10 Commentary: An Embarrassment of Number

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WHO IS A NUMBER EXPERT?

When Catherine Sophian first asked me to comment on these chapters, I began wondering about what essential quality of mine she was seeking to exploit. So I took a look at the list of discussants from the previous 17 Carnegie Cognition Symposia in order to determine what common attributes they all shared. I discovered that past discussants have usually, but not always, combined such features as charm, grace, good looks, cultivated tastes, entertaining style, and an air of casual hilarity. Included in the collection of several dozen discussants were aging gurus, old friends, stand-up comedians, incisive critics, departmental loiterers, and even retired ballplayers.

However, the defining criterion for previous discussants—and the one that I hope Catherine used—is *expertise*. It seems pretty clear that one chooses a discussant by looking for someone who is an expert in the area.

Having concluded that expertise was the key factor here, the next step was to determine whether or not number development is one of my areas of expertise. How can I be sure of that? How do you decide what you *think* you are an expert in? Although there is a lot of research (much of it done here in Pittsburgh) about how experts think, there is not much known about self-defined expertise.

Lacking any rigorous approach, I decided to use my favorite definition of expertise. It comes from a friend of mine with a world-wide reputation in a discipline unrelated to psychology. He once told me that when attempting to draw the boundaries of his own areas of expertise he used the following criterion:

> If you hear a fact about area X, and if you are embarrassed that you do not already know that fact, then you consider yourself an expert in area X.

This definition, together with the chapters in this section, confirm my hunch that number development *is* one of my areas of expertise. Strauss, Curtis, Cooper, Miller, Shrager, and Siegler presented us with new ideas in theory, in methodology, and in empirical results, and I blush not to have known them all in the first place. Hence the title of this commentary: *An Embarrassment of Number.*

Perhaps I can extricate myself from total mortification by providing a structure within which we might evaluate the various contributions. Rather than list fatal flaws and brilliant breakthroughs for each chapter, I outline a research agenda on the development of number skills. In doing so, I will try to get those of you outside the area of number development to see the world through the lens of those committed to solving some of its problems, and to appreciate the difficulties we face. My questions about these chapters are organized around three related topics: (1) the number concept; (2) developmental processes; and (3) information-processing models.

WHAT'S SO SPECIAL ABOUT "NUMBER"?

Why do we study the number concept? What makes number different from, say, shape, or color, or heaviness? We do study the perceptual basis for color, but not the color concept *as such.* "Conservation of color" is just not an issue in developmental psychology. Why is that? After all, we do have a clear set of well-established color constancies. We generally do not expect color to change abruptly, and when it does, we infer a color-changing transformation. What then, is so special about number?

I will try to answer the question indirectly, by focusing on a few key issues about number. Some of what I have to say is speculative, but speculation is absolutely necessary if we are to construct theories of quantitative development. It does my heart good to see such careful experimentalists as the authors of these chapters—some of them the best in the business—beginning to postulate developmental mechanisms. Nevertheless, I am aware that proximity to speculation elicits bizarre behavior from many psychologists, ranging from apology to apoplexy, so instead of *speculation,* I call these comments *design criteria* for a model of quantitative development.

DESIGNING A NUMBER CONCEIVER

One way to appreciate the issues here is to ask the following question: If you were to build a system that "had" the number concept, with what properties would you endow it? Contrastively, how would it differ from a system that *lacked* the number concept?

Formulating issues in terms of the design of an information-processing system is not just a rhetorical device. It forces the conceptual clarification of issues that otherwise remain ambiguous and it suggests appropriate experimental investigations. For example, the most fundamental decision in the design of a system that "has" quantitative knowledge is how to represent quantity. As I try to illustrate in the next section, even this basic issue is not yet resolved.

What is a Quantitative Symbol?

There are two senses in which we may talk about quantitative symbols. In the first, *quantitative symbol* refers to the internal representation produced by encoding processes operating on any quantitative features of the environment. Early versions of quantitative symbols may be inaccurate, or they may have only partial information about all the quantitative aspects of the external situation they purportedly represent. In this first sense, then, a quantitative symbol is whatever gets produced by processes that attempt to encode quantity. Cooper's "subitized states" are an example of this use of the concept of a quantitative symbol.

The second sense of quantitative symbol applies to internal symbols that have all the essential properties of quantity. Most importantly, in the case of number, quantitative symbols in the second sense have both cardinal and ordinal properties. That is, given two such quantitative symbols, the system can determine their relative magnitudes. Otherwise, the system can only determine their sameness or differentness. Thus, it can determine that three is less than four, but only that red is not the same as blue or long or three.

What is it about the internal representation for the things we call quantities in general, and numbers in particular, that makes them special, that endows them with properties unlike other symbols? And how does such a representation develop? More specifically, how does *quantitative symbol* in the first sense develop into *quantitative symbol* in the second? Ten years ago, Wallace and I addressed these questions (Klahr & Wallace, 1973), but our proposed answers seem to have sunk like stones. However, I have not since seen any other serious proposals, so let me resurrect some of our ideas here.

I propose a specific form for representing discrete quantity, and then, in this section and subsequent ones, I describe some possible developmental consequences of this assumption. Consider the early representation for a pair of identical objects, shown in A of Fig. 10.1. The total collection X is represented by two identical symbol structures, each comprised of some elementary symbols that completely characterize the objects. If the objects being represented here were, say, fingers, we could think of this early representation for two fingers as "a finger and a finger."

This is a redundant and inefficient representation, and at some later stage, the system adopts an alternative representation for a set of identical objects. B in Fig. 10.1 shows one possibility. The complete object is represented only once, and its

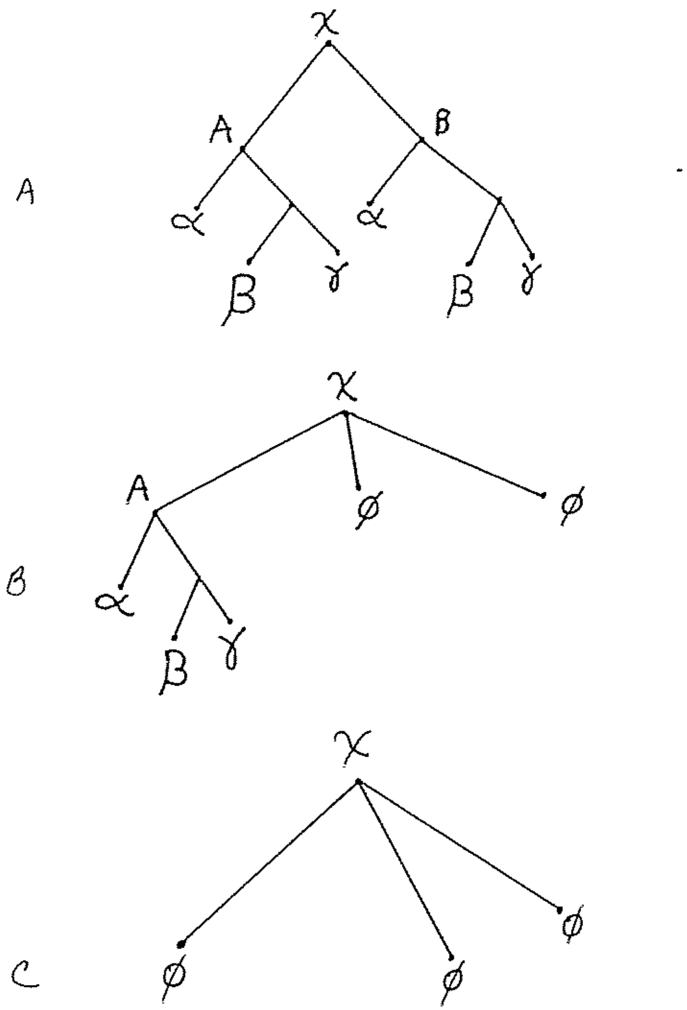

FIG. 10.1. Hypothetical forms for representing discrete quantity. a) Rudimentary and redundant representation for a pair of specific objects; b) More efficient and abstract representation for 3 specific items; c) Most abstract representation for any three items.

other occurrences are represented by a kind of internal "ditto" mark. In the representation for three things shown here, we have something like: a finger and another and another. For quite some time, the infant may maintain many different object-specific representations for small sets of identical objects, particularly the ones he or she frequently encounters. This idea is elaborated by Strauss and Curtis' notion of item specificity and number specificity. Even as adults, we

have several ways of expressing two things, depending on what those things are: pair, couple, set, team, twin, brace, and so on.

Eventually, the system exploits the structural similarity between the representations for the same amount of different things, and develops a representation for cardinal number that is independent of the thing being quantified. Such a representation for "threeness" might be just the simple list of null markers shown in C of Fig. 10.1. This representation differs from the previous one in the same way that "three" differs from "three dolls."

All that I have described so far is a representation for cardinality, not ordinality. As Cooper notes, these early representations for number, or "subitized states" as he calls them, are initially unordered. However, as we will see, ordinality is inherent in the symbols, and their particular form plays a crucial role in quantitative relations.

Extracting Ordinality from Cardinality

Strauss and Curtis have a very interesting idea on this. First, they endow the baby with an innate ability to detect magnitude differences. Then they suggest that the covariation of discrete and continuous quantities enables the infant to order the numerical symbols according to the ordering of the corresponding continuous, nonnumerical representations of physical dimensions.

I think that there are two problems with this idea. First, it seems unlikely that the required cooccurrences are really there. In general, covariation of continuous ordinal and discrete cardinal quantities are quite rare. In the standard conservation situation, length, density, and number do not cooperate. In the infant's world, two hands remain two hands no matter how far apart or close they are, fingers spread and close and yet remain the same in number, two blocks and three blocks can be fit into the same space, and so on.

Strauss and Curtis, seated in their armchairs opposite mine, disagree: "In the natural environment, differences in discrete quantities naturally covary with differences in continuous quantities." This difference of opinion illustrates the importance to any developmental theory of a good account of the environmental inputs to the developing system. I return to this point later. It also underscores the need for detailed naturalistic studies of infants' transactions with their quantitative environments (c.f. Langer, 1980).

The second problem with the Strauss and Curtis idea is that it finesses the issue of what it means to make ordinal judgments about continuous quantity, because we can ask of continuous quantity the same question that Strauss and Curtis ask of number: How do we know that infants are responding to order and not just to difference in magnitude? What does it mean to know that one sound is louder than another rather than just different from another? Even if we are sure that an ordinal judgment is being made, how can we represent that internally? Of course, we can build in ordinality with respect to continuous quantity and then just transfer it to discrete quantity, but that seems to take a lot of the fun out of the endeavor.

Cooper has a different proposal, one that is in some respects quite similar to my own view, but different in important ways. His proposal is similar in the extent to which it emphasizes the importance of the infant's ability to encode quantity before and after transformations, and to detect regularities under different classes of transformations. (Although Miller's subjects were older than those being discussed here, his careful analysis of children's understanding of the relevance of different transformations is quite important and might be adapted for assessing younger children's transformational knowledge.) However, Cooper does not go quite far enough in explaining how he would account for ordinality solely on the basis of transformational correspondences. If it were that simple, then why do other, nonnumerical domains not acquire an inherent ordering of their own? For example, the baby might notice that pouring milk into water makes the water turn white. Why do the symbols for white and clear not get ordered via the milk-pouring transformation?

Cooper alludes to an answer, one that I would like to take a bit further. I think that the *form* of the quantitative symbols, such as the ones shown earlier, enables the system to generate ordinal information. There are two unique properties of quantitative symbols that produce an implicit ordinality in representations for cardinality. The first property derives from the form of the symbols. When the innate symbol-processing routines compare two such symbols for sameness or differentness, they get a little more information than they need. If the symbols are different, that difference is represented by the actual structure of the residual symbol. This residual is itself a quantitative symbol, capable of being matched to another, and this provides the rudiments of internally generated ordinal knowledge. Once this knowledge is available, then the system can use the results of transformations in the manner suggested by Cooper.

The second source of ordinality derives from another unique property of the encoding of small, discrete quantities: multiple representation. When three objects are encoded, so are subsets of two and one. This multiple activation of "detectors" or subitizers occurs asymmetrically, however. When three items are encoded, symbols are generated for two and one as well as for three, but, of course, when two items are present, there is no encoding of three. This inherent asymmetry of cardinality encoders provides the data base for the ultimate representation of ordinality via internal analysis of correspondences. (See Wallace, Klahr, & Bluff, in press, for a full explication of the process.)

The Role of Counting in Childhood and Infancy

What is the role of counting in the development of number concepts in general and in arithmetic computation in particular?

First, let us consider young children, and then we will look at infants. As Siegler and Shrager's work indicates, children appreciate the tremendous utility

of finger counting. In fact, children's use of finger counting shows an interesting correspondence with the cognitive skills of primitive cultures. Although anthropological analogies are considered bad form in psychology, I cannot resist quoting from Dantzig's (1954) book, first published nearly 50 years ago, on the history of number:

> wherever a counting technique, worthy of the name, exists at all, *finger counting* has been found to either precede it or accompany it. And in his fingers man possesses a device which permits him to pass imperceptibly from cardinal to ordinal number. Should he want to indicate that a certain collection contains four objects he will raise or turn down four fingers *simultaneously;* should he want to count the same collection, he will raise or turn down these fingers *in succession.* In the first case he is using his fingers as a cardinal model, in the second as an ordinal system. Only a few hundred years ago, finger counting was such a widespread custom in western Europe that no manual of arithmetic was complete unless it gave full instructions in the method [p. 11].

Before you get too smug at the idea of adults counting on their fingers, try the following exercise: Sit on your hands and then figure out what month it will be 7 months from today.

It is clear from the work of Siegler and Shrager that preschoolers are quite sophisticated in the deployment of their counting technology. What about infants? The important issue here is the role of counting in the *development* of the number concept. Is counting the *basis* of true quantitative thinking, or is it a *reflection* of it, an acquired technology to extend already-developed quantitative processes and representations into higher numbers, with increased speed and accuracy?

In their excellent summary of the empirical work on infant quantification, Strauss and Curtis make a convincing critique of Gelman's view of the primacy of counting. As they note, it seems implausible that infants have the capacity to systematically scan visual arrays, to coordinate an internal tagging process with that scan, to partition the array into tagged and untagged items, and to use the "cardinality principle" to label the set with the last of the ordered tags. Cooper's brief-exposure studies further discredit the notion that infants count.

On One-to-One Correspondence

The role of one-to-one correspondence in quantitative development is still controversial. My own view is that it is a relatively late acquisition (c.f. Klahr & Wallace, 1976, pp. 76–80), but I do not have space to elaborate that view here.[1]

[1] Viewing one-to-one correspondence as a late acquisition is not inconsistent with the assumption of innate processes that underlie the account of internal symbol comparison described earlier. One-to-one correspondence is a high-level strategy for producing or comparing external collections with specific ordinalities. It requires noticing, marking, tagging, and so on. The comparison of internal symbols is a low level, automatic, innate process.

Rather, I want to argue for an alternative interpretation of Miller's results. His clever procedure for getting kids to generate "fair" distributions may or may not actually index their understanding of one-to-one correspondences, for they may simply be executing a distributional procedure that has been socially transmitted without any real appreciation of its quantitative basis. That is, for the youngest children, the one-for-you, one-for-me procedure may simply be a ritual associated with the eliciting conditions of "fairness." Miller argues against the ritualistic copying view by noting that parents rarely attempt to fool children by confusing number of pieces with total amount, but one could equally argue that there are many occasions in which children must accept that equal number is equivalent to equal amount (as in pieces of pie, fruit, etc., with obvious variation in size).[2] As Miller notes, it is clear that children are treating the thing that is equitably distributed as number of pieces, rather than total amount, but do they really believe that each turtle has the same amount to eat? If children could choose which snack to consume, would they be indifferent to the "fair" distributions they have created?

Nevertheless, although I quibble over some of his interpretations, I think that Miller's idea of assessing children's abilities to *produce* specific quantitative outcomes, rather than just assess them, has begun to add important information to our understanding of quantitative development, and it is worthy of extension. In particular, his procedure for assessing children's ability to judge area—in which children count out a specific number of tiles to "cover" an area—provides a challenging alternative to N. H. Anderson's (1974) information-integration model.

ON DEVELOPMENTAL PROCESSES

A complete account of the development of some skill should take a position on three issues:

1. What is the innate kernel of processes and structures with which the system is endowed?
2. What are the developmental mechanisms—that is, the self-modification processes?
3. What are the environmental experiences that, in combination with the innate kernel, provide grist for the developmental mill?

If I could control the format of all the chapters in this volume (or, while I am fantasizing, *all* publications about cognitive development) I would require au-

[2]My disagreement with Miller is another example of arguments based on assumptions about the naturally occurring quantitative experiences of the child (c.f. my earlier comments on Strauss and Curtis). The only way to resolve such differences of opinion is to observe what really happens, as in Siegler's study of parental input of addition problems.

thors to indicate clearly what they have to say about each of these issues. My own summary of how the four chapters address the three questions is as follows:

Strauss and Curtis make a clear statement of their position on what is not necessarily innate. They systematically rule out some of the knowledge structures that might, at first blush, seem to underlie children's numerical performances. Such properties are thus eliminated from contention as necessary members of the innate kernel. They then suggest that "very possibly" there are innate cardinality detectors, but that these have, at first, no ordinal properties. However, ordinality is inherent in innate magnitude-discrimination processes that operate on continuous perceptual dimensions such as size, brightness, length, and so on.

With respect to developmental mechanisms, Strauss and Curtis simply mention that infants' "ability to make [ordinal] judgments with continuous quantity may serve as the foundation for both their eventual knowledge of discrete ordinal concepts and their knowledge of how discrete cardinal and ordinal properties are related." Implied but not specified in their account are processes that can actually build on this foundation. The crucial environmental experience for this (implicit) process to operate is the covariation of differences in continuous and discrete quantity mentioned earlier.

Miller alludes to a notion that humans may be endowed with self-teaching mechanisms that are sensitive to specific information. One such innate mechanism might be sensitive to the results of experimentation with measurement procedures. Miller also seems to view rudimentary forms of counting as "spontaneous." Rudimentary counting provides the requisite stability for the detection of quantitative constancies.

This, in turn, provides the basis for Miller's model of the developmental process, which consists of the "gradual elaboration and refinement" of measurement procedures. Finally, according to Miller, the major environmental experience that drives all of this is the child's practical need to construct and evaluate quantitative equivalences.

Like Strauss and Curtis, Cooper also posits an innate mechanism, a "numerosity detector," that provides cardinal information. He proposes that this base of unorganized "subitized states" undergoes an extensive and subtle bootstrapping process that is sensitive to quantitative transformations and their effects on numerosity. His series of decision trees represents a modest step toward a process model of how this bootstrapping might take place. The requisite environmental experiences, in Cooper's account, are repeated occurrences of transformations involving small (in both set size and increment) discrete quantities. I have to give Cooper high marks for the way in which he has clearly stated his position on the three key issues. In particular, his entire research program is commendably motivated by relatively well-specified working hypotheses about developmental mechanisms.

The Siegler and Shrager chapter is harder to evaluate. On the one hand, their model for performance on the addition task itself, and for the learning of the interitem associations, is more precise, better articulated, and better supported empirically than anything else we have in this set of chapters. They also propose an important environmental regularity—parental input—to account for initial (but not innate!) interitem associations. On the other hand, with respect to both the innate kernel and the general developmental mechanisms that affect the strategy-choice process, they have less to say. Nevertheless, given the unambiguous way in which they have cast their performance model, they are in a good position to address the other issues with equal precision.

ON INFORMATION-PROCESSING PSYCHOLOGY

This final section of my commentary has to do with the potentially rich interplay between the central issues in cognitive development, and the concepts and methods of what is generally known as *information-processing (IP) psychology*. I illustrate a few ways in which recurring issues in cognitive development might be resolved by constructing systems that exhibit the phenomena of interest, and I suggest that current IP models require substantial extension before they can fully capture some of these phenomena.

On Tacit Knowledge

Developmentalists, including many of those writing in this volume, seem compelled to talk of children's tacit knowledge. How is tacit knowledge expressed in information-processing models?

1. Does a calculator have tacit knowledge of the principles of arithmetic?
2. Does a system that knows the "addition facts" have tacit knowledge of the "multiplication facts"?
3. Does an adult have tacit knowledge that each letter appears only once in the alphabet?

In order to construct an information-processing model that has tacit knowledge of some domain, we have to decide which of several possible definitions of "tacit" is intended.

1. TACIT1: Derivable by examination of internal structures, but not explicit before such examination. (The alphabet example.)
2. TACIT2: Computable indirectly from current processes and data structures, or directly if there is memory for results of past processing. (The multiplication example.)
3. TACIT3: Consistent with that knowledge, but nowhere derivable without external intelligence. (Calculator example.)

Here are some examples from number development; I leave the mapping of these examples onto the set of definitions as an exercise for the reader.

1. I proposed earlier that ordinal knowledge was tacit in the representation for cardinality.
2. In Siegler and Shrager's model the associative strengths reveal some surprising tacit knowledge in children's own assessments of their confidence. Recall that the associative strength is used in two very different ways. Its primary use is to determine the likelihood of producing a tentative answer. But it also is compared with the criterion. In this second use, it is as if the child, having produced an answer with some likelihood, is now asking, "Is this answer sufficiently likely for me to say it with confidence?" It seems that with adults, one could test this multiple use directly by getting confidence ratings of some of their retrievals. Note that this form of tacit knowledge is often studied by developmentalists under the rubric of "metacognition." Siegler and Shrager's concluding comments about metacognition and strategy choice shed some welcome light on this often murky area.
3. Miller also deals with tacit knowledge. He concludes from children's performances on his equivalence-creation task that they have tacit knowledge about measurement in general and one-to-one correspondence in particular. He argues that "children can access invariant information about quantity without possessing any more general awareness or understanding of invariance."

Strategy Choice

How do we know what to do? This sort of question is so fundamental that most psychologists would rather not raise it, but it is the core of Siegler and Shrager's work on strategy choice. They have made a series of fascinating discoveries about children's arithmetic skills, and such discoveries are clearly relevant to the full development of number concepts.[3] Siegler and Shrager hope to extend their basic ideas into domains unrelated to arithmetic in particular or to number in general. Thus, I comment only on the *general* implications that their work might have for information-processing models of cognitive functioning.

The basic question is how you decide what to do. As I noted earlier, it is unusual for psychologists to confront this question so directly. Siegler and

[3]Those of you who are familiar with Siegler's previous work may be surprised to find him working in the general area of number development, but, in fact, he has been interested in number for quite some time. This long-term interest is best revealed by looking at the titles of a few of his papers published over the past several years: "*Three* Aspects of Cognitive Development" (Siegler, 1976); "*Seven* Generalizations about Cognitive Development" (Siegler, 1981); "*Five* Generalizations about Cognitive Development" (Siegler, 1983); The Development of *Two* Concepts (Siegler & Richards, 1983). The efficiency of Siegler's research program is evidenced by the fact that he has focused, thus far, exclusively on prime numbers.

Shrager have not only confronted it, they have provided a remarkably complete and empirically supported model of how children make a specific decision. The completeness of their answer derives in part from their considerable ingenuity, and in part from the limited scope that they have placed on the general question. Rather than address the general issue of "what to do," they have focused on a simpler one: How do you decide whether to retrieve or recompute something? However, they have great aspirations for this model.

I suspect that as Siegler and Shrager generalize their model to work in other domains, it will start to approach the form that people in artificial intelligence use to characterize the same general question. In artificial intelligence these issues go under the rubric of *method selection*. Given the full set of weak and strong methods than one might use to solve a problem, how does one design a system to make the choice intelligently? Recent work by Laird and Newell on "Universal Subgoaling" and "A Universal Weak Method" may be relevant here (Laird & Newell, 1983a, Laird & Newell, 1983b).

A key idea in artificial intelligence is that intelligent behavior consists of search through a problem space from an initial state to a goal state. The general search problem (search in the artificial intelligence sense, not in the more specific use of Siegler and Shrager) is to find operators and apply them, evaluating progress toward the goal. In the addition case, Siegler and Shrager propose a few operators: direct retrieval, external representation, and internal representation. What Siegler and Shrager call search is just one particular type of application of the retrieval operator. At some point, elegantly and fully specified by Siegler and Shrager, the retrieval operator is abandoned and the other operators get their shot at producing a solution.

Siegler and Shrager focus on stable facts, acquired over a protracted period, such as the "addition combinations." They give other examples of similar decisions about compute versus recall, such as spelling a word. They might have focused on a wide variety of similar situations, such as those in Table 10.1. Each example shows a situation in which you would be likely to use retrieval, and one

TABLE 10.1
Operator Selection for Memory Tasks

Retrieval	*Other*
2 + 2	4 + 5 [Count fingers]
What comes after A?	What comes before H? [Start at A, search forward]
Pittsburgh population	Wilmerding population [Use almanac]
Lunch Today	Lunch last Sunday [Directed Associations]

in which you would be likely to use some other operator (as indicated in brackets) to compute the answer.

Note that the last example does not refer to stable knowledge, but rather to a temporary value of a state variable. Nevertheless, it seems that people make the same kind of recall versus compute decision for this sort of transitory knowledge as they do for stable facts like the addition combinations.

The Role of Development in Information-Processing Psychology

In closing, I would like to make a few comments on the appropriateness of having these papers about developmental issues form the core of the Carnegie Symposium on *Cognition*.

This volume is based on the 18th Symposium in the series. It is the third one devoted to cognitive development (Farnham-Diggory, 1972; Siegler, 1978). If you compare the volume from the 1972 symposium with the chapters in this volume, it is clear that the intervening decade has produced substantial advances in the quality of research in cognitive development: in the sophistication of the questions being addressed, in the analytic power of the methodology being used, and in the conceptualization of the theories being proposed.

One common attribution for the source of this rapid advance in research in cognitive development is the "information-processing revolution" in adult psychology. Most cognitive psychologists who study adult behavior are of the opinion that (quoting one of my colleagues) "the theoretical issues in developmental psychology are usually defined by the theories of adult performance." If this is the case, then it is only a matter of time until developmental psychologists adapt the adult theories and paradigms for use with children.

I think that such a viewpoint is incorrect. One cannot fully understand the adult system without understanding *first* its developmental history. This is perhaps the one common theme in the diverse contributions of such profoundly influential psychologists as Freud, Piaget, and Skinner. A strong case for the primacy of a good developmental theory has been well stated by Don Norman (1980):

> in the study of adult cognition there seems to be the implicit assumption that once we come to understand adults, children will simply be seen to be at various stages along the pathway toward the adult. Perhaps. But perhaps also that the complexity and experience of the adult will forever mask some properties. Automatic behavior masks the underlying structure, pushing things beneath the conscious surface to the inaccessibility of subconscious processes. Well established belief and knowledge systems mask their content [p. 18].

The primacy of developmental issues is nowhere more evident than in the challenge that development presents to theories of self-modification. I have long been an advocate of the relevance to developmental theory of self-modifying information-processing models (c.f. Anderson, Kline, & Beasley, 1980; Langley, in press, Lewis, 1978; Waterman, 1975). Nevertheless, it is still the case that the models currently available are inadequate to explain many fundamental developmental phenomena. Although they can often account for taking a system from state N to $N + 1$, they fail the crucial induction step: They have not (yet) addressed the question of the innate kernel of information processes. That is, many of the currently available models have little developmental tractibility, although they may provide a reasonable account of the learning mechanisms in an already well-developed system.

There is no reason to believe that this is an inherent limitation of the approach, but there are still some difficult problems to solve. The construction of truly developmental models requires advances in two areas.

1. First, we need theoretical advances in the area of information-processing models, especially in the area of system architectures. Right now it is very difficult to figure out how to start a system with very little—with an innate kernel—and have it evolve into an intelligent system. Even if we solved the conceptual issues, we are currently limited by the tools at our disposal. A serious information-processing model of development—that is, one that actually went through an important developmental process—requires more speed and storage capacity than is currently available on any of the machines around. However, within a few years, we may see specialized production-system machines that run 1000 times faster and that can support systems of 100,000 productions. With such tools at our disposal, we are likely to see some exciting advances in information-processing models of development.

2. Second, we need a more robust empirical foundation. The work described in these chapters exemplifies just the sort of necessary theory-driven methodology. The results from such studies help to constrain and evaluate the proposed models.

It should be clear that the creation of information-processing theories of cognitive development will have a profound effect on theories of adult cognition. As the chapters in this part and in the rest of this volume attest, developmentalists do their best work not when they attempt to harvest the riches of work in adult cognition, but rather when they plow their own rows, and sow their own intellectual seeds.

I look forward to some very embarrassing moments in the years to come.

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