CHAPTER 19

The Role of Knowledge, Strategies, and Aptitudes in Cognitive Performance: Concluding Comments

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Several years ago, a conference was organized with the aim of identifying the merits and shortcomings of current research on memory development. In the resulting publication by Weinert & Perlmutter (1988) the contributors agreed that despite the progress that has been made in understanding children's memory, much remains to be learned about the natural course of memory development. As apparent from the book's organization, strategies, metamemory, and the knowledge base were conceived of as major determinants of memory development. Further, the roles of social and motivational contexts as well as implications of individual differences were discussed in detail.

Typical of more recent research is a shift in emphasis from the separate study of determinants of memory development to the analysis of possible interplay among strategies and knowledge in memory performance (cf. Bjorklund, in press; Schneider & Pressley, 1989). Given the enormous interest and intensive research activities in the field since the mid-eighties, one major reason for organizing a second conference on the topic only a few years later was to get a better picture of the kind of progress that has been made recently in the area of memory development.

Another goal of the second conference was to broaden the perspective on research in the domain of memory development by including research on interactions among knowledge, strategies and aptitudes conducted in areas of cognitive development other than memory. In our view, memory processes represent a specific class of general problem-solving activities. Accordingly, the issue of generalizability of findings from memory research to thinking, reasoning, and comprehension processes in other problem-solving tasks deserves special attention.

In the following, we will use the state-of-the-art represented in the above book as a frame of reference for our comments. In doing so, we will first focus on a few general issues that were already addressed as potential problems in the mid-eighties and seem similarly controversial today. Next, we will turn to some theoretical issues and empirical findings that, at least in our view, demonstrate recent progress. Basic agreements and (possible) disagreements which were unveiled during the conference and are reflected in this book will be high-

lighted. In closing, this chapter will discuss issues that, in our opinion, have been thus far neglected and should be addressed in future research.

Persistent Problems

From the very beginning of the conference, we realized that controversy still remained over several conceptual issues already discussed intensively during the earlier meeting. In our view, this is mainly due to the fact that we are not dealing with precisely defined psychological constructs but with a collection of "fuzzy" concepts: Terms like strategies, domain-specific knowledge, or metacognitive knowledge all have in common that they denote ill-defined categories (see also Ornstein, this volume).

According to Wellman (1983), there are four basic definitional features of fuzzy concepts:

> First, the concept encompasses an essential, central distinction. However, this distinction serves to anchor the concept, not intentionally define it. Second, prototypic central instances of the concept are easily recognized. However, third, at the periphery, agreement as to whether an activity is legitimately metacognition breaks down; the definitional boundaries are truly fuzzy. Related to this, and fourth, different processes all of which partake of the original distinction may be related only loosely one to another. Thus the term metacognition or metamemory serves primarily to designate a complex of associated phenomena. (pp. 32f)

We would like to add that this characterization of the fuzzy concept metacognition can be easily generalized to concepts like cognitive strategies or the knowledge base. Regarding the complex concept of metacognition, persistent problems are associated with the frequent difficulty in distinguishing between what is meta and what is cognitive, and with the concept's usage in referring to two distinct areas of research, namely, *knowledge about cognition* and *regulation of cognition*. While more recent models of metacognition (e.g., Borkowski & Turner, this volume) represent progress in that they theoretically link components of metacognition with strategies, the two sources of confusion continue to exist. For example, the interchangeability of cognitive and metacognitive functions is particularly evident in recent reviews of text processing (cf. Baker & Brown, 1984; Garner, 1987). As to the distinction between metacognitive knowledge and the regulation of cognition, we intuitively assume that both components of metacognition should be closely related. However, testable models specifying the interplay between metacognitive knowledge and the regulation of cognition for a variety of problem-solving tasks are still lacking. Given these persistent problems, it seems appropriate, even now, to refer to components of metacognition as "mysterious mechanisms" (Brown, 1987; Brown, Bransford, Ferrara, & Campione, 1983).

In our view, a similar appraisal applies to the numerous conceptualizations of knowledge found in the contemporary literature. The long list of knowledge components addressed in the cognitive development literature (and also in this book) includes terms like semantic knowledge, episodic knowledge, conceptual

knowledge, the knowledge base, world knowledge or epistemic knowledge, content knowledge or domain-specific knowledge, procedural and declarative knowledge, and so on. Given this impressive flexibility and inconsistency in terminology, there is no doubt that even experts in the field get confused about possible denotations and connotations of the concept. For example, how should we conceptualize the difference between the knowledge base and world knowledge (if there is any)? What about differences in the representations of conceptual knowledge and the knowledge of content domains? Please note in this regard that the term "domain" has also been used inconsistently by researchers (cf. Ceci & Nightingale, this volume). Again, we would like to argue that the state-of-the-art has not changed considerably during the past 5 years, although much more research has been carried out on this topic. We do concede that most contributors to this volume (more or less) explicitly refer to some type of underlying semantic network model when considering the aspect of knowledge representation. We also acknowledge that recent reviews on the role of conceptual knowledge and content knowledge in memory development provide in-depth analyses of the mechanisms by which knowledge mediates memory performance (e.g., Bjorklund, Muir & Schneider, in press; Chi & Ceci, 1987; Muir & Bjorklund, this volume), leading to insights certainly not common in the mid-eighties when the earlier conference took place. However, we believe that some of the problems raised by Ornstein and Naus (1985) have not yet been adequately addressed. That is, we still see the necessity to define knowledge more specifically and to chart its development in a more systematic fashion. It not only seems important to find a generally acceptable definition of the term "domain," but also to deal seriously with the issue of how to characterize children's knowledge of specific domains within a developmental perspective. For instance, how is knowledge structured and changed within the memory system? Are there prefabricated mental structures, or should we assume an hierarchical system of multiple representational facets of information which can be reconstructed by some classes of available control processes?

During the course of this conference, we soon realized that the issue of strategy definition has remained controversial. About 5 years ago, several heaty discussions concerned the issue of whether strategies are deliberately instigated (i.e., voluntary). While some researchers (e.g., Paris, 1988a; Paris, Newman, & Jacobs, 1985) offered definitions retaining an intentionality attribute in strategies, others defined strategies as *potentially* conscious and controlable activities, ones that are nonetheless sometimes carried out automatically and unconsciously (cf. Pressley, Forrest-Pressley, Elliot-Faust, & Miller, 1985). Strube (this volume), in his discussion of the contribution by Ceci and Nightingale, Hagendorf, Kluwe, and Siegler, explicitly points to the inconsistency in terminology he notices in surveying the chapters. In his view, the crucial problem concerns the proper definition and reference to automatic versus controlled processing. From his own research on retrieval strategies, Strube is lead to believe that most complex strategies are open to automatization, a view also shared by van der Meer (this volume).

If we interpret the message of most of the chapters in this volume correctly, then strategies should be conceived of as goal-directed processes which may involve automatic, nonstrategic operations, but which are potentially available to consciousness. As Bjorklund, Muir, and Schneider (in press) noted, such a definition is not without problems, because consciousness is itself a sticky issue. More specifically, the requirement that strategies be potentially available to consciousness leads to biased observation, since such strategies would be noticed more in older children and adults than in younger children. Thus, Bjorklund et al. emphasize that the stipulation on children of any age (particularly young children) to prove they are aware of what they are doing before one can declare them strategic is a serious shortcoming of such a definition. Hence, it appears that we are still far away from a strategy conceptualization that is unreservedly acceptable to the majority of cognitive psychologists. However, the lesson learned from the two conferences is that, regardless of how strategy is defined, the definition should be explicit to avoid fundamental misunderstandings.

Major Accomplishments

In spite of the various persistent conceptual problems mentioned above (which have certainly complicated our discussion), it was not particularly difficult to locate areas where considerable progress has been made during the past few years. As it is thoroughly impossible to discuss all the relevant issues raised in this volume, given the constraints on space, we have selected a few particularly interesting and worthwhile topics for more detailed scrutiny. In our view, major accomplishments during the second half of this decade relate to (1) early strategy–knowledge interactions, (2) the development of comprehensive cognitive or metacognitive models representing strategy knowledge interactions, (3) the systematic inclusion of motivational, educational, and cultural factors in the analysis of strategy-knowledge interactions, and (4) an increasing tendency towards explicitly considering individual differences and aptitude issues in the analysis of these interactions.

The Interplay Between Knowledge and Strategies in Young Children

Our opinion about how competent and strategic young children can be has changed considerably during the past 20 years. In the 1970s, children younger than age 6 were thought to be generally nonstrategic in cognitive endeavors. Research conducted during the late seventies and early eighties was effective in changing this view dramatically. With respect to early memory development, Wellman (1988) gave an impressive account on preschoolers' early memory strategies, emphasizing that memory activities in young children are strategic and mnemonic, that young children's memory strategies are variable and frequently employed, and that they can have significant impacts on memory performance. Wellman referred to the importance of asking broader questions

about the development of memory strategies like, where do effective strategies come from, or how are they developed. In speculating about possible explanations for early strategy developments, Wellman pointed to the importance of initially faulty strategies; the assumption was that effective strategies evolve directly from earlier faulty strategies. Note that young children's knowledge base was not explicitly considered in this explanatory approach.

In our view, the contributions by Perner (this volume) and Sodian (this volume) go above and beyond the explanatory approach offered by Wellman (1988) in that they additionally emphasize the conceptual basis for young children's strategy development. Much of what is known about this issue stems from an active research program dealing with young children's acquisition of a *theory of mind*, that is the acquisition of a set of explicit and interconnected concepts for representing mental states (cf. Olson, Astington, & Harris, 1988).[1]

Perner's (this volume) conceptualization of "experiential awareness" is strongly influenced by a theory of mind approach. The basic assumption is that young children's understanding that informational access leads to knowledge should influence young children's episodic memories, in Tulving's sense. By using a slightly rephrased definition of episodic memory ("episodic memory mediates the remembering of *personal experience of events*"), Perner predicts that great improvement in free recall (unparalleled in *semantically cued* retrieval tasks) should be found as soon as young children develop experiential awareness. If this view can be supported by empirical data—supplementing and extending the preliminary findings presented by Perner—it would mean that a type of basic metacognitive knowledge seems crucial for early memory development.

In a similar vein, Sodian (this volume) illustrates the importance of young children's theory of mind for the development of cognitive strategies. Her research supports the conclusion that "theory changes" in children's intuitive epistemology (understanding the conditions of knowledge formation) provide the conceptual base for developmental changes in strategy use.

In sum, then, the contributions by Perner and Sodian both indicate that early memory development may be strongly influenced by the acquisition of knowledge components. Although these assumptions have not yet been adequately tested, and specific problems still need to be addressed (cf. Bullock, this volume), the two chapters give account of recent progress in the area of early cognitive development, pointing to fruitful directions for further research.

Modeling Strategy–Knowledge Interactions

While the importance of the role played by one's knowledge base for remembering was already demonstrated in the late 1970s, actual evidence of the interaction between strategy development and the knowledge base was not available

[1]We hasten to add that Henry Wellman is particularly active in this research area.

until the 1980s (cf. Chi & Ceci, 1987). With regard to developmental trends in strategy–knowledge interactions, controversial issues until the mid-eighties frequently involved questions of predominance: Although most researchers basically agreed that both the knowledge base and strategies influence cognitive performance, they disputed about the relative importance of these two factors (cf. Bjorklund, 1985; Ornstein & Naus, 1985).

In our view, the contributions to this volume indicate that debates concerning this specific issue can no longer be regarded as particularly fruitful for development of the field. As a matter of fact, researchers committed to a knowledge base approach concede that the imbalance created in their reviews of the field is intentional and aims at countering the traditional view that cognitive development can be largely conceived of as the development of strategies and metacognition (cf. Chi & Ceci, 1987). The relative importance of knowledge versus strategy factors may depend on the type of problem-solving task, the age of the problem solver, and many other factors (cf. Brown et al., 1983 for a nice illustration of the complexity of this issue).

On reviewing the chapters of this book we have reached the basic agreement that a model indicating a feedback loop or the bidirectionality of influences seems to be the preference of most authors. Their assumption is that specific knowledge affects the acquisition of strategies, and that strategy deployment in turn influences the construction of specific knowledge. As Hagendorf (this volume) points out, cognitive strategies and domain-specific knowledge are best viewed symbiotically, a view he thinks can be easily generalized from memory research to the development of problem solving and perception.

In our opinion, the research presented within this general framework leads to several interesting insights. For example, the issue of how specific knowledge influences choices among competing strategies is not only new but also theoretically challenging (cf. Siegler, this volume). The new message is that strategy use cannot be seen as a group-level phenomenon, indicating that children of a specific age consistently prefer a specific strategy over others, but that individual children may use multiple strategies when solving identical problem-solving tasks. This finding seems striking in view of the large body of empirical evidence from training studies showing that children have difficulties with discovering metacognitive information as they work with strategies (cf. Ghatala, Levin, Pressley, & Goodwin, 1986; Pressley, Levin, & Ghatala, 1988). According to these studies based on memory research, children are less likely than adults to discover that one of two alternative intentional strategies can better facilitate a particular type of required performance. Siegler's findings suggest that this metacognitive bottleneck is often bypassed with potential strategy choices in familiar domains not influenced much by explicit, metacognitive processes but rather functioning largely through a simple associative learning mechanism.

Even more provocative is that Siegler's discovery was mainly based on young children's self-reports. While a single-strategy-use model fit the data well, a multiple-strategy-use model relying on children's reports fit the data

even better. This finding indicates that the large body of literature questioning the veridicality of verbal reports as data may somewhat overstate the case, at least when children are concerned (cf. Ericsson & Simon, 1980; Nisbett & Wilson, 1977). Siegler's results show that relying solely on derived measures of strategy use may not always give a true impression, a finding that has also been demonstrated in the case of organizational strategies (cf. Schneider, 1986). Finally, Siegler's results point to a problem in model testing procedures often neglected in empirical research; i.e., the fact that a model fits the data well does not exclude the possibility that an alternative theoretical model may fit the data significantly better.

As indicated by Siegler, formal modeling approaches seem important for our better understanding of the strategy–knowledge interactions. Given that the many competing information processing models developed by cognitive psychologists and experts in artificial intelligence are not easy to evaluate with respect to their possible implications for this issue, the systematic approach taken by Rabinowitz (this volume) seems particularly constructive and helpful. By focusing on the role conceptual knowledge can play in the processing of information, Rabinowitz identifies major limitations of two broad categories of information processing models (i.e., models using the mind as computer metaphor versus models using the metaphor of mind as brain) dominating the field. The major problem of models using the computer metaphor is that conceptual knowledge is reduced to an enabling role, as *active* strategic processes operate on a *passive* knowledge structure. On the other hand, the distinction between knowledge structures and strategic processes is eliminated in connectionist models using the metaphor of mind as brain. Consequently, Rabinowitz points out that neither model category seems suited to adequately capture the dynamic nature of conceptual knowledge and the control of processes afforded by strategies. Instead a hybrid model of information processing is needed to represent conceptual knowledge in terms of a dynamic knowledge system associated with an active processing component. While we generally agree with Rabinowitz' view, we hope that future conceptualizations will give more thorough consideration to the developmental perspective, thus modeling patterns of strategy–knowledge interactions that may change quantitatively and qualitatively as a function of increasing "maturity" or speed of the information processing system.

The modeling approaches described so far are comparatively narrow in perspective, since they focus solely on the interplay between strategies and either conceptual or domain-specific knowledge. We do not claim that this is the perspective adhered to most closely by the majority of contributors to this volume. Rather, we believe that the major discrepancy apparent in the various contributions concerns the degree of complexity in modeling approaches that the authors are willing to accept and the level of generalizability of interactional patterns across domains they are ready to tolerate.

Some contributors to the volume seem to hold the view that the feedback

loop between strategies and knowledge operates domain-specifically. For example, Ceci and Nightingale (this volume) note that there is less across-task consistency than the traditional view would lead one to suspect. Note, however, that Ceci and Nightingale refer to a broadened view of domain-specificity. On the other hand, Muir and Bjorklund (this volume) favor a conceptualization in which the effects of knowledge "decontextualization" increase with age. Finally, Borkowski and Turner (this volume) argue that strategies possess attributes and characteristics that are transferable across time, settings, and tasks.

How to account for these differences? The view offered by Ceci and Nightingale represents the prevailing theoretical trend based on experimental research that strategy use is limited by domain boundaries and the available knowledge within each domain. As children grow older, they acquire more knowledge within a given domain, which may also lead to an expansion of domain boundaries and decontextualization of knowledge effects as described by Muir and Bjorklund. Borkowski and Turner's divergence of opinion seems due to a difference in their research focus: Borkowski and associates have been interested in *strategy transfer* for some time now, an issue certainly less interesting for researchers who are primarily concerned with the role of domain-specific knowledge on cognitive performance.

Not surprisingly, the theoretical model proposed by Borkowski and Turner is very complex. This model of metacognition (also known as the Good Strategy User Model) was first developed by Pressley, Borkowski, and O'Sullivan (1985) and later extended by Borkowski, Pressley, Schneider, and colleagues (cf. Borkowski, Johnston, & Reid, 1987; Pressley, Borkowski, & Schneider, 1987; Schneider & Pressley, 1989). While the original model was built primarily to substantiate metamemory theory, its components can be easily applied to a wider range of cognitive activities. We do not want to reiterate Borkowski and Turner's detailed description of the model but would like to just point out a few interesting implications. In our view, one of the major advantages of this model is that it accounts for different declarative knowledge components (i.e., specific strategy knowledge, general strategy knowledge, relational strategy knowledge) which are conceived of as interactive and mutually dependent causes of strategy use and cognitive performance. In addition, the model emphasizes the importance of so-called Metacognitive Acquisition Procedures, that is, higher level processes that help children learn more about lower level strategies. These processes which are allegedly the heart of metacognition, giving it transsituational applicability, have also been referred to as self-regulation and executive functioning.

A second advantage of the model concerns the conceptualization of General Strategy Knowledge first introduced by Pressley et al. (1987). Borkowski, Pressley, and their associates believe that a unique property of General Strategy Knowledge is its motivational character. That is, general knowledge about the value of behaving strategically may result in expectations about self-efficacy,

which, in turn, may motivate children to confront new challenging learning tasks. Thus, General Strategy Knowledge is also essential for strategy generalization in that it creates attributions with respect to self-efficacy and beliefs about the growth of the mind. In our view, one of the specific merits of the model is that it stimulates a broader research perspective, including motivational and other contextual factors, which will be discussed in more detail in the following section. On the whole, the model implies that components of metacognitive knowledge can play an important role in cognitive performance (see also Kluwe, this volume).

One of the shortcomings of the model presented by Borkowski and Turner is that domain-specific knowledge is not represented. This is somewhat surprising, because in an earlier chapter Borkowski and associates emphasized that future research on metamemory should explicitly account for the relationship between domain-specific knowledge and individual metamemory components (cf. Borkowski, Milstead, & Hale, 1988). We should note, however, that domain-specific knowledge has been included as a major component in updated versions of the Good Strategy User Model (cf. Pressley, Borkowski, & Schneider, 1987; in press). As the contributions by Pressley, Wood, and Woloshyn (this volume) and Schneider, Körkel, and Weinert (this volume) show, there is empirical evidence that metacognition and strategy use interact with domain-specific knowledge: Metacognitive processes are helpful in activating and employing prior knowledge, and prior knowledge in turn improves the accuracy of metacognitive processes.

The Impact of Motivational, Educational, and Cultural Factors

It is not really a new insight that a broader perspective is needed to better understand cognitive development definitively. As a matter of fact, several papers presented at the earlier conference were devoted to the impact of social and motivational contexts on memory development (cf. Ceci, Bronfenbrenner, & Baker, 1988; Paris, 1988a; Verdonik, 1988). Basically it was agreed that information-processing models of cognition are by themselves insufficient devices for understanding children's capabilities, as well as for charting the transitional mechanisms of development. As Ceci et al. (1988) pointed out, it is only through adding real-world complexity to our models that we can understand more about the nature of cognitive development. As the issue has been highly familiar and generally accepted for some time, progress made in this particular area of research is vital. In our view, one of the specific advantages of the more recent approaches can be seen in the attempt to theoretically link issues of context effects to existing information processing models, thus trying to provide a more comprehensive, multidimensional model of cognitive development. This perspective is apparent in the chapter by Ceci and Nightingale

(this volume), as well as in the contributions regarding the impacts of metacognitive knowledge.

As pointed out earlier, Borkowski's and Turner's model of metacognition (this volume) highlights and explains the importance of executive processes and attributional beliefs in producing general problem-solving skills. Emphasis is placed on insight into the importance of effort and the value of effort in influencing task outcomes: children who possess metacognitive knowledge about useful strategies *and* who realize that effortful strategy deployment results in improved performance will be more motivated to work hard, as compared to peers who attribute academic outcomes to uncontrollable factors (see Borkowski and Turner, this volume, Carr, this volume, and Kurtz, this volume, for empirical evidence supporting this view). It appears, then, that the model of metacognition described by Borkowski, Pressley, and associates may serve as a good starting point for exploring the interfaces of motivation and cognition in various problem-solving activities.

As noted by Kurtz (this volume) and Carr (this volume), focusing on the interplay between "skill and will" (Paris, 1988b) seems to be a step in the right direction, but probably not a big enough one to fully understand the complexity of relationships among cognitive, metacognitive, and motivational variables. It is now well established that home and school environments have an impact on cognitive development. As a consequence, the roles of parents and teachers in influencing children's knowledge acquisition have been thoroughly explored in recent studies (cf. Carr, Kurtz, Schneider, Turner, & Borkowski, 1989; Kontos, 1983; Moely, et al., 1986; Stevenson, 1988). As parenting and teaching styles vary systematically both within and across nations and cultures, cross-cultural studies seem further necessary for a better understanding of the universals and individual variations of cognitive development (Kurtz, this volume). Complicating the matter even more, Carr (this volume) emphasizes the need to more closely address issues of development across the life span. Her point is that the knowledge base, cognitive strategies, metacognitive components, and motivational factors have changing roles at different stages of development. According to this view, issues of intraindividual change over time need to be addressed via longitudinal research designs.

The increasing interest in contextual and cultural influences on cognitive development indicates that we are ready to expand our perspective and to ask the "bigger questions" in developmental psychology (Appelbaum & McCall, 1983). However, we must still resolve how to best deal with the complexity of both the theoretical framework and research designs, particularly from a methodological point of view. While causal modeling or structural equation modeling (SEM) procedures favored by Kurtz (this volume) and Carr (this volume) represent powerful general tools for the analysis of longitudinal data, they should not be conceived of as panaceas (see Rogosa, 1988; Schneider, 1989, for a discussion of selected problems of SEM models). In particular, the problem of adequately testing the data fit in SEM models still needs to be solved,

and the problem of cross-validating SEM models should receive closer attention. Moreover, narrowing the knowledge gap between statisticians and researchers seems to be a particularly important precondition for proper usage of SEM procedures in the behavioral sciences. By and large, SEM procedures can be seen as a promising statistical tool for the complex analyses proposed by researchers interested in environmental and cultural effects on cognitive development.

Individual Differences and Aptitude Issues

Participants in the earlier conference generally agreed that the topic of individual differences in cognitive development has been neglected in research following the information-processing approach (cf. Knopf, Körkel, Schneider, & Weinert, 1988; Weinert, 1988). In contrast, contributions to this volume document an increased interest in detailed analyses of how knowledge, strategies, and individual differences interact to produce cognitive functioning (cf. Siegler, this volume).

Two major approaches can be identified: while most authors equate individual differences with aptitude-related differences (e.g., Borkowski and Turner, this volume; Muir & Bjorklund, this volume; van der Meer, this volume), others use the expert–novice paradigm to explore the impact of a rich knowledge base on strategy deployment and cognitive performance (Schneider, Körkel, & Weinert, this volume; Staszewski, this volume).

Regarding the first approach, an interesting theoretical treatment is presented by Ceci and Nightingale on the topic of how knowledge development is related to intellectual development. The term "cognitive complexity" is used to indicate intelligence in the traditional view, redefined as the result of efficient processes (strategies) operating in elaborated knowledge domains. As noted by Strube (this volume), this means that cognitive style is back, but reconceptualized in terms of the information-processing approach. Interestingly enough, this view of cognitive complexity/intelligence implies that it is primarily domain-specific in childhood, and may eventually become "trans-domainal" in adulthood. Needless to say, this conceptualization does not have much in common with the trait concept of intelligence.

Ceci and Nightingale emphasize the role of knowledge in intellectual development, a perspective also shared by Muir and Bjorklund (this volume). From their analysis of memory differences among children with different learning abilities (i.e., learning-disabled vs. nondisabled children, gifted vs. nongifted children), Muir and Bjorklund conclude that knowledge-base factors are an important source of differences among children of differing intellectual abilities.

However, by using a different memory paradigm, van der Meer (this volume) arrives at a different conclusion. In her study with retarded and normal children, observed performance differences were not due to differences in the conceptual knowledge base but to differences in the spontaneous use of this

knowledge. Similarly, Borkowski and Turner (this volume) conclude, from a review of training studies, that poor learners fail to invent mature strategies to meet changing task demands. While the findings reported by Borkowski and Turner appear to support the assumption that retarded children suffer from (lower level-) strategy deficiencies, the authors take this explanatory approach. They further argue that retarded children lack metacognitive, executive skills which seem to be important for learning because they assist in the implementation of lower–lower strategies. In our view, the elaborative interrogation procedures introduced by Pressley, Wood, and Woloshyn qualify as an example of such a skill, in that it directs the activation of prior knowledge related to new to-be-learned information.

What can be inferred from the discrepant results? We tend to believe that while all the authors report valid findings, their validity may be restricted to specific domains and task requirements. In future research, systematic comparisons should explore the importance of (the lack of) knowledge, strategies, and metacognitive skills across different domains and for different problem-solving situations.

There is definitively more agreement as to the crucial role of domain-specific knowledge when experts in a designated domain are compared with novices. The tremendous impact of a rich knowledge base on memory performance is apparent in the results reported by Schneider, Körkel, and Weinert (this volume). Younger soccer experts outperformed older soccer novices on various measures assessing children's memory and comprehension of a story dealing with a soccer game, thus reversing the typical age trend. Similarly, Staszewski (this volume) provides ample evidence for the outstanding role of a particularly rich knowledge base in turning a long-distance runner into an exceptional mnemonist. In the study described by Staszewski, it was shown that an extensive, well-organized knowledge base enabled a subject to (a) encode elaborate representations of random-digit lists and (b) retrieve this information accurately, efficiently, and flexibly. It is the specific advantage of the single-subject design chosen by Staszewski that made fine-grained analyses of the respective roles of semantic encoding and retrieval process in memory performances available, demonstrating how knowledge, practice, and strategies relate to expert performance.

The contributions by Schneider et al., as well as by Staszewski show that general aptitudes are less important when it comes to explaining expert performances. Staszewski reports that his subject was of average aptitude and emphasizes that knowledge acquired through experience, rather than innate talents or abilities best accounts for extraordinary capabilities. Systematic comparisons of low-ability versus high-ability experts and novices conducted by Schneider et al. led to the conclusion that ability deficits can be compensated by a rich domain knowledge. On the other hand, Schneider's et al. first results suggest that procedural as well as declarative metacognitive knowledge may interact with the knowledge base in yielding optimal performance.

Future Research Perspectives

Drawing inferences about future research perspectives from the present state-of-the-art can either be a somewhat monotonous task or a risky endeavor. Certainly, our concluding remarks have highlighted a number of well-defined issues that should be addressed in future research. These issues have either been raised specifically in designated chapters or are indirectly inferred through comparisons between the different contributions.

Considerable progress has been made during the past decade in the empirical analysis and theoretical modeling of the interplay among content knowledge, strategy use, and cognitive performance. However, our scientific knowledge in all these areas is still rather vague and inadequate. This applies especially to the development of cognitive competencies and the emergence of individual differences. There are also a great many discrepancies among the different theoretical positions postulated. In our view, one of the reasons for this state of affairs is that most of the hypotheses formulated are derived from designs using a single experimental paradigm and are focusing on just one domain. Consequently, the validity of such findings across several domains remains unclear, creating the risk of overgeneralization. Two things are needed to correct this state of affairs: First, to back away from the compulsion toward novelty so prevalent in psychological research in formulating research questions, and to avoid working within the confines of one self-selected paradigm. Second, to conduct more replications of studies while systematically varying one critical variable. Only by taking such steps will conditions be created in which theoretical abstraction supercedes vague, empirical generalizations. It is also necessary to supplement the analysis of group data in conjunction with single subject designs, since intraindividual variability of cognitive processes is greater than has been assumed by static models (Siegler, this volume; Ceci & Nightingale, this volume). Only the systematic expansion and completion of ongoing research along with a reduction in the units of analysis will allow for greater precision in investigating such aspects as the rules which govern the interplay of knowledge concerning different qualities and quantities, processes influencing metacognitive understanding, motivational attitudes, and the use of specific strategies as determinants of cognitive performance.

Our present state of knowledge suggests that similar performance levels will be attained under different (sufficient but not necessary) sets of conditions, that many functional compensatory effects prevail among different cognitive resources, and that similar sets of cognitive prerequisites will produce differing levels of performance within different task contexts. Many important research issues and the corresponding strategies can be characterized according to this point of view. Nonetheless, are we really interested in predicting and making recommendations for the course of future research? Given that progress in science is not only achieved through hard work and repeated effort, but that creative thinking is an equally essential element, is it not reasonable to expect that the current state of research will suffice to prompt researchers to ask many dif-

ferent questions generating new approaches and finding creative answers for unresolved problem areas. It is hoped that a creative and open attitude will mark future research. We would like to illustrate this point by using a specific example to ask a series of related questions.

The example chosen concerns the role of aptitudes or abilities in cognitive development and human performance (for a general discussion see Ackerman, 1987; Ackerman, Sternberg, & Glaser, 1989). Although the title of this book incorporates the term "aptitude," the individual chapters contain very few references to empirical studies and theoretically substantive inquiries. Does the expert–novice paradigm really tell the whole story in implying that the level of domain-specific knowledge, as opposed to the impact of general intellectual aptitudes, completely dominates understanding, problem-solving, and memory performance in content-specific tasks (Schneider, Körkel, & Weinert, this volume)? How is such expertise acquired? Can one acquire different kinds of knowledge simply by being properly motivated and by having a so-called power of long-term will (Nietzsche), or do aptitude-related constraints exist which influence the speed, quality, and (possibly) the asymptote of knowledge acquisition? What do differences in the quality of knowledge depend on, when defined as a quality embedded in a differentiated and hierarchically organized network, with variable mental representations (especially various forms of abstract representation), as well as flexible access to and adaptability of the knowledge base in varying contexts? Are individual differences in the quality of knowledge due only to available prior knowledge, time spent learning, effort, and quality of instruction, or are these differences influenced by more or less general aptitudes, at least during the early stages of content knowledge acquisition? Assuming that aptitudes do have such an impact, the next question to be addressed is whether compensatory effects occur between aptitude levels and learning effort, or whether aptitude-dependent limitations exist at least for very complex bodies of knowledge. Could it be that the level of relatively general aptitudes (under otherwise constant conditions) affects the speed of knowledge acquisition and the quality of the knowledge structure, thus making quantity and quality of knowledge acquired within a given time span better indicators of aptitudes than the measurements of general intelligence scores? Can findings showing the negligible effects of aptitudes on the acquisition and use of knowledge in simple task performance be used to make conclusions about the role of aptitudes in developing fundamental skills solve more difficult tasks?

Do genetic determinants of the development of the knowledge and the different forms of information processing exist? If so, how do such genetic determinants affect the acquisition, storage and use of the knowledge base? What role is played by individual learning processes in this context? We are not primarily referring to the short-term learning processes examined in most training studies, but to the long-term acquisition of knowledge and skills spanning many years. Finally, how significant are diverse cultural and educational contexts in this respect?

With regard to knowledge acquisitions, these are some of "the bigger questions" (Applebaum & McCall, 1983) which will be posed by future research in developmental psychology. Answering these questions presupposes not only the continuity of normal science, but also the development of new theoretical models and empirical paradigms. However, such changes in scientific thinking cannot be predicted or can only be predicted at great risk. Hopefully, therefore, the formal presentations and the informal discussions during the second Munich Conference have stimulated many fresh, new, and sophisticated ideas. Perhaps at a third Munich Conference we will be able to acquaint ourselves with the resulting research.

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