accurate observer. How was the value for precession bungled? In order to get the zero point of the stellar coordinate system, it is necessary to relate the daytime position of the sun to the nighttime position of the stars, no mean task. According to Neugebauer, the fault must lie largely in the observational technique, which involved measuring the star Spica with respect to the moon during a lunar eclipse. Clearly, the theories of the motions of the sun and moon are tightly bound up in this procedure. Since Ptolemy remarks that Hipparchus's eclipse data were seriously marred, Neugebauer points to this as the crux of the problem.

Neugebauer writes,

> In all ancient astronomy direct measurements and theoretical considerations are so inextricably intertwined that every correction at any one point affects in the most complex fashion countless other data, not to mention the ever present numerical inaccuracies and arbitrary roundings which repeatedly have the same order of magnitude as the effects under consideration. In the history of the most causal of all empirical sciences, in astronomy, the search for causes is as fruitless as in all other historical disciplines.

The difference in attitude between Neugebauer, a mathematician who has immersed himself in the languages and techniques of ancient science, and R. R. Newton, a physicist who is eager to extract specific results on the deceleration of the earth's rotation, is shown perhaps most clearly in their respective reexaminations of the lunar eclipse of A.D. 135.

According to Ptolemy's epicyclic model, the moon approached twice as close to the earth at quarter phase as when new or full, a situation clearly in conflict with the simplest observations. Ptolemy not only chose to ignore this untenable discrepancy, but in determining the lunar distance he picked the time of closest approach. The result was badly wrong, 40 earth radii instead of 60; nevertheless this apparently confirmed a model that had quite satisfactory distances for eclipses. The erroneous answer at quadrature, which fit so well with all the rest of the theory, was achieved by reporting a lunar position off by 2/3 of a degree.

Did Ptolemy forge this observation, as R. R. Newton would have us believe? Or do we just have here evidence of "uncontrollable" observational and "quite unnecessary" trigonometric inaccuracies yielding "one of the most unsatisfactory topics in the whole Almagest"?

In my own opinion, Newton's attack on Ptolemy is at the very least anachronistic. Ptolemy, using clumsy mathematics invented only a generation earlier, made possible for the first time calculations of the local circumstances of solar eclipses. His planetary theory allowed tolerably accurate predictions to be made for over a millennium. It is hard to imagine that such success rested on fabricated observations. It is nevertheless possible that, in those days before error theory was understood, selected observations were adjusted for pedagogic purposes and thus recorded in the *Almagest* in close agreement with the theory—a theory undoubtedly resting on far more data than Ptolemy specifically reports.

In summarizing the section on lunar parallaxes Neugebauer gives both an evaluation of Ptolemy and an effective appreciation of his own *History*:

> No ancient astronomer had any possibility of analyzing sources of errors in observations made long before his time or at far distant localities. It makes no sense to praise or to condemn the ancients for the accuracy or for the errors in their numerical results. What is really admirable in ancient astronomy is its theoretical structure, erected in spite of the enormous difficulties that beset the attempts to obtain reliable empirical data.

He goes on to say, about Ptolemy,

> Without the cinematic theories of the Almagest it would have been impossible to introduce, on the basis of better observational techniques, those improvements which found their explanation in Newton's celestial mechanics.

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Changes in Biology

Crystals, Fabrics, and Fields. Metaphors of Organicism in Twentieth-Century Developmental Biology. DONNA JEANNE HARAWAY. Yale University Press, New Haven, Conn., 1976. x, 232 pp. $15.

This is a curious and unusual book. The author argues that in biology a revolution, in Thomas Kuhn's sense, occurred in the early portion of this century when the mechanistic paradigm "and its associated metaphors" of Jacques Loeb and others were "successfully challenged" by a "nonvitalist organicism." What is meant by the latter is the idea that, however analytical and dissecting one's experiments, one should not forget the whole live beast. To substantiate her argument the author has chosen to discuss in detail the works and the views on the philosophy of science of Ross G. Harrison, Joseph Needham, and Paul Weiss.

A major part of the book contains excellent reviews of the biological life work of these three distinguished scientists. The case for the attribution of organicist views is clear with respect to Needham and Weiss, but less so with respect to Harrison. In fact, on p. 95 the author shows that Harrison rejected the "emergence" of Lloyd Morgan and the "holism" of Jan Christian Smuts. But these, by a subtle distinction that is discussed in some detail at various points in the book, are considered vitalistic forms of organicism. Harrison rates as an organicist because of his profound concern for problems of polarity and symmetry, the basis of many of his beautiful experiments.

My difficulty is that I am neither a historian nor a philosopher of science, and feel rather like a swine unable to appreciate the shower of pearls. As a working biologist and an amateur admirer of Thomas Kuhn's book, I would not have thought of the shift from Loeb's mechanism to the generally accepted organicism of the 1940's as a scientific revolution. In the modern history of biology I would have selected only three genuine revolutions: Darwin's conception of natural selection, Mendel's genetics and its marriage to cytology, and molecular genetics. My simple-minded interpretation of the whole cycle of events from the 19th century onward involving vitalism, mechanism, organicism, reductionism is that these changes are a rather crude reflection of the fact that biologists go through periods when they think they know everything (mechanism, reductionism) and others when they react against the simplicity of such views and stress that there is more to the problem than meets the eye (vitalism, organicism). All this is really an indication of our anxieties, or lack of them, about our progress.

At any one time, the experimentalist carries on regardless of the prevailing (or his own) optimism or pessimism about our ultimate understanding of living systems. A splendid example is given in *Crystals, Fabrics, and Fields*. When Hans Driesch discovered regulation in sea urchin embryos in 1891, he was so convinced that no machine could behave in such a way that he turned to vitalism. I do not share the author's view that this was the beginning of a new paradigm (at best it introduced a miniparadigm), but think instead that Driesch did not know so much about machines as we do today. It is true that because of the extraordinary success of molecular biology and genetics we are now in a period of confidence that sharp progress in the analysis of mechanisms of development is at hand. But I predict that in some years to come it will be clear to everyone that although we have made progress (in the manner of "normal science," in Kuhn's terminology), there are still matters which need to be understood for which we simply don't have the tools. At that juncture we will certainly not return to vitalism, and perhaps not even to organicism, but will turn to some more sophisticated way of labeling what we still need to know.

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Perceptual Development

Infant Perception. From Sensation to Cognition. LESLIE B. COHEN and PHILIP SALAPATEK, Eds. Academic Press, New York, 1975. Two volumes. Vol. 1, Basic Visual Processes. xvi, 426 pp., illus. $22.50. Vol. 2, Perception of Space, Speech, and Sound. xvi, 246 pp., illus. $17.50. Child Psychology Series.

The scope of this compendium on the development of perception in infants is broad, the topics it covers ranging from the characteristics of single neurons to perceptual-cognitive processes. The chapters vary in quality and should be read selectively.

Volume 1 focuses on fundamental visual processes, beginning with Daphne Maurer's introduction to the anatomy of the visual system, electrophysiological methods of assessing its activity, and the corneal reflection technique of determining where an infant is looking. These reviewers recommend concentration on the eye-movement monitoring techniques.

Bernard Karmel and Eileen Maisel proffer a neuronal activity model of infant visual attention. A lack of close acquaintance with the vast literature on neural and animal behavioral development is apparent in this chapter, and sections 2 and 3 in particular might be skipped. Instead of the details, the reader should concentrate on the fundamental points: that stimulus saliency is determined in part by the phylogenetically developed structural and functional organization of the nervous system; that visual fixation time follows an inverted-U function of the density of pattern contour at the retina; and that contour density is reflected in the late negative component (L–P) of the visual evoked potential curve in infants younger than 6 weeks but in the early positive component (P_2) at later ages. However, the tidiness of the concept of contour density is challenged in a later chapter by Robert Fantz, Joseph Fagan, and Simón Miranda, who argue that many stimuli used to manipulate contour density (checkerboards, for example) actually incorporate two stimulus dimensions, the size as well as the number of elements in the pattern.

In the most ambitious chapter of volume 1, Philip Salapatek reviews the neurophysiological and behavioral literature on pattern perception in infants, including anatomy, saccadic localization, visual scanning strategies of geometric and social stimuli, and attention to differences in stimulus pattern. This chapter is a monumental contribution to our comprehension of perceptual development. Salapatek draws on the vast literature on the anatomical and neurophysiological development of humans and subhumans to explain and describe what must underlie much of early human perceptual development. In an intriguing epilogue, he conjectures that the earliest meaning attributed to distance or space cues beyond simple discriminability may be rooted in differences in eye movement patterns required to scan particular forms.

Whereas Karmel and Maisel and Salapatek are concerned with the neurophysiological basis of perception in the first 2 months of life, Fantz, Fagan, and Miranda survey the behavioral literature on visual capacity (discrimination), selectivity (attention), and recognition (memory) predominantly in midinfancy. There are substantive contributions here—the salience of curvilinear as compared to rectilinear patterns, the joint role of size and numerosity of elements in a stimulus pattern as determinants of fixation, the import of the postmenstrual as distinguished from the postnatal age of the infant for a variety of perceptual dispositions, and the similarities and differences between Down's syndrome and normal infants in attentional behavior and recognition memory. But the review is convoluted, primarily because the field has relied on comparisons between isolated pairs of stimuli, rather than using independently scaled dimensions of stimuli with at least three levels and applying scaling techniques, especially multidimensional ones, to fixation preferences.

In the final chapter of volume 1, Leslie Cohen and Eric Gelber perform a valuable function in bringing together data, many of them generated in studies of other phenomena, bearing on memory in the infant. The study of memory has great potential importance, and the chapter raises issues of interest, such as the retention capability of young infants, transitions in memory ability that occur at 2 to 3 months of age, and sex differences in encoding and memory. But research in this field is difficult and complicated, and this chapter represents only one point of view. For example, Cohen and Gelber propose that the familiarization of an infant to a repeated visual stimulus can be understood better by plotting the infant's looking time backward from the trial on which the infant reaches a criterion of habituation based on proportional decline in looking time. The combination of such a habituation criterion and backward plots capitalizes on chance variations in looking time, however, and many of the attributes of the curves presented are artifacts of the approach. Several of the ideas suggested by these curves may be correct, but this method does not provide convincing support for them.

Volume 2 includes chapters on the infant's perception of space, objects, social beings, and speech and sound. Albert Yonas and Herbert Pick provide an epistemological inquiry into the perception of spatial representation, suggesting that generalization be withheld until results converge on a common interpretation across a variety of stimuli and responses, an admonition reminiscent of Garner, Hake, and Eriksen's strategy of "converging operations." It is still a cogent recommendation.

Then T. G. R. Bower, in the volume's most intriguing piece, argues that an infant cannot perceive the third dimension unless it knows that objects that have gone out of sight still exist—that is, unless the infant has a grasp of object permanence. Bower believes that even very young infants possess this knowledge, despite the conventional literature that places its appearance late in the first year. He argues that the typical procedures used to assess an infant's knowledge of object permanence (for example, determining if the infant attempts to retrieve an attractive object it has seen being covered with a cloth) are tests not of knowledge of object permanence but of knowledge that an object has not changed when it is placed inside another object.

Those fond of criticizing Bower will find much ammunition with which to attack this position. For example, the argument rests prominently on two studies by Bower and Wishart (*Cognition*, 1972). In Bower's chapter he claims that infants are as unlikely to reach for an object placed under a transparent cup as for one hidden under an occluder and that this demonstrates that the object is transformed by being inside the transparent cup. But according to Bower and Wishart's original data four times as many infants (albeit