

gathered not only through archeological field survey but also from a wide variety of sources including classical authors and inscriptions, early European travelers, and modern statistical studies.

This volume is an important contribution, from both the theoretical and the factual points of view, to the archeological literature concerned with the Aegean. Though previous archeological excavation reports have frequently contained specialist appendixes concerning environmental and other matters, such studies have usually lacked a coherent conceptual framework within which all the data can be analyzed and brought to bear on a central theme. The editors are correct in implying that the detailed reconstruction of the archeological-historical sequence for a certain area should not be an end in itself but should serve as a starting point for discussion of possible explanations for the observed changes. Although the idea of such comprehensive studies is not new, such an approach has been adopted only on a small number of Aegean and Near Eastern projects, and publication of the reports of most of these is still pending. This volume also demonstrates the importance of human geographical studies in the determination of factors influencing settlement systems. Geographical models can be of great assistance to the archeologist engaged in such analysis; conversely the archeologist alone can provide the essential data to allow the geographer to extend his or her studies back into prehistory. Perhaps the most important contribution of this book is the evidence it affords of the value of cooperation between scholars from a variety of disciplines all bringing their own kinds of expertise to bear on a specific problem.

Trade and the exploitation of Melian raw materials, especially obsidian, figure prominently in the volume. A detailed new geological study of the obsidian sources was undertaken, and numerous additional trace element analyses (by means of x-ray fluorescence and neutron activation) were carried out. The obsidian derived from the two Melian source areas (Sta Nychia and Demenegaki) can now be clearly separated, but no evidence was found for differential exploitation of the two sources. The claim that this study represents the first detailed characterization of an obsidian source in the Old World is not exaggerated; of interest for characterization studies in general is the conclusion that little variation may be expected in the composition of samples derived from the same obsidian flow in the Mediterranean area. It is also of interest to note that, despite

evidence for the utilization of Melian obsidian as far back as the 11th millennium B.C. at Franchthi Cave on the Greek mainland, the earliest known sites on Melos itself date to the Late Neolithic period (later fourth millennium B.C.), and only in the succeeding earliest phase of the Early Bronze Age (3300 to 2700 B.C.) is clear evidence encountered for permanent settlement.

Although the objective of using Melos to illustrate the dynamics of culture change in general proved difficult to fulfill, the volume represents a considerable

advance over the level of studies frequently appended to published excavation reports. Matters for further research have been delineated, and the value of the underlying comprehensive approach has been clearly demonstrated. It is to be hoped that comparative research in other regions will demonstrate the extent to which similar processes may be observed with regard to cultural change.

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Affliction in the Workplace

Mass Psychogenic Illness. A Social Psychological Analysis. MICHAEL J. COLLIGAN, JAMES W. PENNEBAKER, and LAWRENCE R. MURPHY, Eds. Erlbaum, Hillsdale, N.J., 1982. xvi, 270 pp. \$29.95. Environment and Health.

It was June, the height of the production season for women's clothing in the Northern mill. For several weeks previously, workers had been complaining about the bite of an insect, and the plant had been sprayed several times to no avail. Suddenly, in an 11-day period, 62 people became afflicted with symptoms they believed to be associated with the insect bites: nervousness, burning in the calf, numbness in an extremity, feeling like a "balloon ready to burst," and inability to turn the head. Of these cases, 59 were women in the dressmaking department or the adjacent cloth-mending department. Since as many as 11 persons were afflicted on the first day, the plant had to be shut down. Baffled physicians found a few specimens of a small insect the size of a mite but could find no relation between the insect and the symptoms. By the tenth day, physicians declared that, though the symptoms were certainly real and of great discomfort to those experiencing them, the least important factor was the bite of the insect; anxiety and nervousness seemed most pervasive. Although the physicians were hesitant to use the phrase, the newspapers were already calling it an "episode of mass hysteria."

A brief account of the "June bug epidemic," the subject of a case study published by Alan C. Kerckhoff and Kurt W. Back in 1968, appropriately opens this most interesting and useful book on a

phenomenon the authors call mass psychogenic illness (MPI). In its broadest sense MPI refers to any "outbreak" of illness-like behaviors for which a biological basis is initially suspected but for which a biological cause is not found or, in retrospect, the biological cause adduced is judged not to be credible. Thus, the term covers such strange behaviors as the "dancing manias" of the Middle Ages in which hundreds of persons would engage in dancing to exhaustion (the Italian version was considered by people in that age to be the result of the bite of the tarantula). It also includes instances of demonopathy in which "epidemics" of strange cries, convulsions, abnormal movements, and the like were attributed to demonic possession (late 16th and early 17th centuries). Coming closer to our own time, MPI includes school children fainting and vomiting during the rehearsal of a school play and episodes (usually ranging from several hours to a week or 10 days) of fainting, nausea, vomiting, and feeling dizzy occurring in visible and public form among workers in modern industrial work settings.

Anyone coming to the collection of papers in this book expecting to find accounts of such phenomena as dancing manias and demonopathy will be disappointed. The symposium on which the book is based was sponsored by the National Institute of Occupational Safety and Health, and with one exception, to be discussed later, the authors take as their purview the occurrence of MPI in 20th-century industrial work settings. This is in keeping with the interests of two of the editors, Colligan and Murphy,

who published a definitive review of the literature on MPI in work settings in 1979 (a summary of that article is contained in this volume). It may also reflect a desire to hold the situation relatively constant to get some sense of relevant explanatory variables. In any case, a more accurate title for the book would be "Mass Psychogenic Illness in Industrial Work Settings."

One comes away from the book with a strong sense that the methodological, epistemological, and political problems of studying MPI are daunting ones. It is to the credit of the contributors that many of them confront these problems directly. Consider the following: An MPI episode (in the industrial setting) lasts on the average from a few hours to 10 days. By the time the medical and technical experts have ascertained that no biological cause can be found to account for the behavior, several weeks may elapse. Even if the social scientist is quick enough to grasp the possibility that an outbreak of illness is MPI, it is some time after the event that he or she can get access to the participants. Thus all studies of MPI in industrial settings have been retrospective studies in which eyewitness interviews are the basic data base. Moreover, one is never sure that there is no biological vector to be considered. Imagine if members of the American Legion meeting in Philadelphia had come down in larger than usual numbers with a flu-like disease with symptoms only somewhat more strange than other types of flu. Suppose there had been no deaths? Would the attempts to find the cause of the condition have been as thorough as the search that led to the discovery of the Legionnaire's disease agent? Would some enterprising social psychologists have come upon the scene once the medical people had left and have started studying an interesting example of MPI? In other words, MPI is always a residual category with biological factors probably never entirely ruled out.

Robert F. Boruch's discussion of evidence and inference in research on MPI enlarges usefully on these matters, particularly focusing on issues of diagnosis, misdiagnosis, and the relationship of symptomatology to measurement. For instance, he points to the difficulty of comparing studies in terms of uniformity of symptomatology and to the importance of checking on relapse rates in affected individuals. In line with these comments, Jerome E. Singer points out that something gets called MPI when it becomes dramatic, visible, and an obvious impediment to the functioning of the

organization. But indeed it is often the case that silent suffering, expressed in higher absentee rates, consultation with doctors outside the workplace, and the like, may be much more widespread and may have been occurring long before the "outbreak" (and may persist long after).

The politics of studying MPI is equally perplexing and extremely sensitive, testing the ethical commitments and responsibility of researchers in this area. On the one hand, management may have a commitment to avoid the cost of cleaning up the workplace and would welcome a "diagnosis" of MPI ("it's all in their heads"). On the other hand, where labor is well organized, union officials may have an equal commitment either to resist such a "diagnosis" or to use it as an example of "stress-related factors" indicating poor working conditions. This is a minefield for the "objective" social scientist interested in understanding the basic processes underlying the phenomena. Indeed, the study of MPI would make a fascinating case study in the relationship of scientific research and policy-making.

The politics of MPI research is well illustrated in the papers by the subtheme of sex differences. Data show that 80 to 90 percent of all persons affected in MPI episodes in industrial work settings are women. Explanations for this fact fall into two categories. In the papers by Colligan and Murphy and by Singer, Baum, Baum, and Thew, explanations are given that stress either purported feminine expressive and somatizing styles and the consequent conflict between the role of worker and the role of housewife or the fact of menstruation. The latter is considered important on the grounds that it periodically makes women more sensitive to internal sensation, which in turn is important for one theoretical explanation of MPI (see below). To Kerckhoff, all this sounds uncomfortably close to saying that "women are just like that." He would like us to remember that women are often stuck in the lowest-paid, most boring and repetitive jobs, with little possibility of advancement. He would also like us to consider that this means that women are stuck in the least attractive, noisiest, most physically stressful work. The issue, he implies, is not the nature or role of women but the economic and social situation that subjects women to the sort of stress that predisposes to occurrences of MPI. Joseph E. McGrath takes a middle position. He doubts that the occurrence of MPI in women can be accounted for on biologic or experiential bases. However, he would like to see further research on the extent to which

women expose themselves to certain information and on the ways in which women acquire and process information. The political implications for change in the workplace of any of these positions are quite clear.

By implication, a political problem exists with the term MPI itself. To say anything is "psychogenic" in origin is to bias one toward a certain set of causative "agents," and, at an extreme, opens one to the temptation to "blame the victim" for the problem. A similar point could be made with respect to Sidney M. Stahl's thought that the term be replaced by "mass sociogenic illness." McGrath makes a very good suggestion that these phenomena be retitled "multiple occurrences of unexplained symptoms" (MOUS). Indeed, this is only one of many good suggestions McGrath makes in his paper on conceptual and methodological problems in MPI research. Along with the Boruch paper on evidence and inference in MPI research, McGrath's paper makes for highly important reading for anyone interested in undertaking research on this phenomenon.

The book is subtitled "A Social Psychological Analysis." As one might expect from modern psychological social psychologists, the conceptual framework favored is a perceptual-cognitive information-processing approach to social influence. It follows that a principal theme is the way in which information comes to be the "infecting agent" (McGrath). The potential MPI victim is usually exposed to a fairly complex set of information: the fact that an initial occurrence, or triggering event, has taken place, the fact that symptoms are occurring in others, and the nature of the symptoms themselves. Information may come through three different sources: direct verbal statements individuals make to one another, interpretations individuals place on each other's behavior, and interpretations individuals place on their own behavior and internal sensations.

McGrath makes the important point that embedded in all theoretical accounts are the problems of individuals' differential exposure to information and differential susceptibility to it. A general model for MPI incorporating those concerns emerges when one considers four of the papers in this book as a group. Pennebaker describes the clever experiments he and his colleagues have carried out to ascertain the conditions under which people pay attention to internal sources of information (for example, tiredness, stomach rumblings) and the conditions

under which they will label these feelings as symptoms of illness. An interesting point is that when individuals are sensitized to internal sensations, that is, when they monitor their sensations more closely, they are more likely to perceive these sensations as symptomatic of illness.

Singer, Baum, Baum, and Thew carry the analysis one step further. They apply a familiar social comparison perspective. Thus, they note that when people are physiologically aroused and have no ready explanation for the arousal they seek information from others to provide labels for that state. Since MPI appears to occur when individuals are in some state of stress, with its implications for physiological arousal, these authors' approach appears to have merit. (However, it should be noted that controversy surrounds some of the experiments on which this perspective is based—see Zimbardo, Ebbesen, and Maslach, *Influencing Attitudes and Changing Behavior*, Addison-Wesley, 1977, and Singer and Schachter's response, *Journal of Personality and Social Psychology*, 37, 989 [1979].) In line with this approach, Singer *et al.* look for characteristics of others that make it likely that individuals will be exposed to information about internal states and that make individuals more susceptible to labeling their internal states as "illness."

For a social psychologist, it is of great significance that MPI does not occur in a random fashion but seems to "infect" people who either are in direct perceptual contact with each other or are part of a social network (hence the earlier term, "contagion"). It makes sense that people who know each other will look toward each other for information or will be influenced by friendship ties and relative status within the network. Stahl makes this point in exploring an approach using the "emergent norm" concept to understand MPI. As MPI develops, his idea goes, labeling oneself as ill becomes increasingly legitimated and more people, particularly if they are within the network, will become infected. This explanation for the persistence and spread of MPI works well for Stahl's example, a truly Dickensian picture of a "data processing center" in which all affected workers punched IBM cards on old and noisy keypunches in one large room lacking soundproofing. There was little time for lunch, rest, or conversation with friends (although visual contact with friends was continuous). Additionally, workers were underpaid and rushed and had execrable relations with man-

agement. Stahl found a remarkable sharing of symptoms among those in the same friendship network. He interpreted these findings as indicating the development of a norm legitimating illness as an escape from an impossible situation. Of course, this idea will not help explain who gets MPI first (not infrequently, it is an isolate or a peripheral member of a sociometric network) or why it occurs simultaneously in unrelated individuals, as sometimes happens (a phenomena called "convergence").

Kerckhoff's contribution to this shared approach is to note that susceptible individuals not only differ from those unaffected by felt stress, certain personality characteristics, and position in a social network, they differ by their beliefs in the credibility of the illness explanation. This idea, of course, is consonant with Pennebaker's concept of the importance of the conditions under which one labels internal feelings as illness. Illness explanations that are too different (how different is not entirely clear) from one's own ideas about illness are unlikely to have as great an impact on the appearance of symptoms.

The framework that emerges from these four papers seems to fit data on characteristics of affected as opposed to unaffected individuals in MPI "outbreaks," situational factors characteristic of such "outbreaks," and psychological aspects of the work environment, as reported for the "June bug" outbreak and the extensive set of cases reviewed by Colligan and Murphy.

Yet, satisfying as this emerging framework seems in some respects, in others it seems incomplete. First, as McGrath suggests, if MPI is about social influence, it would have been useful to include some of the social psychological theory and vast empirical literature on persuasive communication and attitude change. A second point is raised by Jonathan L. Freedman. From his review of earlier theories of "contagion" he suggests a modern version of the psychodynamic notion of "superego restraints" and their effect on the occurrence of nonnormative behavior, such as MPI symptoms. In other words, he asks what conditions lead to a lowering of internal restraints on behavior and how this information might help us to explain the occurrence and form of the symptoms. This psychodynamic analysis adds an important dimension lacking in the cognitive-perceptual information-processing account.

François Sirois's paper highlights a third missing element. In what sense

does an explanation of MPI in industrial settings generalize to dancing manias, demon possession, and other MPI phenomena? Sirois does not have a good answer to this question, but he does provide a descriptive typology of MPI episodes. These categories help broaden the analysis provided by other social psychologists in this collection.

Fourth, it is in a paper on stress and coping by Susan Folkman and Richard S. Lazarus that one becomes aware of what is most importantly missing from the social psychological account offered earlier. Folkman and Lazarus's approach is to ask how the individual learns to cope with and adapt to particular situations over time. In brief, the person evaluates how difficult the situation is and how well equipped he or she is to deal with it in terms of the resources at hand. Thus stress is an emergent phenomenon from the interaction of the person and the situation. We are, therefore, asked to consider the individual in his or her embeddedness within organic or physiological, interpersonal, and social systematic contexts. Since the person exists at each of these "levels," the important question concerns the interaction of these levels in determining behavior. Folkman and Lazarus are critical of the use of the concept of stress in other papers in the volume because the authors do not analyze the level at which stress occurs—at the level of the physiological states (physical stress), psychological states (anxiety), psychological traits (sex-role identification), or social process (social conflict). Nor, from Folkman and Lazarus's perspective, do these authors indicate how interaction of these levels of stress affects the occurrence of MPI symptomatology. Such an approach supplements the cognitive-perceptual information-processing approach by asking us to look at the individual in the totality of his or her functioning within both the immediate and larger sociohistorical context.

Finally, one further but related missing element may be noted. The second paper in the book is an account by W. H. Phoon of outbreaks of "hysteria" at workplaces in Singapore. Likewise, Colligan and Murphy briefly summarize Chew's work on MPI in industrial settings in Taiwan. A reader will note that there are fascinating differences as well as similarities between these cultures and our own. For instance, weeping, screaming, convulsions, and aggressive behavior are all part of the symptomatology in Singapore and Taiwan; how different from the more sedate dizziness,

fainting, nausea, and the like seen in the American workplace. Will the model developed in the book hold up in these and other cultures? Or might these other cultures give us new hints for different models? How interesting it would have been to have a cultural anthropologist give an analysis of the meaning of MPI in these other cultures!

It is clear that this book has much to recommend it. The papers by McGrath, Boruch, Schmitt and Fitzgerald (the last on statistical analysis of aggregate data), and Singer (on political and other matters) are worth the price of admission in alerting the researcher to the subtle difficulties of work in this field. Moreover, the book is a fascinating portrait of social psychologists struggling to make sense out of a phenomenon all the more important because of its economic and social consequences. Indeed, it is impressive to see that current social psychological thinking can supply a mostly satisfying account of MPI in the work setting. If I were teaching a course on "how social psychologists go about their job," I'd assign this book. Finally, the book performs a valuable service, perhaps by indirection, in showing how much more powerful it might be to exploit the resources of allied disciplines, such as cultural anthropology, political sociology, and social history in interdisciplinary understanding of MPI. I eagerly await *that* book.

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Language in the Legal Process

Linguistic Evidence. Language, Power, and Strategy in the Courtroom. WILLIAM M. O'BARR. Academic Press, New York, 1982. xvi, 192 pp. \$23.50. Studies on Law and Social Control.

William O'Barr's book is a concise, lucid summary of a series of studies of language and communication in the courtroom conducted by O'Barr and his colleagues at Duke University. Publication of the book, and of several others in recent years (*1*), establishes the empirical study of language and communication in the legal process as an important endeavor of both academic interest and real-world significance. For those who are familiar with the various articles previously published by the O'Barr team, the book will provide little new informa-

tion, though it will be handy to have the results of the research summarized in one volume. The clear, simple style of the book will make it accessible not only to social scientists but also to members of the legal profession and to the intelligent lay reader.

O'Barr, an anthropologist, headed an interdisciplinary team consisting of two social psychologists, a graduate student in both law and anthropology, a law professor, and a linguist. The research was designed to examine some of the ways in which how something is said, rather than what is said, matters in the courtroom. The effects on judgments of courtroom testimony of four sets of factors were studied: "powerful" versus "powerless" speech, hypercorrect versus formal speech, narrative versus fragmented testimony, and simultaneous speech by witnesses and lawyers. The research combined ethnographic analysis of trials tape-recorded in a North Carolina courtroom and controlled experiment. Informed by the ethnographic work, the researchers created experimental variations of a given portion of testimony, keeping the content—the "facts"—the same and changing aspects of style or form.

The book is divided into seven chapters. After laying out some of the reasons why a consideration of form in language is important (chapter 1) and examining some of the distinctive features of both written and spoken legal language (chapter 2), the author provides an overview of assumptions lawyers make about how language and communication work in the courtroom (chapter 3). The main point of chapter 3 is that these assumptions, for example that too many qualifications of an answer have a bad effect on the decision-maker, whether judge or jury, are really hypotheses that ought to be put to empirical test. Chapter 4 explains the research methods used and the rationale for using them and shows how the research questions derive from current work in sociolinguistics, social psychology, and social anthropology. Chapters 5 and 6 are devoted to the presentation of findings. The book concludes with a discussion of implications for both law and social science of the results of these studies.

The heart of the book is chapter 5, which summarizes the results of four sets of experiments designed to test the effects of the four sets of linguistic factors outlined above. The most striking results are those obtained on the effects of "powerful" versus "powerless" speech styles on judgments of the credibility or

intelligence of a witness. Consider two ways in which a witness might answer the question "What is the first thing you remember after the accident?"

I guess I remember coming in to the Duke Emergency, and I thought I was at the police station because it seemed like there were so many policemen around.

I remember coming in to the Duke Emergency, and I thought I was at the police station, because there were so many policemen around.

The content—the "fact"—is the same, but the form is different. In their ethnographic work O'Barr and his associates found that the former, hedging style is not unique to women, as some had previously thought (see, for example, 2). Using experimental recordings of testimony in the "powerful" and "powerless" styles, the O'Barr team demonstrated that both a woman and a man speaking in the powerless mode are judged less credible, less truthful, less competent, less intelligent, and less trustworthy than a person of the same sex speaking in the powerful mode. This implies that a person telling the truth but speaking in a powerless style may actually be judged to be less credible than a person who is lying but does not hedge and qualify his or her answers.

As for the effects of narrative versus fragmented (short-answer) testimony, the results are highly equivocal. The author's interpretation of them is, by his own admission, highly speculative and, in this reviewer's opinion, is too convoluted to be convincing (or is it that his hedges have reduced the credibility of his interpretation?). The researchers focused primarily on how narrative versus fragmented testimony affects subjects' and jurors' attributions of what a lawyer thinks of a witness, rather than on how the type of testimony affects judgments of the witness, as is the case with the experiments on "powerful" versus "powerless" speech and hypercorrect versus formal speech. Differences emerge between the judgments of subjects with and without legal training, but what these differences mean is by no means clear.

By not using judgments of the witness as the dependent variable in all four sets of studies, the authors have lost an opportunity to assess the relative importance of each of the four sets of linguistic factors in determining how a witness is perceived. A comparison of the results of the first and third studies suggests, however, that powerful style is probably more important than formality.