This article introduces a collection of essays on continuity and discontinuity in cognitive development. In his lead essay, J. Kagan (2008) argues that limitations in past research (e.g., on number concepts, physical solidarity, and object permanence) render conclusions about continuity premature. Commentaries respectively (1) argue that longitudinal contexts are essential for interpreting developmental data, (2) illustrate the value of converging measures, (3) identify qualitative change via dynamical systems theory, (4) redirect the focus from states to process, and (5) review epistemological premises of alternative research traditions. Following an overview of the essays, this introductory article discusses how the search for developmental structures, continuity, and process differs between mechanistic-contextualist and organismic-contextualist metatheoretical frameworks, and closes by highlighting continuities in Kagan’s scholarship over the past half century.

In his essay, “In Defense of Qualitative Changes in Development,” Kagan (2008) addresses a contemporary controversy: Are the cognitive functions seen in infancy preserved into and beyond childhood or are they so transformed in ontogenesis that they should be viewed as qualitatively different functions? Kagan mounts an argument for the need for caution in the resolution of this debate. The five commentaries address Kagan’s points and offer additional arguments and empirical data bearing on the continuity–discontinuity issue. Taken together, the articles demonstrate the diverse ways that developmental scientists conceptualize, empirically study, and draw conclusions about continuous versus qualitative change.

Delineating the Questions

Kagan (2008) observes that scientists from a number of disciplines take as their primary goal “to explain how a phenomenon of interest developed its observed form” (p. 1606). He illustrates his point by drawing from evolutionary biology, embryology, and, his focus here, developmental psychology. After providing some historical perspective on prevailing developmental beliefs of earlier eras, Kagan critiques the currently popular view that individual development is best characterized as quantitative rather than qualitative change. Typical of such accounts are nativist theories and research programs that imbue infants with an assortment of underlying and fundamentally adult-like competencies that need merely to be triggered and sharpened by contact with the world. Kagan notes that the theoretical and empirical claims for preservation of function from infancy to maturity have been offered in two major arenas of contemporary developmental psychology, one the preservation or continuity of early emotional attachment, the other the preservation or continuity of early cognitive functions. It is the second of these he addresses in the core of his article.

Kagan identifies two major problems with contemporary research programs that promulgate cognitive continuity. The first concerns language. Kagan argues that too often identical labels are used for behaviors that might, on the surface, appear identical, but that, at root, might be distinct. One illustration he gives is the use of the term imitation to label an infant’s tongue protrusion in the presence of an adult’s tongue protrusion. In the neonate, protrusions might reflect infants’ tendencies to use their tongues to explore nearby objects; in the older infant, they might indicate infants’ representation-based copying of another person’s behavior. Applying the identical term—imitation—to both earlier and later instantiations of an observable behavior, “tempts less sophisticated readers to assume that the two phenomena are exemplars of the same process” (p. 1607). Kagan thus urges the use of different terminology for cognitive functions that are what he calls schematic (containing only perceptually available information) from those he calls semantic (containing additional, based...

I am grateful to Michael Chandler and Willis Overton for their thoughtful comments on an earlier draft of this article.

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information as well). Of course, just as someone wishing to leave room for qualitative change may argue that identical labels for earlier versus later behaviors are misleading, so, too, someone arguing for continuity may argue that different labels are misleading. Despite the possible legitimacy of both positions, the former arguably has more traction because—as in any coding system—one can collapse categories later, but one cannot recover distinctions that were initially ignored. Even then, though, one would still be left with the dilemma of deciding—in the absence of agreement about what underlies any given observable behavior or how to differentiate one observable behavior from another—when to apply which label.

Kagan’s second major critique revolves around the empirical database that he claims is far too weak to support conclusions about infants’ cognitive functions that are routinely drawn by contemporary investigators. Kagan, like others before him (e.g., Haith, 1998) faults the field for its heavy reliance on looking time methods. His critique is in part based on the varied bases for looking time per se and in part based on the more general point that converging methods and data are needed in research. Kagan draws from research in three domains—the concept of number, physical solidity, and object permanence—to illustrate his conceptual and methodological points related to continuity and transformation. He ends his article by urging the use of a greater range of measures and greater attention to the role of the maturing brain.

Accepting the Challenge

Quinn (2008) accepts Kagan’s challenge as an empirical challenge. Quinn agrees with Kagan that the findings from many empirical studies with infants have indeed been flawed and overinterpreted. He argues, however, that some investigations do use the kinds of empirical approaches to study infant cognition that Kagan recommends. To illustrate, he selects the example of infant categorization and draws from his program of research to illustrate how research using multiple approaches (e.g., looking times, event-related potentials, computational modeling) converge on the conclusion that category structures of infants (e.g., for cats and dogs) are indeed similar to those of adults. Thus, Quinn makes the case for why, to use Kagan’s terminology, schematic categorization may indeed be linked, continuously, to semantic categorization. Interestingly, though, the “structural similarity” of Quinn’s illustrative focus is on whether infants’ and adults’ assignments of animals into cat versus dog categories match. It is not, for example, on whether infants and adults share an understanding of the logical relations between and among categories (e.g., cats, dogs, animals) such as would be taken as a hallmark of classification understanding by Piaget (1952).
Redefining the Challenge

Spencer and Perone (2008) join Kagan in his defense of qualitative change, writing from their explicit commitment to dynamical systems theory (DST). Before assuming highly congruent perspectives, however, it would be wise to remember Kagan’s first point about problems stemming from the use of identical words for what may be fundamentally different constructs or behaviors. To Spencer and Perone’s credit, they explicitly note the confusion that results from discrepant definitions, and they thus begin their commentary by providing their theory-embedded definition of the term “qualitative.” Specifically, and as explained in detail in the commentary, the term qualitative change is used within DST to refer to changes from one stable attractor state to another. One feature of their approach is that the behaviors being modeled are unfolding on the second-to-second timescale. Another is that qualitative transformations (as defined within DST) can emerge from continuous changes. Spencer and Perone argue that the study of qualitative shifts over broad periods of time (as in the years between infancy and childhood) show little about the way behavior in real time is organized. Their approach is thus based on examining tiny units that are intentionally removed from the kinds of analyses and constructs promulgated by Campos et al. (2008; see also Witherington, 2007).

Redirecting the Challenge

Adolph and Robinson (2008) offer a perspective that redirects the focus of discussion. They argue that much of the conversation about development has emphasized early versus late states per se, and that scientists interested in progressions would be better served by turning their attention away from characterizing beginning, end, and intermediate states (stages) and toward the process of developmental change. They, like Spencer and Perone, ground their work in a systems framework, although the two groups provide different kinds of illustrations. Spencer and Perone illustrate their work by running tens of thousands of computer simulations of neuronal behavior. Their figure 1 shows “real-time changes of time (as in the years between infancy and childhood) show little about the way behavior in real time is organized. Their approach is thus based on examining tiny units that are intentionally removed from the kinds of analyses and constructs promulgated by Campos et al. (2008; see also Witherington, 2007).

Redefining the Challenge

Mueller and Giesbrecht (2008) join Campos et al. (2008) in arguing for the importance (and legitimacy) of examining any particular behavior at any particular moment within (rather than isolated from) an extended developmental sequence. Furthermore, they note the value of interpreting earlier steps in view of final steps, citing Piaget’s method of recurrence as a model. This approach contrasts strikingly with the second-to-second methodology of DST discussed by Spencer and Perone (2008), which is intentionally focused on the “local details of behavior.” The bulk of Mueller and Giesbrecht’s commentary is directed to the broader epistemological differences between contemporary neonativist approaches that emphasize continuity across age (e.g., Baillargeon, Spelke, Wynn) and classic theories that identify transformations across age (e.g., Piaget, Werner, Vygotsky). They situate their discussion of programs of empirical work within the philosophical traditions of rationalism and empiricism, explain ways in which different epistemological premises lead different theories to pose fundamentally different questions and, relatively, to use fundamentally different methods. Mueller and Giesbrecht illustrate these points by contrasting nativist versus constructivist accounts of both representation and reasoning, drawing specific contrasts between the research of Baillargeon and Piaget.

At the End of the Joust

Jousts are categorized as à plaisir (for pleasure) or à l’outrance (to the death). The joust ensuing from Kagan’s tossed gauntlet surely has not resulted in the death of either the continuity or the discontinuity positions. What, then, can be concluded from this collection of articles?
First—as argued in virtually any article addressing virtually any domain—the field needs more and better empirical work. Particularly important are programs of research that bring converging methods to bear on a given conclusion. This point is made emphatically and clearly by Kagan in his lead article, and it is endorsed in all five commentaries. Campos et al. (2008) provide some additional principles for how empirical work can be better; Quinn (2008) provides specific illustrations of better empirical work that can serve as a model for converging designs. Spencer and Perone (2008) as well as Adolph and Robinson (2008) discuss alternative methodologies and analytic tools for new kinds of empirical research.

Second—and again as recommended in virtually all developmental articles—the field needs more and better research on developmental process and not just on products. This point is made most explicitly in the two commentaries by Adolph and Robinson (2008) and by Spencer and Perone (2008). Observing and modeling behaviors as they unfold over time can reveal trajectories and patterns that would otherwise be invisible in the behavior itself. At the same time, though, the collection of commentaries makes it clear that recording and modeling observable incremental behaviors does not in itself resolve the problem of identifying process. That is, one can document and model behavioral differences at increasingly more microscopic levels of analysis, but this alone need not reveal causal mechanisms, at least not the kinds of causal mechanisms that may be sought. An illustration of this issue is found in Müller and Giesbrecht’s (2008) discussion of the contrast between the way that Baillargeon (2008) and Piaget (1963) approach infant reasoning. They argue that Baillargeon endows the infant with nativist principles or computational devices that process encountered information. Given these devices, the infant is in essence merely a spectator. In contrast, Piaget endows the infant with the functional activity of assimilation. This assimilation (and its conjoined twin, accommodation) entails the infant’s active engagement with the world, thus rendering the infant an intentional agent. But are both kinds of processes scientifically legitimate or even real? The answer to this question is, “It depends.” That is, an explanatory process that is legitimate within one theory (e.g., assimilation and accommodation of Piaget) is dismissed as ephemeral by another (as when Klahr labeled these the “Batman and Robin” of developmental psychology; see Adolph & Robinson, 2008). These conflicting answers lead directly to the last point (which in essence encompasses the prior two).

The third message that may be derived from these articles is that the field needs to attend more actively to the metatheoretical context in which research is designed, conclusions are drawn, and disagreements are discussed. A failure to contextualize theories and empirical data in metatheory results in scholars misconstruing one another’s meaning, or, alternatively, ignoring one another’s meaning altogether. This point is developed most explicitly by Müller and Giesbrecht (2008), but it pervades the collection of articles. Constructs (e.g., concept of number, cat and dog categories) are formulated in radically different ways depending on the metatheoretical frameworks from which they emerge; likewise, the search for origins and processes differs.

As discussed in detail by Overton (2006, 2007) and Witherington (2007), theories rooted in the principles of a mechanistic–contextualist framework necessarily assert the existence of a base level of pure elements (e.g., behaviors, neurons, genes) that combine additively to produce wholes. Hence, the strategy of mechanistic–contextualist approaches is to study ever finer, directly observable units, with the goal of reaching a foundation that reveals chains of cause and effect elements. This is the mechanistic–contextualist interpretation of “process.” In contrast, theories rooted in an organismic–contextualist framework eschew the assumptions of additivity of elements and the ultimate goal of identifying a bedrock foundation. Instead, their holistic perspective asserts that truly novel forms or dynamic systems emerge as the system acts upon the world. What emerges is thus different from the sum of the parts; integration yields novel systemic properties that are not characteristic of any of the parts. There is still continuity insofar as later forms subsume earlier forms, but there is also discontinuity or qualitative change.

To take a perhaps worn but still useful analogy from chemistry, the viscosity, color, and drinkability of water are emergent characteristics of water that could not be predicted from or observed within the properties of its constituent hydrogen and oxygen. Explicating the moment-by-moment processes by which electrons from hydrogen and oxygen merge in the outer rings of the molecules would still fail to explain or predict the qualities of water. Kagan’s example of the movement from a perceptually based representational system to a symbolically based representational system is a more immediate developmental example. Within the mechanistic–contextualist perspective, empirical work entails the search for the bedrock appearance of the symbol at birth (or perhaps later, but still merely triggered by some biological or environmental stimulus). In the contrasting
organismic–contextualist perspective, empirical work entails identifying an emergent property of a system arising from actions and integrations of earlier sensorimotor action systems. Here, symbolic representation would not be expected to be apparent in earlier forms, any more than qualities of water would be expected to be apparent in hydrogen and oxygen. Neither approach will be validated by closer observations or by converging data. Their adequacy will be assessed according to the criteria that are generally applied in the selection of any adequate scientific explanation such as the explanation’s depth, coherence, and logical consistency.

In the end, then, it should be clear that the current collection of articles does—as claimed in the initial paragraph of these introductory comments—“demonstrate the diverse ways that developmental scientists conceptualize, empirically study, and draw conclusions about continuous versus qualitative change.” It should be equally clear, though, that these thoughtful contributions do not converge on a single position. As long as scholars approach development from different epistemological frameworks and meta-theories, there will be plurality, not consensus.

**Continuity in Scholarship on Continuity**

Irrespective of one’s position on continuity in the study of individual development, in the everyday meaning of the word, it is incontrovertible that there is continuity in Kagan’s scholarship. Over half a century ago, Kagan and Moss (1962) opened their seminal book on the Fels longitudinal study by saying: “The search for stability in physical and biological systems is a lure that attracts much scientific inquiry. The stability of that system we popularly call human behavior has long whetted the curiosity of most men, be they scientists *qua* scientists or scientists *qua* parents” (p. 1). Save for the now anachronistic use of the word *men* rather than *people*, this opening sentence could have been pulled from Kagan’s current article.

And, roughly two decades after the appearance of the Fels book, but still roughly three decades ago, Kagan coedited with Brim a collection on *Constancy and Change in Human Development* (Brim & Kagan, 1980a). What made that volume so novel at the time was first, its consideration of stability and change across the entire life span, and, second, its incorporation of non-Western concepts of stability and change. The chapter by Kagan (1980) within it provided a rich discussion of how the concept of continuity has been conceptualized from a range of disciplines, eras, and philosophical positions.

Despite the long history of work on continuity by Kagan and others, key questions remain unresolved. As discussed in the commentary by Müller and Giesbrecht (2008), and as amplified in both earlier and contemporary analyses of these issues (e.g., Kagan, 1980; Overton, 2006), these questions cannot be resolved simply by better and better empirical work. In this context, it is useful to recall Brim and Kagan’s (1980b) early observation: “Constancy and change are inventions of the mind, of course, a matter of subjective magnitude” (p. 2).

Given that readers will approach the articles that follow with different theories and metatheories, it is unlikely that all will derive the same conclusions about continuity and discontinuity from reading them. All will, however, come away from this collection thinking more deeply and productively about the rich conceptual and empirical work on continuity already in our books and journals, and better equipped to think more deeply and productively about the related work that will undoubtedly continue to fill our literature in the decades to come.

**References**


