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Book Reviews

Cutting, J. E. *Perception with an eye for motion*. Cambridge, Mass.: The MIT Press, 1986. Pp. 321. ISBN 0–262–03119–1. £29.95.

Few issues in visual perception have generated as much heat as the debate concerning Gibson's direct perception and the possible use of perceptual invariants by the visual system. The only other such debate that springs to mind centres on the question of whether the visual system uses Fourier analysis for pattern recognition (the two debates may be linked—does a suitably scaled spectrum constitute an invariant for the object that produced it?). Cutting's book stands firmly in Gibson's camp, with its emphasis on the information in the optic array, rather than Marr's emphasis on process, or Helmholtz's pre-occupation with unconscious inference.

In accordance with this theoretical perspective, the first section of the book examines the information for vision, beginning with the author's view that "the human visual system is a sophisticated geometry-analysing engine", recovering invariant information about the shapes of objects or the relations among objects. Several chapters cover types of projection, the optic array, picture perception, and philosophical issues concerning space and geometry. Much of this material is engaging, informative, and well written. The section concludes with a chapter on invariants, which, according to Cutting, can only be accepted as such if they are specifiable as a real number or as an ordered relation among real numbers.

Only a third of the way through the book is motion perception introduced for the first time. A series of experiments addressing two issues is presented: First, how is rigidity (or the lack of it) detected in perspective projections of moving objects? Second, how do we judge our direction of locomotion through a static environment ("wayfinding", as Cutting calls it)? The experiments have not been published before, although they have been presented at various conferences. None of them is robust enough to bear the heavy burden of theoretical analysis that precedes and succeeds them, though I felt more comfortable with the treatment of wayfinding than the treatment of rigidity.

Taking rigidity first, Cutting explores the idea that the visual system "picks up" the cross ratio, which concerns the perspective projection of four collinear elements on a planar surface. Consider four points, *ABCD*, along a

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line on the surface of an object. The canonical cross ratio is defined as $(AD{:}BC)/(AC{:}BD)$; the product of the distance between the outermost points AD and the distance between the innermost points BC, divided by the product of the distances between the remaining non-adjacent points AC and BD. It remains constant regardless of the position from which the points are viewed (unless the position falls on the line itself), if the object remains rigid. Deviations from rigidity change the cross ratio, so the ratio is a correlate of rigidity and can be classed as an invariant (there are a total of six numerically different cross ratios for the same four points, though they change in different ways with deviations from rigidity, and Cutting concentrates on just one). To test whether observers use cross ratios in judgements of rigidity, Cutting describes a series of experiments using computer simulations of a 4-element planar surface undergoing 3-D motion. He introduces a non-rigidity (a small perturbation in the position of one element), and determines whether its detectability co-varies with changes in the canonical cross ratio between the elements or simply with the linear extent of the perturbation in position. The cross ratio predictions are more successful than the displacement predictions, but this is not convincing evidence for the use of cross ratios. If observers base their judgements on rigidity, one would expect performance to be correlated with changes in the ratio (and, indeed, with other measures of rigidity) rather than with simple linear displacement. Cutting himself seems aware of the problem and discusses another correlate of rigidity—local element density, which could also account for the data. He also enumerates some severe limitations in the utility of cross ratios: they require exactly four elements that must be collinear, and the choice between the six available ratios seems arbitrary. Nevertheless, Cutting concludes that the cross ratio invariant *is* used to judge "rigid flatness in rotating and toppling objects" (p.142). I was not convinced.

Turning to wayfinding, the author discusses at some length the possible sources of information, concentrating on the focus of expansion. He rejects all of them except differential motion parallax. Parallax specifies the direction of gaze with respect to the direction of locomotion, so if observers can use parallax, they must be able to judge direction of gaze during locomotion. Experiments are described in which subjects viewed a simulated approach to 12 vertical wires grouped into three depth planes and had to discriminate direction of gaze for each approach (left vs. right vs. ahead of the direction of locomotion). Cutting found that in such displays containing no information other than parallax, judgements were accurate down to angles of about 5° between gaze direction and movement direction, an acceptable level of performance. However, procedural details dilute the force of the findings. For each subject, different gaze angles were presented in separate blocks of trials, beginning with an angle of 20°. If performance exceeded chance, the angle was halved in the next block, and the procedure continued until

chance performance was reached. Observers may have been initially rather poor at the judgements, but the progressive decrease in angle over blocks of trials may have allowed them to learn the discrimination as they went along. This objection is reinforced by the fact that observers were given feedback after each trial. Why was the experiment run in this way? Cutting anticipates such objections, falling back on the argument that "learnability" should play an important role in perceptual theories, but they weaken his position.

The concluding section of the book is devoted to a debate on classes of perceptual theory, in particular the distinction between direct perception and indirect perception. Cutting advocates a third class of theory, "directed" perception. He characterizes direct perception as involving a 1-to-1 mapping of invariant information to object property, whereas indirect perception involves a 1-to-many mapping from information to object in which the different object properties consistent with the same information are sifted by means of inference and hypothesis. His preference is for directed perception, which involves a many-to-1 mapping, so that a given object property can be specified by many different sources of information, and the visual system must choose which information to use. This seems to me a modified version of direct perception, as there is no role for inference, and process is not mentioned at all, but invariants figure largely. The discussion is thought-provoking, but the notion of directed perception leaves many important questions unasked. For example, why must the system choose, can it not use more than one source of information at a time instead? On what basis is the choice made? Most importantly, the total concentration on rich information and its "pick-up" here and throughout the book ignores huge and important questions concerning how the visual system actually codes and analyses the information. "Elements" figure largely in the treatment of cross ratios; what is an element, and how is it coded? How might a cross ratio be made explicit? What visual processes detect motion parallax? Despite the supposed richness of the information in the optic array, upon which Cutting places so much emphasis, the relatively primitive abilities of current machine vision systems bear witness to the difficulties to be surmounted in using it effectively. These are surely major issues that must be addressed by any theory of perception.

Finally, a couple of niggling complaints. First, the author uses notes, numbered in the text and gathered together at the end of the book. I found this practice extremely annoying, because the numbers are prone to introduce nagging doubts—is there something useful and/or informative lurking in the back of the book? Often there is, but getting there is very disruptive and destroys concentration. Second, extensive use is made of lengthy verbatim quotations, often from rather old and obscure sources. These no doubt demonstrate the author's scholarship, but the switch of styles is often jarring and distracting.

The book is a curious mixture. It contains lucid treatments of such topics as invariants, optic flow, and motion parallax. The experimental evidence is rather unsatisfying and seems too thin to justify Cutting's devotion to his theoretical position. The book is consequently of interest mainly to specialists; if you find these topics attractive, then it may be rewarding; otherwise its perspective may be too narrow to sustain interest.

GEORGE MATHER

Kimble, G. A. and Schlesinger, K. (Eds.). *Topics in the history of psychology, Volume 1*. Hillsdale, N.J.: Lawrence Erlbaum Associates, Inc., 1985. Pp. 409. ISBN 0–89859–312–3. £36.00.

In this first volume of a two-handed taster on the history of psychology, the editors have invited papers on what is usually thought of as the hard-nosed end of the subject. Thus, Lorrin Riggs writes magisterially on vision from the seventeenth century to the present, that most fruitful era of interchange between physiology and psychology; and Dember and bagwell complement the coverage with one on the more general issue of percpetion. Although I believe that such a chapter is needed in such a book, there are so many unquestioned (but highly questionable) presuppositions in the text (for example, about the nature of science and the importance of behaviourism to perception) that I seriously query the value and insightfulness of the material. Regurgitated textbooks do not provide a sound basis for historical scholarship.

The same can unfortunately be said of Kimble's own chapter on conditioning and learning, where the same old pantomime horse (front legs Thorndike and/or Watson, back legs various, viz. Tolman, Hull, and Skinner) is turned out to enjoy its usual undemanding canter. There is also a curious problem on page 26 over dates and the laggardly way that American psychology took up Pavlov. Perhaps Kimble has forgotten that the United States did not enter the First World War until 1917, some eleven years after the lecture by Pavlov that he cites. Moreover, the reason why America shunned Pavlov may have been that America could not at the time see the point of it at all.

The chapter by Cooper on comparative psychology seems in part to share the same curious problem with dates. We are told on page 139, for example, that on the death of Aristotle (322 BC) "Europe retreated into a scientific slumber", only to re-emerge into the scientific light during the Renaissance. So much for Arabic science, the post-Aristotlean Greeks and medieval Christian thinkers such as St Augustine. Like most of the book's contributors, Cooper is really only at ease with the America of the 1950s onwards