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Maturational Factors in Human Development

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The rationalist-empiricist argument with respect to the human capacity for language has been extensively debated in recent years (Chomsky, 1975; Fodor, Bever, and Garrett, 1974; Lenneberg, 1967). What in human language is due to genetic endowment, and what is due to interactions with the linguistic community? Rationalists and empiricists agree that there are contributions from each; they differ in their specifications of how much is innate, what kinds of things can be innate, and what kinds of interactions with the environment affect acquisition.

Related to nativism is the explanation of developmental change. It is natural though not logically compulsory for the extreme rationalist to expect major maturational contributions to development. Similarly, it is natural for the extreme expiricist to expect that most development is due to learning. Thus Eric Lenneberg, who was committed to the rationalist position, wrote, "It can scarcely be doubted that the development of language capability is somehow related to the maturation of the nervous system" (Lenneberg, 1974). He went on to bewail the lack of hypotheses about what changes in the brain might be relevant to the acquisition of the language. Nothing has changed, of course, since Lenneberg wrote those words. Whatever is known about maturational changes of the nervous system has not yet been made to yield specific hypotheses about the course of language development. These observations also apply to perceptual and cognitive development more generally.

Perhaps it is premature to attack the issue of a maturational component to behavioral change from this direction. Lenneberg might also have gone on to bewail the small number of hypotheses based on behavioral studies about which aspects of language development might be affected by maturation of the central nervous system. The papers in part 1 suggest that for human development the issue can fruitfully be addressed from the direction of behavioral studies alone, perhaps more fruitfully at this time than from studies of development of the human nervous system.

What is meant by a maturational component to linguistic, perceptual, or conceptual development? No genetic program is carried out in the absence of environmental input, including genetic programs for development. All behavioral development results from a series of interactions between neural substrate and feedback from the world. By the maturational component to conceptual development we mean the developmental changes resulting from the aspects of growth or reorganization of neural substrate that are determined by the genetic program.

A hypothetical example might clarify what is meant by a maturational component. Suppose that all genetically programmed developments in the nervous system are completed by the second decade of life. Suppose then that a blind man regains his sight and learns to discriminate faces. We could describe his acquisition of face-recognition skills in terms of changes in his representations of faces and in the processes by which he recognizes familiar faces and encodes previously unfamiliar faces. By hypothesis there would be no maturational component to this development, since all maturational changes in the state of his nervous system are complete. We must also describe a child's acquisition of face-recognition skills in these same terms. But in the child's case it is possible (but only possible) that some maturational changes affect this developmental course. The challenge, of course, is to specify reasonable hypotheses about the maturational factors that might contribute to development and then to bring relevant evidence to bear on those hypotheses.

Waber comments that in an earlier era of psychological inquiry, assessing the maturational component to development was considered a legitimate and exciting topic for research. In light of the progress made in developmental neurobiology since then, some of the work from that era seems naive and was certainly inconclusive but, I would argue, not fundamentally misconceived. Most of the work of that earlier period concerned motor development (Carmichael, 1926; McGraw, 1935, 1943; Gesell and Thompson, 1943). The starting point was Carmichael's classic demonstration that the emergence and organization of swimming in infant frogs and salamanders is under maturational control. He showed that young animals paralyzed for the five-day period during which normal controls begin to swim, swam identically to the controls when the paralyzing drug wore off.

In the case of development in human infants, three kinds of evidence were taken to support the hypothesis of similar maturational control of the emergence of motor skills. First, descriptive work established detailed invariance in the sequential steps of motor development across children. This invariance holds at fine levels of detail and sometimes appears arbitrary with regard to imaginable experiential sources. In many cases these sequences were strictly contemporaneous in freely developing identical twins (Gesell and Thompson, 1943). Second, deprivation studies were devised to directly mimic the logic of Carmichael's experiments. Dennis (1940) compared the onset of walking in Hopi infants strapped to cradle boards for nine months with Hopi infants allowed to move freely, creep, crawl, and stand like American children. The age of onset of walking was identical (fifteen months) in the two groups. In another study (that makes one grateful for current HEW guides on use of human subjects) a mother lent her newborn twins to two experimenters for nine months. During this period the twins were kept lying on their backs all day, sometimes with their hands bound or secured under sheets, with no toys and no opportunities to interact with each other. Caretakers did not smile at or vocalize to the babies. Nonetheless, the emergence of all the major infant milestones (smiling, cooing, sitting, grasping) in the first nine months was within the normal range for both twins (Dennis and Dennis, 1935). Finally, Gesell and his co-workers developed the method of co-twin control, in which one member of a pair of identical twins was given practice and exercise in some emerging skill while the control was denied it. For example, at forty-six weeks one twin was given six weeks of practice, ten minutes a day, at climbing stairs. The other twin was denied access to stairs. After this period the practiced twin performed the skill better than the control, but the control climbed the stairs spontaneously, and training was markedly more efficacious for the control twin when begun at this time. Two weeks of training for the control twin were equal to the six weeks for the experimental twin, and after three weeks the control twin had completely caught up (Gesell and Thompson, 1943).

As is typical of American developmental psychology of the time, there was little theoretical work on alternative models of how maturation might contribute to development. The basic concept was maturational readiness; the child cannot benefit from relevent experience until his nervous system has matured in some relevant respects. In the case of motor development two candidate sites had their partisans: the cerbral cortex (McGraw, 1943) and the cerebellum (Shirley, 1933). Arguments in favor of each were indirect; each was known to play a role in the control of motor activity in adults, and each was known to develop markedly during infancy.

Although this evidence is suggestive, it is certainly not conclusive. No maturational argument follows directly from invariant sequences. Further, Dennis's deprivation studies and Gesell's method of co-twin control depend crucially on the successful identification of the experience relevant to the acquisition of a particular skill. In the absence of more detailed hypotheses about mechanisms of development, we should be skeptical about the ease with which this can be done. Finally, although these studies demonstrate the theoretical need for a concept of readiness in explanations of development, they do not succeed in demonstrating that readiness emerges as the result of maturation of the nervous system. A concrete example is the method of co-twin control used in a study of vocabulary acquisition (Strayer, 1930). At nineteen months a pair of twins was separated for five weeks. During this period the experimental twin was given two hours of vocabulary drill daily and was otherwise provided with her normal environment. The control was cared for by loving caretakers who hummed to her, played games with her, gestured to her, but did not speak. The control twin apparently remained cheerful through all this and gestured and pantomimed a great deal by the end of the period. Not surprisingly, the experimental twin's vocabulary was much greater. The twins were reunited, and the control twin was given training treatment that had been given the experimental twin. Day for day, the control twin learned faster, and Strayer concluded this was due to maturational readiness. But the control twin's conceptual development had not been impeded; her daily activities, including play with toys, had been normal. It is equally possible that the readiness that allowed her to make better use of the same vocabulary training was conceptual (see Brown, 1958; Bowerman, 1977; Leehey and Carey, 1978; Carey and Bartlett, 1978, for discussions of the conceptual component to vocabulary acquisition). It is possible that maturational factors played no role whatsoever.

I am not denying that motor development is largely under maturational control or even that vocabulary development may have a maturational component. My point is simply that the available evidence relevant to these hypotheses is inconclusive with regard to vocabulary development and merely highly suggestive with regard to motor development.

The work reported in the four papers in part 1 differs from the early work in several respects. First, its domain is not motor development but perceptual and conceptual change. Second, it concerns not infancy but development in childhood through adolescence. Most important, this work attempts to make very clear the kinds of maturational influences on development that are supported by the behavioral data. This is a necessary step in constraining hypotheses about an actual maturational mechanism.

Recent advances in developmental neurobiology have provided animal models of maturational mechanisms of various kinds. There are three familiar examples.

- The genetic program specifies critical periods during which the conditions of input determine permanent characteristics of the nervous system (Hubel and Wiesel, 1970).
- Immaturity of the functional organization of particular areas of the brain places upper limits on the capacities that can be achieved at that point in development (Goldman, 1972).
- Species-specific behavior patterns emerge at a predetermined time, and some features of their organization are relatively uninfluenced by environmental variables (Nottebohn, 1970).

In all these cases, as in the case of motor development, the goal is to relate changes at the behavioral level to maturational events within an individual. Carey and Diamond entertain a fourth maturational hypothesis of this type—that maturationally induced changes in the nervous system cause a temporary disruption of behavior. Waber's research departs from the tradition that examines the maturational components in an individual's developmental history. She relates stable differences in cognitive profiles among different people to differences in their rates of maturation in childhood and, especially, adolescence.

The goal of the papers in this section is to reopen the issue of the maturational component to cognitive development as a legitimate, and empirically tractable, area of research. The approaches of Waber, Carey and Diamond, Rose, and Denckla, Rudel, and Broman differ in specifics but share common assumptions. First, they share the belief that psychological evidence for maturational factors in developmental change must precede and constrain specific hypotheses about neural mechanisms. Second, assuming that such behavioral evidence is forthcoming, they share the belief that specific hypotheses about mechanism can and must be formulated and tested. Finally, they look to the burgeoning field of developmental neurobiology as a source of animal models for maturational mechanisms. In Rose's case the role of the animal model is most transparent. Although a much wider net for relevant data (from normal development, brain-damaged adults, learning-disabled children, lesioned animals) is cast and much more theoretical latitude allowed, it should be obvious that the approach in these papers is continuous with that of the twenties and thirties.

In sum, like Lenneberg, we believe that genetically programmed changes in the nervous system must play a role in the explanation of human conceptual and linguistic development. Unlike Lenneberg, we do not start from what is known about maturation of the human nervous system. We propose to start from behavioral evidence for a maturational component to human development. We then use what is known about the development of nervous systems in general to generate more specific hypotheses about what maturational changes in the human nervous system might underlie these maturational influences that have been supported by behavioral evidence.

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