

## Transcontextual Validity in Developmental Research

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WEISZ, JOHN R. *Transcontextual Validity in Developmental Research*. CHILD DEVELOPMENT, 1978, 49, 1-12. Recent critiques of the generalizability of developmental principles raise an important question: Can we expect the psychological study of human development to yield durable principles, valid across changes in time, culture, and cohort? While the answer is probably yes, this answer must be qualified by an analysis of the form which enduring principles are likely to take, the limitations and strengths they are likely to display, and the process by which they are likely to be discovered. In unearthing and validating durable developmental principles, the complementarity of diverse methodologies (including "lab" and "ecological" approaches and designs of varying complexity) is a major asset. In addition, social relevance and scientific advance are potentially complementary objectives. Our discipline, it is argued, is at its best in a state of creative tension—between the pursuit of answers to contemporary social questions and the quest for firm scientific principles.

In a recent paper, Bell and Hertz (1976) argue that changes in our society are outpacing the reporting of reliable developmental findings. They cite evidence of substantial change in parental punishment practices over the past 75 years (Erlanger 1974); in feeding, weaning, and toilet training practices during the period from 1932 to 1957 (Bronfenbrenner 1958); and in adolescent scores on personality and ability measures over a 2-year period (Nesselroide & Baltes 1974). From such findings, Bell and Hertz conclude that "if both long- and short-term changes in parent and child behavior are occurring . . . obviously, research progress must be faster. Otherwise, findings may no longer be applicable to the populations for whom they are intended" (1976, p. 6).

Bell and Hertz maintain that, while progress in some areas (e.g., development of sound learning principles) "keeps up with or even exceeds the pace of public assimilation" (p. 7), in other areas—notably socialization research—"there has been a failure to reach stable findings" (p. 7). Perceiving such instability in psychology generally, Cronbach (1975) lists numerous findings which reveal the apparent

obsolescence of earlier psychological generalizations in several content areas. After labeling Bronfenbrenner's (1958) findings on changing child-rearing patterns as "perhaps the best example of all," Cronbach contends: "Generalizations decay. At one time a conclusion describes the existing situation well, at a later time it accounts for rather little variance, and ultimately it is valid only as history" (1975, p. 122).

The alleged failure of prominent generalizations to actually generalize has been a popular theme among developmentalists in recent years. Buck-Morss (1975), for example, has marshaled evidence that principles of Piaget's theory of cognitive development cannot be applied to nonindustrialized societies. She argues that the "psychological universalist" position that is taken (with qualifications) by Piaget (1966) builds its assertion that a general theory of cognitive development is possible on the assumption that formal structures of thought are universal because they are abstract (in contrast to the content of thought, which is said to reflect, in its concreteness, environmental factors). Buck-Morss argues that the form of cognitive activity is

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itself social content, and thus that the familiar Piagetian milestones along the route to cognitive maturity are reflections of a particular socioeconomic structure, not some universal truth. Simpson (1974) has leveled a similar (though more diversified) assault on Kohlberg's (1969, 1971) claims for the universality of his scheme for moral development, arguing that Kohlberg's formulation is ethnocentric and culturally biased. Central to Kohlberg's position are the claims that his stages define an invariant developmental sequence and that they reflect hierarchic integration, with each stage representing a reorganization of "less advanced" stages (see Rest, Turiel, & Kohlberg 1969). Kurtines and Greif (1974) have now demonstrated that (a) the empirical work cited to back these two claims actually provides little clear-cut support for either, and (b) there is evidence from several sources suggesting that the claims are incorrect. With regard to learning theory, recent "intrinsic motivation" research (see Condry 1977; Lepper, Greene, & Nisbett 1973; Lepper, Sagotsky, & Greene, in press) has revealed at least one situation and one group of children for which it appears that pairing a particular response with positive reinforcement reduces the subsequent probability of that response. In addition, the success of attacks on the generality of behavioral principles as applied to language development is now generally acknowledged. Finally, certain writers (e.g., Meacham 1972, 1976; Reese 1973, 1976) are now arguing that the facts of memory development are strongly influenced by the social setting in which one develops and cannot be adequately explained by means of general behavioral principles. The dependence of both social and cognitive developmental patterns upon cultural and social class factors is being increasingly emphasized in the theoretical literature (e.g., Havighurst 1976; Riegel 1972, 1975; see also Senn 1975).

As cross-cultural, cross-time, and cross-cohort data hammer away at what some had believed to be developmental and psychological "universals," an important question arises: Is not the validity of all developmental findings likely to erode with changes in time and culture? The question is a legitimate one and has been answered in the affirmative by many who have pondered it. Critics of the scientific status of social disciplines such as developmental psychology have long argued that the phenomena we study differ from the lawful phenomena of the natural sciences in that they are capricious and unstable across

time and setting, influenced by the free will of the "subjects," colored by the values of the investigator who studies them, affected by the public's assimilation of prior information from our own research, and so overdetermined that complete experimental control is impossible (for discussion of these issues, see Beloff 1973; Brown 1963; Feigl & Brodbeck 1953; Gergen 1973; Hempel 1965; Schlenker 1974). These arguments suggest the concomitant view that short-term, context-bound validity is the best we can hope to attain. Along with a number of developmentalists, I believe that we can expect more enduring principles to emerge from our discipline. However, in light of the critiques cited earlier, I think there is a clear need to contemplate the form which "enduring principles" might take, the limitations and strengths which might inhere in such principles, and the methodology by which they may be discovered. It is to such a contemplative exercise that this paper is devoted.

### Transcontextual Validity and Developmental Research

In essence, this paper addresses the problem of *transcontextual validity*—that is, veridicality of principles across contexts. To the extent that a developmental principle can be shown to hold good across physical and cultural setting, time, or cohort, it can be said to possess this form of validity. In developmental research, evidence of transcontextual validity can consist of both empirical data and logical argument: data demonstrating that explanatory or predictive statements hold true of human development across distinct contexts; argument to the effect that "it could not logically be otherwise." These complementary forms of evidence can be seen, for example, in research on ontogenetic sequence in the cognitive-developmental tradition, where argument for the logical necessity of an ordered sequence of stages is combined with empirical demonstrations of cross-cultural similarity (see below for further discussion of the current status of this evidence). It is this more transcendent form of validity which has traditionally been identified with "science," and in the absence of such validity many would conclude that the term "social science" is something of a misnomer (see Gergen 1973; Koch 1974; Schlenker 1974).

To focus upon transcontextual validity is to effect a rather subtle shift away from debates over whether given developmental

principals are "universal." To assert a universal law is, of course, to make an empirically unverifiable statement, since, as Popper (1959) has noted, "We cannot search the whole world in order to make sure that nothing exists which the law forbids" (p. 70). Consequently, it seems more useful to evaluate our principles with respect to their stability across changes in context, recognizing that if they are universally valid we shall never know it. From this perspective, an important question is how to identify statements about human development that are apt to show high levels of trancontextual validity.

*A continuum of generality.*—Probably the most important characteristic of such statements is their position on the continuum from empirical statements (relatively specific, concrete, and circumstantial in form) to universal propositions (relatively general, abstract, and universalistic in form). (See Kaplan [1964], Schlenker [1974], and Sears [1975] for discussion of a similar distinction.) As statements about human development approach the "empirical statements" end of this continuum, their prospects for displaying substantial trancontextual validity diminish. Consider, for example, Schlenker's (1974) contrast between the empirical statement, "A promise of a lollipop will get a child to finish his spinach at suppertime," and the universal proposition, "Expectations of positive reinforcement will increase the probability of a contingent response." The context-boundness of the first statement is self-evident; the second, because of its greater generality and abstractness, is a superior candidate for trancontextual validity.

Note in this connection that the kinds of social change which Bell and Hertz (1976) describe would serve only to invalidate relatively specific principles. Changes over the last 75 years in punishment practices, for example, might leave a descriptive account of punishment dated but have little impact on general principles concerned with relationships between punishment and child behavior. Likewise, shifts in feeding, weaning, and toilet-training practices as a function of social class might be alarming to an investigator whose findings have shown "how the social classes differ," but not to one who has accounted, in general terms, for why they differ.

*Social and scientific relevance.*—The usefulness of universal propositions may be less evident than that of empirical statements because, as Nagel (1961) has noted, the level of

abstraction necessary for these propositions sets them apart "from the familiar notions used in the daily business of social life" (p. 466). Empirical statements, because their specificity allows them to address contemporary social questions directly, are apt to be of greater immediate interest to society. Precisely because of their specificity, however, empirical statements of direct social relevance, however valid when first generated, are apt to have a distinctly limited social value half-life. The limited durability of each specific statement constitutes a difficulty inherent in the pursuit of specific answers to specific social questions as ends in themselves. When an empirical statement is established as valid for a particular context (e.g., a specific classroom or network of class, school, and neighborhood), its validity for any other context (including the same network 2 years later when, say, the social class structure of the neighborhood has changed or the school has a new principal) is unknown. Trancontextual principles reflecting durable commonalities across settings are, because of their formal generality, not as likely to lead immediately or directly to recommendations for social policy. Yet, it seems reasonable to assume that a stable body of broad, general truths about human development will facilitate the discovery of more specific empirical statements which can, in turn, yield information of more direct use to society.

Similarly, in the development of our science, broad general principles of demonstrated trancontextual validity are potential contributors to the deductive enterprise. A corresponding virtue of the more specific empirical statements to which the search for direct answers to contemporary social questions can lead is their potential for enhancing the inductive aspect of our discipline. As numerous different empirical statements accumulate, each one having demonstrated validity for a specific context and all having some bearing on a common content area, the likelihood increases that durable common threads among the disparate statements will be identified. Thus, from the general we may derive the specific, from the specific the general, and so on in a continuing dialectic.

*Trancontextual principles and "common sense."*—Apart from their contribution to this dialectic, however, principles sufficiently broad and general to display trancontextual validity are unsatisfying to many policymakers and developmental psychologists. Orville Brim (in Senn 1975), discussing the work of

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the Advisory Committee on Child Development, reported that an attempt to summarize "what we know about children" (to support policy recommendations) resulted in "such general platitudes, such as *children developed better under conditions of autonomy and nurturance and support—that kind of level—that it became fatuous. So the committee decided not to summarize it or even state anything about what is known about child development*" (p. 75). The notion that an intelligent overview of our best efforts over several decades can produce only "fatuous platitudes" consisting of what Brim later calls "common sense" could be profoundly depressing to developmentalists—unless we recognize that there is nothing inherently fatuous or platitudinous about general principles of the type referred to by Brim. The idea that autonomy, nurturance, and support are best for children would have contradicted the views of many an eighteenth-century Englishman or even many a twentieth-century American reader of Watson's (1928) child care manual. Much of our common sense is only common to those of our time and culture; and within that context much of today's common sense is common only in the sense that it is widely dispersed, and this because the public has readily assimilated the results of our own work, adopting general principles which seemed not at all platitudinous when first accepted. In the early part of this century, American common sense had it that the intellectually gifted child was weird, introverted, and awkward at sports; today's common sense, shaped largely by developmental research at Stanford (e.g., Terman & Oden 1947), maintains precisely the opposite. As Marian Radke Yarrow (in Senn 1975) notes, a number of principles growing out of developmental research have been quietly woven into the fabric of our society without its direct awareness. When this process leads to more humane or sensible treatment of children, or when it merely prevents repetition of historical mistakes, the developmental principles that are responsible—those that have, without fanfare, been incorporated into society's fund of common sense—can hardly be considered either fatuous or lacking in social utility.

Recall the proposition, "Expectations of positive reinforcement will increase the probability of a contingent response." This principle might appear to be grist for the mill of William F. Buckley, who argues that the psychologist's function is merely to "certify the obvious." Yet the simplicity and generality which make both operant and respondent

principles seem obvious and commonsensical are the very properties which make them applicable to a broad variety of therapeutic, educational, and child-rearing problems (see Achenbach 1974). And, regardless of whether one favors behavioral approaches to problems of spinach eating, bed wetting, impulse control, math anxiety, etc., it is clear that such approaches (a) are now widely used, (b) in their sophistication, easily surpass plain common sense, and (c) would not exist but for the broad, general, and deceptively simple principles of their respective theoretical traditions.

*The alleged pertinacity of developmental content.*—Yet, as indicated earlier, the principles of learning theory are, in some respects, apparently failing the test of transcontextual validity. Arguments to this effect in the areas of intrinsic motivation, language development, and memory development have already been noted, along with critiques of the generalizability of Piagetian and Kohlbergian principles. Finally, the area of social behavior and socialization is perhaps the most often maligned for its failure to yield transcontextual principles, and Bell and Hertz (1976) and Gergen (1973) have suggested that this may be true in part because the social phenomena under investigation are constantly in flux.

Taken collectively, the perspectives described in the preceding paragraph might suggest that not only are candidates for transcontextual validity difficult to identify among developmental phenomena, but that, among those which have been identified, it may only be a matter of time until a context is encountered in which they no longer apply. It would be unfortunate if a generalized pessimism about our scientific status and prospects were thus engendered, because successive invalidation and reshaping of general principles is actually a sign of good health within a discipline, be it social, scientific, or both. This process, however, may create an impression of continually crumbling principles, of continual failure in the quest for enduring truths, which can be discouraging unless one recognizes that what is actually happening is a process of refinement.

The recent critiques of the transcontextual validity of Piagetian, Kohlbergian, and behavioral principles are best interpreted as contributions to this progressive refinement process—not as evidence of an inherent intractability of developmental phenomena. For, while these critiques may undermine some behavioral and cognitive developmental

principles, other principles from these theoretical streams seem to emerge stronger as a result of the critical attention. For example, after gathering cross-cultural and cross-class evidence to demonstrate the "socioeconomic bias in Piaget's theory" (p. 35), Buck-Morss (1975) acknowledges that the sequential order of the principal stages in Piaget's scheme is generally supported by the evidence; and further support for the transcontextual validity of the general Piagetian developmental sequence comes from a review of research comparing development in retarded and nonretarded children (Weisz & Zigler, Note 1).<sup>1</sup> Similarly, after gathering evidence to demonstrate the "cultural bias" of some aspects of Kohlberg's theory, Simpson (1974) acknowledges that other aspects (e.g., developmental trends in the use of intentionality in moral judgments) have actually held up across as many as a dozen cultures thus far. Research on "intrinsic motivation" (e.g., Lepper et al. 1973) appeared to raise questions about the law of effect; certain high-interest activities (e.g., drawing, by children showing previous interest in drawing) followed by a presumably "positive state of affairs"—that is, receipt of an anticipated reward—were subsequently less likely to occur than before the reward was available. However, further research and debate stimulated by this finding (see Lepper & Greene 1976; Lepper et al., in press; Reiss & Sushinsky 1975, 1976) have demonstrated that this effect occurs only where the rewarded individuals have no reason to anticipate further reward; the principle that "*expectation of reward increases the likelihood of a contingent response*" thus emerges from the debate with stronger support than before. So, findings that seem to undermine our basic principles may often carry an important flip side in the form of strengthened support for closely related principles.

Despite the problems posed by the subject matter in socialization and personality development, there is evidence that the same process is working even in these areas. Schlenker (1974), for example, has listed a number of topics within the social and personality domain (e.g., social comparison processes, "social facilitation" effects, status and dominance hierarchies) from which important, broadly applicable principles already seem to be emerging. White's (1959) discus-

sion of effectance motivation provides another example. Senn's (1975) interviewees in several instances suggest durable general principles relevant to the rearing and education of children. And broad cross-cultural analyses (e.g., Hofstetter 1956; Sears 1961) point to a number of possible transcontextual principles in the realm of socialization and personality development, as do Rohrer's (1975) ambitious "worldwide" studies of the effects of parental acceptance and rejection.

### Methodology in the Pursuit of Transcontextual Validity

If it is true that principles of broad transcontextual validity are susceptible of discovery and that they are apt to have both social and scientific value, a difficult question arises: How is one to unearth these broad, general principles and effectively validate them? The appropriate tactic, in my view, involves an interplay between approaches to research that have often been viewed as antagonistic.

*Lab versus field: a simplistic analysis.*—While most developmental research falls somewhere between a purely laboratory and a purely field approach, the distribution appears to be bimodal, with higher frequencies toward the two ends of the continuum (as is the case in most social and behavioral sciences; see Coffield [1970] and Labouvie [1975]). Furthermore, the distinction between these two approaches has definite psychological and historical reality for developmentalists. In the 1930s and 1940s, Kurt Lewin (e.g., 1931) argued forcefully for a "Galilean" approach to lawfulness in human behavior and a tightly controlled experimental methodology patterned after that of the physical sciences. Many students of human development followed Lewin's lead. However, more recently, investigators who share the Lewinian interest in the discovery of broadly applicable developmental principles and the construction of a science of human development have grown concerned over apparent limitations of the laboratory approach. Baldwin (1967) accused developmental researchers of social-learning-theory bent of constructing a "mythology of childhood" in which effects demonstrated in laboratory research are assumed to reflect the actual development of children in real life. In a similar

<sup>1</sup>In fact, the evidence that there are stable regularities in cognitive development is strong enough to have spawned derivative theories—theories that assert their own developmental "universals." Two examples are Slobin's (e.g., 1970) account of grammatical development and Boshé's (1975) developmental account of category formation.

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spirit, Bronfenbrenner (1974, Note 2) has faulted most developmental research for its use of "situations that are unfamiliar, artificial, short-lived, and requiring unusual behaviors that are difficult to generalize to activities in the real world" (Note 2, p. 1; see also Chapanis 1967; Willems & Raush 1969). Bronfenbrenner has argued that, to enhance the generalizability of their findings, investigators should strive for ecological validity, designing experiments which involve settings, time frames, roles, and activities that resemble real (or potentially real) life in its naturally occurring state.

Despite the strengths which Bronfenbrenner rightly attributes to the ecological/field approach, such research suffers from an important weakness: Those very environmental factors which constitute the *sine qua non* of "ecologically valid" research are prone either to obscure the general principle(s) which developmental phenomena have in common across contexts or to leave more than one valid principle commingled so that none appears with the singularity that our limited inferential powers require for discovery or confirmation to occur.<sup>2</sup> It is precisely the task of rendering explanatory principles clear and unmingled that is the forte of the carefully controlled "laboratory" experiment. To illustrate, let us consider the following variation on a familiar analogy.

If I wished to discover principles that govern the falling of objects, I might station myself beneath a tree in autumn and observe falling leaves. Unperturbed by the influence of variations in wind direction and velocity or leaf shape and size, I might determine that the central tendency of the falling leaves was to follow a southerly path 10° off the vertical. Subsequent experiments beneath other trees with different prevailing winds might gradually lead me closer to the truth about how "unadulterated gravity" operates, but this truth might have been quickly discovered in one simple, though ecologically invalid, experiment conducted in a vacuum chamber (a

nonenduring, not naturally occurring setting).<sup>3</sup>

Similarly, in determining (or testing a hypothesis as to), say, whether the concept of identity or of transitivity has developmental precedence, observations in various natural settings of children's behavior with various naturally occurring tasks involving the two concepts (but, inevitably, differing intellectual demands as well) might leave my view of the principle under study obstructed by factors in the social situation and the games themselves, which are of no particular interest to me at the time. In this case, a good deal might be learned rather quickly by means of an artificial setting with the social situation carefully structured and a contrived, short-lived task with demands on such intellectual processes as attention and memory carefully minimized.

Sometimes field research is not appropriate to the task at hand. While the general principle we wish to discover may be a single process which operates in all relevant real-life situations, in each it may be *confounded* with so many uncontrolled factors that the best naturalistic approach available is essentially an averaging of cases (as in watching leaves fall "naturally" from a tree). Averaging is an exceedingly poor approach to scientific inquiry; the frequency of given natural events in any setting is essentially *accidental*, and thus any conclusion derived from an average of observations will be partly a product of chance (see Lewin 1931). In one sense it is true that lab research tells us what *can* happen, while field research tells us what *does* happen (McCall 1977; Sears 1975); yet it must be emphasized that, even at its best, the field approach tells us what *does* happen in one or a few limited settings which will never be exactly replicated in time or space. This fact should help us to put in proper perspective the argument that field research assesses "real life," while lab research taps only an arranged set of circumstances. In fact, both approaches tap arranged, limited, nonreplicable sets of

<sup>2</sup>It is this constraint which our own limitations place on the usefulness of naturalistic/ecological methods that I believe accounts for a problem noted by Zigler (1963). The naturalistic method, he complained, has contributed all too little to the development of explanatory models or theories because it has led to the construction of "explanatory concepts so little removed from the phenomena under study that they are merely shorthand expressions of them."

<sup>3</sup>Of course, even this quintessential laboratory means of enhancing the clarity of observation does not resolve all problems of "explanation" inherent in naturalistic approaches. For example, it would still be possible to account for *why* the leaf falls by reference to a process (gravity) or to input-output relations ("the detaching of leaves" [the independent variable] "causes them to fall" [the dependent variable]). See Bowers (1973) for further discussion of this issue.

circumstances. The fact that one approach samples "arrangements" produced by forces outside the control of experimenters forms the basis for only a very modest claim on "real life."

It is true, of course, that in neither the vacuum chamber experiment nor the psychological lab experiment described above would we learn precisely how the forces under study (gravitational or developmental) appear in specific real life settings where a variety of uncontrolled factors operate, and this is not a trivial limitation. But in both experiments, since our primary goal is to render explanatory principles as clear and as unconfounded with alternative principles as possible, our primary consideration in selecting methodology is control.<sup>4</sup>

What must be recognized is that, while both ecological and laboratory methods have their failings, both possess distinctive merit as well. Ecological or field methods, traditionally favored by those who seek direct answers to contemporary social questions, can reveal whether (and, if so, how) principles of human development operate in the context of selected aspects of naturally occurring reality (and if we seek only to answer specific social questions, this information may often be all we need). The laboratory method, traditionally the favorite of those who seek to promote our scientific development, can provide the means of separating and controlling alternative explanatory factors that is needed if we are to perceive clearly the principles we wish to describe. The exclusive use of either method leaves us with *lacunae*, and Labouvie (1975) may be right—that one's choice of a particular research method is apt to be largely a function of the kinds of uncertainty one is willing to live with. Yet, a thorough search for transcontextual principles of development

would not tolerate uncertainty when means of reducing it are known. Both a clear, relatively unconfounded view of principles under study and cross-validation of these principles in some naturally occurring settings should be components of any claim to transcontextual validity. For this reason, a plurality of methods, integrating the virtues of field and lab research, is indicated.

*Lab plus field—an integrative analysis.*—It is one thing to call for field-lab integration but quite another to explain how to effect it. Several writers (e.g., Sears 1975; Willems 1973) have offered suggestions in this regard (see also Bijou, Peterson, & Ault 1968, for a related proposal). McCall (1977) has suggested that developmental psychologists could learn a good deal from epidemiologists about how to marshal evidence from differing research strategies. In the approach McCall outlines, which resembles Garner, Hake, and Erikson's (1956) strategy of "converging operations," the following reasoning would operate: "If a laboratory study demonstrates that under certain conditions X can lead to Y, if there is a relationship between X and Y in naturalistic settings, if X can be imposed in a naturalistic or quasi-naturalistic environment and it leads to Y, then these several observations coalesce on the tentative proposition that X does cause Y in naturalistic circumstances" (1977, p. 11).

Perhaps the most detailed analysis of how field and lab approaches might interact is that of Parke, Patterson, and Gottlieb (in press), who point out that the labels "lab" and "field" actually subsume several independent dimensions of research which are often regarded as inherently linked. For example, (a) the setting in which the research is carried out (physical location, stimulus field, persons present, etc.), (b) the type of research design

<sup>4</sup>Bronfenbrenner (1974, 1977, Note 2) has argued that the "controlled" lab experiment is not really well controlled or likely to lead to valid findings because it fails to control subjects' reactions to the novelty of the experimental setting, the task, or the experimenter (see also Argyris 1968; Koch 1974). While this argument deserves attention, it is useful to recognize, as do many texts on the philosophy of science, that every event (on both the social and physical worlds) is novel. The essential uniqueness of each event ensures that there will always be some novelty to which children or others involved in research may respond idiosyncratically, regardless of whether the research takes place in an ecological or laboratory setting. Each of Bronfenbrenner's own hypothetical examples of ecologically valid research projects (Note 2) contains the potential for such bias. For instance, a proposed study of the relationship between manipulated income maintenance and parent-child interaction could obviously be affected by family members' apprehension over being observed, their wish to be "faithful subjects," their opposition to being "guinea pigs," or various combinations of such uncontrolled reactions to novel elements introduced by this nonlab investigation. The point: While laboratory research introduces novel elements to which subjects may respond with uncontrolled bias, this fact does not distinguish laboratory research from research labeled ecologically valid. In both cases, there is good reason to work toward improvement in our methods of assessing and controlling the influence of subjects' responses to these novelties.

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(e.g., degree of manipulation and control employed), and (c) the kind of data-collection system (e.g., degree to which "natural" processes are assessed) are all dimensions which can vary independently along a "continuum of naturalness" (Thoman, Becker, & Freese, Note 3). Ideally, the particular level of naturalness versus constraint that an investigator selects for each of these dimensions should be a function, not of an unwavering loyalty to "experimental control" or "ecological validity," but rather of a variety of practical, ethical, and theoretical considerations (for a list see Parke et al., in press).

A simple example may help to illustrate how this selection process can interact with the convergence-of-method approach advocated by McCall (1977). In a recent series of experiments (Weisz, in press), I investigated children's reward preferences when rewards varied along both the material-symbolic and the immediate-delayed dimensions. For theoretical reasons, it was necessary for the research to assess children's choices with the two reward dimensions unconfounded. Since orthogonally dimensional reward arrays are rarely found in real life, it was necessary to offer children a carefully constrained set of reward choices. Thus, on research dimension *c* in the preceding paragraph, a relatively nonnatural data collection system was selected, because theoretical concerns dictated the need for a collection system that could not be found naturalistically. Early in the series, other theoretical and practical considerations dictated the need for the reward-choice procedure to follow a contrived learning task in the presence of only an unfamiliar adult; thus, on research dimensions *a* and *b* above, setting and research design, respectively, the procedure fell toward the non-natural end of the continuum. However, after the theoretical issues which required these nonnatural procedures had been addressed via controlled experimentation, it was deemed important to determine whether the results could be replicated under less controlled, more naturalistic circumstances. Accordingly, a third experiment in the series assessed prize choices of children on Halloween, at a costume party and in homes visited during trick-or-treat rounds. In this procedural shift, both setting and research design were nudged closer to the naturalistic pole of the continuum, in that children were in familiar surroundings, among familiar people, and engaging in an activity (prize selection) which was an expected part of their Halloween fes-

tivities. The fact that this experiment replicated the findings of the less naturalistic pair which preceded it substantially bolstered the transcontextual validity of the findings, despite the fact that the data collection system (orthogonally dimensionalized reward array, with standardized instructions) remained relatively constrained across all three experiments.

This study also illustrates the fact that the locus of the independent variable and the locus of the dependent variable are potentially independent. In discussing "field-lab interface" designs, Parke et al. (in press) note that each cell of the  $2 \times 2$  table formed when one crosses locus of independent variable (field vs. lab) with locus of dependent variable (field vs. lab) defines a research design. Each of the four designs has specific advantages that make it especially appropriate for the investigation of a particular subset of developmental phenomena. For example, the design in which the independent variable is located in the field and the dependent variable is laboratory based is particularly useful when one wishes to gauge the effects of a naturalistic variation or field manipulation on behaviors that have a low base rate of occurrence. In the reward choice study described above (Weisz, in press), I wanted to assess the influence of children's personal investment in the rewarded accomplishment on their reward preferences. Personal investment, a difficult characteristic to manipulate laboratory style, was defined in terms of whether children had participated in designing or making the Halloween costume for which they were being rewarded. Children's participation, or lack of it, in costume production was a field-based independent variable, useful because its validity appeared to surpass that of alternative lab manipulations. The child's reward choice was a lab-based dependent variable, useful because opportunities for children to choose from orthogonally dimensionalized reward arrays have a "low base rate" of natural occurrence.

In summary, both lab and field approaches are useful tools in the pursuit of transcontextual principles. The respective strengths and weaknesses of the two approaches argue against the exclusive use of either. Instead, transcontextual validity can be best demonstrated when findings of lab and field research converge at a common principle. Perhaps the most promising development in this regard is the growing recognition that



the multidimensionality of the terms "field" and "lab" permits a variety of useful integrative designs, each with its own combination of naturalism and control.

*Beyond the lab-field debate.*—In addition to balancing naturalism with control, the investigator pursuing durable developmental principles faces other challenges related to design. The "general developmental model" proposed by Schaie (1965) represents a challenge to researchers interested in principles that *explain* "developmental" change—a challenge to disentangle developmental, secular, and generational change. Researchers interested in principles that *describe* developmental change confront a challenge in the form of Baltes's (1968) bifactorial model (see Schaie & Baltes 1975).<sup>5</sup> The complex designs advocated by Schaie and by Baltes have been integrated with cross-cultural designs by Eckensberger (1973), who proposes procedures for teasing apart the effects of development, secular change, generational change, and naturally existing cultures, and artificially created cultural groups. McCall (1977) may be right, that such elaborate proposals have adverse effects, in defining a "proper" longitudinal methodology that is "beyond the time and financial means of our discipline."<sup>6</sup> However, it is often useful to think in terms of ideals, and the ideal reflected in Eckensberger's proposal may, in fact, be an appropriate concluding theme for this section; for, in a general sense, it is precisely through comparisons of developmental levels, cohorts, times of testing, and cultures (both natural and artificially contrived) that we are likely to recognize the most enduring developmental principles.

### For Society and Science: Two Mutually Useful Goals

How successful have we been in our pursuit of these transcultural principles? It is difficult to say, in part because the ongoing process of refinement described earlier leads to uncertainty as to which principles will last, and in what particular form. It is equally difficult to say whether we are developing adequately as a science, because there are no

adequate standards for comparison (see Bromley 1970). To put the problem in perspective, however, let us pose a companion question: Are we making adequate progress in meeting the contemporary needs of society? Clearly, *this* question is not easier to answer. What can be done at this point is to suggest means by which both our scientific development and our contributions to society may be enhanced.

*A science of translation.*—A need that is recognized by investigators and policy planners alike is for a set of principles to direct the conversion of developmental investigators' truths into truths about social policy (see Marian Radke Yarrow's comments in Senn [1975]). Perhaps we require an algorithm A, consisting of guidelines for the translation of general developmental principles into broad sets of boundaries within which policy ought to fall, and an algorithm B, consisting of guidelines for the translation of specific empirical statements into statements about specific policy alternatives. Such a development might provide a bridge to span the gap between the world of the developmental fact and the world of the political decision. It might also serve to debunk the myth that broad, general principles are without social relevance.

*A science of moderate complexity.*—Social scientists of nearly all persuasions have been criticized for dealing with only the simplest aspects and using only the simplest models of reality. Riegel (1972) has chided developmentalists for focusing on "mechanistic rather than dialectic" considerations. Bronfenbrenner (Note 2) has called for a conception of development which attends to "the progressive, *mutual accommodation over time* between the growing human organism and its environment," and a conception of the environment which attends not only to the immediate setting but also to higher-order influences (e.g., political ideologies) which impinge upon the person. In a similar spirit, McGuire (1973) maintains that the future of social psychology holds a shift away from simplistic linear models of one-directional causality and the companion distinction between dependent and independent variables. It is likely that, as the study of human development grows in sophistication, it will move over

<sup>5</sup>In a useful complement to the work of Schaie and Baltes, Goulet, Hay, and Barclay (1974) have discussed the existence of short-term, cyclical phenomena which may interfere with the identification of "true" developmental, cohort-, and time-related change when sequential research strategies are used.

<sup>6</sup>McCall notes, for example, that a design proposal by Schaie (1965), if applied to the Berkeley Growth Study (with yearly assessments through age 40), would require more than 5,500 subjects and 81 years to complete!

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closer to the sort of paradigm which Bronfenbrenner and McGuire envision, with multivariate models to explain bidirectional relationships, feedback loops, parallel processing, and other complex phenomena. Such movement can only be welcomed, since there can be little doubt that the reality within which each individual develops is more complex than the models we now use to represent it.

However, caution is in order, because there is almost certain to be a point of diminishing returns at which further increases in the complexity of what we report will only diminish our usefulness. Cronbach (1975) has described the "hall of mirrors" we enter as we begin to attend to interactions, with the prospect of third- and fourth-order interactions giving way to those of ever higher order. At the end of this hall of mirrors we are apt to find a purely idiographic enterprise. We need to recognize that seeking common denominators of human development is an important task—that it is primarily the discovery of unifying principles that makes it possible to reduce uncertainty.

Yet, the models of reality which we now use may well be simpler than necessary. And even now, within our humble efforts to trace and explain developmental processes, one can detect the faint hints of pure tone that will lead us toward a paradigm within which we can capture more of the true multidimensionality and multidetermination of human development. As we approach such a paradigm it may well be that, as McGuire implies, differences between explanatory models (and hypotheses and predictions) oriented toward immediate social relevance and those oriented toward transcontextual regularities will diminish.

In the meantime, however, let us agree that there is room in the developmental arena for the pursuit of both immediate social relevance and transcontextual validity, and let us recognize that the resulting cross-pollination can enhance our corporate fecundity. Our discipline is at its best in a state of creative tension—between the pursuit of direct social relevance and the quest for firm scientific principles. The two pursuits may often (though not inevitably) be carried on by different investigators, using somewhat different methods and oriented toward different values. The one pursuit may contribute primarily to the immediate well-being of contemporary society, the other to the emergence of a developmental science with long-term benefits

to society. But it is essential that proponents of these two complementary pursuits engage in their work with mutual respect and that neither seek to impose gratuitous limits on the scope of developmental inquiry.

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