habits; he calls them simply "blaming the victim." For example, some data show that poor people are more likely than the nonpoor to say that they work mainly for money. According to some sociologists, this shows lack of commitment to the Protestant work ethic and explains why the poor seldom become (future-oriented) nonpoor. One could suggest, however, that the data reflect basic differences in the economic situations of the poor and nonpoor rather than value differences. The available evidence, in fact, does not show that the poor lack achievement motivation.

It is difficult, however, to recognize our pejorative labels and the underlying hypotheses. A clarifying exercise is to take a "trait," reassign it to the "good" group, and change the label. For example, take "illegitimacy." Assume that young unwed mothers on welfare are most often from the middle rather than the lower class. Assume also that an unwed mother who keeps her baby and goes on welfare shows admirable child-orientation, self -direction, and independence from her family of origin. In contrast, then, an unwed pregnant woman who has an abortion, gives up her baby for adoption, or enters a forced marriage with an unloved or high risk partner is exhibiting dependency and excessive conformity to social pressure. And finally, rename illegitimacy, "the free birth rate." Development of this line of thinking would allow us, among other things, to reconsider the assumption that unwed motherhood is difficult and wedded motherhood is bliss.

On a more scholarly level, I would propose that poverty researchers at least see what happens if they abandon the umbrella deficit hypothesis that says data showing the poor to differ from the nonpoor also show deficiencies among the poor. Such an exercise might well reveal some hidden theoretical biases. (See Cole and Bruner, 1971, for directions on how to take this cold bath.

Noncorrespondence between theoretical and operational variables. Another problem which is often overlooked by time-pressured reviewers is noncorrespondence between what researchers say they are measuring and what they are actually measuring. For example, Goodwin (1972), in an otherwise excellent study of work motivation, found a related set of questions which he dubbed "lack of confidence in ability to succeed in the work world." Four items concerned lack of commitment to work (e.g., work for money, just to make a living) and four suggested that to get ahead you have to be lucky and likable. Goodwin reasoned that emphasis on money indicates a lack of confidence about one's earning ability while emphasis on luck indicates uncertainty about the effectiveness of effort. Therefore, he concluded, the whole cluster of items reflected confidence in ability to succeed in the work world. However, he had no direct evidence for this assumption. Since the cluster so clearly consisted of two distinct themes, neither of which directly concerned confidence, the finding that the items formed a cluster is most safely interpreted as an artifact Unless the same relationship turned up in other samples or with other similar items, Goodwin's reasoning should be viewed as speculation. (However, it has not been and his "findings" about lack of confidence among poor young men are widely-cited.)

Reiss (1975) pointed out a particularly serious version of this problem which he saw in delinquency research: the use of prevalence data to answer questions concerning incidence. A variant can be found in the poverty literature: The prevalence of welfare experiences is high among the poor. However, only a small proportion of this population-at-risk is on welfare at any one time and only a tiny proportion have been on welfare more than once or for long time If one is interested, say, in the causes of welfare dependency, one should study the small group with a high incidence of welfare experiences (or who have spent much time on welfare) rather than the larger group who have been or are on welfare at some time.

<u>Too simple models and uncritical variables.</u> Whatever area of social scientific inquiry a reviewer is concerned with, one is bound to end up calling for better analysis and theory. It <u>is</u>

useful as a caveat emptor warning, however, to cite some specific examples of problems with theory in the poverty literature.

First, as an example of inadequate analysis, take the Moynihan hypothesis about the breakdown of the Black family. Moynihan (1965) made a statistical finding about the growth rate of Black female-headed families into a hypothesis concerning general pathology in Black culture. Farley (1971) took the data and with further analysis showed that the growth of Black female-headed families was not associated with a decline in Black family stability. Instead there were more unwed mothers establishing their own households rather than living in others' households, over time. The data do not reflect family decay but an increasing autonomy among Black unwed mothers. This would be evidence of family pathology only if one assumed that it is most healthy for unwed mothers to live with their own parents and not in a separate residence

An example of too-simple models is provided by Blum and Rossi (1968) in their discussion of the relevance of socioeconomic variables to poverty. Correlations between class and a myriad of variables tend to be reliable and easy to demonstrate. It is tempting to build a theory around such findings. However, socioeconomic variables are seldom the critical variables. They are reliably related to the dependent variables of interest most often through their reliability as indices of underlying causative variables-which tend to be more difficult to measure. As Blum and Rossi. conclude, "We,. know a lot about what the differences are among socioeconomic groups, but very little about <u>why</u> such differences exist" (P. 349).

A related problem is inadequate consideration of <u>both</u> personal and situational factors as causes of behavior. For example, consider the chronically unemployed man who seems not to search for work. On one hand he may want to work but knows that available jobs are beyond his strength or skill or otherwise impossible to take (situational constraints). On the other hand, he may be basically lazy. Perhaps it's fortunate for him that jobs are hard to get, or perhaps not. Careful consideration both his attitudes toward work and the job market and the interaction of such factors is necessary if we want to be sure we really understand the situation.

Furthermore, if we do infer attitudes to be important causes of behavior, we must take care to infer the appropriate attitudes. For example, a man's willingness to move to another town to get work has been taken as evidence of job flexibility and thus of positive attitudes toward work. However, willingness to move hasn't proved to be a very good measure of job flexibility (Wright, 1975). It seems more likely that willingness to move reflects the strength of friendship and kinship ties.

10. Problems created by the structure of academic disciplines and the road to academic success. Many of the problems described above are compounded by certain characteristics of the scholarly trade. First, the volume of social science reports makes it impossible for a single reviewer to examine carefully all the literature in any but the narrowest areas. In most social science fields, articles are accompanied by an abstract, which may be all that most consumers read. Abstracts vary in specificity; some may list basic research details while others list only interpretive conclusions. Either way, problematic aspects of the research design and findings may not be mentioned. For example, a small but statistically significant difference may be reported in the abstract and a hapless, time-bound reviewer who just needs to show awareness of others' research in a write-up of his or her own research will miss the fact that the difference is meaningless. The next researcher in the area may rely on the secondary report, and in this fashion a limited or unreliable finding can mushroom into something close to a sociological law. (See Hedley and Taveggia, 1977, and Macaulay, 1979, for examples of this process in the areas of job satisfaction and jury studies, respectively.)

Furthermore, findings may not only mushroom in significance, they sometimes get turned inside out. Berkowitz (1971) and Yarrow et al. (1968) describe what is generally a problem when relying on secondary sources, the probability that a finding has been refashioned through leveling and sharpening as it is restated by successive citers. Berkowitz's article concerns the odyssey of a minor finding from an early piece of research through various permutations to its final home in textbooks where it is cited for a thesis that it does not support, much as a rumor is made more understandable or congruent with expectation, as it spreads through a group. Once the final polished product enters the conventional wisdom it is very difficult to see that the data base needs reexamining and that perhaps a whole consensus of opinion is false.

Second, differences are more rewarding for a scholar than similarities in several ways. Only statistically significant differences are usually publishable. A scholar's promotion depends on publishing, and so one must seek such differences in order to advance professionally. Thus repeated findings of similarity where differences are sought, or failure to find differences where others did, seldom make it into print while one aberrant finding of a statistically significant difference tends to get published and to become the only known evidence regarding some question. (See Barber, 1976 for a thorough description of this problem.)

Also, the chance to work up publishable theory is greater with differences than with similarities and it seems that reports that highlight differences are intrinsically more interesting than report*-highlighting similarities — unless one is involved in a debunking effort. In the latter case the results may be hard to publish because the journal editors and their colleagues constitute the old guard being attacked, and they get defensive.

This equation of statistical significance with publishability and theory development has wide-reaching and long-lasting pernicious effects on the development of both theory and policy. (See Walster and Cleary, 1970, and Walster and Tretter, 1974, for constructive discussions of this problem. It is on the basis of this that we might designate as major heroes in the poverty literature the authors of the Michigan panel study of income dynamics (Morgan et al., 1974) They comment on the "significance" problem as follows:

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> " The capacity of the human mind to find regularities, focus on the unusual, and combine things is such that there is great danger of pouncing on findings that 'fit.' The reader should be warned that in spite of everything, negative conclusions are more trustworthy than positive ones. If we are unable to find any evidence that a certain variable matters, . then in the absence of serious measurement problems it is likely that it does not matter. But if we find an intriguing relationship for which we can elaborate a neat theory, the possibility remains that it is a chance finding (Vol. 1, p. 8)

Finally, the isolation of individual disciplines results in disciplinary parochialism. The economist relies on naive, intuitive psychological assumptions and the psychologist relies on naive, intuitive economic assumptions.

There are few rewards for doing otherwise. In order to achieve academic tenure one must impress one's colleagues within one's discipline. The economist's naive psychology constitutes the conventional wisdom of his or her senior colleagues and the acquisition of psychological expertise might lead to questioning,, this wisdom hardly the thing to do when those senior colleagues hold your career in their hands. Interdisciplinary effort is, applauded in the abstract but in reality it tends to draw some' scholars away from what seems to be their own discipline's mainstream. A. wise junior scholar will realize this. (See, for example, advice given to tenure aspirants by Nitsche, 1978.) Furthermore, the geographical separation of disciplines on a campus makes it unlikely that scholars working on the same problem in different disciplines will discover each other. In short there are a variety of factors that converge on the maintenance of pockets of ignorance within disciplines and nonsharing of insights and information between disciplines. The lesson of this for the consumer of social science research is to be wary of even the most prestigious scientists when they step beyond the boundaries of their own discipline.