COMMENTARY ON CHAPTER 7:

The "What" and "How" of Development: Really Two Separate Research Agendas?

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As already pointed out by Lightfoot and Folds-Bennett (this volume), the participants in the Bernried meetings had different and often conflicting opinions about many issues concerning human development. Among these issues, the distinction between developmental processes and developmental mechanisms and its implications for developmental research attracted special interest, leading to both stimulating and controversial discussions. If my recollections of those debates are correct, the majority of participants took the position that the distinction between process and mechanism has important implications, a view elaborated by Lightfoot and Folds-Bennett (Chapter 7). A stable minority, however, including myself, saw the distinction as presenting a conceptually complicated and confusing problem. In particular, these participants felt uncomfortable with the term *mechanism,* a word that is derived from the notion of mechanics and that suggests a decontextualized model of the human mind analogous to a machine with moving elements (see also Sigel, 1986). These participants argued that comparably neutral terms like *developmental transitions* or *sources of developmental changes* seem better suited to address the issue of explanation.

Admittedly, the controversy concerning terminology—although interesting from a theoretical point of view—addresses only a minor

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problem when compared to the issue of judging the importance of the distinction between (descriptive) processes and (explanatory) mechanisms for developmental research and theory. This is the topic Lightfoot and Folds-Bennett have chosen for their interesting and provoking essay. When reading through the chapter, I was immediately reminded of the discussions in Bernried: Similar to the debates at the monastery, the ideas expressed in the chapter very much stimulated my thinking about issues of cognitive development. However, they also generated some confusion on my part. Given the amount of stimulation/provocation provided by this chapter, I thought it helpful to comment on its major points.

Before discussing the issues in detail, I will first outline what I see as the most important assumptions treated in Chapter 7. These can be summarized as follows: (1) Like many leading developmental theorists, the authors assume that the question of explanation is more important than the question of description. Accordingly, future research should give more emphasis to the issue of developmental mechanisms. (2) Traditional developmental methodology based on ANOVA models cannot achieve this goal: Because the available methods focus on the analysis of group means, they may lead to incorrect inferences concerning developmental changes in individuals. Instead of using "interindividual" methods, future research should focus on methods of "intraindividual" or "genetic" analysis. (3) Today's research can profit from looking at the past: For example, Lewin's experiments based on his field-theoretical approach and Vygotsky's studies on concept formation (strongly influenced by Piaget's "clinical method") seem well-suited to give deep insights into the "lawfulness" of children's development. (4) *Conclusion:* Issues of developmental processes and developmental mechanisms require different methodologies; they should be kept distinct.

In the remainder of this chapter, I will elaborate on these four major points.

Should Our Research Focus More on the "Mechanisms" Issue?

At first glance, the answer to this question seems clear. There is little doubt that explaining development is more important than simply describing it. Like Lightfoot and Folds-Bennett, I fully agree with Flavell's (1984) contention that the question of explanation should be

"the ultimate objective of any science" (p. 188). Unlike Lightfoot and Folds-Bennett, however, I am not convinced that the state-of-the-art of developmental theory requires a particular emphasis on mechanisms, at least not of the sort described in recent models of cognitive development.

Why this negative attitude? In my view, a careful reading of the volume edited by Sternberg (1984), devoted to the exploration of mechanisms of cognitive development, elicits some skepticism concerning the fruitfulness of this enterprise. First of all, it is surprising to see that there is little overlap among the six models of cognitive development presented in this volume (cf. Sigel, 1986). As Flavell (1984) put it: "Whenever one sees six different theories supposedly trying to explain the same thing, one should suspect that Truth is not yet at hand!" (p. 190).

A second problem is that most theoretical approaches operate at a high level of abstraction, making a large number of "bold and imaginative claims" (Flavell, 1984) about various aspects of cognitive development. In principle, there is nothing wrong with trying to capture the "big picture" of human development, a goal that most of these theories try to reach. Given the high level of abstraction, however, it is difficult to see how the theories could be falsified by experimental test.

Third, and most important in the present context, the basic concept of developmental mechanism is never directly addressed in the various theoretical approaches (cf. Sigel, 1986). The large collecion of "mechanisms" includes transformation rules, differentiation, coordination, hierarchical integration, discrimination, encoding processes, equilibration, and adaptation. Do these terms indeed denote explanatory concepts? Let us take Piaget's concept of equilibration as an example. Aebli, one of Piaget's most prominent European students, has serious doubts in this regard (see Aebli, 1984). In his view, Piaget's (unproven) core assumption is that children typically strive for consistency in their thinking. Whenever children experience a state of imbalance, they try to overcome it, a process called "equilibration." According to Aebli, the term *equilibration* does not provide an explanation for developmental change. While it indicates *that* consistency was reestablished, it does not tell us *how* this might happen.

Unfortunately, this is true for most concepts considered as mechanisms in developmental theory. As emphasized by Sigel (1986), the mechanisms do not inform us about what governs developmental change. For example, to what extent is change controlled by

children's social interactions? Or what is the impact of biological/maturational components? We need to know much more about the various sources of developmental change and their interrelations at different age levels.

Does this mean that we should intensify our search for more salient developmental mechanisms, as Lightfoot and Folds-Bennett suggest? Although this must be an important goal in the long run, I do not think that current research should focus solely on issues of explanation. In my view, problems of generalizability inherent in many contemporary models of cognitive development stem from the fact that the available data base is still weak. Despite the large number of empirical studies on numerous issues in cognitive development, the number of robust, cross-validated findings is comparably restricted (see van der Veer, van IJzendoorn, & Valsiner, in press, for a discussion of the problem of replicability). Thus more emphasis should be given to a careful *description* of developmental changes over the life span. I do hope that an emphasis on systematic data collection will eventually enable us to come up with low-level inferences about mechanisms of developmental changes that are better founded than many of the high-level "explanations" offered in contemporary developmental theory. But do we really need a different methodology to accomplish this goal? This is what I want to discuss in the next section.

How Bad Is Our Developmental Methodology?

As briefly mentioned above, Lightfoot and Folds-Bennett believe that current problems of the state-of-the-art of developmental theory are closely linked to the use of ANOVA models in data analysis. Because ANOVA models focus on group means, they are of limited utility in understanding the development of individuals.

I agree with the authors that a focus on ANOVA models restricts our knowledge about individual differences and their changes over time. Along with Lightfoot and Folds-Bennett, I also believe that ANOVA models still dominate developmental research. In my view, however, a more serious shortcoming of traditional developmental methodology is that it has been dominated by cross-sectional analyses, comparing different age groups at one point in time. While many have voted for longitudinal designs, only a few have actually used

them. When Lightfoot and Folds-Bennett raised the issue that we need "intraindividual methods," I first thought they were referring to longitudinal methodology. A more careful reading of the chapter, however, revealed that I was wrong in this regard. The authors distinguish between two conceptual models of developmental analysis—the analysis of developmental functions and the analysis of individual differences—claiming that both approaches employ longitudinal designs that are essentially *interindividual*.

Frankly, I have problems with this claim. I still believe that one of the major advantages of longitudinal methods over cross-sectional designs is that they allow for *intraindividual* analyses in addition to interindividual comparisons. In particular, recent developments in growth curve modeling such as the Hierarchical Linear Modeling (HLM) procedure developed by Bryk and Raudenbush (1987; see also Chapter 4) represent a nice example for the progress we have made in this regard: HLM not only gives an estimate of (intraindividual) growth curve parameters but also provides a statistical procedure that tries to "explain" the variation found among intraindividual growth curves. Despite the various conceptual and methodological problems of longitudinal studies (for overviews see Rogosa, 1988; Schneider, 1989), there is no doubt that we have made considerable progress regarding the design and analysis of developmental studies over the last few years. As a matter of fact, we have overcome many of the disadvantages linked to the (cross-sectional) ANOVA methodologies alluded to by Lightfoot and Folds-Bennett.

What Can We Really Learn From the Past?

As you may well imagine, I was curious to learn more about the type of "intraindividual analysis" that Lightfoot and Folds-Bennett conceive of as "the king's road" for the explanation of developmental change. In order to clarify their position, the authors refer to the theoretical framework elaborated by Lewin and by Vygotsky, focusing on an experimental method called "genetic analysis." Because this term has several connotations in the literature, I discuss the major characteristics of the experimental approaches used by Lewin and Vygotsky before judging their importance for the explanation of developmental changes.

LEWIN'S VERSION OF "GENETIC ANALYSIS"

Lewin's "detour problem" is presented as an illustrative example for genetic analysis within the field theory paradigm. In this task, a child must navigate around two different types of barriers in order to reach a piece of chocolate (see Figure 7.2). As Lightfoot and Folds-Bennett point out, young children typically have more problems with the type of barrier that requires initial movement in a direction opposed to the location of the chocolate. In Lewin's terminology, this means a movement away from the "positive valence" associated with the piece of chocolate: Solution to this problem requires a "psychological restructuring" of the problem situation in that the first movement, although away from the "valence," becomes the first of several steps toward the chocolate. According to Lewin, successful performance in this task depends on the child's age (older children perform better than younger ones), on the strength of a valence (the stronger the valence, the more difficult the task), and on the ability to restructure the task as a set of related movements rather than a series of separate movements.

Undoubtedly, this is an interesting experimental task. It is difficult, however, to see how this "genetic analysis" differs from more modern approaches used in experimental methodology. One possible difference may be that Lewin operated with theoretical concepts like *field force, valence,* or *psychological restructuring* that he took for granted, whereas modern experimenters probably would like to make these concepts more accessible and testable in their experimental designs.

For example, Lewin's claim that performance in the detour problem task varies as a function of the strength of valence could be easily tested by varying the degree of attractiveness of the target. Lewin's theory would be confirmed if even older children fail to solve the more difficult barrier problem when the target is very attractive to them. It would be falsified, however, if we found a positive correlation between degree of target attractiveness and successful task solutions, regardless of age (i.e., the more attractive the target, the higher the percentage of successful solutions).

In my view, Lewin's experimental procedure cannot be conceived of as qualitatively different from modern experimental methodologies. The major difference concerns terminology: Modern problem solving theories would probably avoid Lewin's rather abstract explanations of successful task performance like "an understanding of the

difference between physical and psychological realities" or "a successful construction of the life space" in favor of more concrete, observable behaviors. Thus it is really difficult for me to see how Lewin's approach of genetic analysis can lead to the detection of general "developmental mechanisms." The explanatory value of a term such as *psychological restructuring* seems similarly restricted as the Piagetian concept of *equilibration.*

VYGOTSKY'S VERSION OF "GENETIC ANALYSIS"

Lightfoot and Folds-Bennett use Vygotsky's studies of concept formation based on his "method of double stimulation" as an example for his version of genetic analysis. This method involved presenting subjects (i.e., children, adolescents, and adults) with two sets of stimuli. For example, children were given a number of wooden blocks that could be sorted according to features such as color, shape, height, and size. The underside of each block, not seen by the subjects, contained one of four nonsense words. Children were asked to select all blocks that might be of the same kind. Whenever a block was "wrongly chosen," the experimenter turned over the block, indicated that it had a different word on it, and asked the child to continue sorting. The experiment consisted of a sequence of child sorts and experimenter corrections, until the child correctly sorted the blocks as indicated by the nonsense words. As a main result, Vygotsky found that the ability to regulate one's action by auxiliary means (i.e., the use of nonsense words) was not fully developed before adolescence where "all existing functions are incorporated into a new structure. . . , become parts of a new complex whole." (Vygotsky, 1934/1986, p. 108).

What is the essentially new information about developmental changes in children's concept formation that we can derive from Vygotsky's approach? According to Lightfoot and Folds-Bennett, the core message is that "the new complex whole assembled during adolescence forms the basis of all higher mental functions; activity becomes sign-mediated as cultural mental functions supersede natural mental functions in the determination of behavior." In my view, however, we do not learn much about "natural mental functions" in Vygotsky's experiment, mainly because the method of double stimulation forces children to adopt rules provided by the experimenter. It is difficult to see how this methodological approach fits with the

basic theoretical assumption of interdependence between developing individuals and their environments, a claim inherent in both Lewin's and Vygotsky's theories.

As emphasized by Lightfoot and Folds-Bennett, Vygotsky's approach was strongly influenced by Piaget's "clinical method," a procedure highly regarded even in contemporary textbooks of developmental psychology. This method, which involves first confronting children with a problem solving task, then urging them to come up with a solution (judgment), and finally asking them to justify their judgment, has been assumed to be an elegant procedure to explore the way children make use of their existing "cognitive structures." However, critics like Aebli are not convinced that the clinical method tells us much about children's problem solving in everyday situations. At about the time when Piaget's theory was introduced into American psychology, Aebli (1963) published his critical analysis of the Piagetian procedure. In his view, the clinical method leads to instable, ad hoc productions that are of no relevance for children's decision making in everyday problem solving situations. In subsequent experiments conducted by Aebli and his coworkers (cf. Aebli, 1984; Riesen, 1988), it indeed could be shown that children's performance in Piagetian conservation tasks does not predict their behavior in similar everyday problem solving situations. For example, when confronted with the problem that there was not enough tea available in a glass that should be given to a sick child, all of the children who did not master Piaget's conservation task (i.e., who believed that transferring liquid from a broad, low glass into a thin, high glass will produce more liquid) recommended adding new tea in order to help the sick child; none of the children spontaneously suggested that the experimenter should pour the tea from the glass into a (thinner and higher) tea bottle in order to produce more tea for the sick child. Of course, most of these "nonconservers" found the experimenter's idea of pouring the tea from the glass into the tea bottle helpful in order to improve the sick child's situation. However, when asked to decide between the options of either transferring the tea from the glass to the bottle or to add a small amount of tea to the liquid already in the bottle, almost all of the children preferred the latter possibility.

If Aebli is right—and there is reason to agree with his position—using the clinical method does not give us much insight into the relationship between the child and its environment. It is difficult to imagine that Piaget, Lewin, or Vygotsky were able to create experi-

mental test situations that suitably mirrored the "constructive opportunities" afforded by children's everyday-life, natural environments. This is not to say that these great researchers were unable to stimulate our thinking about human development. Undoubtedly, the opposite is the case. What I doubt is that their experimental methodology gets us to the ambitious goals Lightfoot and Folds-Bennett have in mind.

Thus I do not see any new feature in this type of intraindividual analysis that overcomes the basic shortcomings of traditional experimental research. Like most contemporary experimental paradigms, the "genetic method" does not attend to the child's perspective regarding the task. Rather, the child is viewed as a passive respondent faced with tasks of relevance to the experimenter (cf. Sigel, 1986). In order to better understand the outcomes of experimental tests, it seems important to know more about children's constructions of the situation and their interpretation of this experience.

Do We Really Need Different Agendas for Studying "Descriptive" Processes and "Explanatory" Mechanisms?

As the reader may have inferred from the previous section, I am not convinced that searching for "general laws of development" or general "explanatory mechanisms" will benefit much from the "genetic method" described above. Actually, I do not believe that searching for rather global developmental mechanisms constitutes a promising research strategy for the future. The research findings presented in recent books and journal articles on issues of cognitive development suggest that there is much more variety, domain-specificity, and complexity in cognitive development than indicated by the six theories presented in Sternberg's book. As noted by Flavell (1984), there is more variety in *what* gets developed and also more variety in *how* developmental changes get accomplished. Accordingly, we may be obliged to devise specific theories for specific transitions.

How do we get to such theories? In my view, approaches such as the hierarchical linear modeling procedure devised by Bryk and Raudenbush (1987), based on the analysis of individual growth curve parameters does have the potential to promote our understanding of interindividual differences in intraindividual developmental changes for a wide range of domains. Another promising methodological approach—although probably not similarly suited for complex statistical

analysis—is what Siegler and Jenkins (1989) have labeled the "microgenetic method." The two key properties of microgenetic methods are that (1) the same subjects are observed over an extended period of time, and (2) their learning is subjected to intensive trial-by-trial analysis, with a goal of inferring the underlying processes that gave rise to both qualitative and quantitative aspects of learning (cf. Siegler & Jenkins, 1989, p. 9). One of the major advantages of this approach over traditional longitudinal methods is that the time intervals between measurement points are very small, thus allowing for the assessment of transitional periods (e.g., the identification of the exact point at which a new behavior was first shown). Siegler and Jenkins demonstrated the utility of this approach for studying strategy construction in young children, showing that careful observation and description of the children's problem solving activities over many trials led to the identification of a set of transitional processes preceding the discovery of a counting strategy.

In my view, this approach combines intra- and interindividual analysis in that it is based on a collection of single-case studies, enabling the researcher to compare individual developmental paths in order to detect commonalities that can be conceived of as more general transition rules. Although the microgenetic method entails certain disadvantages because it is expensive and based on small sample sizes, it not only provides us with a detailed picture of the idiosyncracies of individual performance, but may also allow low-level inferences about "developmental mechanisms" better suited for "explaining" behavioral changes than the global conceptualizations predominant in current developmental theories.

As I have emphasized before, I was very impressed by the way Lightfoot and Folds-Bennett handled the very complicated issue of processes and mechanisms of cognitive development. Their chapter helped in stimulating my thinking about this issue in several ways. It is mainly due to space restrictions that I have focused on those points where we have different opinions. Obviously, the main difference between our positions is that Lightfoot and Folds-Bennett ask for different research agendas in order to explore descriptive processes versus explanatory mechanisms, whereas I believe that careful and sophisticated experimental (longitudinal) research can help in reaching both goals. Thus my conviction is that we do not need different methodologies but more fine-grained, domain-specific analyses than before in order to move from descriptions of development to the detection of

transitional mechanisms. As several longitudinal studies on cognitive development are currently conducted, we soon may be in a position to evaluate this claim.

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