what is special in that social system, what permits it to generate *rational* consensus, or, if one wishes to be skeptical, the appearance of rational consensus. If the social system of a scientific community is to be compared with a political system, differences must be considered as well as similarities. What distinguishes a scientific community, its methods of achieving consensus and the kinds of consensus it achieves, from those of a political party or a national state? Blissett never faces that problem straightforwardly. And all the time he is going through his abstract theorizing he is putting off fulfillment of his initial promise to show how " 'hidden systems' of influence and persuasion actively shape scientific perception but [why *but*? why not *and*?] vary in relation to institutional configurations of power" (p. xiii).

In the last third of the book he finally gets down to cases. The result, like the endlessly delayed climax of an erotic melodrama, is a banality. We discover a gap between ideal and reality in the scientific community. The ideal to which scientists are pledged is the impersonal search for truth; in practice many of them are self-centered careerists. Blissett made this discovery by "in-depth interviews" (45 to 90 minutes each) with 29 scientists, and by a questionnaire (consisting of 33 items) mailed to 1500 scientists (854 came back).

The meager quantity of this research is not as disappointing as its vague quality. His questionnaire avoided substantive issues in any particular science. It asked for agreement or disagreement with generalities of this type (pp. 207, 211):

> Within my discipline there is a small group . . . who highly influence what kinds of evidence are acceptable for the empirical confirmation of hypotheses.
>
> Scientists are skeptical even about their own findings until other scientists have evaluated them.

In his interviews Blissett did get involved in particulars, but he reports them so gingerly that he hardly begins to test his initial hypothesis about hidden systems of influence and persuasion shaping the content of science. The closest he comes is in a five-page review—consisting mostly of a few long quotations—of the dispute concerning the structure and function of membranes in living systems. The opponents' catty remarks about each other's motives and methods might have launched him on a real test of his hypothesis. If he had been sufficiently bold and industrious, he might have tried to determine what kinds of techniques have been most influential in moving the scientific community toward one or another theory of membranes. Then he might have avoided the astonishing banality of his present conclusion: "With the emergence of better techniques of research, the dispute will subside." A commonly accepted view will then be incorporated in the textbooks. Until then "interpersonal movement and conflict" will mark this field of science (pp. 142–43).

When Blissett's conclusions are not banal, they are obscure or inconsistent. He suggests, for example (p. 159), that

> The historical style of decision-making in science—the one emphasizing criticism, controversy, and spontaneous consensus—may be replaced by one stressing conflict, competition, and manipulated consensus.

Yet he has never made a meaningful distinction between "criticism" and "conflict," between "controversy" and "competition," and he has scoffed so much at the notion of "spontaneous consensus" that the reader is startled to see it suddenly labeled "the historical style of decision-making in science."

Blissett probably has good research instincts, for he touches on a good assortment of case studies for investigating "politics in science." In the dispute over membranes he picks a scientific community aware that an important problem is unsolved. In the Velikovsky affair he lights on a community overwhelmingly convinced that there is no problem except the definition of professional competence. In the debate over causality in quantum mechanics, he finds a community of physicists largely indifferent to a problem they consider philosophical. The trouble is that Blissett only touches on such cases, and never gets seriously involved in showing "how institutional activity refines and extends [or coarsens and limits, I would add] our perceptions of reality" (p. 196). That is a very worthy goal he sets himself. Let us hope he serves it better in his next book.

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