

Methodological and Conceptual Problems and Issues in Attribute x Treatment Interaction (ATI) Research

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Twelve methodological and conceptual problems and issues in ATI research are discussed: the impossibility of manipulating organismic variables, the intercorrelation of organismic variables, whether to use "actual" or "perceived" environment measures, the classification and measurement of environmental characteristics, whether to use broadly or narrowly defined attributes and treatments, whether it is necessary to conceptualize both the person and the environment in theoretically comparable terms, whether ATI research should be theoretical or atheoretical, the importance of ordinal and disordinal interactions, determining regions of significant difference in interactions, the confounding of between- and within-class regressions with treatment regressions, and the neglect of intermediate treatments. It is concluded that there is no clear reason to retreat from the ATI orientation and that ATI research may prove to be a cornerstone in the *active* recognition of the interactive complexity of behavior determinants and may represent an important evolutionary step in psychology in general.

Une douzaine de problèmes d'ordre méthodologique ou conceptuel reliés à la recherche sur l'interaction attributs-traitements (ATI) sont abordés dans cet article: l'impossibilité de contrôler les variables organiques; les intercorrelations entre les variables organiques elles-mêmes; le choix que l'on doit faire entre les mesures réelles ou perçues de l'environnement; la classification et la mesure des caractéristiques de l'environnement; la décision d'utiliser des attributs et des traitements définis soit en termes généraux soit en termes spécifiques; la nécessité hypothétique de conceptualiser en termes comparables la personne et son environnement; la question à savoir, la recherche ATI devrait-elle être théorique ou non théorique?; l'importance des interactions ordinales ou non ordinales; la détermination des seuils de signification des interactions; l'impossibilité de différencier entre les régressions inter et intra-classes d'une part, et les régressions sur les traitements d'autre part; le fait de ne pas tenir compte des traitements intermédiaires. Somme toute, il semble n'y avoir aucune raison substantielle pour abandonner l'approche ATI. Au contraire, cette approche pourrait fort bien s'avérer la pierre angulaire pour établir définitivement la complexité-interactive des déterminants du comportement et marquer un pas décisif pour le développement de la psychologie en général.

Historically, the two dominant branches of scientific psychology, general experimental psychology and psychometrics, have different roots and have grown along different paths. Both the experimentalist and the psychometrician attempt to formulate principles in manners that do not foster, or in most cases do not even permit, the incorporation of the content and the structure of the other's approach. The experimentalist concentrates on variance which can be attributed to treatment variations; the psychometrician concentrates on variance which can be attributed to individual differences.

Ignoring the importance of student variables in the area of instructional research leads to research hypotheses concerned with the general effectiveness of instructional procedures, such as whether a student-centred approach is more effective in promoting learning and higher student morale than a teacher-centred approach. Psychometric researchers have analogously ignored variations in instructional methods while seeking stable organismic correlates of academic performance, student satisfaction, and other educationally relevant outcome variables.

Attribute x treatment interaction (ATI) research is an endeavour in the areas of instructional psychology and classroom dynamics to look at educational outcomes in terms of the variance-accounting capacities of the interactions of environmental and personological variables. ATI is a research *orientation*, rather than a research area, within the bounds of instructional psychology and classroom dynamics (Berliner & Cahen, 1973). Slow to accumulate at first, the number of ATI studies has accelerated in recent years and a few reviews of the literature have been published (e.g., Berliner & Cahen, 1973; Cronbach & Snow, 1977; Snow, 1976). It is hoped that ATI research will facilitate the development of an adequate theory of instruction by gradually fabricating a matrix of pupil characteristics and learning situations in relation to several educational product variables (Salomon, 1972). Because of its relatively prototypical nature in dealing with person x environment interactions, ATI research will surely contribute to thinking in many areas of psychology. By concentrating on a reasonably well-defined microsystem – the classroom – much may be gained regarding the problems, pitfalls, and potentials of interactional research in general.

It is not our intention to examine any particular body of ATI research but to discuss some of the problems and issues in methodology and conceptualization which appear to be germane for those pursuing an interest in the study of ATI phenomena for whatever reason. There appear to be 12 major methodological and conceptual problems and issues in ATI research but of course no claim is being made for exhaustiveness. Perhaps one might consider these particular problems and issues to be of an almost entirely methodological nature. However, the available methods tend to guide and limit the manner of conceptualization, and in this more indirect way the problems and issues are also conceptual.

IT IS NOT POSSIBLE TO MANIPULATE AN ORGANISMIC VARIABLE

Any variable which is used as an independent variable in a true experiment (Campbell & Stanley, 1963) must be capable of being manipulated along some unidimensional continuum. By manipulating independent variables and noting changes in dependent variables, it is assumed that causality can be inferred. This is the core of the experimental method on which general experimental psychology rests. It should

be stressed that this core is independent of the statistical techniques employed to make the inferences. As Humphreys and Fleishman (1974) state:

An aura of the laboratory and of controlled experimentation accompanies the use of the analysis of variance, while correlational analysis is perceived as not experimental. Every undergraduate, or almost every one, knows that correlation does not mean causation. It is not as well known that *t* ratios and *F* ratios are also measures of relationship and that it is the ability of the experimenter to make random assignments of persons to treatment groups that distinguishes experimental control from statistical control. To put it bluntly, the analysis of variance has become more popular by far than it deserves. (p. 472)

Personological variables cannot be used as independent variables in a true experiment because they cannot really be manipulated. For example, one cannot randomly assign different levels of intelligence to different subgroups of subjects in the manner in which environmental treatments can be assigned. The most satisfying option is to "manipulate" personological variables circuitously by selecting from a subject pool persons who are high and low on the variable in question and then submitting the high and low groups to one or more treatments. Many researchers who do this seem to assume that a variable "manipulated" in this fashion causally underlies the changes that may be noticed in the dependent variable. The subjectivity of such an interpretation is apparent since personological variables tend to be correlated.

For instance, a researcher employing the authoritarianism construct as tapped by the *F*-Scale (Adorno, Frenkel-Brunswik, Levinson, & Sanford, 1950) might select subjects from a subject pool on the basis of their high and low *F*-Scale scores and note their responses on some dependent variable in the face of one or more treatments. However, since authoritarianism and intelligence are negatively correlated (Jacobson & Rettig, 1959), a theoretical explanation might be misleading if authoritarianism theory is used. It is quite conceivable that the results may also be explicable in terms of intelligence. Such a quasi-experimental design can never ensure valid cause and effect inferences.

Therefore, there is resistance on the part of experimentalists to accept the interactional challenge; it means giving up, in part, their quest for confident causal deduction, which is their hallmark and their most prominent advantage over the personologists. All those who enter the confines of the ATI paradigm, or any person $\times$ environment paradigm, must be prepared to accept something much less than reliant causal interpretations.

ORGANISMIC VARIABLES TEND TO BE INTERCORRELATED

Obviously, the first and second problems are closely related. On closer examination, though, they are separable. The first stresses the barrier

which must be overcome before some researchers can adopt the ATI approach, whereas, the second deals more with the implications for generating valid predictions from theory for further ATI studies. In other words, the problem arises in interaction research that one cannot be sure on which of two correlated personological variables to make a theoretical interpretation and hence deduce further hypotheses in regard to an interactive relationship.

For example, McCann and Fisher (1977) found both dogmatism and perceived teacher directiveness *and* intelligence and perceived teacher directiveness to be interactively related to student satisfaction. More dogmatic and less intelligent students were more satisfied when they perceived the teacher to be directive; less dogmatic and more intelligent students were more satisfied when they perceived the teacher to be nondirective. Separate theoretical explanations based on either the dogmatism or the intelligence construct could probably have been derived for the two interactive relationships, and almost certainly would have been if the interactive relationship involving dogmatism had been found by one researcher and the interactive relationship involving intelligence by another. However, since dogmatism and intelligence have been found to be negatively correlated (e.g., Zagona & Zurcher, 1965), the possibility that the interactive relationship involving dogmatism and the one involving intelligence were redundant or overlapping was seriously entertained. Using stepwise multiple regression strategies (Cohen, 1968), it was found that the variance accounted for by the interaction of student intelligence and perceived teacher directiveness could be statistically explained by the significant interaction of student dogmatism and perceived teacher directiveness. Alternatively, the variance accounted for by the interaction of student dogmatism and perceived teacher directiveness could not be totally explained by the interaction involving student intelligence. The variance accounted for by the latter could be viewed as a subset of the variance accounted for by the interaction involving dogmatism. Therefore, it would seem more reasonable in this case to explain the relationship and deduce further hypotheses on the basis of the theory of dogmatism rather than on the theory of intelligence. Caution must be exercised though in using this strategy: stepwise regression does not identify the *causal* variable in a correlated set.

The point is that in dealing with correlated personological variables one must proceed in an open-minded eclectic manner and be prepared to move across construct barriers and perhaps often even totally into the territories of foreign constructs, if the statistical variance accounting framework is taken seriously. The alternative is to argue that one's initial construct in actuality subsumes the subsets comprised of the apparently competing constructs, but the problem is really one of more accurately measuring one's initial construct.

SHOULD MEASURES OF THE ACTUAL OR THE PERCEIVED ENVIRONMENT BE EMPLOYED?

Perception is the essential core of cognitive functioning. If it was the case that we see what we see because things are what they are, perception would pose few problems. For all intents and purposes the actual environment could be substituted for the perceived environment since the latter would be equivalent for all persons within the same actual environment. Perception might then be looked upon as a constant across individuals, a link in the initiation and execution of responses but of no use in explaining variability in behavioural differences across individuals within the same actual environment. However, extensive research has made it abundantly clear that what is perceived cannot be fully derived from the nature of the stimulus alone.

Cronbach and Snow (1977, p. 471) stress that future "studies will have to reckon with the evidence that the student's perception mediates the interaction and generates personality-dependent ATI effects even where the teacher is 'constant.'" Acknowledgement of the importance of perception in ATI phenomena has deep implications for the manner in which research is conceptualized and conducted. It is not simply a matter of substituting a "perceived" measure for an "actual" measure.

In regard to the importance of student perceptions in ATI work, Cronbach and Snow were most impressed with the results of two studies by Dowaliby and Schumer (1973). In the first study, Dowaliby taught one section of an introductory psychology course according to a student-centred prescription and another according to a teacher-centred prescription. It was found that scores attained by high-anxiety students were higher on two multiple-choice tests based on course content in the teacher-centred class than in the student-centred class, while the converse occurred for low-anxiety students. In the second study, students in an introductory educational psychology course responded halfway through the term to an anxiety scale and a scale designed to tap the perceived structure of the course. Two course examinations were used as dependent variables. Surprisingly, when the extreme scorers on the perceived structure measure were retained for analysis, essentially the same disordinal interaction emerged as in the earlier manipulative study. That is, even though the teaching style was held constant, high-anxiety students received higher grades when they perceived the course to be structured, while low-anxiety students did better when they perceived the course to be unstructured.

The Dowaliby and Schumer findings spurred Cronbach and Snow (1977, p. 508) to state that "the student's perceptions of his teacher may be just as significant a source of interaction as the teacher's actual style." This assertion implies that both the subjective treatment and the objective treatment can be fruitfully utilized in ATI research. No grounds for

choosing one or the other were put forth. Pervin (1968) has expressed a similar view within the broader context of interactional research, stating that a choice between actual and perceived environments is inappropriate, that instead we should look at the conditions under which one or the other accounts for more of the variance or is most useful, and that both kinds of data should be collected whenever possible.

Sells (1963a, 1963b) advocates the use of measures of the objective features of the environment in interactional research. According to Sells, the "perceived" approach obscures the nature of environmental factors and confounds the interaction of person and situation variables. He concludes that a perception term is not necessary since "the individual's perception of his world is largely included in the personality and motivational questionnaire items" (1963a, p. 8) used to tap the organismic aspects of the interactionistic formulation. This argument seems insufficient because there is a failure to note that certain personality and motivational dimensions may indeed reflect individual differences in perception but these dimensions are not necessarily, or perhaps not even usually, of interest in the interactional hypothesis being tested. In other words, the personological dimension in a particular study may not reflect individual differences in perception of the environment in the particular study but may interact with perceptions of the environment in the particular study.

Sells is not alone. The usual approach in ATI research has been to look at the interaction of actual environmental factors and personological variables in relation to educationally relevant outcome variables, an approach which does not allow for the importance of individual differences in environmental perception. A less common strategy has been to consider the interactions of perceived environmental factors and personological variables in relation to educationally relevant criteria. This method says little about the role of the actual environment and does not comment upon why individuals may hold different perceptions of the same actual environment. The two approaches may reach different conclusions within the same context.

Cronbach and Snow (1977, p. 513) have emphasized that interaction effects may change over time because the proper match of persons and initial instruction can alter the very personological dimensions on which the initial matching was based. It might also be added that perceptions of an instructional treatment may also change over time and thereby change the interaction effects over time even though the actual instructional treatment and the relevant personological dimension have remained constant. This may be one source of inconsistency in findings of studies employing actual treatments and studies employing perceived treatments.

One spin-off in using perceived environment measures tabulated at the individual level is that the denominator degrees of freedom in the associated *F*-tests and the range and variance of the "environment" variable may be increased, making it mathematically easier to attain a

suitable level of statistical significance if a relationship indeed exists. Type II errors are minimized. This is especially important in instructional research in the field because of the relatively limited number of teachers and classrooms which can usually be procured for any particular piece of research.

HOW SHOULD ENVIRONMENTAL CHARACTERISTICS BE CLASSIFIED AND MEASURED?

More basic than the issue of whether to use measures of the perceived or actual environment is the problem of how to classify and categorize the environment, be it from a "perceived" or "actual" perspective. Shulman (1970) has stressed that "aptitude-treatment interaction" will probably be "an empty phrase as long as aptitudes are measured by the micrometer and environments are measured by the divining rod" (p. 374). Mitchell (1969) has underlined that techniques for measuring varieties of instruction are especially global and imprecise.

Experimentalists, who have monopolized the study of the environment until fairly recently, have been more concerned with reductionistically selecting what they feel to be crucial predictors of behaviour and manipulating the essential factor within an experimental paradigm. What appears necessary is a psychometric approach to environmental factors, in which environmental dimensions (including other actor's behaviours) are incorporated into measures of the environment. Advances have been made in this direction (e.g., Anderson, 1973; Steele, House, & Kerins, 1971; Trickett & Moos, 1973). Such taxonomies of educational environments should contribute greatly to ATI methodology; the "A" and the "T" should produce greater resonance.

IS IT MORE BENEFICIAL TO USE BROADLY DEFINED OR NARROWLY DEFINED ATTRIBUTES AND TREATMENTS?

Bracht and Glass (1968) have asserted that the potential for confirmation of interaction effects is much greater when narrowly defined personological and environmental variables rather than broadly defined constructs are studied. Contrary to this position, Cronbach and Snow (1977) and Snow (1976) conclude that research showing high consistency of results employ fairly general personological variables and use real, broadly categorized, instruction over a long period of time. Such score-keeping before we have really learned how to play the game is probably somewhat destructive and misleading in the long run. The more important answers to the question of relative benefit would seem not to revolve so much around the proportion of confirmations gained for each approach but around the ends to which ATI research is seen as a means.

For those researchers most interested in applying the findings of ATI studies as an empirically based means of more or less grossly individualizing instruction in the not too distant future, working with broadly defined

personological and treatment variables may be most beneficial. If, though, the researcher is more interested in arriving at a deeper analytic understanding of the phenomenon in question, in determining the cognitive dynamics or processes involved, then the molar approach is too coarse and further degrees of reduction are necessary.

Cronbach and Snow (1977, p. 160) have pointed out another problem in employing narrowly defined personological variables, particularly ability variables. If a specific, narrowly defined ability variable is found to interact consistently with a treatment, is one to attribute the relationship to the specific ability or to the more general ability of which it is considered to be a part? It appears that multitrait-multimethod designs (Campbell & Fiske, 1959) are needed to achieve a sound understanding of the relative contributions of specific and general dimensions. This problem is similar to that surrounding the intercorrelation of organismic variables which was discussed earlier in the present paper.

It might also be added that broadly defined variables would seem to be most manageable or "beneficial" in field research while narrowly defined variables are more suited to the laboratory approach. For example, when one does long duration research in a real classroom situation, classifications of teaching behaviour as global as "directiveness" seem to be called for, while elements of such a classification can be broken down and used individually or in fabricated combinations more effectively in the laboratory.

IS IT NECESSARY TO CONCEPTUALIZE BOTH THE PERSON AND THE ENVIRONMENT IN THEORETICALLY COMPARABLE TERMS?

Interactive theory is exceedingly difficult to build if the units used for the organism are not theoretically similar to or compatible with the units used for the environment. Hunt (1971) considers this to probably be the major obstacle to ATI research programs. Most personality models vary in the degree to which structural or dynamic characteristics are emphasized. For example, the construct "cognitive complexity" is structurally oriented while "need for achievement" is dynamically oriented. Accordingly, Hunt complements his cognitive complexity measure with measures of environmental structure or organizational complexity in his ATI work. Conversely, a variable such as need for achievement can be more successfully employed in ATI research if the salient environment is characterized in terms of potential for need gratification, as in Atkinson's theory of achievement motivation (Atkinson & Feather, 1966). If the personological and environmental variables are not in comparable units, or looked at from tangential perspectives, the ATI results may not be readily explainable in terms of either the initial organismic construct or in terms of the initial environmental construct. Consequently, the potential for further valid theoretically based predictions is gravely threatened.

SHOULD INTERACTION RESEARCH IN INSTRUCTION BE THEORETICAL OR ATHEORETICAL?

Salomon (1972), Berliner and Cahen (1973), and most others in the ATI camp feel that ATI findings which do not suggest explanatory principles or that are not directed at elaborating such principles have little lasting importance. Shulman (1970), on the other hand, feels that it might be in the long term advantageous to ignore conventional psychologizing for the present and take a relatively atheoretical course in the study of education.

It would seem mandatory for an ATI researcher to have at least vague theoretical grounds to predict potentially interacting variables in order to make a decision as to what variables to measure in any given study. A truly random selection of variables is probably never made. However, even a "shotgun" approach with a number of less than randomly selected variables is fraught with problems and seems destined to end up with no more, or not many more, significant interactions than would be expected by chance (e.g., Goldberg, 1972).

The issue appears to be not so much that of atheoretical researchers versus theoretical researchers but of eclectic researchers versus researchers who are strongly committed to particular kinds of "pet" variables. For example, if a cognitive complexity measure can account for all of the variance in a particular interactive relationship which can also be explained by an intelligence measure, but the intelligence measure cannot explain all of the variance accounted for by the cognitive complexity measure, then we should look at the cognitive complexity measure as an explanation, or at least make further predictions on the basis of it.

For the present it seems advisable to stick to the criterion of how well a particular construct predicts independent variance in the outcome variable. Molecular approaches within a given area (e.g., the student variables which interact with teacher directiveness in relation to grades and satisfaction) should guide a number of studies with different variables from different schools of thought, and when many results are in, perhaps a pattern will emerge on which to build a more molar ATI approach within that given area.

ARE ORDINAL AND DISORDINAL INTERACTIONS BOTH IMPORTANT IN ATI RESEARCH?

Lubin (1961) defined a disordinal interaction as one in which the treatment lines cross within the sample range of scores on the personological variable. Bracht and Glass (1968) modified, and made more stringent, the definition of a disordinal interaction. An interaction was considered to be disordinal only if the difference between the two treatments at two levels of a personological variable were both significantly greater than zero and opposite in algebraic sign. For example, an inter-

action is only disordinal if one treatment is significantly better for subjects high on a personological variable and a second treatment is significantly better for subjects low on the personological variable. Otherwise the significant interaction is considered ordinal and, according to Bracht and Glass, is not of much value in the ATI paradigm.

However, Berliner and Cahen (1973), Cronbach and Snow (1977), Hunt (1975), and Snow (1976) all disagree with Bracht and Glass and feel that disordinal and ordinal interactions are both useful. Berliner and Cahen offer an example of the potential utility of an ordinal interaction for the educational decision maker. Suppose that Treatment X is superior to Treatment Y at all sample levels of a personological variable but Treatment X is far superior to Treatment Y at the higher levels of the personological variable. If the cost of Treatment X is the same or no more than that of Treatment Y then all learners should receive Treatment X. However, if Treatment Y is cheaper than Treatment X then a decision might be made to give students higher on the personological variable Treatment X and students lower on the variable Treatment Y. Of course, it might be argued that the decision has not been made on the *basis* of the ordinal interaction – not calculated solely from the information of the ordinal interaction – and that the ordinal interaction has merely suggested a disordinal interaction with “gain per unit cost” as the dependent variable. However, this anti-ordinal argument fails to negate the importance of the information derived from the ordinal interaction from either a theoretical or an applied perspective.

The Bracht and Glass test for disordinality may be unduly severe and they apparently now are ready to discard it (Cronbach & Snow, 1977, p. 93). In any case, their distinction should not be used as justification for refusing to acknowledge reported ordinal interactions. Both kinds of interaction information are useful but one must be especially careful in the use of disordinal and ordinal information in theory and practice, and avoid confusion of their different implications.

THERE IS A PROBLEM IN DETERMINING REGIONS OF SIGNIFICANT DIFFERENCE IN INTERACTIONS

A statistically significant ATI, ordinal or disordinal, does not offer much practical information to a decision maker faced with the task of assigning students to instructional methods on the basis of scores on the personological variable. The interaction itself does not provide information as to the critical values on the personological variable which discriminate between those who will do well with one method, those with another, and those who do equally well with either method.

The Johnson-Neyman technique (Johnson & Neyman, 1936) can be used to determine the range or ranges of points on the personological variable for which reliable treatment differences are found. However, the technique is unwieldy – and consequently seldom used – and little is

known about the effects of unreliable measurement in relation to the regions of significance (Berliner & Cahen, 1973). Cahen and Linn (1971) compared the Johnson-Neyman technique to two other more recent competing methods, the Potthoff technique (Potthoff, 1964) and the Erlander and Gustavsson technique (Erlander & Gustavsson, 1965). Each technique produced different regions of significance. They concluded that controversies similar to those surrounding the best methods of post-hoc analysis of differences between means after the analysis of variance appear to be imminent regarding regions of significance in interaction effects.

It seems worth pointing out for at least two reasons that statistical significance is not everything and in some cases is nothing. First, a significant interaction may not be of sufficient magnitude to warrant consideration in application. Second, as Cronbach and Snow (1977) have emphasized, nonsignificant findings which indicate a similar pattern over a number of similar studies are particularly important in ATI research because of the limited power of most ATI studies.

OUTLIERS CAN PROFOUNDLY AFFECT REGRESSION SLOPES

Snow (1976) briefly discusses the problem of outliers in ATI research. An outlier is defined as an unnaturally deviant data point, that is, a data point, which cannot "be predicted by psychological variables that make sense in context" (p. 87). Apparently, present techniques for detecting outliers are unsuitably specialized and developed and there are serious problems in deciding if data points are indeed deviant.

The real problem for ATI research is that regression slopes can be deeply affected by a few outliers. A very few outliers can render an interaction significant when in fact there would be no interaction if no outliers were included in the analysis. A few outliers can also change the nature of the interaction effect. Furthermore, the disproportionate effect of outliers increases dramatically as the number of data points is reduced. Attempting to spot potential outliers by plotting data points *before* doing any analyses is strongly recommended.

BETWEEN- AND WITHIN-CLASS REGRESSIONS MAY BE CONFOUNDED WITH TREATMENT REGRESSIONS

Cronbach and Webb (1975) have pointed to a profound problem in interpreting the results of ATI studies in which the popular method of assigning a number of classrooms to one condition and a number of classrooms to another condition, or at least classifying a number of classrooms as being of one type and a number of classrooms as being of another type, is employed. When subjects are treated individually in a laboratory study, the results are always interpreted as resulting from the individual's response to the treatment. However, when subjects are treated in groups there are two other potential explanations.

One possibility is that the group as a unit may be affected by some process. For instance, the teacher may include less material in the course if the mean intelligence of the class is particularly low, and therefore on a standardized measure of achievement the class as a whole may do relatively poorly. The other possibility is that there may be comparative effects within a group of subjects. Suppose that one method is replete with rewards for students with high ability. A student's experience of success is in part then governed by whether he is in a class where the mean intelligence is above or below his own intelligence.

Between-class regressions are influenced by the first class-level process while within-class regressions are influenced by the second comparative effect process. Both between- and within-class regressions are influenced by the individual effect. Unfortunately, the problem is that no sure statistical method has been developed for separating these three kinds of regressions in ATI research.

IN ATI RESEARCH, SUBJECTS ARE SELDOM GIVEN AN INTERMEDIATE TREATMENT

In a study in which subjects are presented with strongly contrasted treatments, significant interactions may occur. However, there is the possibility that all subjects may actually prefer, or perform better with, an intermediate treatment. Furthermore, this preference may not be at all related to the standing on the personological variable in question. This problem is similar to the practice in main effects research of not presenting an intermediate treatment to test for the possibility of curvilinear relationships.

As an example, consider part of a study by McCann and Hamilton (1978). On the Student Perception of Teacher Style (SPOTS) scale (Tuckman, 1968), several teacher behaviours are rated by a teacher's students and the mean score is taken as an index of the teacher's degree of directiveness, with lower scores indicating a more directive style. According to Tuckman, directive and nondirective teacher behaviours can be divided into those which are on a "structure" dimension and those which are on an "interpersonal" dimension. Items of the following form are circled according to each student's perceptions:

When we are working on a group project or on a committee the teacher

1	2	3	4	5	6	7	8	9
Tells us exactly what to do.			Suggests ways that the project might be handled.			Lets the group members decide how the project should be handled.		

In the McCann and Hamilton study, two brief profiles of a hypothetical professor who was either directive or nondirective on the structure dimension of teacher directiveness were derived from the polar descriptions of the structure items on the SPOTS scale. Each of the two profiles was responded to in terms of expected degree of satisfaction if confronted with such a professor in an elective course. Students also completed a Dogmatism Scale (Rokeach, 1960). Respondents were then asked to suppose that they could choose a professor with the characteristics that they wanted so that they would be maximally satisfied. The SPOTS items from which their professor description had been developed were presented in the conventional format, and they were asked to respond to each item by circling one of the digits from 1 to 9 to reflect the behaviour with which they would be most satisfied. Analysis revealed that dogmatism and the structure dimension of teacher directiveness were interactively related to satisfaction with the hypothetical professor in the expected fashion. The lower the student dogmatism, the higher was the satisfaction with the nondirective professor, while the higher the student dogmatism, the higher was the satisfaction with the directive professor. However, the total SPOTS scores for optimal satisfaction were not related to dogmatism; students preferred teacher characteristics which were intermediate on the directiveness continuum. In terms of mean responses, 6% were in the directive third (1–3), 61% were in the intermediate third (4–6) and 33% were in the nondirective third (7–9).

In summary, student dogmatism seemed to moderate satisfaction when the teaching style was extremely directive or nondirective on the structure dimension, but both dogmatic and nondogmatic students preferred a style that was not excessive in either direction. The potential pitfall for the educational researcher, the theoretician, and the educational decision maker is rather obvious.

ATI RESEARCH IN THE FUTURE

Given the methodological and conceptual problems that have been touched upon in this paper, it may seem somewhat presumptuous for ATI researchers and theorists to suggest that the approach become even more complex. Nevertheless, increased intricacy appears to be the order of the future for ATI work.

Perhaps the most widely advocated increase in sophistication is the notion of several in the area (Berliner & Cahen, 1973; DiVesta, 1973; Glaser, 1972; Rhetts, 1972; Snow, 1976; Tobias, 1976) that serious attention must be paid to the nature of the tasks students are asked to perform and the underlying cognitive processes determining success and failure on these tasks. With a knowledge of how different subgroups of students process the task demands, it is reasoned that instruction could then be designed to adapt to these individual processing characteristics (Glaser, 1972). Tobias (1969) has suggested that the ATI framework may

have to be renamed "ATTI" to take into account the interaction of task attributes with attribute x treatment interactions. DiVesta (1973) similarly recommends a three-way interaction approach which he refers to as the "TTPI" (trait x treatment x process interaction) model.

Rhetts (1972) also argues in favour of careful early analysis to determine which individual differences may be related to performance, and then designing instructional treatments to adapt to these differences. However, Rhetts thinks that such research should be done in two stages and the essential paradigm should remain "ATI" instead of "ATTI" or "TTPI." He feels that a three-way paradigm is in general a "premature move except in specific three-way hypotheses" (p. 276). In Rhett's approach, the task process questions are asked first and until significant performance differences among students have been established the research is not cast within an interactional paradigm.

Cronbach, who initiated the ATI paradigm in 1957 (Cronbach, 1957), now also recognizes that it is ultimately insufficient (Cronbach, 1975) since interactions cannot be confined to the first order: "Once we attend to interactions, we enter into a hall of mirrors that extends to infinity. However far we carry our analysis – to the third order or fifth order or any other – untested interactions of a still higher order can be envisioned" (p. 119).

There is an obvious note of despair in Cronbach's statement but such a conclusion is not surprising. Mischel (1973) has likewise concluded that personality research must inevitably become the study of higher order interactions and McGuire (1973) has come to a similar conclusion in regard to social psychological research. Mitchell (1969, p. 698) follows the same vein when he declares that "we are multitrait individuals responding to multicharacteristic environments, and the total pattern of these interactions determine our behaviour."

The fate of "general" laws (or empirical generalizations) based on organismic *or* environmental variables *or* ATI *or* even ATTI seems obvious in light of the foregoing pronouncements. Less than obstinate scientific psychologists might choose to adopt a humanistic viewpoint. However, even though ultimate interactional psychology may strain our current methodological and conceptual capacities, the threat of higher-order interactions qualifying lower-order interactions does not seem to be a clear reason to retreat – not any more so than any other scientist should retreat from empirical research simply because he senses the infinite complexity of the universe. What has to be internalized is the necessary leap in the level of complexity – the increase in the number of conditional qualifiers – that is necessary in so-called "laws" of behaviour. ATI research may prove to be a cornerstone in the *active* recognition of the interactive complexity of the determinants of human behaviours and may represent an important evolutionary step in psychology in general. In light of its ancestral alternatives, it seems worth the effort.

NOTE

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