Reconciling Piaget with structural learning

The main purpose of our book Structural Learning and Concrete Operations: An Approach to Piagetian Conservation (1960) was to help reconcile Piagetian concepts with American operationalism. Although Christiane Gilliéron (Geneva) and Stephanie Thorton (British) appear to recognize clearly the contributions made toward this end, Frank Murray (in his review; CP, 1982, 27, 462-463) appears to lack this recognition. Moreover, several crucial points are either overlooked by Murray or only grudgingly acknowledged. In the latter regard, for example, Murray mentions that we have provided "a more detailed account of the comparison rules [than that by Piaget] . . . that may figure in the child's reasoning" (p. 463). This acknowledgment loses much of its weight, however, coming at the end of a review that suggests that we are simply adding another entry to the long list of Piagetian training studies. In this regard, Murray puts us at once both in the American camp, in that (according to Murray) Genevans are not likely to be impressed with our adding anything new, and not in the American camp, because we are engaging in a monologue with ourselves.

Now it may be that Murray has missed much of what we were trying to say, and in that sense we were having a monologue. But his comments seriously distort both facts and our intent. The small kernel of truth in what Murray says is that, as Gilliéron (1982) puts it, "the authors are putting themselves in a difficult position. They are admitting the relevance of Piagetian concepts . . . , being thus open to American criticism. In addition, they have [treated] conservation tasks as
problems, thus [making themselves] liable to Genevan attacks” (p. 167). She adds, “The very attempt of treating Genevan theory operationally is demanding . . . the [Scanduras have made] a substantial contribution to an old debate” (p. 167).

Thornton (1982) agrees that “the contribution . . . [made by the Scanduras] is real. This is not just another training study. Rather, it is a substantive analysis of a domain of conservation which goes some way towards bridging the gap between Piagetian insights and the Anglo-Saxon demand for an empirically operationalized approach” (p. 164; italics added).

Among the more important points that Murray missed are the following. He implies (apparently falsely, judging from Gilliéron’s review) that Genevan would not be impressed because we provide students with solution rules but with no means for deriving them. Nor, according to Murray, do we explain how higher order rules are acquired. Murray is wrong on both counts. The higher order rules identified reflect this derivational ability precisely, and their roots in even more basic reasoning skills (represented as simpler and more fundamental higher order rules) were fully explicated. Moreover, after training on these rules, our students’ reasoning on conservation tasks could not be distinguished from those of natural conservers.

Murray completely overlooks what should have “popped out” as perhaps the single most important empirical result. As Thornton (1982) put it, “The Scanduras derive an experimental program in which children should learn a higher order rule for conservation, but one which would not be perfectly general. This . . . allows them to predict a specific pattern of decalage across posttest tasks . . . their analysis does indeed allow prediction of decalage effects” (p. 164). Or, as Gilliéron (1982) put it, “The experimental subjects seemed to conform very well to the model. . . . The significance of their performance cannot be underestimated” (p. 169).

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References

On bridging gaps
One cannot hope to bridge the gap or reconcile the differences between two competing models of development without first coming to terms with the two models themselves. Had the Scanduras presented the models in a way that recognized the appropriate scholarship of the last twenty years, there is reason to believe their efforts to bridge the gap would still have failed. The issues that separate the Genevan and mechanistic models are, after all, not empirical issues and consequently cannot ever be resolved empirically. There simply are, for example, no data that will ever tell us whether a change in a child’s behavior is qualitative or merely quantitative. This is a deeper matter than whether or not the Genevan concepts can be operationized because, of course, they can and have been. It is, instead, a matter of the fundamental assumptions made in each model about what kinds of behavioral changes are developmental and what kinds are not. The Scanduras’ analysis of conservation simply does not address the issues that divide the models and, for that reason, holds no promise for bridging that division; nor does it tell us anything about the basic developmental fact that four-year-olds differ from seven-year-olds in their solutions to conservation problems.

With respect to the empirical portion of their claims, it is regrettable that their training study falls short of the standard required for scientific journals, so no firm empirical findings of any sort can be claimed. In any case, that the Scanduras provided children with a rule, even if it is formally derivable from other simpler rules, that led the children to give conservation judgments that were indistinguishable from “natural” conservers does not mean that natural conservers solve the problem like the trained children or vice versa; nor does it mean that the trained children even derived the higher order rules from the other rules, let alone that their solution to the conservation problem carried with it the feeling of necessity that marks genuine conservation.

Finally, if we follow the Scanduras’ logic for bridging gaps, how in the end are we to make use of the fact that hundreds of other researchers, using very different analyses of the child’s conservation competence, have also succeeded in training children to conserve or that other plausible schemes exist for the prediction of the decalage phenomena?

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On clarifying confusion
Murray’s reaction simply restates his view that the structural learning approach fails to address fundamental issues that divide Genevan and “mechanistic” models. He barely alludes to what the fundamental issues we purportedly do not address might be.

Consequently, our initial response to Murray’s reaction was simply to stand pat on our original statement (the book). His comments do not come to grips with what we said, and he attempts to interpret our work exclusively in terms of Piagetian concepts or “mechanistic empiricism”—as if these were the only two possible ways of conceptualizing human knowledge and behavior. Granted, for example, other schemes for explaining decalage may exist, but to date no other scheme has made it possible to manipulate experimentally horizontal decalage in individuals.

On the one hand, Murray speaks of the “feeling of necessity” as something inconsistent with structural learning, not recognizing both that the scientific core of the former (i.e., that which can be observed within the domain of interest) is captured by our higher order derivation rules and that Genevan stage constructs are a special case of the idealized prototypic competence that forms the core of ALL structural learning theories. On the other hand, Murray’s implicit call for larger “n” fails to recognize both the high statistical reliabilities of our results and our use of empirical methods that are intrinsically deterministic rather than normative (e.g., Scarduna, 1977, 1978) as in tests of “mechanistic” models.

As other reviewers have stressed, “This is not just another training study.” We invite psychologists to read the book and decide for themselves.

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References
Low-level lead exposure: Are there harmful effects?

Robert Gregory has provided the audience of psychologists with a highly laudatory review (CP, 1982, 27, 429–430) of Needleman’s Low Level Lead Exposure: The Clinical Implications of Current Research. Because the clear purpose of the book is to promote a single position and because the issue of low-level lead effects is far from settled, comments about the book and the position promoted therein are in order.

Methodological issues are paramount in this area of research. Let us look at the Needleman et al. (1979) study. This study is seriously flawed (Ernhart, Landa, & Schell, 1981a, 1981b), and 1 should here refer to only the most critical problems. Parental intelligence and other confounding variables were controlled in some, but not all, of the analyses. (The graphs provided in support of a dose-response effect for behavior ratings may well reflect father occupation, father education, and parent IQ rather than lead.) The authors fail to recognize that statistical controls of confounding, as in analysis of covariance, necessarily undercorrect (Reichardt, 1979). This is important when effects are small, as they are in these studies. The study also suffers from differential subject loss in the high- and low-level group. Aside from the Wechsler Intelligence Scale for Children-Revised, all significant findings were obtained with unstandardized experimental procedures. No attention was given to the failure to obtain positive effects with the important standardized tests of school performance and standardized measures of perceptual-motor abilities. The number of subjects is very small relative to the number of variables. Most important, the study is a prime example of a common problem—the use of independent univariate statistical tests for a large number of variables without consideration of t-testwise error rate (Cohen & Cohen, 1975, pp. 155–162). With fifty independent statistical tests, the probability of finding any one or more analysis significant (alpha = .05) under the null hypothesis is .92. If the investigators had followed the probability correction procedures mentioned in a footnote to their tables, they would have been able to report statistically significant results only for two trial blocks of the unstandardized reaction time procedure. Clearly, multivariate procedures would have been more suitable.

The situation is even worse for the EEG study reported in the reviewed book (Burchiel, Duffy, Bartels, & Needleman). In this, 320 outcome measures obtained for 41 children were analyzed by univariate tests. Of these, 6.2% were significant at the .05 level, 2.5% at the .02 level, and .6% at the .01 level. These “significant” EEG variables were then combined with the psychological variables from the parent study that reached statistical significance. These were entered into further analyses (same subjects) that, of course, yielded significant differences between groups.

Simply put, there is no statistical justification for Needleman et al.’s (1979) conclusions. When my coinvestigators and I obtained a very few marginally significant results in our investigation (Ernhart et al., 1981a), we considered the direction of bias associated with these methodological issues in our study, in the Needleman study, and in other research. We concluded that if there are intellectual and behavioral effects of low-level lead exposure, they are minimal.

Some of the other chapters in the reviewed book are useful contributions to the literature if one is aware that all of them were selected or written to be consistent with the Needleman position. Gregory mentioned that the book ignores researchers who are skeptical of the conclusion of low-level lead effects and he wonders “how advocates such as Needleman view this other body of research.” Needleman is deeply committed to his position. Given this commitment it is no surprise that he did not include alternative views in his book. This firm belief may explain his failure to recognize the problems in his studies (Needleman, Bellinger, & Leviton, 1981).

Strong positive conclusions in the absence of valid findings are not harmless. Failure to conduct research adequately can lead to a loss of credibility and weakens the trust the public has in our work. In this situation we should also be concerned for parents whose anxieties about possible diminished intelligence in their children are inappropriately directed toward low-level lead effects. Most important, making low-level lead exposure a scapegoat for mental retardation and behavior problems causes us to ignore the very real problems of those children who are most likely to be exposed to low levels of lead. These are the children who are most likely to live in substandard housing, to be poorly nourished, to have a mother who is herself a child, to have no visible father, to witness or be victims of drug abuse, alcoholism, violence, and stress. This is the real epidemic that we are ignoring.

The book provides some useful material for the knowledgeable researcher and the astute and experienced teacher of research design. It most certainly is not a “good starting point for the uninitiated”.

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References


Low-level lead exposure: The case for harmful effects

Ernhart claims that the Needleman et al. (1979) study is “seriously flawed.” I described this study as a “methodological tour de force” in my review, so obviously I disagree. The argument that the graphs provided in support of a dose-response effect for behavior ratings may well reflect father occupation, father education, and parent IQ rather than lead” will appear specious to readers who view Figure 2 in Needleman et al. I believe it is extremely unlikely that any covariate is so highly correlated with both lead level and the teacher’s ratings of 2,146 children.