LOCAL SOCIAL DISRUPTION AND WESTERN ENERGY DEVELOPMENT  
A Critical Review

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The assumption that energy development causes social disruption in western communities is based on undocumented assertions, questionable interpretations of evidence, and superficial analyses. Theory used implicitly in research on western communities that are affected by energy development is inconsistent with the history of the region and with recent developments in community research. The literature provides suggestions for research, but improvements in theory and methodology will be needed if responsible contributions to energy development policy are to be made in this research.

The research literature on western energy development (reviewed earlier by Murdock and Leistritz, 1979) provides an important first step toward developing a scientific understanding of the social effects of rapid energy-related growth at the local level. This literature is ripe with substantive suggestions of topics and issues needing precise conceptualization and analysis. The need for taking the next step—that is, for developing precise conceptual

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and analytical approaches—is demanded not only by requirements of scientific methodology but also by the controversial nature of the subject being investigated (Freudenburg, 1981:240). Journalistic accounts of life in western "boom towns" (Kelly, 1980; Feldman, 1980; Kittredge, 1981), which might influence public opinion, and policy discussions (Christiansen and Clack, 1976; Gilmore, 1976; Cedar and Salasin, 1979), which might influence state and federal action, express the assumption that local social effects of western energy development are severe and negative.

This report reviews the research basis for this assumption. The first section of the review appraises the evidence by which many studies conclude that rapid, energy-related growth is causing increases in rates of crime, marital instability, alcoholism, suicide, mental breakdown, and other behavioral disruptions in small communities in the western states. The second section examines the theoretical basis of the proposition that such a finding would be expected. The third section discusses a research perspective from which an improved understanding of the local social consequences of energy development might be pursued.

**EVIDENCE OF DISRUPTIVE EFFECTS**

The general tone of a number of papers on western energy development is expressed by the opening sentence of that most frequently cited. According to Kohrs, "The history of power production—synonymous with boom development—in Wyoming is a dismal record of human ecosystem wastage" (1974: 1). On the basis of his experience as clinical director of a counseling center in Wyoming, Kohrs argues that overcrowding and inadequate planning for meeting needs have produced "drunkenness, anomie, mental discord, suicide attempts and teen-age rebellion" (p. 2). He describes what he calls the "Gillette Syndrome":

A housewife, after fighting mud, wind, and inadequate water and disposal systems, a crowded mobile home and muddy children all day, snaps at her husband as he returns from a 16-hour shift. He responds by heading back downtown and spending the night at a
bar drinking and trading stories with men from similar circumstances [p. 3].

This pattern, says Kohrs, leads frequently to divorce. Suicide attempts, he says, occur at the rate of one per 250 people. He cites data from unspecified sources\(^1\) to support assertions about increases in divorce, depression, school dropout rates, juvenile delinquency, criminal activity, welfare caseloads, mental breakdown, and other social problems. Assertions by Kohrs might be taken to suggest hypotheses for research. Unfortunately, these assertions are taken by many researchers as primary documentation of social disruptions (Gilmore, 1976: 536; Gould, 1978: 20; Foster, 1977: 9; Freudenburg, 1976: 11; Cortese and Jones, 1977: 8; Little, 1977: 408-409; Albrecht, 1978: 80).

A second widely quoted paper, by Gilmore (1976), argues that rapid population growth resulting from increased basic employment in energy development strains the capacity of a small community to provide “goods, services and intangibles” (p. 536). He describes a situation in which growth leads to institutional breakdowns in the labor market, the housing market, and the system for financing public services. Services, he says, tend to lag far behind demand. Alienation, suspicion, and criticism of authorities increase. Mental health clinic caseloads increase dramatically. Newcomer wives in mobile homes are isolated from the community. Young people drop out of schools to take jobs in construction, and mining productivity declines by 20 percent. He is describing, of course, an imaginary town, but one which he claims to be typical (1976: 535). The problems, he says, are spelled out in the report of a case study and in the paper by Kohrs (p. 540).

An examination of the case study report (on Sweetwater County, Wyoming) shows that it indeed claims that such problems occurred (Gilmore and Duff, 1975) but that it does so without indicating any source of evidence in support of that claim. The authors claim that rates of alcoholism, broken homes, suicide and suicide attempts, and “down-and-outs” increased (1975:12), that crime rates went up, and that prostitution and drug dealing were introduced to the county during the mining and construction boom of the early 1970s (p. 14). The data presented
in this report are findings of an opinion survey that did not deal with these phenomena. The totally undocumented claims about increased pathology used to introduce the survey data are cited elsewhere, unfortunately as are the claims by Kohrs, as primary documentation of social disruptions (Gould, 1978: 20; Foster, 1977: 9; Freudenburg, 1976: 11; Cortese and Jones, 1977: 82; Albrecht, 1978: 80; Little, 1977: 402; Murdock and Leistritz, 1979: 252).

Flaws in scholarship are apparent in this literature in citations of undocumented assertions as evidence, questionable interpretations of empirical data, overgeneralization of conclusions, and absence of controls in measures of relationships.

A pattern of undocumented assertion being cited in subsequent papers as documentation may be traced through several papers. Foster (1977) cites Kohrs (1974) and Gilmore and Duff (1975) and adds that a staff member of mental health clinic in a growing town said that "long-time residents are the ones most affected by the disruptions," and that "the long-time resident is more likely to become an alcoholic or suffer from mental illness" (1977: 9). O'Hare and Stinson (1977: 3) cite Foster (1977) as a source of evidence that long-time residents are thus disposed, and they cite Little (1977) and Gilmore and Duff (1975) as providing evidence of other disruptions. Little cites Gilmore and Duff and Kohrs as providing evidence of these (1977: 408-409).

Murdock and Leistritz, though generally critical of the quality of research on social impacts of energy development, report that "higher [than previous] rates of divorce and increased marital strains have been recorded in many impacted areas" (1979: 251) and that "several researchers provide strong documentation of a general increase in crime, drug abuse, mental illness, child abuse, and related problems in impacted communities among both new and long-time residents" (p. 252). A review of the sources they cite for these statements fails to uncover any confirmed evidence of the increases to which they refer. One source is the case study of Sweetwater County (Gilmore and Duff, 1975) discussed above. Another is a paper by Albrecht (1978) discussing changes he would expect to occur, partly on the basis of earlier reports by
Kohrs and by Gilmore and Duff. A third source also contains no data on the problems indicated by Murdock and Leistritz, but reports survey data showing no significant difference between population samples of two growing and two stable counties in proportions believing that crime had or had not increased in recent years (Thompson et al., 1979). The fourth source cited by Murdock and Leistritz contains data used by the author to indicate that crime increased in two North Dakota counties during construction of an antiballistic missile site. No quantitative data were available on one of the counties, and the range of estimates of increased crime by police officials were, according to the author, "too far apart for credibility" (McDonald, 1976: 65). Sheriff's office data presented on the second county show increases in numbers of complaints, warrants, and investigations but give no information on number or types of offenses committed (p. 66). Thus, documentation of the conclusion is not found in any of these sources.

Questionable interpretations of empirical evidence are given in several papers. Freudenburg cites a community caseload review in Craig, Colorado, as containing evidence that complaints and incidents of problem behavior increased enormously on a percentage basis—crimes against persons by 900%, child behavior problems by 1000%—as the population doubled (1978: 7). The report of this review reveals that these percentages are based on small initial numbers—two crimes against persons, three child behavior complaints (Lantz and McKeown, 1977: Table II). Weisz says that admissions to mental health facilities serving Campbell County, Wyoming, increased greatly during a period of growth because of increased stress (1979: 42). The data he presents (pp. 41-42) can be used to compute admission rates per 1000 residents. These show a modest, irregular increase: 27 in 1975, 31 in 1976, 29 in 1977, and 34 in 1978. Little presents data on population and crimes in Page, Arizona, where construction of a large power plant began in 1971 (1977: 417). The population changed from 1439 in 1970 to 3603 in 1971, 5618 in 1972, 7240 in 1973, and 6902 in 1974. Crime rates per 1000 residents can be calculated from his data, as follows: 275 in 1970, 240 in 1971, 252
in 1972, 190 in 1973, and 95 in 1974. Comment might be expected on why the crime rate dropped as construction proceeded and the population peaked. Instead, the author (1977: 422) says:

A community with a rapidly growing population that is not integrated into the social structure and is highly transient, that is, a boom town, would be expected to experience an increase in crime rates. In this respect, Page is not atypical.

This is an inaccurate interpretation of the data, even with the qualification, noted by Little, that population increased "more rapidly than crime in some years" (p. 422).

Several papers contain generalizations covering a number of types of disruption. This ignores previous evidence that some of the rates which might be treated as disruptions, such as homicide and suicide, might be negatively correlated (Martin, 1968: 90) and is questioned by findings of studies of energy development communities in which quantitative indicators of two or more dependent variables are used. Thompson reports that the rate of property crimes increased during a period of growth in Campbell County but that divorce rates and welfare payments did not increase (1980). Brookshire and D'Arge (1980: 535-539) report that in 36 western cities some crimes increased with growth while others did not.

The study by Brookshire and D'Arge is one of the few using multiple regression methods, but the period covered, 1970-1972, predates the period in which much of the energy-related growth has occurred in the region. A recent paper on the Northern Great Plains reports that bivariate associations of the divorce rate with measures of local population size and growth become insignificant when structural characteristics of the localities are controlled, but the analysis uses divorce data for 1970 only (Thompson et al., 1980). Additional studies such as these, with more recent data, are needed to test claims made by others.

Inadequate attention in this literature is given to the possibility that increased incidence of recorded behaviors might result from such factors as changes in the recording system, changes in age composition of the local population, or changes in budgets, staff
sizes, or policies of agencies responsible for locating and dealing with the behaviors.\textsuperscript{4}

Murdock and Leistritz (1979: 273) conclude their review of the literature on disruptive social effects of western energy development with the observation that "the available information is sufficient to validate the levels of concern often expressed regarding such impacts." The present review of cited evidence, in contrast, finds no acceptable basis for such a conclusion which refers to "a general increase in crime, drug abuse, mental illness, child abuse and related problems" (Murdock and Leistritz, 1979: 252). Assumptions in this literature seem to be informed substantially by hearsay evidence. Serious flaws in scholarship are apparent in the treatment of sources and in interpretations of data.

It therefore cannot be assumed that research shows disruption, in the sense of increased rates of what are commonly called social problems, to be caused by rapid, energy-related growth in small communities of the region. By the same token, conclusive evidence to the contrary is not available. Research is needed to test the hypothesis that such changes occur.

\textbf{THEORETICAL ASSUMPTIONS IN THE LITERATURE}

Why would rapid, energy-related growth in small communities of the western states cause increased rates of individual and family disruption? A large number of specific postulates are suggested in the literature on western energy development and others might be drawn from previous research. Kohrs, Gilmore, and several others cited above emphasize crowding and excessive demands on public services as sources of distress. Gold (1974, 1976) reports that distress results from contrasts in lifestyle, interests, and resources of newcomers and oldtimers, especially between newcomers and ranchers, whose viewpoint he has examined through ethnographic research. Others suggest that a class of drifters referred to as "riff-raff" (Kohrs, 1974: 3; Freudenburg, 1976: 17) contribute, at least temporarily, to local
distress. Little (1977: 414) argues that transient populations are associated with high crime rates and asserts that transiency can be indexed by the proportion of mobile housing units in a community (1977: 418). Several papers argue that distress results from changes in local social interaction which are caused by growth (Carnes and Friesema, 1974; Little, 1977: 410-412; Cortese and Jones, 1977). Energy-related growth in rural areas is said to cause a breakdown in informal social controls (Little, 1977: 415), antagonisms among former friends and between newcomers and oldtimers (Little, 1977: 411), and a reduction in neighboring (Cortese and Jones, 1977). Albrecht (1978: 79) argues that growth impacts and impacts associated with “diversification” interact in relatively small, highly homogeneous areas “to compound potentially deleterious sociocultural consequences.” An additional source of distress identified in some discussions of energy development is described as domination and exploitation of the rural community by outside interests (Albrecht, 1977: 86-87; Massey, 1980).

Research on western energy development is not based on a cogent theory, but many of the postulates suggested in discussions of local social effects relate, at least implicitly, to a theoretical perspective that can be traced to the typological approach used earlier by Ferdinand Toennies, Emile Durkheim, Henry Sumner Maine, Robert Redfield, and Ernest Becker. A conceptual distinction is drawn between rural and urban forms of social organization. The rural form is assumed to be based on informal, sentimental ties among people who share a common tradition and whose lives are rooted in symbiotic interdependency. The urban form is assumed to be based on formal, impersonal rules which regulate exchanges within large, dense concentrations of people from dissimilar backgrounds. Several essays, written early in the twentieth century, argue that social distress and interpersonal hostility occur less frequently in the rural community than in the urban community because of the impersonality of social life of the latter (Wirth, 1938: 23; Sorokin and Zimmerman, 1929: 56-57; Simmel, 1950). The urban form of social organization is observed from this perspective to spread among societies and among communities in modern society. Durkheim’s concept of anomie is
cited to account for what is seen as extreme disruption associated with change (Wirth, 1938: 13). Local social life is assumed to be increasingly dominated by events and organizations in the larger society (Tilly, 1974; Warren, 1978).

The conclusions of many discussions of western energy development are consistent with this theoretical perspective. Cortese and Jones (1977: 85) assert, on the basis of their case study observations, that the changes "add up to the process of urbanization or, depending upon one's orientation, modernization." Gold (1974: 9) reports that in communities experiencing rapid growth because of increased coal mining "the traditional 'gemeinschaft' type of rural society is being superseded by a much looser form of social organization." Freudenburg (1976: 15) argues that "sheer change" in the process of energy development causes anxiety and maladaptation and that "a social fabric can stretch only so far and so fast without shredding, particularly in the case of a tightly knit community." Other writers report that energy development communities are being exploited and dominated by outside interests. Albrecht (1978: 86-97) says:

In many ways, we are now experiencing a form of domestic colonialism. Capitalist exploiters are developing vast energy resource deposits in what are basically rural areas of the country. The profits from such exploitation are usually exported from the area while many of the problems—such as environmental degradation and increased social pathologies—are left behind.

Massey (1980: 198) says:

Energy-impacted communities are providing energy development operations with necessary labor and are absorbing most of the impact-induced problems. . . . The company towns of Appalachia were reflections of a corporate mentality that treated labor as a process separate from everyday life. The more modern outlook for corporations is to recognize the interrelations of these two spheres and to pursue its [sic] management efforts in both.

Thus urbanization, rapid change, and outside domination are asserted to be sources of social pathology in western energy development communities.
The utility of these assumptions for understanding potentially disruptive effects of energy development can be evaluated on several grounds. One has to do with validity of assumptions about prior trends and conditions in these communities. Do (did) these communities have the type of social organization described as rural in the classical typologies? The general history of communities of the region suggests that, with few exceptions, they did not (Vogt, 1955; Kraenzel, 1955, 1980; Pang and Hanson, 1968). The relatively short histories of most western communities would not be sufficient for development of the kinds of relationships indicated in the classical rural type. In fact, it is doubted by some sociologists that this type has ever occurred in the United States (Dewey, 1963: 66; Mann, 1965: 4; Bernard, 1973: 96). Further, the histories of communities of this region have been marked, in most cases, by recurring conflicts and upheavals and by crucial, fluctuating ties to the larger society. Conflicts between oldtimers and newcomers described in modern times recall earlier conflicts between ranchers and homesteaders and earlier ones between white settlers and original residents (Nellis, 1974: 229). Periods of heavy immigration and outmigration have occurred frequently in the region in response to droughts, cycles in cattle markets, and "boom and bust" conditions in mining (Kraenzel, 1955). Reports of a persisting, individualistic bias in the culture of the region (Pang and Hanson, 1968) are inconsistent with the assumption that the communities are of the classical rural type. Conflicts among local groupings have persisted over many years in some communities, and the claim that some ranchers are "coming back to gemeinschaft" in defensive response to energy development (Gold, 1974: 7-8) contradicts the assumption that an integrated rural society existed before the recent development. The historical evidence supports, instead, the observation that the form of social organization described as urban in the classical typologies has prevailed, more or less, in this area and that local communities in this region have developed in close contact with trends and forces in the larger society.

A second point to be evaluated is the assumption that rural life, in this region or elsewhere in modern society, is less stressful than urban life. Theorists who emphasize a difference in stress levels in the classical forms of social organization also argue that these
differences tend to be reduced as urbanism spreads throughout a society (Wirth, 1938; Simmel, 1950). Attempts to measure rural-urban differences in stress levels in recent years have produced mixed, inconclusive results (Flax et al., 1979: 27). Several studies show the incidence of psychological distress to be higher in urban settings than in rural settings, but others show the opposite (pp. 20-26). An extensive review of the available evidence for the National Institute of Mental Health concludes that higher levels of extreme psychological distress appear to be more frequently found in rural communities than in urban communities in modern society, but that many questions remain unanswered concerning the adequacy of measures of this difference (Flax et al., 1979: 27).

A third point to be evaluated is the assumption that disruption occurs because of the speed and volume of change with which community residents must cope during energy development. Studies of national trends correlating stress indicators with measures of economic change provide support for the theory that change can produce stress (Henry and Short, 1954; Brenner, 1973). Studies at the individual level indicate, though not conclusively (Rabkin and Stuening, 1976), that vulnerability to stress-related disorders can be increased by major changes in one's life. Whether change actually produces widespread distress in western communities which experience energy-related growth is an unanswered question. In the absence of reliable data, an answer might be suggested by assumptions about prior community conditions and trends. If these communities, like others in modern society, are highly interconnected economically and socially with the larger society, many of the norms which regulate social life and many of the influences on the behavior and psychological well-being of people might be more or less independent of changes and social characteristics in the immediate community of residence (Srole, 1972: 581-582). In communities (and in a region) in which change has been occurring with some regularity in the past, it would seem likely that social and personal mechanisms for coping with change would have developed. National survey evidence presented in one study suggests that people who move frequently tend to develop skills to help with their own adjustment to new situations and to help others with
adjustment problems (McAllister et al., 1973). A number of cases are reported in the literature in which major community changes, including energy development, did not produce apparent increases in social distress (Breese et al., 1965; Carr and Sterner, 1952).

At the community level, it would be useful to distinguish between stress experienced by entrenched power figures, such as some ranchers, merchants, and politicians whose status is threatened by the changes, and stress experienced by others who might have increased opportunities as a result of the changes. Time-series analyses at the national level show that distress is more likely to result from changes involving losses than from changes involving gains (Brenner, 1979: 38). The distributions of losses and gains in communities which change because of energy development are yet to be clarified in research. Findings of recent studies suggest that stress can stimulate mobilization of community resources to attempt to deal with problems. If this were to happen in energy development communities, old problems as well as those related to growth might receive public attention. Thus, the proposition that rapid change causes distress is based on assumptions that are less well supported by evidence than is implied in recent discussions of energy development.

An additional point for evaluation is the assumption that communities are being dominated and exploited by outside interests. The assumption that most benefits leave the community while most costs are left in the community (Massey, 1980; Albrecht, 1978) is the basis of a debate over responsibilities of federal and state agencies to subsidize costs of adjustment of community infrastructures (Brookshire and D'Arge, 1980: 542; Gilmore, 1976). The extent of participation of government agencies or of energy development companies in attempts to mitigate growth-related problems has not been systematically studied. It would seem more appropriate to regard this as a variable than to assume that there is little such participation. It would also seem appropriate to regard community activeness in seeking and securing outside assistance as a variable that could influence both the amount of assistance received and the level of distress associated with change. This variable is ignored, generally, in the energy development literature. Further, more attention in research needs to be given to understanding why energy develop-
ment communities vary in use of strategies which are now available to influence the behavior of energy development companies. Leverage exists in federal and state regulations governing the siting of power plants and other installations, in the granting of mineral development permits, and in community eligibility for various forms of intergovernmental transfers (Murdock and Leistritz, 1979: chap. 11).

It also is not clear that outside domination of community welfare is uniquely associated with energy development or that such domination is new to these communities. Strong extralocal influence in local social and economic life is characteristic of virtually all communities in modern society (Tilly, 1974) and is a persisting historical characteristic of communities in the western energy development area.

The perspective on the community used in much of the western energy development literature is inconsistent with prominent current perspectives in the sociology of community. Warren (1978: chap. 13) discusses the need, in modern society, to view the community, not as a "concrete collectivity," but as an "interaction field." The former view was common in earlier decades. The community was seen as a more or less complete and self-contained local society. Evidence of increased significance of outside forces in influencing local conditions and events stimulated the conclusion that the community was being "eclipsed" by the "mass society." This led, in turn, to the proposition that small communities could be expected to do little, if anything, to influence the outside forces which were controlling their development or decline.

The alternative view is that a dynamic form of community has emerged in modern society to replace the static, self-contained form indicated in the older view (Kaufman, 1959). The modern community, says Warren (1978: chap. 13), is a field of interacting forces with a tendency toward systemic unity. As a field it is dynamic and is composed of the interplay of local and extralocal interests, neither of which can be ignored in understanding the emergent outcomes of the interaction.

This latter view also supports the proposition that concerted action by local residents can influence the outcomes of change, even in the face of massive forces from the outside (Wilkinson,
Retention of the earlier view in discussions of "domestic colonialism" in energy development leads, understandably, to the same conclusions as those reached in the literature on the community a quarter of a century ago. This also leads to neglect of a potentially important variable for explaining differences among communities in response to externally induced change.

Conclusions reached from limited evidence are consistent with theoretical assumptions that have guided much research on energy development. The assumptions are questionable but have not been tested adequately. Thus we do not know, on the basis of scientific evidence, whether energy development leads to increased rates of divorce, crime, mental illness, alcohol abuse, child abuse, and other disruptions in small towns; and we do not know, on the basis of explicitly relevant evidence, whether a rural form of community life is being overrun, as claimed, by the urban society in its quest for energy. The development that is occurring, therefore, presents not only a challenge to the protection and enhancement of human welfare but an opportunity for answering critical issues in theory as well.

**SUGGESTIONS FOR FUTURE RESEARCH**

Science requires an intimate knowledge of subject matter, a systematic way of thinking about the subject, and an objective procedure for examining evidence. Applied social research also depends on a commitment to using science to enhance the social welfare of people. The literature on western energy development provides an intimate view of some aspects of local response to rapid change; and there is little doubt that the authors of this literature are committed to mitigating the problem they describe. The middle parts of the framework for a useful social science are deficient in this literature. Theory and methods must be improved if the social welfare of people is to be served by the research.

The theoretical and methodological resources of social science are much richer than displayed in this applied field. The first prescription for an improved approach is to abandon, or at least to test, the theoretical perspective that implicitly guides much of
the research; the second is to rely on replicable measures rather than speculative assumptions and hearsay as sources of evidence. Beyond these broad prescriptions, an enumeration of needed research on local social effects of western energy development might cover the gamut of emphases in the contemporary study of community structure and change. A useful focus for research, however, would be to delineate general issues in theory and policy which can be clarified through objective study of local changes associated with energy development. Theoretical essays on the significance and problems of the community in modern society provide one view of such issues.

Walton's (1967) hypothesis about the local effects of extralocal linkages (he argues that these might cause an increase or a decrease in equity and/or integration under certain conditions) is an example of a general proposition that could be examined in this empirical context. Tilly (1974) asks, "Do communities act?" and suggests that effective mobilization to solve local problems is rare in modern society but might occur under conditions similar to those described in western energy development communities—conditions of geographic isolation, intense competition for control of the use of territory, and a history of local activeness. Wellman and Leighton (1979: 365) ask what they call the "community question," that of "how large-scale divisions of labor in social systems affect the organization and content of interpersonal ties." Warren (1978) writes of a tendency toward systemic unity in the dynamic social field that now constitutes the local community. Wilkinson (1979) asserts that such a tendency reflects a fundamental human disposition to create community in local social life, despite change and apparent conflict in the local environment.

These theoretical questions and propositions have apparent policy relevance and indicate some of the dimensions of energy development that can be examined profitably through research. The fact is that we have little research-based knowledge of the local social consequences of rapid growth in population and employment in small towns and rural areas, because, prior to the past decade, there was little such growth to study (over the history of rural sociology in America) in these areas. Energy development in the West is providing a distinctive, and in many ways unique,
opportunity for testing theories and clarifying issues which have become central academic and public concerns.

Whatever the theoretical and policy foci, there is an obvious need for methodological improvements in research on energy development, beyond the elementary prescription that reliability and validity of measures should be evaluated. Perhaps the most obvious of these is adoption of a multivariate, probabilistic logic in analytic design. Most of the dependent variables treated in research to date are likely affected by such variables as local population composition, institutional practices in detecting and reporting the incidence of certain behaviors, and secular trends in the larger society. Little use is made of control variables and comparative designs in the studies reviewed above.

A probabilistic approach, rather than a narrow causal logic (Kerlinger, 1973: 393), is needed because of the complexity of empirical relations likely to be found in the process of community change. If the community is viewed as a complex, emergent field, for example, procedures will be needed to take account of covariance among multiple sets of variables, representing, respectively, forces from outside the local area, characteristics and patterns in local social life, the interaction of these, and changes over time. The search for dimensions of the simultaneous associations of a large number of variables is suggested by this open, dynamic concept of community.

Finally, both theory and methods need to be refined to take advantage of opportunities to study (and, it is hoped, to influence) the relationship between the social well-being of individuals and the process of community change. Life in the West, according to some observers (Pang and Hanson, 1968; Kraenzel, 1980), has been a "hard" one for many individuals, but likely has been a "good" one for many as well. The idea of a multidimensional, dynamic community field can be extended to a concept of "multiple possibilities" in the relationship between the individual and the community (Yinger, 1965: 42-5). The interaction of the multiple possibilities in the process of change at both community and individual levels must be conceptualized and measured precisely if useful, responsible conclusions are to be drawn from the study of social consequences of rapid energy development.
CONCLUSION

The conclusion of this review is that for research on the local social effects of energy development to be useful, it must take account of the complexity of forces affecting social life, and it must use rigorous scientific methods to describe and interpret the operation of those forces. The contributions of early literature to “consciousness-raising” about the potentially negative effects of energy development need not be disparaged. But the next step in developing sound research is long overdue.

NOTES

1. Kohrs (1974) presents ten tables on conditions in Campbell County and other counties. He does not indicate sources of the data in the first nine tables, and says that the tenth (on quality of local services) was provided by a research assistant (page 7). One of the tables (Table II) shows the ratio of marriages to divorces in Campbell County and in two other Wyoming counties in 1970. He refers to this in the text as “the ratio of divorces to marriages” (page 5) and argues that differences among the counties in this single year indicate changes which occurred in Campbell County because of energy development. He offers no data on marriages or divorces in previous years. His estimate of the rate of suicide attempts is given in the text (page 3), with no indication of how this estimate was made other than in the previous reference to his own clinical experience (page 2).

2. The only item in the survey that even indirectly relates to any of the claims noted above was one on local perceptions of problems. “Crime and drugs” was indicated as one of the three most pressing problems by 13% of the respondents (Gilmore and Duff, 1975: 106). This was the eighth most frequently listed category of problems.

3. In a work of the scope of that by Murdock and Leistritz, inadequate documentation of one point should not be taken as a major flaw in scholarship. They provide an excellent overview of energy development in western states, and a useful synthesis of what has been found in social impact research, generally with appropriate caution and reservations about the quality of evidence. Their compilation of attitude surveys (1979: 273-281), which are not discussed in the present review of studies of behavioral disruption, is an important contribution to the literature. Lack of documentation of the conclusion about disruption is crucial, however, in that this conclusion is reached on perhaps the most important policy issue in social impact assessment—that of whether rates of crime, drug abuse, mental illness, child abuse, and related problems should be expected to increase as consequences of energy development. The fault must rest largely in the literature they reviewed, where undocumented claims by Kohrs and by Gilmore and Duff are relied on in many discussions. Criticism must also be qualified by the fact that published versions of two of their sources (Gilmore and Duff, 1975; and Thompson et al., 1979) are cited here, rather than the preliminary drafts which they cite. A telephone call to the Denver Research Institute, which issued the earlier draft of Gilmore and Duff’s study in 1974, provided the information that copies were no longer available and that everything in the report was
included in Gilmore and Duff (1975). An earlier draft of Thompson et al. (1979) was available and did not differ from the published version on the points cited.

4. Lantz et al. report an increased incidence of alcohol-related problems over a period of rapid growth in Craig, Colorado, but note that the figures are probably inflated because "a person may have been arrested several times for driving while intoxicated, as well as having been admitted to the hospital for detoxification" (1980: 71).

5. Studies by Murdock and Schriner (1978, 1979), summarized by Murdock and Leisritz (1979: 255-273), compare communities in predevelopment, development, and postdevelopment phases on measures of individual satisfaction and socioeconomic welfare. These comparisons suggest that most local residents have favorable attitudes toward development before it occurs and after it has occurred, that changes in occupational and income distributions during development tend to be of short duration, that newcomers appear to benefit more than oldtimers from opportunities created by growth, but that oldtimers have more opportunities for upward mobility in growing communities than in those which do not grow. Whether these findings would hold in longitudinal comparisons is unknown.

6. Findings in Luloff and Wilkinson (1979: 147) and Humphrey and Kranich (1980: 588) indicate that local mobilization tends to increase with severity of some local problems.

7. The argument can be made that some of the flaws in scholarship noted above are explained by the recency of large-scale energy development in many parts of the West and by pressures on social scientists to complete impact assessments with greater speed than might be required by the use of precise methods and theories. That argument, even if valid in the past, should not hold in the future. Precision in measurement and thoroughness in analysis are elementary requisites for sound contributions to policy and theory. If energy development policy is not informed by careful research and analysis, decisions are likely to be made on speculative grounds, and resources for mitigating real problems are likely to be poorly used. This point is elaborated by Freudenburg (1981: 240).

8. This is not to argue against the usefulness of case studies or of participant observation methods of research. Rapid energy development of "boom" proportions is occurring in a relatively small number of the counties of the western energy development area, and perhaps can best be studied through comparative case studies. Participant observation is perhaps the only method (certainly one of the best methods) of acquiring certain types of data. What is meant by a "multivariate probabilistic logic in analytical design" is a commitment to examining competing explanations of observed reality. This commitment can be expressed in qualitative analysis as well as in quantitative analysis.

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RESPONSE

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A recent essay by Craig Vetter has this to say about sociological research in Gillette:

Mark Twain could have told them what they were going to find, and they found it: murder, robbery, assault, child abuse, wife beating, divorce, alcoholism, depression, madness and suicide all out of proportion to the number of people in town. They began calling it Gillette syndrome, and then, in the best traditions of sociology, they began to argue whether it really existed or was just a statistical aberration built of shabby data [1982: 118].

A sociology of sociology, if not a sociology of knowledge, might be constructed in this exchange, but at the cost of energy that should be spent in concocting more precise tests of more soundly based propositions than have dominated the literature to date.

After responding to some of the points raised in comments on our article, we propose to end our part in this dialogue so as to get on with the task of figuring out likely social consequences of rapid growth in small communities. We will respond only to comments about matters relevant to the topic, not to remarks about studies of attitudes and perceptions, about studies outside the region covered by the review, or about ourselves, our sponsors, and our motives. We hope that Finsterbusch is correct in his prediction that future research on social disruption in energy development
communities will be better than past research. Progress would be retarded, however, if some of the judgments about the quality of evidence made by him and others were accepted.

The article does not discount data presented by Lantz and McKeown, though we have reservations about it. It criticizes use of the enormous percentages which are used shown in their report to indicate the magnitude of changes. The problem with these, as Freudenburg now says, is that they are based on small initial numbers—he and Finsterbusch say the numbers we cite should be 4 and 6; we and Lantz and McKeown say that they are 2 and 3 (as monthly averages for a two-month period). The numbers are too small for percentages to be reliable, regardless of the amount of time covered—which Finsterbusch says would make a difference—or the number of them cited—which Freudenburg says would make a difference.

Finsterbusch also says that double counting of individuals by agencies would not affect comparisons through time. This underestimates the tendency of agencies to expand in growing communities. The potential for multiple (not merely double) counting increases with the number of counters, and, as Gold observes, expanding agencies need and tend to acquire new cases. Multiple counting is less of a concern to the purpose of the caseload review conducted by Lantz and McKeown (which was to give agencies information on service demands) than to the purpose of estimating the number of persons who might need the services of one or more agencies.

Finsterbusch also notes that Lantz and McKeown found no evidence of significant changes in record-keeping procedures in Craig. Freudenburg (in the first of his two 1980 papers cited in the comment by Murdock and Leistritz) reports that the recordkeeping system in the sheriff's office was changed in 1976, "destroying comparability." (He makes comparisons from 1975 to 1976, and from 1975 to 1977, anyway, on the dubious assumption that the ratio of city police reports to sheriff's department reports would have been the same before the change as after the change, had there been a city police department before the change.) Changes in Craig in 1976 which could affect the
results of the caseload review (as learned by us through interviews in Craig) include change in the record-keeping system in the mental health center (described below), initiation of an outreach program and hiring of a new director of that center, hiring of a new director of the county social services department, and creation of the city police department referred to above.

The most important of the Freudenburg reports on Craig for indicating disruption, says Finsterbusch, is the analysis of changes in the caseload of the mental health center. In that report, Freudenburg et al. say that the outreach program had little apparent effect on the overall caseload of the center because a detoxification center was also established to handle alcohol abuse problems, which previously had been treated by the center. They do not mention that the detoxification center was opened in 1977 (the outreach program began in 1976) or that alcohol-related problems continue to be among the most numerous of those treated by the mental health center. They also do not mention that the practice was started in 1976 of creating case files on all family members of clients whose treatment required the participation of other family members or that many contacts of center staff with community residents were not recorded prior to 1976 as they were beginning that year. Nor do they mention that the center has a three-county catchment area, including two energy development counties (Routt and Rio Blanco) in addition to Moffatt County, in which Craig is located. Finsterbusch could not have known of these qualifications from reading the article. An increased caseload would be expected, at least from 1975 to 1976, to result from these changes. The authors report that the caseload increased from 133 in 1975 to 208 in 1976; and they interpret this, along with data for previous and subsequent years, as evidence of a disproportionate increase in the boom-era caseload due to an increased rate of mental health problems, an interpretation that ignores the qualifications mentioned above.

For the three years for which their caseload totals appear to be reliable (1976-1978), the increase was from 208 in 1976 to 259 in 1977 and 300 in 1978. With their population estimates for what they call “the functional community” (i.e., Craig and its suburbs),
these totals would yield admission rates per 1000 estimated residents of 25.8, 26.1, and 29.8, respectively, during the three years of the growth period, indicating a small increase. With the Moffatt County population (from census estimates) as base, the rates for these years would be 21.0, 24.9, and 27.3; and with the entire catchment area as base, the rate would be 8.3, 10.0, and 11.0. Thus, there was a small increase in this period but no comparable data are available for the pregrowth period.

The authors use the comparative percentages of oldtimers and newcomers in the caseload to reject the hypothesis that newcomers accounted for the increase; then they use change in the number of oldtimers (not their percentage of the total) to accept the competing hypothesis, which becomes the focus of their conclusions. Such a switch in logic is an unusual analytic step, to say the least. The evidence they present for 1973-1978 indicates that the number of long-time residents in the caseload doubled, but that the proportion of these in the caseload declined, while the total caseload and the community population grew. Their conclusion that this was caused by increased stress among oldtimers ignores the qualifications mentioned above and the fact that population growth can be affected both by increased inmigration of newcomers and by reduced outmigration of long-time residents. There might have been increased stress in Craig, as the authors maintain; but that is not shown in their data. An untested competing hypothesis is that the composition of the expanded caseload was representative of the population served by the center.

In another reference, Finsterbusch ignores the mental health admissions rates we calculated from Weisz's data on Campbell County, Wyoming, citing instead percentage increases, which give a distorted indication of the amount of change in rates. Freudenburg mentions the rates in his comment and says that our figures are wrong. He also says we ignore Weisz's data on admissions to the state hospital for mental health treatment. He is in error on both counts. The rates, as we say, are for admissions to mental health facilities serving Campbell County, and we cite the two pages in Weisz's paper where admissions are recorded, respectively, to the mental health center and the state hospital.
Finsterbusch also ignores cautions in the critical literature we cite on the relationship between life changes and health problems. He says that Weisz’s report of a high sample mean on a life changes scale indicates disruption. The prediction by Weisz that health problems will likely result from such a high mean is made on shaky grounds, given the state of evidence on this subject and the obvious problems of generalizing from individual-level postulates about a causal relationship to a community situation in which many people have experienced such major changes as moving, taking new higher-paying jobs, buying homes, joining churches, and the like. In any case, it is a prediction, not a measure, of disruption. The data Finsterbusch cites on the mental health center in Platte County, Wyoming, are presented by the researchers as a crude indicator of service costs; thus no information is given on programs of the center, changes in record-keeping, or distribution of first admissions and readmissions in the totals. Rates calculated from the data (with admissions aggregated over the 12 diagnostic categories used in the report and population estimates from elsewhere in the report) would be 30.7 (per 1000) in 1975, 31.7 in 1976, and 36.0 in 1977—a modest increase. Finsterbusch notes, as we do, the offsetting changes in rates of property crimes and crimes of violence reported for Gillette. This means that change in the overall crime index, combining the opposing trends, cannot be used, as he uses them to indicate disruption. We have not seen the 1970 paper on Sweet Home, Oregon, which he cites. The 1971 paper which he says also shows a crime increase in that community contains only one relevant measure. Fines and forfeitures increased with construction of a large dam. The authors say that this shows a disruption effect of dam construction.

Murdock and Leistritz say we quote them and others out of context, leave the wrong impression of their focus and conclusions, ignore their qualifying statements, and contribute generally to malaise in the literature. The qualifications they give in the vicinity of the statements we quote from pages 251 and 252 of their book are irrelevant to the point that these unqualified statements are incorrect. After showing that the statements are incorrect, we say that their general conclusion about social
impacts does not hold up in the particular case of the evidence we examined. Our scholarship is especially careless, they say, for not reporting that one of the authors they cite concluded that crime increased, although his data were unreliable. This is incorrect and irrelevant. We say that the author used the data to reach this conclusion. It is irrelevant, because we were looking for evidence, not for claims, in their source. The reader can decide whose scholarship was careless in this.

We have the elephant by the tail they say, the weakest link in a broad field which they reviewed. Clearly it is a minor part of their work, but we had no intention of writing a book review. The tail indeed! We are talking about whether or not there is evidence that energy development contributes to increased rates of divorce, crime, drug abuse, mental illness, child abuse, and related problems. Others would add suicide, suicide attempts, teenage rebellion, street prostitution, juvenile delinquency, and "down-and-outs" to what Freudenburg calls "the dismal list." Where, we ask, is the heart of this elephant? Surely Murdock and Leistritz would not agree with Gold that we are asking trivial questions. As we say, we think their book is fine, though slightly flawed. The state of the art revealed in the literature we review does not indicate that the previous admonitions and prescriptions cited by Murdock and Leistritz have had much effect. New insights, which they say we lack, are needed here less than is adherence to some old rules of evidence and logic. We hope those rules are reaffirmed in the recent papers cited by Murdock and Leistritz (those published in 1981 and 1982) that we have not seen.

Our statement that there are no data on these problems in the report by Thompson et al. should be that there is no reliable evidence from their study that documents an increase in rates. Available information on arrests was collected in the study and is recorded, as Freudenburg notes, in background volumes which supplement the comparative analysis we cite. Changes in police record-keeping in the two energy development counties and passage of several new ordinances in the one municipality studied made change analysis inappropriate. The dramatic increase in arrests in Wheatland which Freudenburg mentions resulted largely from records kept in 1977 in categories where none had
been maintained previously and from enforcement of dog laws, a curfew ordinance, and other such regulations. There is no evidence of increased serious crime (or, as noted above, of increased mental illness) in these data.

The evidence of crime in Rock Springs cited by Freudenburg in Lovejoy's report of an attitude survey in southern Utah is from Lovejoy's notes on a talk given by a policeman to a group of students who took a two-day field trip to Rock Springs in 1976. The information given by the officer has not been appraised for use in research.

Freudenburg also says we should accept Gilmore's word about crime increases in Rock Springs—a curious suggestion for evaluation of scientific evidence. He, along with Gale, says we ignore Little's statement that Page is atypical. That is correct. We are more interested in Little's explicit statement, which we quote that Page "is not atypical," which we take to mean "is typical," of energy-development communities in the response of its crime rate to population growth. Freudenburg says that divorce is mentioned casually in this literature. We find nothing casual about frequent, uncritical citations of Kohrs's claims on this topic. Freudenburg also says, again in agreement with Gale, that we incorrectly cite Cortese and Jones as arguing that Gemeinschaft is disrupted in energy development communities. The Cortese and Jones thesis is one of the clearest applications of Louis Wirth's unfounded propositions about urbanization to be found in recent sociological literature. They argue that change in social structure in these communities is a more important problem for the long run than even the short-term strains (such as increased mental illness) that are often reported; and they say that attempts to reduce strains by improving social services contribute to this problem. Freudenburg implies that we ignore useful theoretical perspectives in the literature and focus instead on use of such simplistic, discredited concepts as "riff-raff" by some researchers. The reader will note that our reference is to his hypothesis that projects requiring skilled workers will be less disruptive than others because less "riff-raff" will be attracted to the community by these.
Gale's comments overstate the role of "project-oriented case studies" in the literature we review. Impact reports to agencies by Lantz and McKeown, Foster, McDonald, Thompson et al., and Gilmore and Duff are examined for the explicit purpose of checking the references of works directed at academic audiences. Also contrary to his impression, most of the studies we review are published and most of the others are reported in papers that can be acquired from authors. We obviously disagree with his point that dual standards should be used in judging whether or not research results can be trusted. He and Albrecht express the opinion that companies have been encouraged to take mitigating actions by results of the research we critique. If so, what is likely to happen when the flaws in that research are uncovered? We agree with Albrecht that good research will be the only acceptable solution. Gale also misinterprets the intended meaning of several of our points. One is the point that theoretical assumptions have forced conclusions that are not supported by evidence. Another is that little previous research in sociology has addressed issues that could be addressed profitably in this applied field. Another is that the balance of evidence summarized in the comprehensive NIMH review of literature (which Gale refers to as "only one study") does not support the common assumption that stress increases with urbanism. Finally, his opinion that the research we review gives adequate attention to controls and competing hypotheses is contrary not only to our conclusions but to those of others who have commented on this literature.

With the opening restrictions we placed on our response, we must ignore many of Gold's remarks. Our view of the value of participant-observation is given in the article. We sense in his comments a spirit of skepticism about so-called evidence we endorse. In that spirit, we wonder not only about how things were in the good old days of Gemeinschafi in this region but also about when those days were. Were they before the first white boomers arrived to take the land from its natives? Were they after the shooting died down between various groupings who wanted the land for their own uses? Were they after the first company town at Colstrip was established, or after the mine closed and only a few
people were left behind? Maybe they were in the early 1970s before all the construction workers arrived and the new town was built with its modern schools, churches, arcade, clubs, recreation center, and what Gold calls “people pollution.” There has been much trespassing in this territory. We hope Gold agrees that there is no proscription against trespass on one’s scene in the search for answers about the social effects of energy development.

Albrecht’s observations on the process of social change point, we believe, to an area in which fruitful work—with sound theory and methods—can be done in this field. It is likely, as he suggests, that a number of variables influence community responses to growth and that combinations of these can have varying effects at different stages of the process. These variables, we suggest, might include previous community characteristics, mitigation policies of companies, and, most crucially, the interactions that occur in planning for growth and coping with change. Albrecht thus suggests what we call a multivariate, probablistic design. Unfortunately, in the paper in which this suggestion is elaborated, he says—without presenting any relevant evidence—that the consequence of a lack of “community imbeddedness” of newcomers in energy development communities, “in many instances, is likely to be increased drinking, more serious marital discord, depression and suicide” (on page 25 of the 1982 paper he cites) and that communities he studied “have experienced increasing occurrences of social problems and personal pathologies reflected in higher crime and delinquency rates, increased frequency of drug and alcohol abuse, the increased evidence of suicide and family conflict, during the early stages of industrialization” (on page 20 of the same paper). He says that he knows these increases occurred.

Despite the lack of clear evidence in the literature, the possibility remains that some of these problems increase with energy development in some communities. Our paper does not dispute the disruption hypothesis, only the claim that this hypothesis has been formulated reasonably and tested in previous research. Hints of a relationship based on intuitive evidence and personal awareness cannot be ignored; but enough hints have
been recorded to move the field beyond what Murdock and Leistritz properly call suppositions and into serious research with the scientific method. There is no issue here of what some might call a "hard science" versus a "soft science" approach, and our critique should not be dismissed as simply an exercise in the use of stringent scientific criteria to suppress what is known by those on the scene. Science is the search for truth, no holds barred. The normative issues in the field deserve our best efforts in this search and in disputation of what is claimed.

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COMMENTARY

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The critical review of the literature on social disruptions associated with large-scale western energy development by Wilkinson, Thompson, Reynolds, and Ostresh is an important contribution. The authors correctly identify some of the problems evident in the early stages of this literature, and one cannot deny the tendency on the part of later authors to continue to cite some of the earliest descriptions of energy-impacted communities, even though the empirical bases for these early descriptions were often questionable. While I agree with many of the criticisms expressed by these authors and while applauding their effort to promote a literature on energy-impacted communities that is more soundly based—both empirically and theoretically—I believe their analysis raises some points that require a response.

EMPIRICAL EVIDENCE FOR COMMUNITY DISRUPTIONS

Wilkinson et al. are particularly critical of a series of papers that tend to conclude that there are major disruptions associated with rapid energy-related growth. Their basic concern is that these papers tend to quote from one another and that few of them actually present empirical support for their conclusions. There is a clear tendency in this literature to identify the same problems again and again without a presentation of actual data about their occurrence. Equally important is the tendency to present descriptive data on a community without any clear demonstration that the problems that are described are in any way causally related to rapid growth.

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However, the literature review provided by the authors is somewhat selective and tends to ignore several studies that are important in understanding what has been occurring in energy-impacted communities. Particularly useful in this regard is the large literature that has developed on Canadian resource communities, especially those located in Northern Alberta (e.g., Jackson and Poushinsky, 1971; Graham Brawn and Associates, 1975; Riffel, 1975; Van Dyke, 1975; Nickels and Ledger, 1976; Hobart et al., 1979; Larson, 1979; Gartrell, 1981). In this literature, some documentation is provided for problems of high job turnover, increased rates of crime, mental illness, illegitimacy, alcohol and drug abuse, marital conflict, and divorce.

One of the important points that emerges from this literature is that it is particularly important when and how one conducts research in a community experiencing rapid growth from energy development. Self-report data on the incidence of criminal victimization, marital discord, family disturbances, and related problems may or may not be reflected in official statistics or in caseload reports kept by service delivery agencies. Much of the Canadian literature is based on self-reports, but it does provide some support for the relatively undocumented assertions that were made in the earlier social impact literature developed in the western part of the United States. In other words, while much of the material Wilkinson et al. cite may not provide sufficient evidence that these problems occur, there are other sources that provide additional documentation.

Also important is the fact that there is a virtual absence of any firm empirical support for the conclusion that these impacts do not occur. Some caution must be exercised when considering the findings of the few studies that report no social impacts on the community associated with energy development. For example, the Brookshire and D’Arge (1980) study cited by Wilkinson et al. not only covers a period that predates much of the energy-related impact in western communities (as noted by the authors) but also fails explicitly to deal with population change within communities. That is, comparing crime statistics for Rock Springs with those for several other communities in 1972 doesn’t mean much. What
one needs is an analysis of change in crime (if any) that occurs within communities during the impact period.

The conclusion by Brookshire and D’Arge (1980: 524) that the secondary effects on wildlife habitat reduction from large population increases are substantially greater than the direct effects on community social characteristics exhibits virtually all of the faults Wilkinson et al. decry: inadequate operational indicators of variables, questionable interpretations of evidence, and superficial analyses. In fact, one almost gets the feeling that sponsorship for one’s research might be the single variable that is most highly correlated with the conclusions reached in a given report. The most blatant evidence of this comes from the U.S. Air Force Draft Environmental Impact Statement on Deployment Area Selection and Land Withdrawal/Acquisition for the MX Missile System, which devoted from two to five times the amount of attention to such topics as impacts on the pronghorn antelope, rare plants, sagegrouse, bighorn sheep and the Utah prairie dog as it did to impacts on human quality of life (Utah Consortium for Energy Research and Education, 1981).

In sum, while some of the early literature on western energy-impacted communities is almost journalistic in its effort to paint a picture of small, rural communities being raped and disrupted by the energy conglomerates, balance will not be achieved by becoming apologists for these energy conglomerates or for other entities like the military that propose programs that would have important consequences for these communities. As Wilkinson et al. suggest, research is needed that is consistent with the usual standards of scholarship, whatever the specific topic of that research.

THEORETICAL ASSUMPTIONS OF THE IMPACT LITERATURE

In large part, the social impact literature is atheoretical. In fact, one is hard-pressed to find any studies that employ theory or that attempt to test systematically any theoretical tradition that might
be applicable (Larson, 1977). However, Wilkinson et al. argue that many of the postulates suggested by this literature relate, at least implicitly, to community typologies that have their origin in the early work of Toennies, Durkheim, Maine, Redfield, and Becker. As the authors note, in this work a conceptual distinction is drawn between rural and urban forms of social organization that is perhaps best captured in Toennies’s concepts of gemeinschaft and gesellschaft. This theoretical tradition assumes that the forces of urbanization and industrialization act essentially to alter the character of society from that which is based on communal attachments to an associational basis (Kasarda and Janowitz, 1974: 328). The forces of urbanization and industrialization are thus assumed to lead to a breakdown in primary group ties, decreasing attachment to community, greater reliance on secondary institutional supports, and so on (Wirth, 1938).

First of all, one should note the large literature that is critical of the assumption of a movement from gemeinschaft-type social organization to gesellschaft-type social organization during industrialization and urbanization. Research on industrialized communities has found extensive informal networks and primary group ties (e.g., Axelrod, 1956; Bell and Boat, 1957; Adams, 1968; Kasarda and Janowitz, 1974). However, when one extends this literature to what is happening in energy-impacted communities, one may be begging a very important question. Research that documents extensive primary and informal ties in industrialized communities has typically been conducted some time after the period of rapid industrialization. On the other hand, research that examines the process of this industrialization often reaches very different conclusions. This is illustrated in Shaw and McKay’s (1942) classic study of juvenile delinquency in Chicago. In discussing the rapid growth that occurred in Chicago around the turn of the century, they note:

This . . . rapid territorial expansion and geometric increase in the population of Chicago implies marked changes in the areas within the city and a rate of mobility quite unknown in stable communities. Likewise, the influx of great numbers of people of such widely different social and cultural backgrounds implies not only lack of
homogeneity but also disorganization and reorganization affecting a large proportion of the population [1942: 25].

In describing the social characteristics of those areas of Chicago that exhibited the highest rates of juvenile delinquency, Shaw and McKay note:

Certain areas . . . lack the homogeneity and continuity of cultural traditions and institutions which are essential to social solidarity, neighborhood organization and an effective public opinion. . . . The economic insecurity of the families, the tendency for the family to escape from the area as soon as they prosper sufficiently to do so, all combine to render difficult, if not impossible the development of a stable and effective form of neighborhood organization in these sections of the city [p. 110].

As I have noted elsewhere (Albrecht, 1982a), perhaps the problem is that we are dealing with two different issues. In terms of the classic characteristics of gesellschaft, urbanized and industrialized areas may not be particularly different from more rural communities once they have become stabilized. That is, individual residents feel a certain degree of attachment to the community; they maintain significant informal and primary ties with kin and friends, and so on. At the same time, communities or areas of communities characterized by the rapid social change that accompanies urbanization and industrialization experience at least a temporary breakdown in many of the traditional social support mechanisms that contribute to such things as community stability, individual identification with community, and quality of life. Thus, areas of cities like Chicago that were experiencing rapid social change, including growth, changing ethnic character resulting from the influx of European immigrants, southern blacks, and so on, were also characterized by high rates of juvenile delinquency, crime, and other important social pathologies. On the other hand, areas characterized by less social change had the time to stabilize; individuals reestablished important primary ties with kin and neighbors, came to identify with community, and developed an overall more stable character that, in turn, was
reflected in much lower rates of crime and related social problems.

What this suggests is that the implicit theoretical orientations that Wilkinson et al. believe guides much of the research on energy-impacted communities does provide some interesting research hypotheses that can be tested. This tradition, therefore, should not be rejected outright.

Two other issues require brief mention here. First, Wilkinson et al. argue that rural communities that are being impacted have not had the type of social organization described as rural in the classical typologies. I would suggest that there are some important exceptions to this argument. One should remember that many of the impacted communities are located in Utah and in the Southwest. This means that many of them have large concentrations of Mormons, Native Americans, and Mexican Americans. One can argue that these communities do exhibit many of the classic characteristics of gemeinschaft and that, to the extent that they do, they stand to be even more severely disrupted by the changes associated with rapid energy-related growth. The histories of these communities have not been so short, as suggested, that they have been unable to develop the kinds of relationships indicated in the classic gemeinschaft typology.

Second, many of these impacted communities exhibit characteristics that make the application of certain other theoretical traditions somewhat questionable, at least without some adjustment. For example, more attention could be paid to the large literature on rural industrialization that has developed in other areas of the country. However, one cannot lose sight of the fact that there are few real parallels to what has happened in energy-impacted communities in the rural West. These experiences often differ from those that have occurred elsewhere in at least the following ways (Albrecht, 1978, 1982b; Cortese and Jones, 1977): (1) The rate of growth has been much more rapid. For example, Valdez, Alaska, had 1100 people in town, and two years later there were 1100 kids in local elementary schools. This occurred despite the fact that most of the boomers came to the area without their families (Bayliss, 1980). (2) The geographic isolation of the
communities means that workers will not be able to live in a nearby urban center and commute to the construction site, as is often the case in other areas of the country. (3) The demographic history of the impacted communities is often quite unique. For example, many of them had larger populations in 1910 and 1920 than they did in 1970. Proposed developments will reverse historical patterns of decline quickly and dramatically. (4) The social and cultural homogeneity of many of the communities does not prepare them to deal with the rapid flux of a different population. This homogeneity is often most noticeable in states like Utah, where in several counties over half of their populations have lived in the same community over 40 years and where over 90% of the residents are members of the Mormon Church. (5) Finally, the virtual absence of a well-developed local infrastructure in the form of adequate schools, medical facilities and personnel, law enforcement, recreation facilities, and so on increases the potential of severe impacts, at least during the early stages of growth.

More attention should be paid to the rural industrialization literature. However, researchers should not lose sight of the characteristics of the impact communities that might be quite unique and critical in understanding the impacts that occur.

Finally, let me conclude with some brief observations on some of the possible positive impacts that have been associated with the development of the literature Wilkinson et al. criticize. The authors note that the extent of participation of government agencies or of energy development companies in attempts to mitigate growth-related problems has not been systematically studied. This is only partly true. A recent series of papers by Metz (1977, 1979, 1980) documents the dramatically increasing role of energy companies in working with communities to help mitigate the impacts their project will have on that community. I argue that this increased role of industry is not an altruistic response on their part but may, in large part, be a function of the potential problems they will create for impacted communities that have been described (granted, often without strong empirical basis) in the social impact literature. While much mitigation on the part of
industry has been designed to help decrease worker turnover problems (which, by the way, are extremely costly to industry), other efforts have been generated either to relieve community anxieties (because communities do not want to experience the horror stories that have been written about other communities) or to meet community and state laws that have been written largely in response to concerns that were identified in the early literature. In other words, if future research projects find that disruptions are less severe than described in the earlier literature, some credit should probably be granted to that earlier literature.

This does not mean that the early research on social impacts has affected energy development policy in an irresponsible way. Rather, it has helped convince industry and state and local decisionmakers that truly social impacts must be fed into the decision-making equation. One can say this and still readily agree with the final conclusion of Wilkinson et al. that future research on social impacts of energy-related growth can and should be more soundly based—both theoretically and empirically.

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COMMENTARY

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BOOMTOWN DISRUPTION THESIS:
ASSESSMENT OF CURRENT STATUS

Science entails the systematic accumulation of knowledge by disproving error and accumulating empirical support for theories and generalizations. In a new area of research generalizations are often fabricated on the basis of rather skimpy evidence and quickly accepted as conventional wisdom. Then come the debunkers who spotlight the flaws in the conventional wisdom and challenge the accepted verities. Thereafter, researchers proceed more cautiously and test more thoroughly their generalizations.

In the new research area of boomtown studies the hastily constructed social disruption thesis has become the conventional wisdom, and Wilkinson and his associates in this journal and elsewhere (1980; Thompson, 1979; Reynolds et al., forthcoming) are its debunkers. The aftermath of the debunking will be a more mature science of boomtown phenomena and, it is hoped, more maturity in social impact assessments. Boomtown studies are a subfield within social impact assessments (SIA), which are a subfield of applied sociology. The story of the debunking of the boomtown disruption thesis, therefore, is a lesson for social impact assessors and applied sociologists.

Social impact assessment (SIA) is a challenging area of applied social science because it requires that social scientists predict how a potential action, usually by government, will affect individuals, groups, organizations, communities, and other social units in the future. The social sciences have a reasonably good record in explaining what has or is happening but a very poor record in
predicting what will happen. For example, Lipset (1979) describes how the social sciences failed to predict most of the important developments in the United States since World War II. Nevertheless, SIA must predict the future state of affairs, assuming that a proposed action will be implemented, and compare it to the future state of affairs if no action or an alternative action will be taken.

In predicting the future, SIA must use knowledge of the past. The consequences of similar actions in the past are projected as the predicted consequences of the action being considered. SIA requires, therefore, the systematic condensation of past case studies into standard information modules (SIM) for specific types of actions. (See Finsterbusch and Hamilton, 1978, for a discussion of SIMs in applied social sciences.) These SIMs would identify the general patterns and the major types of exceptions. Wherever possible, they would specify the factors which determine when the general pattern applies and when one of the alternative patterns apply. The social impact assessor would then identify the unique aspects of the study situation and evaluate whether they would cause a deviation from the general pattern or not. I have tried to summarize the knowledge on prevalent social impact areas in order to facilitate this process (Finsterbusch, 1980).

Unfortunately, the number of carefully documented studies of past cases is very small for many types of actions. As a result, a few well-publicized case studies, often based more on impressions than on systematic evidence, can create a fraudulent stereotype of impacts of such actions. Wilkinson et al., in a paper in 1980 and again in the article in this journal, attempt to show that a fraudulent stereotype has become the conventional wisdom for understanding boomtown social and psychological impacts (see also Thompson, 1979; Reynolds et al., forthcoming). They are successful in demonstrating how the stereotype of boomtown stress and social problems diffused quickly and became conventional wisdom without proper testing. They also fashion a plausible explanation for the popularity of the boomtown disruption thesis, which is that widely held social disorganization, modernization, and urbanization theories are congruent with the
boomtown disruption thesis and predispose social scientists to accept this thesis on very little evidence. Their work has provided a valuable corrective to the stereotype and rightfully demands that the boomtown SIM be based on evidence, not prejudice.

Here I attempt to initiate the task of reconstructing the boomtown SIM out of the shattered pieces of the boomtown disruption stereotype. I begin by critiquing the critique of the boomtown disruption thesis and then identify the parts of the boomtown disruption thesis which have been empirically discredited and the parts which retain some credibility. (For reviews of the boomtown literature see Cortese, 1980; Cortese and Jones, 1979; Finsterbusch, 1980; Freudenburg, 1980, Murdock and Leistritz, 1979).

THE CRITIQUE OF THE BOOMTOWN DISRUPTION THESIS

The Wilkinson critique of the boomtown disruption thesis demonstrates how three works (Kohrs, 1974; Gilmore and Duff, 1975; Gilmore, 1976) provide the basis for the boomtown disruption thesis for many later commentators. On close examination, however, these three works fail to document empirically their assertions about severe community disruption, crime and social problems, and psychological stress in the boomtowns under study (Gillette and Rock Springs, Wyoming). The booms in these towns may have been highly disruptive, but we only have the impressions and undocumented statements of these witnesses on which to rely.

In addition, Wilkinson and his associates review a number of other studies of boomtowns but find in them no strong evidence of the social disruption thesis. For most of the studies they reviewed, their case seems to be well made, but some of the studies do present evidence of increasing rates of social and psychological problems in boomtowns. They deny, however, that this evidence constitutes verification of the boomtown disruption thesis for two reasons. They notice that some rates do not increase at the same
time that some rates increase (Thompson, 1979; Brookshire and D'Arge, 1980), and they judge some data as “not strong” (McDonald, 1976; Lantz and McKeown, 1977; Weisz, 1979). I take issue with both of these arguments. I would judge the data which they criticize to be much stronger than they allow, and I do not think the disruption thesis requires increases in all types of social and psychological problems. In contrast, I think there is fairly strong evidence that boomtowns do cause social and psychological problems, though not in all cases and not necessarily for all types of problems in any single case. Nevertheless, we agree with Wilkinson and his associates that the boomtown stereotype is a gross exaggeration and an impediment to SIAs and policy recommendations for potential boom communities. The boomtown SIM needs to be reconstructed.

SUPPORT FOR THE BOOMTOWN DISRUPTION THESIS

Three major boomtown studies provide reasonably solid documentation for the social disruption thesis. One was reviewed and inappropriately discounted by Wilkinson and his associates, and the other two were not covered in their review. They cite the study by Lantz and McKeown (1977) of the disruptive effects of the boom in Craig, Colorado, but select the two problems with the lowest rates of occurrence and dismiss them as being “based on small initial numbers—two crimes against persons, three child behavior complaints.” Their readers are given the false impression that there is nothing noteworthy in the Lantz/ McKeown study, but this study provides just the kind of data Wilkinson and his associates are asking for. Lantz and McKeown obtained statistics on cases from four sources: the sheriff and police departments, the mental health center, the hospital, and social services. They obtained statistics for only November and December in both 1973 and 1976, but they obtained the total incidents for the entire year from each source and verified that November and December were average months.
The two crimes against persons and three child behavior problem cases dismissed by Wilkinson and his associates actually represent four and six cases for November/December 1973 compared to 40 and 66 cases for 1976. The 900% and 1000% increases indicated in the authors' table should not be taken as accurate measures of the rates of increase for these problems, because larger time periods should be used to compute such rates. Nevertheless, the increases were large and exceeded the 47% increase in population between 1973 and 1976. (Freudenberg, 1978, argues that the real population increase is about 100% if we consider the natural community rather than the community defined by city limits.)

The other five problem areas studied by Lantz and McKeown involved more cases. The number of cases for November/December were as follows:

- substance abuse: 13 in 1973 and 94 in 1976, a 623% increase;
- family disturbance: 27 in 1973 and 122 in 1976, a 352% increase;
- emotional disturbance: 11 in 1972 and 16 in 1976, a 45% increase;
- child abuse/neglect: 23 in 1973 and 52 in 1976, a 130% increase; and
- crimes against property: 58 in 1973 and 187 in 1976, a 222% increase.

Furthermore, the number of cases of the above problems recorded in the four agencies for the two months in the two years were as follows:

- sheriff and police departments: 77 in 1973 and 380 in 1976, a 394% increase;
- mental health center: 11 in 1973 and 15 in 1976, a 36% increase;
- hospital (inpatient): 7 in 1973 and 17 in 1976, a 143% increase; and
- social services: 18 in 1973 and 75 in 1976, a 317% increase.

Statistics for the mental health center were for cases opened. The active caseload or contacts in November 1973 were 80 and 178 in 1976, an increase of 123%.
According to the authors, the November and December figures for the four agencies were in line with the annual totals. The authors also checked for changes in record-keeping procedures and found no significant changes. They think, therefore, that their statistics reflect reality in their general outline, though some of the specific details may not be truly representative. They mention two further caveats. They conclude from their data that citizens may have reported problems to these agencies more readily in 1976 than in 1973, and they note that some individuals may be double counted as repeat offenders or by more than one agency. The problem of double counting, however, would apply to both years and not affect the comparisons. In sum, Lantz and McKeown provide some noteworthy data supporting the social disruption thesis, though some of their specific rates of increase may exaggerate the extent of disruption.

The second major boomtown study to provide empirical support for the social disruption thesis is the work of Freudenburg and his associates in the boomtown of Craig, Colorado (Freudenburg et al., 1977, 1982; Freudenburg, 1978, 1979, 1981). Freudenburg et al. (1977) provide survey evidence that oldtimers expressed much more fear of crime and much higher victimization rates in Craig than in three control communities. They also report a substantial increase in the crime rate on the basis of data from the sheriff and police departments of complaints and “offenses known.” Freudenburg (1978) provides a qualitative description based on 16 months of ethnographic observation of the social effects of the boom in Craig. He concludes that the boom disrupts social patterns in ways that are largely viewed negatively but also notes that most individuals seem to adapt to these disruptions successfully with no significant loss in well-being. Freudenburg (1979) surveys students in Craig and the three control communities and finds the Craig students much more negative about their community, government, school, peers, life in general, and themselves than their peers in the control communities, even when controlling for age, sex, class, and length of residency. They were also more anti-growth. He argues that booms are negatively disruptive for
teenagers and more so than for adults. Freudenburg (1981) produces mainly a negative finding. No significant differences between Craig and the control communities are found in a survey of satisfaction and happiness. Nor are significant differences found between men and women on satisfaction and happiness questions. He does find significantly lower satisfaction/happiness among newcomers to Craig than among newcomers to the control communities.

The most important analysis by Freudenburg for measuring boomtown disruptions is based on the clinical records of the Mental Health Center of Craig (Freudenburg et al., 1982). The center's caseload increased 62% in the early growth years (1972-1973 to 1974-1975) and increased 189% in the boom growth years (1974-1975 to 1976-1978). The authors estimate that the functional community population increased 90% from 1973 to 1979, so the center's caseload increased much faster than the population (the 1980 caseload was 245% larger than the 1973 caseload). The authors also demonstrate that the caseload increases were not simply due to the influx of disturbed people. The number of oldtimers seeking professional mental health counseling doubled between 1975 and 1977 (the boom period). In sum, the work of Freudenburg and his associates documents social and psychological boomtown disruptions in Craig, Colorado, but also demonstrates that some expected negative effects did not occur.

The third major boomtown study which documents social disruptions is Dixon's study of the boom in Fairbanks, Alaska, due to pipeline construction (1978). Wilkinson and his associates ignored this work because they studied western energy boomtowns exclusively. Nevertheless, Dixon provides the most complete study of a modern boomtown in print. She provides statistics on practically every imaginable indicator of impacts, including wages, prices, employment, unemployment, turnover rates, welfare recipients, housing, telephones, traffic, accidents, births, deaths, violent deaths, marriages, divorces, reported runaways, hospital admissions, ambulance and rescue calls, social services, mental health services, alcohol consumption, and
crime. Some indices increase in proportion to population growth, some increase more, and some increase less.

According to police activity records, total complaints increased 93% (9788/5072) from 1973 to 1975, while the population is estimated to have increased 39% (see Dixon, 1978, for a discussion of the difficulties of estimating population). Homicide, rape, and kidnapping did not increase significantly from 1973 to 1975 (14 and 16 respectively), but most other categories of crimes did, including robbery (195%), burglary (36%), other assaults (35%), prostitution and commercialized vice (2 to 68 cases), vandalism (71%), driving under the influence (64%), and disorderly conduct (188%). Drunkenness showed an erratic pattern: 230 cases in 1974, 540 in 1975, and 175 in 1975. Dixon also computed a violent death index, which combined deaths due to accidents, suicides, homicides, and alcoholism. It increased from 42 in 1973 to 61 in 1975 (+45%), but it had been 65 in both of the economically depressed years of 1970 and 1971.

On mental health Dixon reports: "The Alaska State Mental Health Clinic located in Fairbanks admitted 40 to 50 percent more new clients during the pipeline period than before pipeline construction began" (p. 211). In the judgment of the clinic's staff, the increased "caseload did not represent newcomers to the community" (p. 211). Fairbanks Crisis Line reported that crisis calls increased from 29% of total calls to 49% from 1973 to 1975 and that suicide-related calls increased fivefold in the spring of 1975 compared to spring 1973.

Dixon interprets her data on social and psychological trends as having both positive and negative aspects. Crime indicators increased substantially and mental health service and family conflict indicators also increased faster than population; but incomes increased dramatically and the demand for most welfare and social services declined. One cannot say whether the overall effects were good or bad, and citizens have conflicting opinions on the matter. A survey of 408 families in 1975 by John A. Kruse found only 14% saying the community had changed for the better in the previous three years, and 56% said it had changed for the
worse. In fact, "47% of households said that they were the victims of some type of crime in the preceding year" (p. 215). On the other hand, Dixon claims that "Fairbanksans overwhelmingly support construction of a trans-Alaska gas pipeline" (p. 301).

In addition to the major works cited above as supporting the disruption thesis, several minor works deserve brief mention. Weisz (1979) surveyed 215 heads of households in the boomtown of Gillette, Wyoming, in 1977 and found an unusually high level of stress as indicated by the Holmes-Rahe Social Readjustment Rating Scale. Weisz also reports that Campbell County admissions to the Northern Wyoming Mental Health Center increased 101.2% from 1975 to 1978, while the population of Campbell County increased 62.5%. He uses admissions to the State Mental Hospital as an index of severe mental illness, and the admissions from Campbell County increased from 10 to 71 (610%) from 1974 to 1978. Another study with statistics on mental health problems is by Thompson et al. (1979). They found substantial increases (198%) in the boom county, Platte, Wyoming, in the active clients for mental health services who were admitted for a variety of psychiatric disorders. In another Wyoming boom county, McLean, however, this indicator declined 45%. Incidentally, the largest increases in psychiatric disorders in Platte were in schizophrenia and disorders related to alcohol and drug abuse.

In the area of crime statistics two additional studies should be noted. Thompson (1979) finds a 91% increase in the total crime index from 1973 to 1976 (144% increase for 1973 to 1978) for Campbell County, Wyoming, which had a 67% population increase. The per capita increase was 14% (20% from 1973 to 1978), while four other Wyoming counties without booms had per capita increases in the same period of 113%, 34%, 4%, and −15%; so the effects of the boom are not clear. Thompson also calculates that per capita crimes against persons decreased 25% in Campbell between 1973 and 1978, while per capita crimes against property increased 25%. Hogg and Smith (1970, and Smith et al., 1971) studied a miniboom in Sweet Home, Oregon, and found a
140% increase in total crime offenses from 1962 to 1966, while population increased only 18%. The greatest crime rate increases were for grand and petty larceny and traffic violations and the lowest increases were for disorderly conduct and assault and battery.

The above review of boomtown studies indicates that the social disruption thesis has some empirical support. However, very few boomtowns are included in the above review, and specific disruptions or problems do not occur in all cases. Crime rates often increase, but they did not increase in the Page, Arizona, boom of the early 1970s (Little, 1977) or in the World War II boom in Willow Run (Carr and Stermer, 1977). Mental health services admissions or caseloads often increase, but they declined in McLean County, Wyoming (Thompson et al., 1979). Alcohol consumption per capita increased in Craig, Colorado (Lantz et al., 1980), but declined between 1971-1972 and 1974-1975 in Fairbanks, Alaska (Dixon, 1978: 118, 216). The research to date allows us to say that boomtowns tend to be socially and psychologically disruptive, but it does not provide us with a common boomtown pattern or SIM.

I recognize that the above review of boomtown studies does not examine the reliability of the data or consider alternative explanations for the negative changes in social and psychological indicators. Space does not permit such an examination. Most of the cited researchers, however, probed for changes in data recording procedures (potentially the major reliability problem) and were satisfied with the basic comparability of the annual rates they published. Most also sought to determine whether the characteristics of the newcomers accounted for the changes and generally found that they did, but only in part. Increasing problems among the oldtimers also contributed to many of the rate changes. Nevertheless, the cited support for the boomtown disruption thesis must be partially discounted until critiqued more carefully. In no case were all potentially pertinent factors controlled.
THE BOOMTOWN PATTERN

I conclude my examination of the debate over the boomtown disruption thesis with a description of the general boomtown pattern which seems to emerge from this review. Booms disrupt social patterns and cause psychological stress for oldtimers, but most adapt rather well and on the whole show as much satisfaction with life as oldtimers in non-boomtowns. Newcomers to boomtowns seem to do less well than oldtimers. They tend to experience greater psychological stress and express more dissatisfaction. These problems for both newcomers and oldtimers seem to translate into increased use of professional mental health services, but the absolute number of mental health clients is small. On the other hand, booms generally improve the economic position of preboom residents (though not as much as was originally believed; see Little and Lovejoy, 1977) and newcomers, so the net effects of the boom on the quality of life of oldtimers and newcomers is not clear.

Family stress and marital problems are commonly cited as problems in the qualitative descriptions of boomtowns. The quantitative evidence for these problems, however, is slim. Dixon (1978) finds increases in the divorce rates exceeding increases in the marriage rates during the Fairbanks boom, but Thompson (1979) finds the increases in the divorce rate for Campbell and the four comparison counties to be about the same. We should not ignore the qualitative testimony of numerous field observers about the family stress which results from boom conditions, but we have little indication as yet on its severity. According to Lantz et al. (1979), the boom in Craig is related to a jump in child abuse and battered women incidents; but I have not been able to examine the report on which this observation is based (Quality Development Associates, Inc., 1979).

Booms may be particularly disturbing for adolescents, as Freudenburg demonstrates for the boom in Craig and Dixon demonstrates for Fairbanks. Oldtimer adults need not mix much
with the newcomers, but the students must. Some schools change to two shifts. Adolescents are affected when family stress increases. Dixon reports increased parental neglect of young people, and the number of reported runaways nearly doubled from 122 to 243 between 1972 to 1975. Increased drug and alcohol use and related problems are reported for some boomtowns. On the other hand, young people are generally benefited by the job opportunities created by the boom. A side effect of the increased job opportunities, however, can be an increase in the number of high school dropouts and a reduction in college applications (Dixon, 1978).

Finally, the crime rate tends to increase. Usually crimes against persons do not increase faster than the population, but crimes against property and conduct offenses do. Many observers report the weakening of the mechanisms of informal social control in boomtowns and thus a degree of social disorganization. Some surveys find higher levels of concern about crime in boomtowns (e.g., Freudenberg et al., 1977; Kruse in Dixon, 1978), but Thompson et al. (1979) did not.

The reader probably has noticed that I have essentially resurrected the debunked boomtown stereotype. I have tried, however, to remove the exaggeration which afflicted the stereotype and which Wilkinson and his associates have successfully debunked. The patterns identified are not without exceptions and are milder than those portrayed in the stereotype. Two factors in particular are likely to affect the degree of severity of negative social and psychological impacts of booms. First, the degree to which the community is stable, homogeneous, nonindustrialized, folkish, and integrated affects how disruptive the boom changes will be. Wilkinson and his associates point out that many western communities have a history of growth and decline and a fair amount of population turnover, so they should not be shocked by booms. Second, the characteristics of the newcomers must be considered in evaluating boom effects. Part of the exaggerated boomtown stereotype was the stereotypic view of the newcomer as a young hellion. Most of the newcomers to
boomtowns, however, though young, are married workers and their families. In a study of 14 construction projects only 24.6% of nonlocal construction workers were single or divorced (Mountain West Research, 1975). About half (48.9%) were married workers who brought families with them. The remainder (26.5%) were married workers who did not bring their families. For every 100 nonlocal workers, 128 dependents would move to the area (a study of 55 Corps of Engineers projects computed 124 dependents per 100 workers; see Dunning, 1980).

Many other factors affect the degree of social disruption caused by booms, and we have a poor handle on most of them. Further research on boomtowns is needed to increase our ability to predict and mitigate their negative social and psychological effects, and to advance our general understanding of social disorganization and adaptation and of psychological stress and adjustment.

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COMMENTARY

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BALANCE AND BIAS IN BOOMTOWN RESEARCH

Wilkinson and his colleagues offer several contributions to the literature on energy boomtowns of the western United States. One is a call for attention to scientific quality; the second is a theoretical perspective which they suggest would offer a useful guide for future boomtown research; the third is a critical review of the evidence and the reasoning employed in existing research. My comments will focus exclusively on their critical review.

Detailed and critical scrutiny has a time-honored place in scientific research; critiques, however, should be able to withstand the same degree of scrutiny as any other form of scientific reporting. Wilkinson and his colleagues identify two particularly important criteria: "an intimate knowledge of subject matter . . . and an objective procedure for examining evidence."

It is appropriate to ask whether their own article meets these criteria; a careful examination of the evidence suggests that it does not. The article contains a number of valid points, but they are intertwined with a good deal that is misleading or simply incorrect. The result is a critique likely to sound convincing and accurate to persons who are not familiar with the literature, but convincingly inaccurate to those who have examined the evidence carefully. To illustrate this point, this review will focus on three issues identified in the critique: documentation, analysis/interpretation, and "theoretical assumptions."

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Wilkinson and his colleagues criticize existing literature for "citations of undocumented assertions as evidence." The criticism is an important one if it is accurate; examination of the examples provided, however, calls their argument into question.

They are particularly critical of Murdock and Leistritz for concluding that "several researchers provide strong documentation" of a general increase in boomtown crime rates (1979: 252). They claim, for example, that one of the Murdock-Leistritz sources (Thompson et al., 1979)

contains no data on the problems indicated by Murdock and Leistritz but reports survey data showing no significant difference between population samples of two growing and two stable counties in proportions believing that crime had or had not increased in recent years.

The report does contain the survey data they mention; it also contains actual crime data they evidently overlook. Both of the growing counties had substantial increases in arrests in a short period—arrests in one county increased 284% (from 100 to 384) in two years, and arrests in the other increased almost tenfold (from 26 to 248) in three years (Thompson et al., 1979: III.45, III.68, II.97). Given the fact that this documentation of the Murdock-Leistritz conclusion is drawn directly from Thompson's own reports, it is puzzling that he and his colleagues could conclude that "documentation of the conclusion is not found in any of" the sources cited.

Equally puzzling is their criticism of Gilmore, whose work is dismissed as providing "totally undocumented claims." In fact, it is clear from reading Gilmore and Duff (1975)—and it is widely known among boomtown researchers—that the statistics involved came directly from agency records. The credibility of Gilmore's work is quite considerable, not only among researchers but also among major energy companies. Such firms could scarcely be expected to hire a consultant who exaggerates or makes up evidence of negative impacts, particularly when more cooperative expertise is available.
That credibility also appears to be warranted. Gilmore and Duff devoted extensive research to their case study, but, as indicated by the title of the book that resulted from the study, their focus was on "boomtown growth management" rather than on exhaustive documentation of the problems involved. The existence of the problems was so clear that Gilmore and Duff chose to go beyond documentation and to explore potential mitigating measures—a fact, incidentally, which helps call into question the Wilkinson et al. contention that the variable of community efforts to secure outside assistance "is ignored, generally, in the energy development literature." (See, in addition, the very useful "Coping With Growth" publications of the Western Rural Development Center, particularly Butler and Howell, 1980, and Faas and Howell, 1979. For a highly selective list of other relevant works, see also references 9-23 in Freudenburg, 1981.)

**ISSUE: ANALYSIS AND INTERPRETATION**

Wilkinson and his colleagues criticize the fact that one of my earlier papers reported substantial percentage increases in crime rates and child behavior problems without clearly stating that the "percentages are based on small initial numbers—two crimes against persons, three child behavior complaints." The criticism is a valid one, although the numbers are wrong (the base figures were four and six, respectively—Lantz and McKeown, 1979: Table 2). The validity of the criticism is the reason that more recent reports (e.g., Freudenburg, 1980a: 4-5) have contained explicit warnings that "spectacular increases in percentages generally need to be interpreted with caution," and that confidence is drawn primarily from "the general pattern of findings." It is unfortunate that Wilkinson and his colleagues neglected to mention the broader pattern of findings, including the fact that substance abuse rose 623%, from 13 to 94; that family disturbances rose 352%, from 27 to 122; or that crimes against property rose 222%, from 59 cases to 187. In the context of such a pattern, a 1000% increase in child behavior problems—even if it is
from a small base of six cases in 1973 to the noticeably larger figure of 66 cases in 1976—would appear to be worthy of at least a certain degree of sociological attention. It is difficult for scientists to devote objective examination to data that are simply ignored.\(^3\)

The failure to deal with the available evidence may actually be as severe in the critique as it is in the boomtown literature more generally. Wilkinson and his colleagues criticize the Weisz (1979: 41) report of figures on Campbell County (Wyoming) mental health center admissions (although their calculations are in error—calculations from the Weisz figures show admissions rising from 24.8 per thousand in 1975 to 30.7 per thousand in 1978, more than a 20% increase on a per capita basis). As they note, Weisz provides the relevant data, but they do not note that the next page of his report also shows an increase of 610%, from 10 to 71, in Campbell County’s admissions to the state mental hospital (generally the more severe cases) between 1974 and 1978. Nor do they mention that substantial increases in crimes, even on a per person basis, have been reported for a series of boomtowns; instead, they claim incorrectly that the Murdock and Leistritz summary was not supported by the cited reports, and they call into question the “totally undocumented claims” of Gilmore and Duff (1975). Other sources point to greater crime increases than those cited by Gilmore and Duff, who chose not to report some of the more dramatic figures. For example, Lovejoy (1977: 15) provides this summary of the figures he obtained directly from an officer of the Rock Springs police department:

Calls for police services in Rock Springs increased by over 400 percent between 1969 and 1974, and nearly every category of criminal activity had tremendous rates of increase: burglary skyrocketed, drunkenness and driving under the influence of alcohol increased over 300 percent, arrests for prostitution increased from none in 1969 to over 80 in 1975, arrests for concealed weapons increased from 2 in 1969 to over 85 in 1975, drug problems and drug related incidents “exploded,” and juvenile problems increased substantially.\(^4\)

The “underground” or “fugitive” nature of much of the boomtown literature would make it understandable that a
researcher might not be familiar with every single relevant work. Under the criterion of "an intimate knowledge of the subject matter," however, it would seem appropriate for a team of four authors to be familiar with the preponderance of the available and relevant evidence; under the criterion of "an objective procedure for examining evidence," it would seem important for the same researchers to take into account the existence of the data with which they are familiar. Yet Wilkinson and his colleagues explicitly quote the crime data from what may be the only longitudinal study of an energy boomtown where the increase in crimes was roughly paralleled by the increase in population—that of Little (1977). They do not mention Little's explicit warning (1977: 416) that "Page is an atypical boomtown," one which ended a massive boom-bust cycle in 1967 and then began a second boom in 1970.5 (Little's crime and population data are drawn from the second boom.)

The failure to consider the relevant data is particularly unfortunate, in that it could serve to divert attention away from a valid point. While increasing confidence can be gained from a series of independent findings that all point to the same general conclusion, it is not appropriate to assume in advance that all presumed "social pathology" indicators will be positively correlated (Freudenburg, 1980b). This problem is a real one in the boomtown literature—notably with respect to child abuse problems and divorce rates, and particularly with respect to the latter. While divorce rates are often mentioned casually on the lists of "social pathologies" that are presumed to result from boomtown growth, the connection between "divorce" and "social pathology" is an extremely weak one, particularly for statistics from the past two decades, when divorce patterns have been changing throughout our entire society. In addition, actual statistics on divorce are rarely presented in available studies, and even then, the computation of divorce rates is anything but straightforward: High rates of in- and outmigration make it inevitable that a certain proportion of couples will be married in one county but divorced in another. In short, while it is not appropriate to dismiss all relevant quantitative documentation on the basis of isolated and probably unrepresentative
examples—thus ignoring "previous evidence that some of the rates . . . might be negatively correlated" with one another—the critique would have been on more solid empirical ground if Wilkinson and his colleagues had instead argued that inappropriate measures such as divorce rates should simply be dropped from all future summaries of "boomtown problems" unless quantitative evidence of their appropriateness can be provided.

ISSUE: "THEORETICAL ASSUMPTIONS"

Wilkinson and his colleagues do not actually say that researchers see boomtowns as undergoing a transition from Gemeinschaft to Gesellschaft. What they say is that "many of the postulates which are suggested in discussions of local social effects relate, at least implicitly, to a theoretical perspective which can be traced to the typological approach used earlier by Ferdinand Toennies, Emile Durkheim," and other theorists. There would seem to be little reason to devote such a detailed critique to the use of Toennies-style typologies, however, unless those typologies were actually believed quite explicitly; yet the evidence on that point is anything but convincing.

To back up their assertion that "many discussions of western energy development are consistent with" these typologies, Wilkinson et al. cite Cortese and Jones (1977: 85), Gold (1974: 9), and Freudenburg (1976: 15). I do not have access to the Gold work they cite; I do have access to the other two works, however, and both of them say something very different from what they are characterized as having said.

Cortese and Jones (1977: 85) note that the boomtown's "mutually reinforcing cultural changes add up to the process of urbanization or, depending on one's orientation, modernization." This is the sentence quoted by Wilkinson et al. It is a summary of the preceding paragraph, which notes these more specific cultural changes:

First, the communities were becoming more culturally diverse as new people brought in new ideas. . . . Second, diversity meant that
the towns were becoming less provincial and isolated. . . . A third trend [was] toward professionalism and respect for expertise. . . . [a fourth was toward] specialization and bureaucratization. . . . A fifth trend [was] . . . a growing belief that bigger is better, as well as more efficient and cheaper. . . . A sixth pattern, centralization, was spreading into these towns. For some long-time residents the profit motive had been strengthened, a seventh change. . . . As the community grew and more and more strangers moved into town, many people started to rely more on [formal] institutions, an eighth cultural shift. . . . A ninth change is that, at the same time, people became more demanding of their institutions [Cortese and Jones, 1977: 85, italics in original].

Cortese and Jones are clearly sensitive to the contributions of Toennies, Durkheim, and others; yet the above discussion provides little evidence that the preboom community is romanticized, or that it is "assumed to be" composed of people "whose lives are rooted in symbiotic interdependency."

The second example available to me is that of Freudenburg (1976). As Wilkinson and his colleagues note, this paper draws attention to the importance of "sheer change" in boomtown settings. An examination of the paper itself reveals that the reference is not to the work of Toennies et al., but rather to the more recent, more quantitative, and probably more directly relevant research on stressful life events (Freudenburg, 1976: 14):

Using a randomly selected population, Myers et al. (1971) found that psychiatric symptoms were significantly associated with recent experience of (moderately stressful) "critical life events" such as changes of educational, occupational, financial, and marital status; a follow-up study (Myers et al., 1972) found that changes in symptom level were directly associated with changes in the number of events experienced. Haberman (1965), using a strictly non-random sample (alcoholics, their spouses, and a matched comparison group) found that extreme changes in the index scores of a psychiatric screening inventory were directly related to changes in environmental stress. Studies in this area . . . [generally classify about 20 percent of the population] as "impaired"; aside from the fact that the cutoff point is somewhat arbitrary, these rates may be temporarily inflated, as Dohrenwend and Dohrenwend (1965) have noted, by "stressful events in the contemporary situation," particularly among lower class persons.
Is boom development likely to inject "stressful events" into a "contemporary situation?" As the [earlier] statistics indicate, at least quite a few people apparently felt so. . . . Developments can obviously bring a good deal of change, and the social psychiatry literature emphasizes the importance of sheer change in producing anxiety and maladaptation, almost (but not quite) to the point of concluding that the apparent desirability or undesirability of a change is irrelevant (see, e.g., Myers et al., 1971; Gerston et al., 1974).6

It is clear that a number of researchers have used classical theorists as sources of sensitizing constructs, but this fact alone would scarcely seem to warrant the Wilkinson et al. critique—particularly given the obvious connection between boomtown statistics and the kinds of changes Durkheim, for example, might have predicted. Moreover, an examination of the literature reveals that sociologists who have noted the relevance of the Gemeinschaft-Gesellschaft continuum (e.g., Cortese and Jones, 1977; Moen et al., 1981) have generally done so in a way that is reasonable and balanced. The classical theorists are used as points of reference, not objects of worship; they are treated with respect, but they have also been rejected explicitly in several papers. To quote one summary:

The boomtown of this study stands in clear contradistinction to Toennies' Gesellschaft, for example—where individuals remained "essentially separated despite all uniting factors"—or to Simmel's metropolis, where citizens experienced "a slight aversion [toward others], a mutual strangeness and repulsion" (Toennies, [1887] 1963:74; Simmel, [1903] 1950:416). Rather, most of the long-term residents of the boomtown have continued to experience meaningful personal integration within their more intimate social spaces (where, in many ways, it matters the most); this "localized integration" continues even at a time when the social structure of the broader community is undergoing substantial alteration, but it is all the more notable for that reason [Freudenburg, 1978: 50].

Even without such clear counterexamples, it is not entirely clear why Wilkinson and his colleagues would wish to focus on
“suggested postulates” which “relate, at least implicitly, to a theoretical perspective,” which in turn “can be traced to” the classical theorists they actually criticize—particularly when a number of explicit and useful theoretical perspectives are available. For example, Little (1977) has analyzed the importance of locals’ inflated economic expectations (corroborating evidence of which has since been provided by Thompson’s own work). Lovejoy and Krannich (1980) have devoted critical thought to issues of dominance, dependency, and neo-colonialism. Massey (1977) has done much to demolish the simplistic assumption that newcomers or construction workers are “riff-raff,” or that they are social isolates (see in particular Massey and Lewis, 1979). Moen and her colleagues (1981) provide an insightful analysis of the informal but important roles of women in community adjustments to rapid growth. Murdock (1979) has argued that the concepts of human ecology may have much to offer in social impact assessments. This list is anything but exhaustive, but it does begin to provide some idea of the richness and the range of writings not reflected in the Wilkinson et al. critique.

**ISSUE: SCIENCE**

Wilkinson and his colleagues usefully point out that, in addition to systematic modes of thought, “science requires an intimate knowledge of subject matter . . . and an objective procedure for examining evidence.” Their article, unfortunately, falls short on one or both criteria. As a result, their article is misleading, its conclusions are inaccurate, and its tone of scientific certitude is inappropriate.

Existing evidence is clearly suggestive, not definitive, but what the data suggest is that the social consequences of rapid community growth are not uniformly positive. The negative impacts do not appear to be overwhelming; they do, however, appear to be both real and significant. While much of the existing literature on energy boomtowns is lacking in balance, quality, or sociological sophistication, the best available evidence—
including the clear preponderance of quantitative research that is
guided by normal social science standards of adequacy—
contradicts the conclusions of Wilkinson and his colleagues.

Given the critical tone of their review, particularly in
conjunction with the demonstrable inaccuracy of most of their
specific conclusions, the article by Wilkinson and his colleagues
seems destined to arouse a number of intense responses.
Overreaction to the article, however, would be unfortunate, for
two reasons. The first is that the concluding section of their article
suggests a theoretical framework which they believe would be an
improvement over those currently available. The potential
usefulness of this approach should be treated as an empirical
question; it would be unfortunate if the approach were
overlooked or discarded simply because of the intensity of the
reaction against the remainder of the article.

The second reason is that, whatever the degree of inaccuracy of
their specific comments and citations—even if, as it appears, the
error is substantial—one point by Wilkinson and his colleagues is
entirely valid. Much of the existing literature on energy
boomtowns is of dubious scientific validity, and hence of dubious
practical value. The problems, however, include simplistic
assertions about both the positive and the negative consequences
assumed to result from boomtown living. Indeed, as Schnaiberg
(1977, 1980) has noted, the more common tendency appears to be
for the advantages of growth to be exaggerated, while the
disadvantages are ignored or minimized.

Decision-making documents that ignore the evidence on
boomtowns appear to be somewhat less common today than they
were in the middle of the 1970s, although unfortunate examples
have clearly not yet disappeared (see, e.g., U.S. Department of the
Air Force, 1980; U.S. Department of Energy, 1979; see also the
discussion in Freudenburg and Keating, 1982). Throughout most
of the 1970s, however, even the most obvious sociological issues
were often ignored entirely; thus a number of writers explicitly
noted that they were attempting to draw attention to problems
that were being overlooked in most discussions at the time. As the
relevant social impacts have begun to be considered, the need for
an emphasis on negative findings has diminished. It is thus doubly
unfortunate that Wilkinson and his colleagues fail to cite so many of the recent works that provide relevant evidence. (Probably the best single summary of the current boomtown literature is provided in Weber and Howell, 1982, which contains contributions from many of the authors criticized by Wilkinson and his colleagues; most of these papers, incidentally, have been available and widely circulated since early 1980.)

It is appropriate to note again, however, that balance is important. While there are good reasons for professionals to avoid oversimplified accusations as well as inaccurate assertions, the larger message of this discussion may be one of reaffirmation: The progress of science *does* depend on accurate evidence and even-handed analysis. In short, it is to be hoped that the Wilkinson et al. "critical review"—and the responses to it—will together constitute yet another reminder of the importance of being careful in one’s scholarship.

NOTES

1. These calculations draw combined municipality-county totals directly from data in the Thompson et al. tables; the calculations exclude 468 crimes in one of the boom communities for which comparable preboom data are evidently not available. If the additional crimes were included in calculations, both boom counties would have shown crime increases in the 750-850% range. One of the “control” counties had more than three times as many “preboom” arrests as the other control county or either of the impact counties (in 1975, the first year for which comparable data are available). That county experienced a 54% increase in arrests in the next year, from 318 to 489, and a 70% increase over the entire period for which data are available. The other impact county showed a relatively random pattern of arrests, fluctuating between a low of 22 and a high of 37 arrests over a five-year period. These figures remind us that fluctuations in crime rates need to be interpreted cautiously, but they do not begin to approach the magnitude of the changes in Thompson’s impact counties. See also note 5.

2. Some of his recent clients with which I am familiar include AMAX, the Stone and Webster Engineering Corp., the Rocky Mountain Energy Company, and the Electric Power Research Institute. He has also been a consultant to the Environmental Protection Agency, the U.S. Synthetic Fuels Corporation, and the Office of Technology Assessment, among others, and he has received a half-dozen invitations to present testimony before congressional committees.

3. More recent data from the same community also provide general support for this pattern of findings, but except for the findings reported in Freudenburg et al. (1982), these recent data were not previously available to Wilkinson and his colleagues.
4. The 400% increase in calls for police services is consistent with Edgley's (1979: 100-101) report that calls increased from 9,000 to 39,000 over the shorter time period 1970-1973. For similar reports from other boomtowns, see Montana Energy Advisory Council (1975: 5), Lantz and McKeown (1979: 45), or Freudenburg et al. (1977: 5-7); for a brief summary discussion which also notes consistent findings from survey data on criminal victimization, see Freudenburg (1980a).

5. Boomtowns such as Rock Springs, Wyoming, or Craig, Colorado, have more typically shown total population increases or decreases of well under 10% during the entire decade before the beginning of their respective booms. It should be noted that dramatic increases in crime do not appear to be associated consistently with less rapid growth—for example, in communities that have grown 2 to 4% annually—even in cases where that growth is energy-related. Such low-growth communities do not seem to warrant the "boomtown" label, but they are important because they suggest that the effects of growth on crime rates may be strongly nonlinear. A further implication is that cross-sectional studies which attempt to draw longitudinal conclusions—for example, by regressing crime rates against growth rates for a large number of counties—are likely to encounter distorted findings with respect to the genuine "boom" areas. This problem, of course, is further worsened by the fact that there can be more significant variations in record-keeping practices across police departments than there are within departments on a year-to-year basis.

6. The stressful life events literature is clearly not free of all problems, particularly with respect to the proper weighting scheme for desirable-yet-stressful events, such as major promotions (for recent discussions, see Ross and Mirowsky, 1979;Thoits, 1981; Tausig, 1982). Boomtown findings since 1976, however, continue to suggest the relevance of this perspective. In Wheatland, Wyoming—often regarded as a "success story" for impact mitigation efforts, and clearly a community where a large number of problems were successfully avoided—a survey using trained mental health workers as interviewers found that 25% of the females and 35% of the males in the community sample showed a level of distress indicating a need for psychological treatment (Bougst and Marshall, 1981: 6). These researchers report that in representative samples of the national population, roughly 15% of the public would be expected to experience such high levels of distress according to the same methodology (U.S. President's Commission on Mental Health, 1978, as quoted in Bougst et al., 1981: 8). Another survey by mental health professionals in Gillette, Wyoming, found a community sample to have a mean score of 308 "life change units" on the Holmes and Rahe (1967) social readjustment rating scale (Pattinson et al., 1979; Weisz, 1979). This level of stress is categorized by Holmes and Masuda (1974: 61) as indicating "major life crisis." In a prospective study, 49% of the persons scoring over 300 reported illness within nine months of the survey; only 9% of the group with scores below 200 reported illness over the same period (Holmes and Masuda, 1974: 64).

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COMMENTARY

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The three sections of this article critique the emergent conventional wisdom concerning the social impacts of energy development projects in rural areas. Before commenting on each of these sections, some attention must be given to the context in which this research typically occurs.

A significant portion of such research is initiated by government agencies legislatively charged with reviewing the anticipated social effects of large-scale developments. Both the research setting and the interests of the contracting agency in a description of existing conditions and a prediction of likely future states are in marked contrast to the less applied, deductive, theory-building goals of university-based research, even when that research is funded by government agencies interested in basic social science research. These differences are seen in three areas—methodology, professional backgrounds and organizational affiliations of research personnel, and publication outlets. Each has a major impact on research results, and a primary failure of Wilkinson et al. is that this important research context is totally ignored in their critique.

Social scientists involved in project-oriented case studies often find severe constraints on their preferred methodology. Contracting agencies may restrict the use of social surveys, either because of their commitment to Office of Management and Budget restrictions on such surveys (which require a lengthy and complex review process for approval of instruments and survey design) or because of severe time and budgetary limitations. The consequences of these restrictions are clearly reflected in some of the studies examined—combined analyses of available public documents and records (approximating a small-scale "social
indicator" approach) with assembly of qualitative data through direct observation and key informant interviews. In their focus on the survey and documentary base of many studies, Wilkinson et al. ignore the fact that the conclusions reported reflect both the analysis of these "objective" data and reports by local residents. It should be noted, as they fail to, that the reports of such findings typically include a commitment to reporting these local perspectives. For example, Gold (1974), whose ethnographic work on the impacts of coal development is cited at several points, clearly distinguishes the perceptions of the informants and the researchers' analysis. Similarly, as Cortese and Jones (1977: 79) are careful to point out:

Data from the resident interviews and institutional representative interviews were subjected to only the most elementary statistical analysis for this report but were hand-analyzed quantitively and qualitatively to detect trends. Thus, our findings must be received more as those of the participant-observer, based upon our interpretations after having "been there" and upon data from what are accurately termed informants rather than a random sample of survey respondents [italics in original].

While the research context or other methodological limitations obviously do not excuse poor research, it is not appropriate for the authors to assume a similar context for all of the research critiqued and then to base the criticism on standards which are more appropriately applied to basic research initiated by university-based sociologists and published in academic journals.

Second, the funding, disciplinary backgrounds, and organizational affiliations of social scientists involved in social assessment research are often more diverse than is typical of traditional community studies. Indeed, the authors themselves reflect this diversity—at least one is a geographer, two work in academic settings, one is employed by an agency with direct interests in energy development (U.S. Geological Survey), and one is affiliated with a private research firm. The only individual missing is the social scientist employed by the research and development division of a corporation with economic interests in energy development.
Social scientists involved in this research often face special conditions which can influence their research, in addition to the subtle biases which characterize more basic research. For example, within the natural resource agencies which contract for impact research, the ideology of the dominant natural resource professionals (foresters, civil engineers, etc.) may be reflected in agency preferences for resource development and for social research supportive of this development. These factors could have influenced some of the research reviewed in the article, although the “development causes problems” tone of much of it would receive little sympathy from development-oriented agencies. However, it is also possible that the authors’ own perspectives may have been conditioned by the organizational settings within which they work.

A related point is the likely corporate and agency use of the authors’ critique. Organizational entities wishing to expand energy-related development will find ready use for the challenges included in this article. Unfortunately, it is doubtful that these uses will include a careful examination of the veracity and accuracy of the authors’ critiques, especially since their general conclusions are consistent with development interests. Paradoxically, Wilkinson et al.’s criticism of the zeal reflected in the reporting of study results could also be applied to their own strongly worded conclusions that a portion of this research is flawed.

Much of the research is reported in a “fugitive” literature consisting of unpublished papers, reports, and articles in nonsociological journals and popular magazines. These include the “most frequently cited” paper by Kohrs (1974) and several papers by the authors (Thompson, 1979; Thompson et al., 1979, 1980). Of the approximately 25 community case study works cited, nearly half are unpublished papers or reports. This fact makes it very difficult for the reader to assess the criticisms of the authors and to examine the extent to which the specific findings under attack reflect the major conclusions of the research. Further, acknowledgment of the special characteristics of this literature tempers their implicit criticism that the studies fail to show the theoretical relevance and methodological rigor of works
published in academic social science journals. Sponsoring agencies typically care little about the theoretical relevance of the research; and where sophisticated methodological techniques are employed, the usual response is a plea to translate major results into lay language. The major point of the preceding comments is that Wilkinson et al. ignored the conditions surrounding the research under review. Their failure to place the work in its appropriate context is as inexcusable as if someone in a community action institute had critiqued scholarly work in community sociology without acknowledging the context in which that research occurs. The remainder of my comment examines the three sections of the paper. The first question is: In their critique of specific studies, do the authors accurately reflect the methodology, analysis, and conclusions of the researchers under attack?

The authors provide some telling criticisms of a number of empirical studies and provoke the reader to verify their criticisms by reexamining the original sources. Unfortunately, because of some of the characteristics of this literature noted previously, a number of works are unavailable. (The authors apparently had a similar problem—see their footnote 3.) However, an examination of several of the journal articles cited suggests that their criticism is somewhat misleading or fails to correctly summarize the perspective of the author(s). For example, Wilkinson and his colleagues cite Cortese and Jones (1977), among others, when documenting the reliance on Kohrs (1974) as "primary documentation of social disruptions." Cortese and Jones cite Khors only once, and state: "Much has been written about the rise in alcoholism, child abuse, welfare expenses, crime, suicide and divorce." Cortese and Jones also cite Gilmore and Duff (1975: 12-15) and Little (1977). It stretches the point to refer to this use of Kohrs as "primary documentation." In another instance, while it is true that Little does not distinguish between the rate of crime increase and the total number of crimes, he does qualify his statement. What Little (1977: 422-423) actually says with respect to the data is the following:

It can be seen in Figure 1 that the increases in crime and population are roughly parallel. The rates of increase each year
vary considerably, with population increasing more rapidly than crime in some years and less rapidly in others. For example, from 1970 to 1971, as construction activities on the Navajo Generating Station began, the annual number of crimes increased 118.7% while the population increased 150.4%. But the percentage of increase from 1971 to 1972 was higher for crimes (63.7%) than for population (55.9%).

Little (1977: 423) also mentions in a footnote to the above quote that crime statistics during growth periods tend to underestimate the true number of crimes, since police capacity generally lags behind population growth. Elsewhere Williamson et al. state: "Little (1977: 414) argues that transient populations are associated with high crime rates." What Little says is this: "Transient populations, both in the popular mind and in reality, have come to be associated with high crime rates. Boomtowns prove no exception to the rule." A careful reading of the section from which this quote was taken reveals a complex discussion of transient populations and crime rates, and the special law enforcement problems of boomtowns. And, while Little comments on the "not atypical" crime rate situation in Page, Arizona, the general thrust of his discussion was that the community response to the problems of growth was not typical. In a section titled "Page, Arizona: An Atypical Boomtown," Little states:

Population declines in various years between 1957 and 1976 [following an earlier boom period] were very nearly as radical as the growth had been. Nevertheless, during the several growth years, yearly growth rates nearly always exceeded fifteen percent. But Page is a unique community that has survived population fluctuations which could have crippled normal communities. Page is an atypical boomtown not only because it has experienced two booms in less than twenty years, but also because until March 1975 it was a federal community.

One might also question the authors' selectivity in choosing which of a researcher's reports to subject to analysis. For example, they cite three single-authored papers by Freudenburg (1976, 1978, 1981). Reference to a summary of one of his other unpublished papers (Freudenburg et al., 1977, summarized in
Finsterbusch, 1980) gives a somewhat different impression of the research; Freudenburg and his associates did compare perceived conditions in boomtowns with three other nonboom communities. Finsterbusch’s (1980: 145) reading of this paper is apparently quite different than Wilkinson et al.’s examination of Freudenburg’s other work.

It is clear that the social system of Craig was in disarray. Surprisingly, however, the authors did not find individuals in Craig, except for the ranchers, more distressed or unhappy than individuals in other towns. They concluded that individuals are fairly well able to cope with boomtown conditions even when the preboom social arrangements are not able to cope.

It may well be that the results of the research reviewed in this article are so mixed that no firm “conclusions” are possible. If this is the case, as the authors themselves suggest in the last paragraph of this section, caution is indeed needed in selecting specific findings for scrutiny. The authors’ own conclusions are implicit throughout their critique, and these appear to have influenced their uses of the literature under review.

In the second section of their paper, the authors examine three unstated theoretical assumptions underlying social impact research. Their review of these major assumptions is, in large part, an effort to understand preenergy development community conditions and potential urban-rural contrasts.

The initial impression obtained from reading this section is that empirical research with a focus other than energy development impacts fails to support these theoretical assumptions. However, much of the research they cite does not directly address the fundamental question in such research: Will large-scale development significantly alter rural and small-town social life? For example, the historical examples cited which document a pattern of “recurring conflicts and upheavals” between ranchers and homesteaders and white settlers and original residents do not speak directly to conditions posed by large-scale projects involving a large construction force. Only one study is cited (Flax et al., 1979) as pertinent to the issue of
comparative levels of stress in rural and urban areas. In their discussion of Flax et al., the authors themselves refer to “an extensive review of the available evidence” which did conclude that higher levels of extreme psychological stress were more frequent in rural areas. And concerning disruption resulting from the speed and volume of change, the authors cite studies at both the individual and national levels which support the assumption they wish to challenge.

In their discussion of the assumption concerning possible domination and exploitation by outside interests, the authors mention programs of some government agencies and energy companies to mitigate growth-related impacts. Their observation that these efforts have not been systematically studied is not surprising, given their recent origin. What is surprising, however, is the authors’ failure to acknowledge that these programs have, in part, occurred in response to the conclusions of the community-based research they critique. Put bluntly, had government and corporate officials shared the authors’ doubts about the validity of this research, such efforts would be even less available to potentially impacted communities.

In their concluding comments to this section, the authors state: “The perspective on the community used in much of the western energy development literature is inconsistent with prominent current perspectives in the sociology of community.” Are they suggesting that the perspective has somehow lagged behind changes in rural communities? It would seem that a perspective which views communities as moving from “a more or less complete and self-contained local society” to the “dynamic form of community [which] has emerged in modern society” is precisely that which is utilized in most of the rural impact research reviewed in the article. Further, the focus of this applied research is not whether the community will change, as it is assumed that change is inevitable, but how much of that inevitable change is attributable to the impacts of large-scale energy development.

The concluding paragraph in this section reveals the authors’ sense of inconclusiveness and their methodological biases or standards. They state: “Conclusions reached from limited evi-
idence *are consistent* with theoretical assumptions which have guided much research on energy development” (italics added). But in the next sentence they indicate: “The assumptions are questionable, but they have not been tested adequately.” They go on to say that our knowledge is incomplete because of an apparent lack of “scientific evidence” or “explicitly relevant evidence” concerning a variety of social impacts.

In the final section of their paper, therefore, one looks for specific suggestions to guide future research in ways which would avoid the pitfalls they noted.

The first prescription for an improved approach is to abandon, or at least to test, the theoretical perspective that implicitly guides much of the research; the second is to rely on replicable measures rather than speculative assumptions and heresay as sources of evidence.

Given the choice between abandoning and testing theory, I assume that most would choose the latter, especially since Wilkinson et al. provide little in the way of possible alternative theoretical perspectives. In part, of course, the middle section of the article is one such first step, and if the conclusions of the studies critiqued in the first section were systematically linked to the three major assumptions, one might conclude that some indirect testing has already occurred. Their comments on replicable measures, however, both ignore the efforts of others to use quantified social indicators (even though the communities under study seldom maintain systematic data) and unfairly disparage efforts to verify appropriate qualitative data from community residents.

In response to the “obvious need for methodological improvements,” the authors suggest, in their concluding paragraphs, “adoption of a multivariate, probabilistic logic in analytical design.” Because little explanation is given, it is impossible to assess whether this different “logic” would remedy the methodological problems they describe. Most of the studies reviewed already address “such variables as local population composition, institutional practices in detecting and reporting the incidence of certain behaviors, and secular trends in larger society,” although
data limitations often preclude the use of the formal covariance procedures apparently preferred by the authors.

In a footnote (8) they explain that their suggested logic means "a commitment to examining competing explanations of observed reality." A careful reading of much of the research literature reviewed in this article, if approached from a different perspective, would suggest that such a logic already guides the research. The conclusions are typically cast in a probabilistic manner, and most studies utilize a range of variables and several complementary research strategies. The "competing explanation" in most such studies is that the changes that can be expected from energy development, or other large-scale projects, are not significantly different from those expected without the project, and this "explanation" is often formally provided as part of the "No Project" or "No Action" alternative required in environmental impact statements.

In conclusion, the authors have provided a challenging, if uneven, critique of an important, relatively recent body of applied social research. They have, however, few specific suggestions to remedy the problems they define, other than an important caution in the reporting of results. On the other hand, the next time I work on a social impact study, I think I'll tell the sponsoring natural resource agency that I plan to use a "multivariate, probabilistic logic in analytic design" and to view the community as an open, dynamic interaction field in my study of likely emergent outcomes of that interaction. They'll love it.

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COMMENTARY

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Wilkinson et al. rely on several standard numerical indices of social problems to evaluate the “assumption” (reading the entire article reveals that this is their favorite put-down word for what would ordinarily be called a finding) of well-known social impact assessment (SIA) researchers that the rural communities in the West which they have been studying have been experiencing “social problems” and other negative effects of the impacts of energy development projects. Their evaluation of the extent of social problems as revealed by these measures in the findings of the several cited SIA researchers leads them to reject this “assumption,” to declare that this and other “assumptions” in these studies “are informed substantially by hearsay evidence,” and to assert that “conclusive evidence to the contrary is not available.”

First, it is indefensible to rely on mental breakdown rates and other such secondary data as the “evidence of disruptive effects” of Western energy development projects when a wealth of sociologically validated primary data on the social interactions, organizations, and norms to which these indices pertain are available and could be used by the authors to examine directly and thoroughly the negative social effects in question.

Second, dwelling on crime rates and other such indicators of social problems constitutes a big to-do about trivia (i.e., indicators of behavior rather than the behavior itself)—and often misleading trivia at that. For example, reported crime rates in budding boomtowns may be largely a function of the efforts of local law enforcement agencies to justify expanding their staffs,
quarters, and so on. Then, when the agencies have really gotten
good at recording officially all manner of law violations they may
not have previously recorded so scrupulously and have used these
numbers to help achieve the desired expansions, they may justify
the increased expenditures by reporting success in reducing crime
rates. Do these kinds of data really enlighten us about actual
criminal activity in boomtowns? Hardly. One needs to find out
from people who are "in the know" about such matters what is
actually going on versus what is officially reported to be going on.
This kind of investigation includes (developing the kind of
trusting relationship needed to facilitate) finding out from the/a
local doctor how much of what kinds of evidence of alcohol and
other drug abuse, rapes, and cases of physical abuse of spouses
and children he or she is seeing versus what is reported to the law
enforcement personnel versus what these personnel put into the
official reports of the kind Wilkinson et al. seem to believe is the
only acceptable documentation concerning the incidence of
criminal activity in boomtowns. (The doctor is likely to be seeing
several times as many rape and abuse cases as are reported to the
police. And other informants are likely to know about cases that
have not come to the attention of any professional service pro-
sider.)

Third, limiting their examination of social problem data to
indicators of the incidence of crime, illness, divorce, and the like
gives them a very narrow and uncertain view of social problems,
which, predictably, differs markedly from the views that the
several SIA researchers attempted to project. Like so many
important aspects of human community life, social problems
cannot be understood sociologically unless certain qualifying
questions are asked. A principal one, not raised in Wilkinson et
al.'s examination, is this: "social problems" from whose point of
view? From the authors' point of view, social problems in rural
boomtowns in the West are not all that bad when defined solely in
terms of the selected indicators. From the point of view of the
great majority of the residents, newcomers as well as longtimers,
local social problems are very real and very great—at least, this is
clearly the impression one gets when reading the cited reports of
the several SIA researchers. Let me indicate where this impression comes from and why its sources are too sociologically significant to ignore.

The chief source of this impression is the story of what happens to a rural community in the West when its values are affected by the various representatives of industry. Example One: Some of the community's basic values, which are its great strength and charm as a rural community, become its greatest vulnerability and weakness when faced with the rapid growth of industrialization. Thus, valuing, and counting on, honesty, openness, and trust in business and other negotiations puts the community fathers at a great disadvantage in trying to deal with urban-industrial types whose word is definitely not their bond. Example Two: After a long history of doing virtually everything informally and personally, the locals are suddenly engulfed by a world in which personal reputation means little or nothing; votes must be taken on proposed actions previously handled by sensing when there was and was not consensus; bureaucratic expectations and procedures have to be studied, understood, and utilized by the people concerned if they are going to be able to apply for work, bid on jobs, provide materials and services to construction firms, participate meaningfully in EIS and other government agency processes which may fatefully affect their community; and so on. Example Three: The locals have long prided themselves on their ability to help newcomers learn how to "fit in" to the community so as to put down acceptable social and cultural roots and become functioning members of the community. What worries the established residents as much as anything is how to fit the relatively "permanent" newcomers into the community when they come in droves. Such "people pollution," a significant social problem in and of itself, triggers other important social problems. Thus, a reaction of the locals to a large influx of new residents is to tighten their established social circles as a means of retaining an acceptable sense of community, reaffirming their sense of place in the community, assuring that the core of their way of life will remain relatively insulated from the influences of the hordes of newcomers, and so on. Upon becoming aware that it is extremely
difficult to break into these informal gatherings and communication networks, newcomers tend to perceive that the long-timers are "clannish" or "unfriendly." The situation is ripe for we-they relationships of the kind that few of all concerned really want and none knows how to avoid. These examples shuld suffice to indicate that much SIA material that is of enormous sociological interest and relevance is left out of account in the authors' conception of social problems and that their evaluation of how SIA researchers have been handling and assessing social problems is inaccurate and incomplete and thus a misrepresentation of the facts of the matter.

Wilkinson et al.'s insistence on branding solid SIA findings as "assumptions" and "hearsay evidence" begins in the first section and continues through the remainder of the article. Consequently, my remarks will now also pertain to the ensuing sections.

Wilkinson et al. have evidently looked at the world and concluded that it must be flat. They document this conclusion with certain numerical indices and with historical essays prepared by like-minded scholars. Accordingly, they feel justified classifying as "hearsay evidence" the reports of those who say they have been around it and especially the findings of those who claim to have photographed it from outer space. By this I mean that the authors' curious views on what is and what is not sociologically and scientifically acceptable documentation of reported findings have to be analyzed if we are to get to the heart of the problems they evidently have with the findings of contemporary SIAs of industrially impacted rural communities in the West.

The authors' main hangup is that they are acting in the manner of office-bound observers of the human scene whose methodological blinders permit them to see, trust, and accept only written numerical data on the contemporary community, along with certain supporting historical reports. Their methodological ideology renders them unable to bring themselves to look at this scene up close or to actually take seriously into account the findings of sociological observers who do not share this hangup, appropriately footnoted, polite gestures to the contrary notwithstanding. Their de facto commitment to
this particular brand of positivism is so sectarian that they are ever watchful of opportunities to abjure the devilries of modern SIA work. Thus, they try not to miss an opportunity to point out ideological deficiencies in such work by making pious use of such incontestable and argument-clinching adjectives as "scientific" and "objective": "Thus we do not know, on the basis of scientific evidence, whether energy development leads to..." and "Science requires an intimate knowledge of the subject matter [which Wilkinson et al. patently do not have!], a systematic way of thinking about the subject and an objective procedure for examining evidence"—matters in which the cited SIA researchers are allegedly deficient. One reason for not venturing close to the empirical social world they purport to study sociologically is that they fear that doing so would get them into intensive interaction with the people whose world it is, in which case, God knows, there would be no method of avoiding biasing and otherwise contaminating the data. Another reason for acting in the manner of the office-bound sociologist is that up-close investigations of the human scene require the kind of accountability to informants that they eschew, given their belief in a methodology of personal and professional detachment from the research scene and given that their primary accountabilities are to their clients and colleagues. Yet another reason is that sociological fieldwork leads to the production of many qualitative findings—which they categorically reject as "subjective." Their underlying distrust of the ability of people whose scene the boomtown is to make good analytic sense out of it is also a key reason why they studiously avoid doing anything smacking of competent ethnographic research. Their chosen dependence on statistical measures to portray this scene enables them to study only what they can quantify or what secondary data, such as crime statistics, are already quantified. One of their mottos appears to be: If it isn't quantified and on paper, folks, forget it. This approach to studying, and reviewing the study of, human communities reduces them to studying infrastructure much more than structure, the subsocial much more than the social, and indices of human behavior rather than the actual behavior itself; and it
requires them to make assumptions and inferences about culture and social structure that they are unable to check in any direct, empirically grounded, scientifically sensible manner. What I find totally incomprehensible and deeply upsetting about all this is that it is not only produces grossly erroneous and misleading findings and analyses of the findings of others but also gives the uninformed and/or unsophisticated reader the impression that he or she is looking at "hard," "objective," "scientific" data when, in fact, the authors' paper is nothing more than an exercise in rank "scientism"—a kind of flimflammary which uses the trappings of science to produce what is known in the trade as bullshit data—data which, alas!, are simply not faithful, accurate, fair, or credible portrayals of the human scene in question.

Wilkinson et al.'s article is, as intimated, full of errors of omission and commission on which I would like to comment, but space remains for discussing just one of them: "Conflicts among local groupings have persisted over many years in some communities, and the claim that some ranchers are 'coming back to gemeinschaft' in defensive response to energy development (Gold, 1974: 7-8) contradicts the assumption that an integrated rural society existed before the recent development." This sentence contains a nonsequitur. The "back to gemeinschaft" tendency noted in the cited paper clearly referred to as a recurrence of this form of community in the vicinity of Colstrip, Montana, and in no way contradicts what I reported being told by residents of the area about the good old days of stronger rural ties. Indeed, this reference supports the conclusion that a revitalization of Gemeinschaft was occurring in the cited situation. My basic mistake was in not realizing how persistent Gemeinschaft is even when it appears in process of being overtaken by a looser form of organization. This actual, empirically documented persistence also refutes what the authors say in the very next sentence about "the" historical evidence supporting their assertion that such so-called rural areas as the vicinity of Colstrip, Montana, have really had an urban form of social organization all along. This totally incorrect assertion should be tried out on the cowboys and other residents of such
areas, and on the sociologists who have studied them in depth, to see how far it gets with anyone who has intimate familiarity with the actual form of organization there.

To conclude my comments on Wilkinson et al.'s paper, I would like to point out to them how modern SIA research on energy and other natural resource development projects in the West actually is "informed substantially by hearsay evidence," but in ways evidently beyond the ken of those wearing the methodological and epistemological blinders of office-bound sociological research. I would like, in this way, to offer some fatherly advice about how they might begin to shed their blinders and mend their errant ways.

Any sociological fieldworker worth his or her salt generates "hearsay evidence" routinely in the course of cultivating a running interaction between making inferences and checking them with informants, the people who are "in the know" about what he or she is inferring. Studying human behavior up close, in the manner of the competent ethnographer, enables the sociological researcher to transform hunches, rumors, insights, guesses—information of all kinds—into empirically grounded, scrupulously validated, sensibly and fairly portrayed data, the very hardest data that sociologists are presently capable of producing. Don't fool with that bullshit data, fellows. Get out there and develop relationships of mutual trust and respect with other people in boomtowns, especially those who are up to their ears in value-related and other sociologically significant social problems, and find out what the people whose scene it is have to report about what is going on and what it all means to them and their community. Make an intelligent effort to study community structure directly, in depth, and over time, instead of preoccupying yourselves with such hobgoblins as divorce statistics, which, by their very nature, tell precious little about impingements of natural resource development projects on the structure and functioning of the communities in question. To learn about the latter, there simply is no substitute for asking community members to inform you about what's happening, trying to make both folk and sociological sense out of this
information, and then validating and reporting it as described above. Then, and only then, would you start to become qualified and prepared to undertake a literature review of this kind in a way which would merit the serious attention of both SIA specialists and other competent sociological researchers.

As your article now stands, it is at best a source of sociological mischievousness and at worst a highly misleading report on the work of some first-rate SIA researchers. At worst, it also provides a lot of grist for the public relations mills of the unprogressive, unenlightened, and rapacious natural resource development firms of the world that rely on PR bullshit to try to persuade the uninformed, the unsophisticated, and the gullible that their industrial trespasses create no social problems, because they are really making positive and commendatory contributions to the quality of life of the lucky small-town recipients of the manifold blessings of their incursions.

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COMMENTARY

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The article by Wilkinson et al. points out what they believe to be numerous deficiencies in the analysis of the social impacts of western energy development, makes a plea for better scholarship in such analysis, and suggests that what is needed to upgrade the analysis of the social impacts of western energy developments is the careful use of the best of the theoretical and empirical traditions of the social sciences. There is much general merit to the authors' arguments. They are correct in noting that much of the literature on the impacts of western energy development is based on sparse evidence and that a better application of social science theory and methods is essential to the overall improvement of the field of energy impact assessment. However, the authors' careless scholarship, their generalization of a critique applicable to only one area in a very broad field of study as a means of condemning the entire field of study, and the simple fact that what they have said is not new at all suggests that their own analysis is an unfortunate example of the very malaise they decry.

One of the major problems that pervades the paper is that the authors' charges about the quality of other researchers' scholarship is often based on even more questionable scholarly practices. Because other authors similarly treated have also been asked to respond to the article, we shall address this issue only in relation to our own work.

Our work (Murdock and Leistritz, 1979), from which several excerpts are cited by Wilkinson et al., is a relatively comprehensive overview of the socioeconomic impact literature as of the end of 1978. It is a work 363 pages in length which
contains 12 chapters. Although the authors denote (in footnote 3) that given the size of our effort, "inadequate documentation of one point should not be taken as a major flaw in scholarship," they nevertheless contend that "the sources which they cite for these statements fail to uncover any confirmed evidence of the occurrences to which they refer"; "Thus documentation of the conclusion is not found in any of these [four] sources"; and "Serious flaws in scholarship are apparent in the treatment of sources and in interpretations of data."

Two points must be made concerning these charges. First, the two quotes (from pages 251 and 252 of Murdock and Leistritz, 1979) are in fact surrounded by a context of qualifications and admonitions to the reader that the data available on such topics must be used with extreme caution and form but a small part of a chapter covering a wide range of social (not just deviance-related) dimensions. Thus throughout the chapter from which the quotes are taken, we caution:

There is a lack of data on the actual social impacts of energy developments, and the analysis of social impacts is one of the most neglected areas related to the impacts of energy developments on rural areas in the West [1979: 246].

The general literature on social impacts includes a voluminous amount of literature, including such general methodological works as those of Finsterbusch and Wolf, Wolf, Fitzsimmons, Stuart, and Wolff, and other works. . . . Literature specifically on the social impacts of energy development in the West is much more limited [1979: 248].

And in the section from which the quote on divorce is taken, we note: "The effects on social institutions are even less well-known" (p. 251).

Recognition of the limited data in this area led us to devote only limited attention to the social disruption issues. The majority of the analysis presented in the chapter was on such social dimensions as attitudes toward and perceptions of energy development. The sections from which Wilkinson et al. quote are
from two pages in a 36-page chapter, and the analysis in these two pages includes not only a discussion of marital and social disruptions but also effects on leadership patterns, religious, governmental, and other institutions, and effects on the general quality of life in impacted areas. The impression left by the authors that social disruption is a major focus of the effort is misleading.

A second and even more serious problem related to the level of scholarship in the Wilkinson et al. effort is their tendency to criticize through the use of quoted excerpts that, when read in their proper context, connote a conclusion just opposite to that implied by Wilkinson et al. Thus, in denoting our inaccuracy in data interpretation, they quote from McDonald (1976) that estimates of deviance were "too far apart for credibility," which suggests that McDonald had concluded that social disruption had not occurred. In fact, throughout his analysis, McDonald noted that despite the lack of quantitative data, the presence of increased levels of deviance was apparent. When the entire paragraph from which the McDonald quote is taken is examined, the misinformation conveyed by the selective quote used by Wilkinson et al. is all too apparent. Thus, McDonald noted:

> Whatever the exact increase of crime was in Cavalier County in general and Langdon more specifically, it seems reasonable to assume that the impact was both acute and immediate. The range of a 75% increase by Chief Wentz to a 300% estimated increase by Sheriff Haaven seems, at least apparently, to be too far apart for credibility. Yet, given separate jurisdictions, along with the fact that Sheriff Haaven was responsible for eleven towns via contract policing, the respective estimates with their accompanying variances may be highly indicative, while not precisely accurate, of the changing conditions faced by the law enforcement personnel of Cavalier County [McDonald, 1976: 64-65].

Clearly McDonald's overall conclusion was that a quantitative estimate of the level of increased crime could not be given but that there was general evidence (unless the authors consider only numerical rates to be evidence) of increased deviance.
Yet an additional example of the authors’ tendency to use items out of context and thus mislead the reader is their charge:

Murdock and Leistritz conclude their review of the literature on disruptive social effects of western energy development with the observation that “the available information is sufficient to validate the levels of concern often expressed regarding such impacts” (1979: 273). The present review of cited evidence, in contrast, finds no acceptable basis for such a conclusion as it refers to “a general increase in crime, drug abuse, mental illness, child abuse and related problems” (Murdock and Leistritz, 1979: 252). Assumptions in this literature are informed substantially by hearsay evidence. Serious flaws in scholarship are apparent in the treatment of sources and in interpretations of data.

The implication that we concluded that the available evidence about social disruptions was sufficient is incorrect. When the entire text from which the quote from page 273 is presented, it shows that far from suggesting that available evidence was sufficient, we were, in fact, noting the inadequacy of existing data and the need for additional research efforts. Thus, in total we said:

The general discussion in the earlier part of the chapter suggests numerous factors not apparent in the data presented and impacts with varying degrees of speculative and empirical support. The strong adherence to the conservation ethic in the West, as well as the positive perspectives that rural people in the region have of their communities, is widely supported. On the other hand, we know little about the actual impacts on the elderly or on Native American populations or about the impact of developments on interactions between newcomers and longtime residents. Clearly, there is sometimes conflict, but how common or how dissimilar it is from conflict in any other area receiving in-migrants is unclear. There is little to tell us about the long-term effects of any of the factors, simply because recent energy developments in the West involve the use of technologies within a new context and are thus not comparable to events in the past. We do not know whether the implications of developments in Illinois or Appalachia can be applied to the West, or if so, in what respects the generalizations would be appropriate. Overall, then, our general suppositions
about energy development exceed our knowledge base and will only be clarified by additional analyses and research in impacted areas.

Perhaps the best overall generalization that can be drawn about the impacts of energy development on the social organization, values, attitudes, and perceptions of residents in rural areas in the West is that the available information is sufficient to validate the levels of concern often expressed regarding such impacts. There is little doubt that such impacts are likely to be severe and among the most negative experienced by impact area residents. Except for such general knowledge, however, it is necessary to conclude that our knowledge is limited. That additional research is critical is a much used, but clearly the most applicable, overall conclusion in this area of impact analysis [1979: 273].

Although the authors provide footnotes that qualify their charges, good scholarship requires that quotes be used accurately to convey the authors’ intent and not be used in a convenient manner to create a "strawman" for critical rhetoric.

A third and final point concerning the level of scholarship used in the paper is that the authors ignore or are unaware of considerable evidence that would at least in part have supported their contention that energy development does not necessarily lead to increased deviance. Thus, recently completed longitudinal case studies by Gilmore et al. (1981), Murdock et al. (1981) and Freudenburg (1980a, 1980b) tend to find that some selected forms of deviance increase during development. Although other analyses, such as those of Lillydahl et al. (1982), provide contradictory evidence, the authors seem to be unaware of the considerable progress the field has made in the completion of longitudinal analyses of forms of social disruption.

In sum, then, in the process of condemning the level of scholarship in the area of social impact studies, Wilkinson et al.’s own scholarship tends to weaken an argument that, in terms of its stress on the need for additional research, may indeed be quite valid. A thorough, knowledgeable, and appropriately documented critique of the literature may have allowed the authors to make several of the same points without the need to create false impressions about the scholarship of other researchers.
A second major problem in the article is that the authors have chosen to examine critically the state of the elephant by seizing only its tail. That is, they provide a condemnation of the analysis of the social impacts of western energy development based only on evidence from the area of social deviance. This area is perhaps the poorest documented area in social impact analysis (Murdock and Leistritz, 1979). Although the authors are correct in stating that, as a whole, the analysis of western energy development is relatively new and underdeveloped, had they selected other areas of social analysis (such as attitudes toward energy development) or looked at the broader context of socioeconomic impact analysis (and thus examined economic, demographic, public service, and fiscal dimensions as well), they would have found empirical evidence accumulating rapidly (Denver Research Institute, 1979; Leistritz and Murdock, 1981; Finsterbusch and Wolf, 1981). The state of the art, though still in a fledgling stage, is less underdeveloped than the authors' statements suggest. In choosing to attack one of the weakest links in the field, they have been as selective in their criticism of the research area of social assessment as they are of many of its researchers.

A third problem with the Wilkinson et al. piece is that its suggestions for improving the field are not new ideas. The authors fail to cite social assessment literature that has preceded them and made essentially the same arguments. Other impact researchers and ourselves (Murdock et al., 1976; Cortese and Jones, 1977; Albrecht, 1978; Freudenburg, 1978, 1982; Murdock and Leistritz, 1979; Leistritz and Murdock, 1981; Finsterbusch and Wolf, 1977, 1981) have noted the following:

The approaches used in analyzing social impacts reflect the lack of attention paid to these impacts. Despite the extensive attention given to the discussion of the issues and ethics of impact analysis (Finsterbusch and Wolf, 1977), the study of social impacts is largely lacking in the development of sound theoretical approaches for analyzing energy-related impacts. Except for a few attempts either to analyze such impacts as part of a general process of urbanization and modernization, which are thus largely a reflection of classical sociological theory (Carnes and Friesma,
1974); as a part of the great change involving the integration of rural structures into urban structures (Warren, 1977); as a part of the general environmental problems (Catton and Dunlap, 1978); as a possible area for the extension of human ecological theory (Murdock, 1979; Olsen et al., 1977), there has been little theoretical development and even less application of theoretical bases to empirical analysis. As a result, many of the projected social impacts in impact assessments involve little more than contentions that communities will move farther toward the urban end of the rural-urban continuum. As Hassinger notes, however, such contentions concerning rural societies often fail to take into account the fact that rural areas are no longer isolated, nonintegrated areas of American society but have long been moving toward increased integration (Hassinger, 1978). The analysis of social impacts is in critical need of innovative and extensive theoretical development [Murdock and Leistritz, 1979: 247].

Leistritz and Murdock add:

A review of current impact assessment practices reveals a need for additional conceptual and analytical refinement within each major impact assessment category. The need for improved conceptualization and refined analytical approaches is even more apparent when the integration of various impact dimensions is considered. If such conceptual and analytical refinements are to be achieved, it is essential to increase the levels of interest among social scientists in pursuing impact research as a legitimate research area with scientific merit. If the impact assessment research process is viewed as pursuing questions tangential to the major interests of the various disciplines, social scientists will be reluctant to make substantial and continuing commitments to this area of research [Leistritz and Murdock, 1981: 252-253].

In addition, the authors’ call for the use of new and dynamic community-based theories to analyze the social impacts of energy development represents neither a new approach nor a new suggestion (Murdock, 1979; Murdock and Schriner, 1978, 1979; Cortese and Jones, 1977).

In sum, Wilkinson et al. provide a set of assertions derived by selectively quoting western energy impact literature out of
context, condemn the whole of the western impact literature from an examination of but a small part of it, and fail to provide new insights into the conceptual or empirical needs of the area. Rather than providing a step toward the solution of the problems identified the article provides an unfortunate example of the errors of omission, of ideological bias, and of the failure to use the best of the social science tradition that they adamantly condemn. With Wilkinson et al.'s assertion that such practices are unfortunate and retard the development of conceptually and empirically adequate analyses of the social impacts of western energy development we fully concur.

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