Separate and Unequal Is Better

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Some argue that the assumptions of strain, control, and differential association theories of delinquency are fundamentally incompatible (Hirschi, 1969; Kornhauser, 1978), while others deny that they are necessarily incompatible and suggest the possibility of a compromise theory satisfactory to all concerned. For sheer reasonableness, the integrationist approach would seem to have much to commend it: Why should we continue to squabble over petty differences when, with a little concession here and a minor modification there, the larger truth we all love so well would be better served?[1] As a matter of fact, however, integration turns out to be more difficult than this question suggests. When Elliott, Ageton, and Canter (1979) actually face the Solomonic task of resolving the conflicting claims of the three perspectives, they find themselves compelled to agree that it cannot be done. Their solution is to use the terms and ignore the claims of control theory. This allows them to divide the child in two, giving the larger half to differential association and the remainder to strain theory.

Since it seems late to be inquiring after the child's health, it may be more useful here to look briefly at the mediating devices available to those who would resolve the conflicts among these traditional perspectives. For all their popular appeal, the repeated failure of integrationist approaches suggests that there are inherent difficulties that preclude attainment of their avowed goal.

END TO END

One procedure open to the integrationist is to put partial theories end to end so that they describe a developmental sequence. The dependent variables of prior theories become the independent variables of subsequent theories. An overall increase in the ability to account for the final dependent variable is not expected because the last theory in the sequence presumably absorbs the predictive power of those preceding it. In fact, given the reasonable notion that the power and complexity of a theory should somehow be proportioned to what it explains, sequential integrations will normally explain less than the sum of things explained by their constituent theories. In other words, sequential inte-

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1. Students in schools of criminal justice, where the boundaries between disciplines are often obscure, are easily irritated by a focus on differences among theories within disciplines. If all disciplines are "really saying the same thing," within-discipline differences are obviously too petty to merit attention.

grations presuppose important outcomes (e.g., sustained patterns of delinquent behavior) and are inappropriate as explanations of trivial events (e.g., crossing a neighbor's yard without permission). In sequential integrations that cross disciplinary lines, questions about which partial theory is most important will usually seem to be off the point. In others, where each of the constituent theories was originally advanced as a direct explanation of the outcome variables, integrationists face a formidable task: They must argue in effect that all theories but the last one in the sequence are wrong. Unless based on evidence not previously available, this argument will undercut the integrationists' claims to impartiality, since they will have become parties to the dispute they proposed to mediate.

This abstract description of the end-to-end strategy is of course partly based on the discussion by Elliott et al. (But not entirely. For roughly parallel integrations of the same perspectives—and some of the same features—see Lofland and Stark, 1965; Farrell and Swigert, 1978.) To my mind, it illustrates that Elliott et al. have on the whole been true to the logic of their procedure. It also illustrates that this procedure cannot resolve differences among the theories in question. Those whose minds are closed to the idea that, for example, "access to and involvement in delinquent learning and performance structures is a necessary . . . variable in the etiology of delinquent behavior" are unlikely to be persuaded by the argument that this "postulate" is a necessary consequence of an open-minded procedure.

SIDE BY SIDE

Another procedure is to put partial theories side by side and segregate the cases to which they are considered applicable (Warren and Hindelang, 1979). This increases explained variance, avoids questions of incompatibility, and answers the question of relative importance. (Other things being equal, a theory that applies to 40 percent of the cases is "better" than one applying to 30 percent.) Since the side-by-side approach avoids the theory overload problem, it is useful in explaining delinquent acts differing widely in significance.

Being all virtue and no apparent defect, the side-by-side "integration" would seem to be the way to go. The reluctance of Elliott et al. to exploit this strategy fully seems to result from the fact that in the side-by-side approach the definition of delinquency is unrestricted. This approach does not allow a single definition tailored to the needs of a particular theory. Instead, it leaves each subtheory free to define delinquency in its own terms. Having constructed their definition with a causal sequence and differential association in mind, Elliott et al. cannot reopen the definitional question without reopening the question of the adequacy of their integrated theory. (A pure side-by-side approach would require consideration of control theory as a possible explanation of at least some forms of delinquent behavior.)

The major difficulty inherent in the side-by-side approach, as far as I can

determine, is that no one has as yet come up with a way of segregating cases that produces the results suggested. (The notoriously unsuccessful social-class-specific approach to theory is a good example of the side-by-side procedure in operation.) Elliott et al. segregate cases on the basis of the strength of initial "bonds to the conventional order." Those with formerly strong bonds are said to follow a path to delinquency different from the path followed by those who have never developed such bonds.[2] As is usually true with side-by-side integrations, procedures for identifying the two groups are not provided. Furthermore, evidence for the existence of the strain theory path is exceedingly vague. Put another way, I interpret the Elliott et al. side-by-side effort as a bow in the direction of "the most influential and widely used contemporary formulation in the sociology of delinquent behavior," a formulation whose continued influence and wide use are something of a mystery, to say the least.[3]

UP AND DOWN

Integrationists may also raise the level of abstraction to the point where these partial theories become specific applications of a general theory of deviance—or law (e.g., Black, 1976). Such efforts should lead to greater explained variance because what were once considered unrelated processes may now be seen to bear on the same outcome. This procedure too has the defects of its virtues: a marked tendency on the part of integrationists to accept without question the truth of any partial theory their general theory subsumes. (The greater the number of partial theories accounted for, the greater and more powerful the general theory.) What appears to be unusual reasonableness on the part of the integrationist may then turn out to be nothing more than failure to invoke required scientific bases of discrimination.[4]

2. Elliott et al. occasionally mention that strain theory requires a repeated-measures design, while research from within the control perspective has been cross-sectional or "static." I do not find their equation of *superior research design* and *superior theory* convincing. A repeated-measures design may indeed allow the researcher to examine the effects of prior states, but it does not guarantee that such effects exist. In fact, given the empirical record, I would expect the strain theorist to be extremely leery of the hypothesis that prior and subsequent states interact in their effects on delinquency.

3. "We fail to find any support for Cloward and Ohlin's disjunction hypothesis" (Elliott and Voss, 1974:170). "There is little or no support for the hypothesis that a tendency to attribute blame externally . . . increases the likelihood of delinquency . . ." (Elliott and Voss, 1974:170). "Strain theories have a decided defect: they are not consistent with the evidence" (Hirschi, 1969:228). "On the basis of available evidence, Cloward and Ohlin are wrong" (Hirschi and Hindelang, 1977:578). "There are no delinquent gangs of the types described by Cloward and Ohlin" (Kornhauser, 1978:159).

4. In the empirical ("down") equivalent of this procedure, the integrationist simply combines variables from various theories on the grounds that some may explain cases left unexplained by the others. This is the "multiple regression" approach, where predictability is increased by inclusion of variables unrelated to variables already considered. In most applications of this procedure, unfortunately, the presumption of orthogonality among independent variables owes much of its plausibility to the fact that it has not yet been tested.

To some extent, all of these integrative procedures are employed by Elliott et al. As can be seen from their general properties, the life of the integrationist is not easy. He or she is forced to make theoretical decisions (and to abide by their consequences) unguided by a consistent theoretical perspective. If, in desperation, one or another theoretical perspective is adopted, claims to even-handed treatment of all perspectives are no longer tenable.

I think we should be pleased to find that attainment of the integrationist's goal is so difficult. A "successful" integration would destroy the healthy competition among ideas that has made the field of delinquency one of the most interesting and exciting fields in sociology for some time. To their credit, Elliott et al. eventually abandon the integrationist perspective in favor of the theory of differential association. To my mind, differential association is at least of some historical interest.[5] I would not be able to speak so highly of an integrated theory of delinquency.

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5. In his 1939 statement of differential association, Edwin Sutherland limited his dependent variables to systematic crimes in order "to postpone consideration of the very trivial criminal acts" (Cohen et al., 1956:21). In their 1979 statement of essentially the same theory, Elliott et al. restrict their dependent variable to "sustained patterns of illegal behavior" because they "are not concerned . . . with the isolated delinquent act."

By 1947 Sutherland had abandoned "systematic" because a psychiatrist could find no more than 2 systematic criminals among the 2,000 inmates of Indiana State Prison and because his own students found it "most difficult . . . to determine objectively whether a prisoner was a criminal systematically or adventitiously" (Cohen et al., 1956:21). I suspect that in the next go around Elliott and his colleagues will reach similar conclusions about "sustained patterns of illegal behavior." They will see that it eliminates too much of interest and is too difficult (impossible?) to operationalize to justify its retention in a theory of delinquency. (I suspect too that they will see that this definition of delinquency reopens the question of the theoretical relevance of self-report results, since it has not been the definition employed in self-report research.)

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