# Book Reviews

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## The Mind's Staircase

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### Staircase? How Can We Tell?

*The Mind's Staircase* presents a series of studies from the 1980s that recount a major evolution in Case's model of development. The issue that drove this evolution is that of domain-specific versus domain-general principles and processes of development. Case's own earlier neo-Piagetian model (Case, 1985) was primarily a domain-general model, and, as such, was challenged by the positions and results of researchers oriented toward domain-specific conceptions and models of development. This book is essentially Case's response to that challenge, his attempt to integrate both perspectives and to be able to account for both kinds of results.

I will undertake three tasks in this review: (a) to outline Case's theoretical response to the challenge of domain-specific development, (b) to raise a number of questions about the theory and research reported, and (c) to draw from those questions a more general moral for psychology.

### The model

Case's earlier neo-Piagetian model construed development as the progressive construction of higher order control structures, under the constraint of limited, but developmentally increasing, resources of working memory (Case, 1985). Four major stages were hypothesized, with three substages within each:

> [At the beginning of each stage,] a new type of structure is assembled, but [it] can only be applied in isolation; at the second stage, two such units can be applied in succession, but cannot be integrated in a definitive fashion; and at the third, two more such structures can be applied simultaneously and integrated into a coherent system. As a result of this integration, the system acquires the general set of properties that Piaget referred to with such terms as "reversibility" and "compensation." Another result is that the system can now serve as the building block for further progress at the next stage. As a consequence, development "recycles," in [a] recursive fashion. (1992, p. 18)

Case modifies this model to handle domain-specific phenomena by postulating a set of *central conceptual structures* that consist of core semantic units and relations within specific domains or modules of knowledge. These

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conceptual structures provide the representational units with respect to which the control structures of the earlier model are presumed to function. Processing with the units of a conceptual structure, as well as the development of conceptual structures, is postulated to be constrained by the general stage model outlined above. The semantic particulars of a conceptual structure, however, are dependent on experience and culture in a domain-specific manner.

Domain-general constraints on development and domain-general processes of development are accounted for in the same control structure assembly manner as in the earlier model. Case's earlier model, however, had no semantic constraints, no representational modules. The representations with respect to which the control structures were presumed to function were logically free and unconstrained in the model, and were postulated independently for each task. Thus, there was no locus within the model in terms of which domain-specific phenomena might be explained.

Central conceptual structures satisfy that lack, and are necessary for control structure functioning in a given semantic domain. They are domain specific in their semantic development, thus accounting for domain-specific learning and developmental results in terms of the domain-specific semantic, or representational, units in these structures. However, the processing with respect to those semantic organizations, and the general development of those semantic organizations alike, are constrained by the domain-general control structure capacities and constructive possibilities. Forms of information processing and developmental constructions, then, are domain general, whereas the representational units with respect to which that information processing occurs are domain specific.

The central conceptual structure is intended not only to permit the integration of a variety of prima facie disparate results, but is proposed as capturing a number of conceptual convergences as well.

> As I have already mentioned, the one construct in the above set of postulates that is genuinely new, and that serves to give some coherence to what would otherwise be four or five rather disparate and unconnected sets of propositions, is the notion of a central conceptual structure. Interestingly, the feature that allows the construct to play this sort of unifying role is that it bears a strong resemblance to one notion from each of the different theoretical systems that it may potentially help to integrate. The modular notion to which the notion of a central conceptual structure bears a resemblance is the naive "theory"; the neo-connectionist notion it resembles is the "knowledge network"; the relevant sociocultural notion is the "interpretive frame"; the neo-Piagetian notion to which it bears a resemblance is the "executive control structure"; and finally, the parallel classic Piagetian notion is the "operational structure." (p. 370)

Given Case's earlier control structure stage model of processing, central conceptual structures in effect modularize the semantics of what those control structures are presumed to operate upon. Case's new model, then, is a variant of an information processing model, in which the information processing control structures are hierarchically organized in the stages and substages mentioned above, and the semantic elements which are processed are modularized into central semantic domains.

> A set of central conceptual structures was postulated. Each of these structures was hypothesized to represent a core set of semantic relations and to be module-wide in its domain of applicability. However, each structure was also hypothesized to be subject to system-wide constraints on its construction and application. The semantic *content* of such structures, particularly at upper age levels, appears to be dependent on the culture, its symbolic systems, and the institutions within which these systems are acquired and/or utilized. By contrast, the general *constraints* to which the structures are subject appear to be more dependent on a set of systemic factors of a biological and/or neurological sort. In effect, then, central conceptual structures appear to constitute a kind of pivotal point, where the forces of biology and culture meet, and around which children's understanding of their world can coalesce. (pp. 375–376)

Before attending to my questions and evaluations of the specifics of Case's model, I want to make a general evaluative point concerning the attempt that Case has made in this book. He has tried to generate a new model that can integrate and account for the sorts of results that have been generated by his most direct theoretical rivals—modularist innatists and domain-specific "developmentalists"—and to do so without losing the strengths of his original model. It is an attempt that, in both scope and honesty, is rare, and it deserves to be acknowledged as such.

The book reports many studies directed at various properties of this integrated model, whose results are almost uniformly consistent with it. (Some of the earlier studies report results inconsistent with the earlier inability to account for domain specificity—these were among the motivators for Case's theoretical revision.) Nevertheless, I wish to raise a number of questions about these studies and to pose challenges to them and their relationships to the model that Case proposes. Many of these questions and challenges have possible counterchallenges, and counterarguments to the counterarguments, and so on, resulting in a kind of tangle of interrelated issues. I will not propose any ultimate answers to this tangle, but want to indicate its existence. It is my judgment that the existence of such tangles of unaddressed issues is pandemic in psychology, and is symptomatic of a deeper problem. I will take the opportunity of this review to make this broader point—it applies to Case's model, but is not specific to it. It is a domain-general, even *field*-general, problematic.

## The tangle

I will begin with a few theoretical questions, then proceed to the tangle of methodological issues. The theoretical questions focus on some aspects of the model that seem unmotivated and unexplained from within the model itself (Campbell & Bickhard, 1992). For example, why should there be exactly three layers of control structure substages in each stage? Why not two, or five, or varying numbers in different stages and for different domains? Why should exactly three such substages yield consolidation into a new unit that can be used in higher order constructions and that constitutes a *represen-*

*tational* unit at a higher level? Consolidation into a fully integrated system of lower level units might explain being able to make use of the consolidated unit in higher level *control* structures, but that characterization does not explain why it takes exactly three substages to arrive at that consolidation for all stages across all domains, nor does it explain how such a functional, control organization consolidation yields any new *representational* properties.

The incrementations of control structure layers that climb Case's stages and substages are supposed to follow from incrementations of working memory. Working memory, in turn, increases either from consolidation itself or from maturation of larger working memory resources. On either account, why is there an increment of one working memory unit exactly every 2 years? Is there any other maturational or learning or habituation phenomenon that exhibits such precise age specificity, and sequential age specificity?

Why should mental operations be organized into subroutine control structure layers at all? This is certainly a plausible organization, and there is neurological evidence for such an organization for some aspects of motor control, but work in artificial intelligence has shown that there are a number of alternative possible architectures for information processing models of cognition, some of which seem to be highly preferable for appropriate kinds of tasks. Why should children's cognitive organization stick with this particular architecture?

There is an indication that separate working memories exist for each conceptual domain: "Working memory for numbers also failed to develop beyond the 6-year-old level (i.e., two units)" (p. 364). This might be compatible with a purely consolidation model of the incrementation of working memory, in which consolidation would occur separately in each conceptual domain, but the point is not elaborated. A domain-specific consolidation model—however much it might make sense in terms of the control structure layering per se—makes even more mysterious the emergence of a new representational unit with each new stage *and* the lockstep new unit every 2 years of the overall model. The study being discussed here, in fact, makes it clear that a new unit does not emerge each 2 years without appropriate experience in the relevant conceptual domain—so, again, why the precision of sequences of exactly 2-year intervals in general?

Case might respond to these questions by pointing out that the 2-year-interval change is a change in the *potentiality* for a new layer, but that that new layer still must be constructed, and must be constructed separately in each conceptual domain. The 2-year change, then, is permissive, not constructive per se. This model accommodates the challenge immediately above about cross-domain differences in the actual construction of domain-specific representational units (though it still does not address the exactitude of the 2-year-interval sequencing of such *potentialities* of construction), but it makes the emergence of a new *representational* unit at the stage boundaries, and the exactitude of three substages per stage, even more mysterious: Now we must consider two parallel and *exactly* in-synchrony age sequences—one that permits a new control structure layer every 2 years, and one that permits a new representational unit every fourth of such control structure layers. Or, perhaps, the control structure consolidation is postulated to somehow *constitute* an emergence of a new representational unit? If so, this is not explained.

A related set of questions arises from the likelihood that development encounters several different kinds of constraints (Campbell & Bickhard, 1992), each with its own sort of impact on sequencing and emergences. Case's model offers only one basic kind of sequencing constraint that purports to account for development, but what then happens to the other sorts of constraints? These include, for example, task and conceptual constraints (you cannot learn calculus without having already learned algebra), levels of reflection constraints (you cannot reflect on a lower level of reflection if it does not already exist), and so on.

Still another question focuses on the particularities of the control structure layering: Even overlooking the question above about why such an architecture exists, there is nothing a priori about how many subroutine layers are needed for a given task nor about what those layers have to be, even if the number of layers is fixed. In fact, such layering is, from a logical perspective, completely undetermined, and open to unbounded variations: There are an infinite number of programs, with an infinite number of variations in architectural detail, that can solve any particular task. Why, then, should we find the particular layerings and principles of layering that Case proposes for the tasks in these studies? These and related theoretical questions may (or may not) have possible answers. They do not, however, appear to be addressed at all, and this leaves puzzles and doubts.

A purely scholarly point is that the model of general formal structures having multiple instances in differing domains is strongly akin to Piaget's original model. Chapman (1988) has shown that the standard "structures of the whole" interpretation of Piaget was *never* correct, and that he had always proposed a model of formal structures with differing actual instances (at least for concrete operations; I contend, Campbell and Bickhard, 1986, that Piaget proposed that formal operations overcame this fragmentation of concrete operations and generated a genuine "structure of the whole"). The ability to so badly interpret Piaget is in part due to the failure to understand what he was attempting, and, in particular, that Piaget's primary interest was on the properties that accrued to the structures by virtue of their *formal* organization, such as closure and resultant mathematical necessities. The multiplicity of *instances* of the structures, then, was evident to Piaget, and fully consistent with his model, but not of focal interest. The point in this context is that this general form of model looks, in some respects, very similar to the one that Case has proposed here—it would be interesting to see Case's comparisons of them.

I turn now to some methodological questions and challenges, counter-arguments, counters to the counters, and so on. Entry into this tangle will be by way of a question that a critic of Case might ask: "Why aren't the sequences that are found in these studies all 'in the tasks'?" The point here is that, if Task B requires the same thing as Task A *plus some more,* then Task B *must* be solved after Task A—a version of task sequencing constraints mentioned above. The balance beam tasks that ground Case's original model have much of this flavor: Each higher level task is generated by adding a complication to the preceding. If this challenge were valid, it would suggest that something potentially interesting, in some cases, was being noted about the *tasks,* but it would count against the model capturing actual task and developmental processes in the *child.*

I do not know what Case's response to this question would be, but here are some responses he might offer:

1. The task sequencings are in terms of complexity orderings derived from the model of hierarchies of control structures operating on representational units, and *differing* complexity principles could well yield *different* orderings—orderings of the tasks that would *not* fit with the observed results. The model, then, is capturing something about what counts as being in fact complex for the actual processing architecture in the child.

2. An "in the task" interpretation of the results would not make any sense of the 2-year sequencing of next substages in the model.

3. An "in the task" interpretation of the results would not be able to account for the across-*task* synchrony within a conceptual domain, nor the across-*domain* synchrony in entirely different conceptual domains, nor the effects of *training and transfer* that have been shown.

These are strong potential counterarguments on Case's part, but, unfortunately, they move only farther into the tangle: There are counters to the counters. Potential critics are not silenced. For example, to the first charge that alternative complexity principles would not fit the data: Perhaps, but no alternative complexity principles are investigated, so the warrant for this counter is not established.

The counterargument offered on Case's behalf involving the 2-year sequencing is also prima facie a strong one, but it faces, for example, the following counters to the counter: In general, only a few substages are examined in any study, and there is in fact not perfect age synchrony across children. Therefore, the counter here rests on how difficult it is to find sequences of related tasks that exhibit *rough* 2-year intervals of mastery between adjacent levels of *a few steps* in the sequence in *normal* (and, therefore, relatively homogeneous) *populations* of children.

It is clear that the representational units involved in the complexity sequencings of the tasks are free parameters—in the sense that they are not given by the theory, but must be "put in by hand" to make the model work. In some studies in new domains, this even involves preliminary studies to determine the sort of representational unit involved in this new class of tasks. This freedom of unit does not *force* a 2-year interval of mastery in ascending the task sequence. Empirically determining the unit in a new domain is precisely what should be necessary given the logic of the model of central conceptual domains. Nevertheless, the free parameter of representational unit does raise the question of how impressive it should be that such rough 2-year-interval task sequences can be found. This is amplified by the facts that only a few substages are examined in most studies and that the cross-child age synchronies are only rough. Ultimately, there is no answer available to these questions.

Empirical evidence of cross-*task* synchrony within a conceptual domain would seem to greatly strengthen Case's counterarguments to the challenges to his model. Such evidence should argue strongly in favor of the model, and against, for example, an "in the tasks" interpretation. This is so especially because the units across tasks *within a given domain* should not be free to vary across those tasks. The units may be free from one domain to another, but should be basically the same for differing tasks within a domain, so a strong constraint seems to exist here in the cross-task comparisons.

Yes, *but:* Potential critics of Case are still not silenced; insofar as the roughness of the 2-year-interval sequences and the freedom of unit permit the adjustment of sequences so that they *individually* fit the 2-year model, then it necessarily follows that, within those tolerances, differing task sequences will automatically exhibit roughly synchronous 2-year-interval steps—all such task sequences will have been fit to those intervals in the first place. How free, or constrained, is this "fitting" of the task sequence to the 2-year boundaries? I do not have an answer; the issue seems not to be addressed.

The cross-*domain* synchronies, although in some senses having a flavor of being even more impressive in favor of Case's model than the cross-task synchronies within a domain, seem in fact to be methodologically *less* impressive given the freedom of the representational unit across domains. In the extreme, comparing one task sequence in one domain to a task sequence in a differing domain would involve one free parameter of representational unit in each domain, and, therefore, for each task sequence. Again, it is not clear how easy or difficult that becomes, but it seems likely to be easier than finding such matches across tasks within a given domain—that must involve the *same* representational unit across those tasks.

Furthermore, because there is no model of how new representational units are created by consolidations at stage boundaries, and, therefore, no particular theoretical constraint on what those representational units are taken to be at the advent of each new stage, this "free parameter" nature of the representational units recurs in each domain at each new stage. Representational unit is constrained *only* within substages of particular conceptual domains; crossing *either* a domain boundary within a stage *or* a stage boundary yields a new free choice of representational unit. It would seem, therefore, that it is not logically possible to meaningfully compare developmental sequences for more than three substages at a time, whether within-domain or cross-domain, because a sequence with more than three *sub*stages moves into a new *stage* and requires a new free choice of representational unit. Case might wish to argue that these choices of representational units are not as free as I present them, but I find no additional constraints in the theory on what they might be.

In chapters 7 and 12, transfer studies are reported that purport to test the central-conceptual-structure hypothesis in a different way. The basic logic of the studies is that, if representational capabilities are in fact organized into such conceptual structures, then training on a task within a conceptual structure domain should provide the necessary representational units for solving problems on other tasks, so long as they are in the same conceptual domain. As Case suggests:

> The conceptual underpinnings of the various tasks are actually represented in the mind of the child by a common conceptual structure, and . . . training in this structure can play a role in bringing about the developmental transition from 4- to 6-year-old thought. (223)

> The conclusion to which we were led by the training studies was that it is a mistake to see children as assembling executive control structures for each separate task in complete isolation from those for each other task, subject only to an upper bound on their processing capacity. Rather, it seemed more appropriate to view children as assembling a central conceptual structure that is applicable to a broad range of tasks, then utilizing this central structure, more or less successfully, as a guide for assembling the particular executive control structures that each new task may require. (355)

I do not find the training results presented persuasive, again because serious alternatives—rival hypotheses—have not been given strong attention. For example, in a study involving storytelling, the experimental group received training toward the sorts of abilities that were ultimately tested (training to the tasks), and the control group, although also engaged in storytelling activities, were trained in a thoroughly different direction:

> As can be seen from the above outline of classroom activities [for the control group], no effort is made to move beyond the form of story organization and expressive language that children use spontaneously. Instead, the approach focuses on the children's experiences in daily activities and with literature, and develops their mode of expressing these experiences through activities such as conferring, revising, and publishing. (221)

It is not at all clear what this control group was controlling for, and, therefore, it is not clear what support the expected results offer to the model.

Case might argue in response that the tasks involved in the "transfer" part of the study were in fact more distant from the task that was specifically trained than I am giving them credit for, and, therefore, that the finding of transfer was stronger than I am acknowledging. But, once again, this does not settle the matter: First, those "transfer" tasks do not seem at all distant to me, and, without further specification of what the domains are and what counts as "distance" within them, this issue stays at an indeterminate level of subjective impressions; second, the finding of transfer is relative to the control group, and, to repeat, it is not clear that the control group controlled for any strong alternative hypotheses. It seems to me that it is not at all clear what, if anything, can be made of the transfer studies.

In addition, some statistical questions must be raised about the studies in this book: Much of the time, analyses use age as a predictor; at other times, age is partialled out. I find no discussion of when or why either approach is appropriate. I can invent some plausible rationales for using one or the other method, but I have not gone back through the book to see if all usages are consistent with any of the rationales I can devise. I will refrain from attempting to outline the potential rationales, challenges, counterarguments, and so on that have occurred to me in trying to analyze these issues. The basic point remains that statistical considerations add to the tangle of potential unaddressed questions of and challenges to Case's model and results.

Similarly, at times a linear 2-year-interval sequence of attainment is statistically compared with a quadratic trend of attainment with age. There is no discussion of appropriateness. Rejecting a quadratic in favor of a linear sequence might seem to provide strong support to the 2-year-interval model, but, for three substages, the result is not strong anyway because there are no polynomial possibilities *other* than linear or quadratic. For more substages, a quadratic is itself a highly constrained and a priori unlikely alternative model. Treating age nominally would be a stronger contrast for the issue of linear versus nonlinear because it would compare linearity with all possible forms of nonlinearity. Furthermore, in cases where linearity is not strongly supported, the explanation is offered that the right representational unit has not yet been found, adding to the point about the free parameter of representational unit.

Basically, Case picks his units, his tasks, and his analyses. This is not an entirely free set of selections, but it is not shown how constrained those choices are. Therefore, it is not clear how strongly the eventual results that are *consistent* with the model in fact provide *support* for the model. If the results were consistent with *all possible alternative models* as well as with Case's, then they would provide no *support* for Case's at all, no matter how many such consistent results are presented.

In the ideal instance, there would be no free parameters—everything would be determined by the model, results would fit exactly, and no plausible alternative model could account for those results. The ideal form never occurs, not even in physics, but that then requires that assessments be made of how strong everything that remains really is. Such strength is always relative to how strong the alternative models are that are *not* consistent with the data; strength of empirical support for a model is inherently relative to the alternatives that are ruled out by the data. I find no such assessments.

## A diagnosis

The reader will find a tangle of unaddressed, intertwined, theoretical and methodological issues here. It is not remarkable that there is such a tangle—that is the nature of research—but it is remarkable that it is unaddressed. Furthermore, although I have been asked to review Case's book in this instance, there are far better examples of such unaddressed tangles of issues elsewhere in the literature. Case, in fact, is to be strongly lauded both for attempting to take into account a rival class of models and results, and for not succumbing to the myopic particularisms of some of those rival approaches that give up on the notion that there is anything in common in mentality and development.

The diagnosis for such unaddressed tangles that I want to suggest, then, is not directed specifically to Case's book, but includes it within a much broader charge. It is, in fact, a charge directed to contemporary psychology writ large. I will suggest my diagnosis hypothetically, because this is not the occasion for developed argument.

First, psychology is still largely in the thrall of the vestigial and conceptually corrupted neopositivism that it inherited from behaviorism: It has rejected a restriction to strict observables and it has rejected a restriction to associationism, but it has retained a naive inductivism, operational definitionalism, and other unproductive approaches (Bickhard, 1992; Bickhard, Cooper, & Mace, 1985). Fundamentally, psychology is permeated with false conceptions of the nature of theory and of the relationships between theory and evidence.

One aspect of this is an empiricism of the meaning of theoretical terms, together with an instrumentalism of theories. Because, in this view, theories are fundamentally only instrumental for accounting for data, and because meaning is itself already strictly empirical anyway—via operational definitions—the assumptions about science in psychology offer little support to considerations of conceptual and logical analyses of theories. If the data are consistent with the theory, then conceptual or logical challenges carry little weight. Conversely, it pays poorly for anyone to devote much attention to conceptual and logical issues concerning theory, because the rest of the field does not care, or at least has no philosophy-of-science rationale for caring. Consequently, unaddressed tangles of theoretical issues are, in my judgment, quite common in the field.

A second aspect is a focus on confirmatory research and methodology. Supporting data are the gold to be sought. This follows readily from the inductivism that is inherent in behavioristic positivism and is even more strongly urged by the corruption of operational definitionalism. Attention to rival hypotheses, alternative explanations, is limited to strictly methodological alternatives, and even then is not well motivated (just what is a control group for?—*within a strictly confirmatory, inductivist perspective?*). This exclusivity of focus on *methodological* rivals, such as, for example, attention or prior interest, seems itself to have derived from the behaviorists' exclusive focus on cause and control, and on experimental studies to establish cause and control. Design is focused on ruling out *nuisance* variables, not on ruling out conceptual or theoretical alternatives; conceptual alternatives about design and affecting design are rarely developed or addressed. Thus, there are *many* tangles of unaddressed conceptual-methodological issues.

The empiricism of positivism and operational definitionalism carries forward a view of science as somehow "seeing" deeply into the empirical patterns of nature. The more such sightings, the stronger the picture. Multiple sightings across many phenomena can simply cumulate, stitch together, to fill out the overall patterns of nature—in both senses this view presupposes an extremely naive inductivism. There is no logical role in this view for ruling out, for falsifying, conceptual alternatives. Falsifying a hypothesized pattern, of course, is of relevance, because the falsified hypothesized pattern cannot be accepted for stitching into the overall pattern, but a confirmatory "seeing" of a pattern is taken as support for that pattern independent of any considerations of conceptual alternatives ruled out. The very notion of "conceptual alternative" is difficult to conceive in this view: Theoretical meanings are taken to be *constituted* by postulated empirical patterns, so, if two models yield the same empirical pattern, they will be the *same* model. This is a gross confusion about the nature of theories and their relationships to empirical data. Such views of science have long been discredited and understood to be false (Bickhard, 1992), but they still dominate and distort psychology.

Clearly, I do not wish to claim that these points hold for each single instance. I do wish to strongly claim, however, that they hold as a general ideology of science across psychology, and that they are deeply harmful to the science of psychology. Bluntly, a great deal of research in psychology is a waste of time because it does not focus on any important space of conceptual alternatives, but rests satisfied with some nuisance-variable controls and claims of resultant "supportive" results.

*The Mind's Staircase* is a remarkable book presenting Case's attempt to take into account a thoroughly alien set of perspectives and results and generate a new model that could integrate and account for results across the entire span of consideration—in particular, both domain-general and domain-specific aspects of development. Case ends up with a model that seems to have just the right integrations and differentiations to be able to handle both specificity and generality of development. It is even possible that the tangles of challenges that I have discussed are subject to strong dismissals, and that Case's model will be the core of a successful account of development (although I have other theoretical criticisms that make me doubt that: Campbell & Bickhard, 1986, 1992). What is clear, however, is that those tangles are not addressed, and that Case is one of a vast company in that respect.

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## Children's Theories of Mind: Mental States and Social Understanding

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## Children's Understanding of Minds in Social Context

Children's theories about the mind, a topic of philosophical and psychological interest for decades, has become a major focus of current research in developmental psychology. This volume, based on a conference held at Yale University in 1988, contains a collection of important papers describing research projects that span the various facets of this topic. The chapters contribute to the field both by discussing unresolved theoretical issues and by describing new research that helps to answer questions about what develops in the realm of children's understanding of internal states such as belief, desire, and intention. Many of the papers in this volume address one of two key developmental transitions: (a) the emergence of intentionality and of an understanding of others' intentions in infancy, and (b) the emergence of metarepresentation (i.e., thinking of representations as representations) during the preschool years. One further point that is bolstered by much of the research reported in this volume is that children's theories of mind emerge in a social context and have important implications for children's interactions with other people. In this review, we will organize our discussion around these three points.

### Understanding of intention in infancy

In exploring children's understanding of other people's intentions, Douglas Frye's chapter begins by asking when infants' behaviors are first guided by their own intentions. This question has been debated for many years; in Piaget's analysis of sensorimotor development, for example, evidence of intention appeared when infants could engage in means-ends behavior between 8 and 12 months of age. Frye presents new data on infants' surprise at events in which a goal and its means are mismatched. For example, in one study infants had to push on a string (rather than pull on it) to make an object move toward them. Infants did not show strong evidence of intentionality on these tasks until 16 to 24 months of age. Frye claims that this intentional behavior leads to the understanding of intentionality in other people, and that this understanding in turn causes infants to begin to distinguish people from objects and to develop a theory of others' minds. In contrast to Frye's analysis, David Premack proposes that perception of intention in others may be a "hard-wired" ability. Premack suggests that there may be an innate predisposition for infants to infer agency from self-propelled movement. Although the argument that agency may be directly