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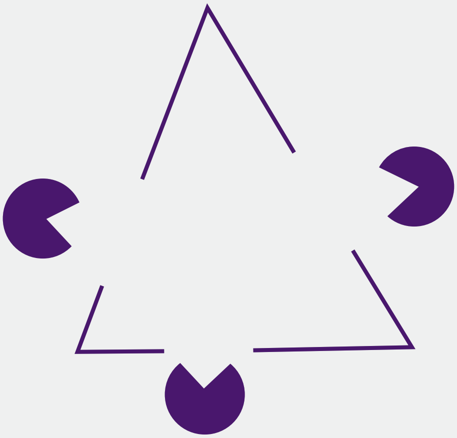

David Marr

FOREWORD BY
Shimon Ullman

AFTERWORD BY
Tomaso Poggio

VISION

VISION

A Computational Investigation into the Human Representation and Processing of Visual Information

David Marr

The MIT Press
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To my parents and to Lucia

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CHAPTER 1

The Philosophy and the Approach

1.1 BACKGROUND

The problems of visual perception have attracted the curiosity of scientists for many centuries. Important early contributions were made by Newton (1704), who laid the foundations for modern work on color vision, and Helmholtz (1910), whose treatise on physiological optics generates interest even today. Early in this century, Wertheimer (1912, 1923) noticed the apparent motion not of individual dots but of wholes, or "fields," in images presented sequentially as in a movie. In much the same way we perceive the migration across the sky of a flock of geese: the flock somehow constitutes a single entity, and is not seen as individual birds. This observation started the Gestalt school of psychology, which was concerned with describing the qualities of wholes by using terms like *solidarity* and *distinctness,* and with trying to formulate the "laws" that governed the creation of these wholes. The attempt failed for various reasons, and the Gestalt school dissolved into the fog of subjectivism. With the death of the school, many

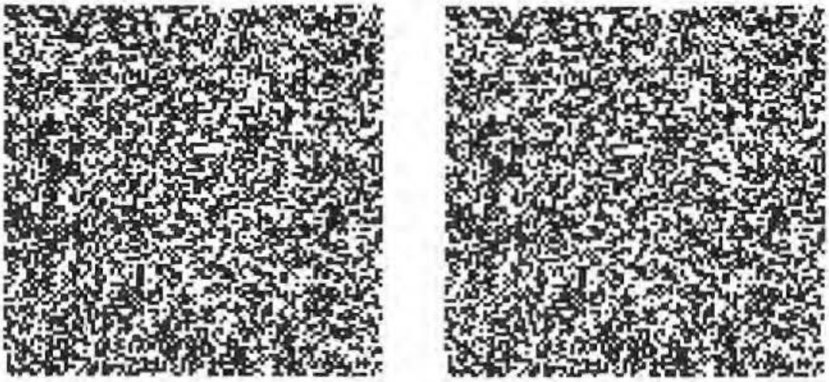

Figure 1–1. A random-dot stereogram of the type used extensively by Bela Julesz. The left and right images are identical except for a central square region that is displaced slightly in one image. When fused binocularly, the images yield the impression of the central square floating in front of the background.

of its early and genuine insights were unfortunately lost to the mainstream of experimental psychology.

Since then, students of the psychology of perception have made no serious attempts at an overall understanding of what perception is, concentrating instead on the analysis of properties and performance. The trichromatism of color vision was firmly established (see Brindley, 1970), and the preoccupation with motion continued, with the most interesting developments perhaps being the experiments of Miles (1931) and of Wallach and O'Connell (1953), which established that under suitable conditions an unfamiliar three-dimensional shape can be correctly perceived from only its changing monocular projection.*

The development of the digital electronic computer made possible a similar discovery for binocular vision. In 1960 Bela Julesz devised computer-generated random-dot stereograms, which are image pairs constructed of dot patterns that appear random when viewed monocularly but fuse when viewed one through each eye to give a percept of shapes and surfaces with a clear three-dimensional structure. An example is shown in Figure 1–1. Here the image for the left eye is a matrix of black and white squares generated at random by a computer program. The image for the right eye is made by copying the left image, shifting a square-shaped region at its center slightly to the left, and then providing a new random pattern to fill the gap that the shift creates. If each of the eyes sees only one matrix, as if the matrices were both in the same physical place, the result is the sensation of a square floating in space. Plainly, such percepts are caused solely by the stereo disparity between matching elements in the images presented to each eye; from such experiments, we know that the analysis of stereoscopic information, like the analysis of motion, can proceed independently in the absence of other information. Such findings are of critical importance because they help us to subdivide our study of perception into more specialized parts which can be treated separately. I shall refer to these as independent modules of perception.

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*The two dimensional image seen by a single eye.

The most recent contribution of psychophysics has been of a different kind but of equal importance. It arose from a combination of adaptation and threshold detection studies and originated from the demonstration by Campbell and Robson (1968) of the existence of independent, spatial-frequency-tuned channels—that is, channels sensitive to intensity variations in the image occurring at a particular scale or spatial interval—in the early stages of our perceptual apparatus. This paper led to an explosion of articles on various aspects of these channels, which culminated ten years later with quite satisfactory quantitative accounts of the characteristics of the first stages of visual perception (Wilson and Bergen, 1979). I shall discuss this in detail later on.

Recently a rather different approach has attracted considerable attention. In 1971, Roger N. Shepard and Jacqueline Metzler made line drawings of simple objects that differed from one another either by a three-dimensional rotation or by a rotation plus a reflection (see Figure 1–2). They asked how long it took to decide whether two depicted objects differed by a rotation and a reflection or merely a rotation. They found that the time taken depended on the three-dimensional angle of rotation necessary to bring the two objects into correspondence. Indeed, the time varied linearly with this angle. One is led thereby to the notion that a mental rotation of sorts is actually being performed—that a mental description of the first shape in a pair is being adjusted incrementally in orientation until it matches the second, such adjustment requiring greater time when greater angles are involved.

The significance of this approach lies not so much in its results, whose interpretation is controversial, as in the type of questions it raised. For until then, the notion of a representation was not one that visual psychologists took seriously. This type of experiment meant that the notion had to be considered. Although the early thoughts of visual psychologists were naive compared with those of the computer vision community, which had had

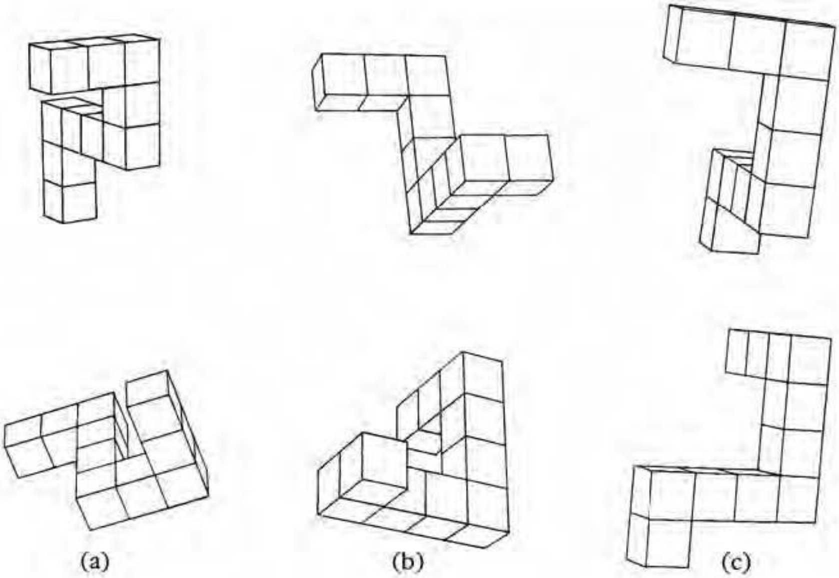

Figure 1–2. Some drawings similar to those used in Shepard and Metzler's experiments on mental rotation. The ones shown in (a) are identical, as a clockwise turning of this page by 80° will readily prove. Those in (b) are also identical, and again the relative angle between the two is 80°. Here, however, a rotation in depth will make the first coincide with the second. Finally, those in (c) are not at all identical, for no rotation will bring them into congruence. The time taken to decide whether a pair is the same was found to vary linearly with the angle through which one figure must be rotated to be brought into correspondence with the other. This suggested to the investigators that a stepwise mental rotation was in fact being performed by the subjects of their experiments.

to face the problem of representation from the beginning, it was not long before the thinking of psychologists became more sophisticated (see Shepard, 1979).

But what of explanation? For a long time, the best hope seemed to lie along another line of investigation, that of electrophysiology. The development of amplifiers allowed Adrian (1928) and his colleagues to record the minute voltage changes that accompanied the transmission of nerve signals. Their investigations showed that the character of the sensation so produced depended on which fiber carried the message, not how the fiber was stimulated—as one might have expected from anatomical studies. This led to the view that the peripheral nerve fibers could be thought of as a simple mapping supplying the sensorium with a copy of the physical events at the body surface (Adrian, 1947). The rest of the explanation, it was thought, could safely be left to the psychologists.

The next development was the technical improvement in amplification that made possible the recording of single neurons (Granit and Svaetichin, 1939; Hartline, 1938; Galambos and Davis, 1943). This led to the notion of a cell's "receptive field" (Hartline, 1940) and to the Harvard School's famous series of studies of the behavior of neurons at successively deeper levels of the visual pathway (Kuffler, 1953; Hubel and Wiesel, 1962, 1968). But perhaps the most exciting development was the new view that questions of psychological interest could be illuminated and perhaps even explained by neurophysiological experiments. The clearest early example of this was Barlow's (1953) study of ganglion cells in the frog retina, and I cannot put it better than he did:

> If one explores the responsiveness of single ganglion cells in the frog's retina using handheld targets, one finds that one particular type of ganglion cell is most effectively driven by something like a black disc subtending a degree or so moved rapidly to and fro within the unit's receptive field. This causes a vigorous discharge which can be maintained without much decrement as long as the movement is continued. Now, if the stimulus which is optimal for this class of cells is presented to intact frogs, the behavioural response is often dramatic; they turn towards the target and make repeated feeding responses consisting of a jump and snap. The selectivity of the retinal neurons and the frog's reaction when they are selectively stimulated, suggest that they are "bug detectors" (Barlow 1953) performing a primitive but vitally important form of recognition.
>
> The result makes one suddenly realize that a large part of the sensory machinery involved in a frog's feeding responses may actually reside in the retina rather than in mysterious "centres" that would be too difficult to understand by physiological methods. The essential lock-like property resides in each member of a whole class of neurons and allows the cell to discharge only to the appropriate key pattern of sensory stimulation. Lettvin *et al.* (1959) suggested that there were five different classes of cell in the frog, and Barlow, Hill and Levick (1964) found an even larger number of categories in the rabbit. [Barlow *et al.*] called these key patterns "trigger features," and Maturana *et al.* (1960) emphasized another important aspect of the behaviour of these ganglion cells; a cell continues to respond to the same trigger feature in spite of changes in light intensity over many decades. The properties of the retina are such that a ganglion cell can, figuratively speaking, reach out and determine that something specific is happening in front of the eye. Light is the agent by

> which it does this, but it is the detailed pattern of the light that carries the information, and the overall level of illumination prevailing at the time is almost totally disregarded. (p. 373)

Barlow (1972) then goes on to summarize these findings in the following way:

> The cumulative effect of all the changes I have tried to outline above has been to make us realise that each *single neuron can perform a much more complex and subtle task than had previously been thought* (emphasis added). Neurons do not loosely and unreliably remap the luminous intensities of the visual image onto our sensorium, but instead they detect pattern elements, discriminate the depth of objects, ignore irrelevant causes of variation and are arranged in an intriguing hierarchy. Furthermore, there is evidence that they give prominence to what is informationally important, can respond with great reliability, and can have their pattern selectivity permanently modified by early visual experience. This amounts to a revolution in our outlook. It is now quite inappropriate to regard unit activity as a noisy indication of more basic and reliable processes involved in mental operations: instead, we must regard single neurons as the prime movers of these mechanisms. Thinking is brought about by neurons and we should not use phrases like "unit activity reflects, reveals, or monitors thought processes," because the activities of neurons, quite simply, are thought processes.
>
> This revolution stemmed from physiological work and makes us realize that the activity of each single neuron may play a significant role in perception. (p. 380)

This aspect of his thinking led Barlow to formulate the first and most important of his five dogmas: 'A description of that activity of a single nerve cell which is transmitted to and influences other nerve cells and of a nerve cell's response to such influences from other cells, is a complete enough description for functional understanding of the nervous system. There is nothing else "looking at" or controlling this activity, which must therefore provide a basis for understanding how the brain controls behaviour' (Barlow, 1972, p. 380).

I shall return later on to more carefully examine the validity of this point of view, but for now let us just enjoy it. The vigor and excitement of these ideas need no emphasis. At the time the eventual success of a reductionist approach seemed likely. Hubel and Wiesel's (1962, 1968) pioneering studies had shown the way; single-unit studies on stereopsis (Barlow, Blakemore, and Pettigrew, 1967) and on color (DeValois, Abramov, and Mead, 1967; Gouras, 1968) seemed to confirm the close links between perception and single-cell recordings, and the intriguing results of Gross, Rocha-Miranda, and Bender (1972), who found "hand-detectors" in the inferotemporal cortex, seemed to show that the application of the reductionist approach would not be limited just to the early parts of the visual pathway.

It was, of course, recognized that physiologists had been lucky: If one probes around in a conventional electronic computer and records the behavior of single elements within it, one is unlikely to be able to discern what a given element is doing. But the brain, thanks to Barlow's first dogma, seemed to be built along more accommodating lines—people *were* able to determine the functions of single elements of the brain. There seemed no reason why the reductionist approach could not be taken all the way.

I was myself fully caught up in this excitement. Truth, I also believed, was basically neural, and the central aim of all research was a thorough functional analysis of the structure of the central nervous system. My enthusiasm found expression in a theory of the cerebellar cortex (Marr, 1969). According to this theory, the simple and regular cortical structure is interpreted as a simple but powerful memorizing device for learning motor skills; because of a simple combinatorial trick, each of the 15 million Purkinje cells in the cerebellum is capable of learning over 200 different patterns and discriminating them from unlearned patterns. Evidence is gradually accumulating that the cerebellum is involved in learning motor skills (Ito, 1978), so that something like this theory may in fact be correct.

The way seemed clear. On the one hand we had new experimental techniques of proven power, and on the other, the beginnings of a theoretical approach that could back them up with a fine analysis of cortical structure. Psychophysics could tell us what needed explaining, and the recent advances in anatomy—the Fink-Heimer technique from Nauta's laboratory and the recent successful deployment by Szentagothai and others of the electron microscope—could provide the necessary information about the structure of the cerebral cortex.

But somewhere underneath, something was going wrong. The initial discoveries of the 1950s and 1960s were not being followed by equally dramatic discoveries in the 1970s. No neurophysiologists had recorded new and clear high-level correlates of perception. The leaders of the 1960s had turned away from what they had been doing—Hubel and Wiesel concentrated on anatomy, Barlow turned to psychophysics, and the mainstream of neurophysiology concentrated on development and plasticity (the concept that neural connections are not fixed) or on a more thorough analysis of the cells that had already been discovered (for example, Bishop, Coombs, and Henry, 1971; Schiller, Finlay, and Volman, 1976a, 1976b), or on cells in species like the owl (for example, Pettigrew and Konishi, 1976).

None of the new studies succeeded in elucidating the *function* of the visual cortex.

It is difficult to say precisely why this happened, because the reasoning was never made explicit and was probably largely unconscious. However, various factors are identifiable. In my own case, the cerebellar study had two effects. On the one hand, it suggested that one could eventually hope to understand cortical structure in functional terms, and this was exciting. But at the same time the study has disappointed me, because even if the theory was correct, it did not much enlighten one about the motor system—it did not, for example, tell one how to go about programming a mechanical arm. It suggested that if one wishes to program a mechanical arm so that it operates in a versatile way, then at some point a very large and rather simple type of memory will prove indispensable. But it did not say why, nor what that memory should contain.

The discoveries of the visual neurophysiologists left one in a similar situation. Suppose, for example, that one actually found the apocryphal grandmother cell.* Would that really tell us anything much at all? It would tell us that it existed—Gross's hand-detectors tell us almost that—but not *why* or even *how* such a thing may be constructed from the outputs of previously discovered cells. Do the single-unit recordings—the simple and complex cells—tell us much about how to detect edges or why one would want to, except in a rather general way through arguments based on economy and redundancy? If we really knew the answers, for example, we should be able to program them on a computer. But finding a hand-detector certainly did not allow us to program one.

As one reflected on these sorts of issues in the early 1970s, it gradually became clear that something important was missing that was not present in either of the disciplines of neurophysiology or psychophysics. The key observation is that neurophysiology and psychophysics have as their business to *describe* the behavior of cells or of subjects but not to *explain* such behavior. What are the visual areas of the cerebral cortex actually doing? What are the problems in doing it that need explaining, and at what level of description should such explanations be sought?

The best way of finding out the difficulties of doing something is to try to do it, so at this point I moved to the Artificial Intelligence Laboratory at MIT, where Marvin Minsky had collected a group of people and a powerful computer for the express purpose of addressing these questions.

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*A cell that fires only when one's grandmother comes into view.

The first great revelation was that the problems are difficult. Of course, these days this fact is a commonplace. But in the 1960s almost no one realized that machine vision was difficult. The field had to go through the same experience as the machine translation field did in its fiascoes of the 1950s before it was at last realized that here were some problems that had to be taken seriously. The reason for this misperception is that we humans are ourselves so good at vision. The notion of a feature detector was well established by Barlow and by Hubel and Wiesel, and the idea that extracting edges and lines from images might be at all difficult simply did not occur to those who had not tried to do it. It turned out to be an elusive problem: Edges that are of critical importance from a three-dimensional point of view often cannot be found at all by looking at the intensity changes in an image. Any kind of textured image gives a multitude of noisy edge segments; variations in reflectance and illumination cause no end of trouble; and even if an edge has a clear existence at one point, it is as likely as not to fade out quite soon, appearing only in patches along its length in the image. The common and almost despairing feeling of the early investigators like B.K.P. Horn and T.O. Binford was that practically anything could happen in an image and furthermore that practically everything did.

Three types of approach were taken to try to come to grips with these phenomena. The first was unashamedly empirical, associated most with Azriel Rosenfeld. His style was to take some new trick for edge detection, texture discrimination, or something similar, run it on images, and observe the result. Although several interesting ideas emerged in this way, including the simultaneous use of operators* of different sizes as an approach to increasing sensitivity and reducing noise (Rosenfeld and Thurston, 1971), these studies were not as useful as they could have been because they were never accompanied by any serious assessment of how well the different algorithms performed. Few attempts were made to compare the merits of different operators (although Fram and Deutsch, 1975, did try), and an approach like trying to prove mathematically which operator was optimal was not even attempted. Indeed, it could not be, because no one had yet formulated precisely what these operators should be trying to do. Nevertheless, considerable ingenuity was shown. The most clever was probably Hueckel's (1973) operator, which solved in an ingenious way the problem of finding the edge orientation that best fit a given intensity change in a small neighborhood of an image.

**Operator* refers to a local calculation to be applied at each location in the image, making use of the intensity there and in the immediate vicinity.

The second approach was to try for depth of analysis by restricting the scope to a world of single, illuminated, matte white toy blocks set against a black background. The blocks could occur in any shapes provided only that all faces were planar and all edges were straight. This restriction allowed more specialized techniques to be used, but it still did not make the problem easy. The Binford–Horn line finder (Horn, 1973) was used to find edges, and both it and its sequel (described in Shirai, 1973) made use of the special circumstances of the environment, such as the fact that all edges there were straight.

These techniques did work reasonably well, however, and they allowed a preliminary analysis of later problems to emerge—roughly, what does one do once a complete line drawing has been extracted from a scene? Studies of this had begun sometime before with Roberts (1965) and Guzman (1968), and they culminated in the works of Waltz (1975) and Mackworth (1973), which essentially solved the interpretation problem for line drawings derived from images of prismatic solids. Waltz's work had a particularly dramatic impact, because it was the first to show explicitly that an exhaustive analysis of all possible local physical arrangements of surfaces, edges, and shadows could lead to an effective and efficient algorithm for interpreting an actual image. Figure 1–3 and its legend convey the main ideas behind Waltz's theory.

The hope that lay behind this work was, of course, that once the toy world of white blocks had been understood, the solutions found there could be generalized, providing the basis for attacking the more complex problems posed by a richer visual environment. Unfortunately, this turned out not to be so. For the roots of the approach that was eventually successful, we have to look at the third kind of development that was taking place then.

Two pieces of work were important here. Neither is probably of very great significance to human perception for what it actually accomplished—in the end, it is likely that neither will particularly reflect human visual processes—but they are both of importance because of the way in which they were formulated. The first was Land and McCann's (1971) work on the retinex theory of color vision, as developed by them and subsequently by Horn (1974). The starting point is the traditional one of regarding color as a perceptual approximation to reflectance. This allows the formulation of a clear computational question, namely, How can the effects of reflectance changes be separated from the vagaries of the prevailing illumination? Land and McCann suggested using the fact that changes in illumination are usually gradual, whereas changes in reflectance of a surface or of an object boundary are often quite sharp. Hence by filtering out slow changes, those changes due to the reflectance alone could be isolated. Horn devised a

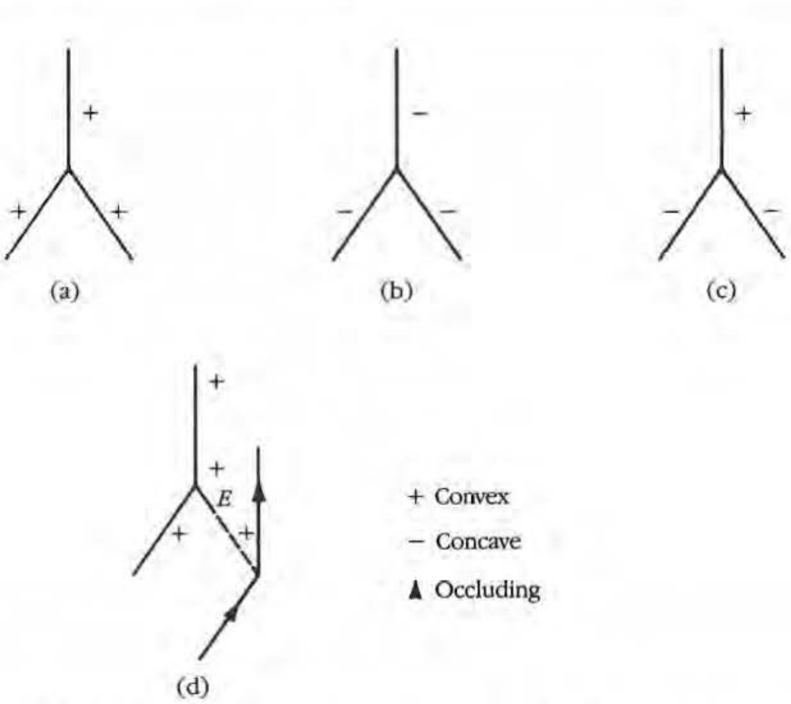

Figure 1–3. Some configurations of edges are physically realizable, and some are not. The trihedral junctions of three convex edges (a) or of three concave edges (b) are realizable, whereas the configuration (c) is impossible. Waltz cataloged all the possible junctions, including shadow edges, for up to four coincident edges. He then found that by using this catalog to implement consistency relations [requiring, for example, that an edge be of the same type all along its length like edge *E* in (d)], the solution to the labeling of a line drawing that included shadows was often uniquely determined.

clever parallel algorithm for this, and I suggested how it might be implemented by neurons in the retina (Marr, 1974a).

I do not now believe that this is at all a correct analysis of color vision or of the retina, but it showed the possible style of a correct analysis. Gone are the ad hoc programs of computer vision; gone is the restriction to a special visual miniworld; gone is any explanation *in terms of* neurons—except as a way of implementing a method. And present is a clear understanding of what is to be computed, how it is to be done, the physical assumptions on which the method is based, and some kind of analysis of algorithms that are capable of carrying it out.

The other piece of work was Horn's (1975) analysis of shape from shading, which was the first in what was to become a distinguished series of articles on the formation of images. By carefully analyzing the way in which the illumination, surface geometry, surface reflectance, and viewpoint conspired to create the measured intensity values in an image, Horn formulated a differential equation that related the image intensity values to the surface geometry. If the surface reflectance and illumination are known, one can solve for the surface geometry (see also Horn, 1977). Thus from shading one can derive shape.

The message was plain. There must exist an additional level of understanding at which the character of the information-processing tasks carried out during perception are analyzed and understood in a way that is independent of the particular mechanisms and structures that implement them in our heads. This was what was missing—the analysis of the problem as an information-processing task. Such analysis does not usurp an understanding at the other levels—of neurons or of computer programs—but it is a necessary complement to them, since without it there can be no real understanding of the function of all those neurons.

This realization was arrived at independently and formulated together by Tomaso Poggio in Tübingen and myself (Marr and Poggio, 1977; Marr, 1977b). It was not even quite new—Leon D. Harmon was saying something similar at about the same time, and others had paid lip service to a similar distinction. But the important point is that if the notion of different types of understanding is taken very seriously, it allows the study of the information-processing basis of perception to be made *rigorous*. It becomes possible, by separating explanations into different levels, to make explicit statements about what is being computed and why and to construct theories stating that what is being computed is optimal in some sense or is guaranteed to function correctly. The ad hoc element is removed, and heuristic computer programs are replaced by solid foundations on which a real subject can be built. This realization—the formulation of what was missing, together with a clear idea of how to supply it—formed the basic foundation for a new integrated approach, which it is the purpose of this book to describe.

1.2 UNDERSTANDING COMPLEX INFORMATION-PROCESSING SYSTEMS

Almost never can a complex system of any kind be understood as a simple extrapolation from the properties of its elementary components. Consider, for example, some gas in a bottle. A description of thermodynamic effects—

temperature, pressure, density, and the relationships among these factors—is not formulated by using a large set of equations, one for each of the particles involved. Such effects are described at their own level, that of an enormous collection of particles; the effort is to show that in principle the microscopic and macroscopic descriptions are consistent with one another. If one hopes to achieve a full understanding of a system as complicated as a nervous system, a developing embryo, a set of metabolic pathways, a bottle of gas, or even a large computer program, then one must be prepared to contemplate different kinds of explanation at different levels of description that are linked, at least in principle, into a cohesive whole, even if linking the levels in complete detail is impractical. For the specific case of a system that solves an information-processing problem, there are in addition the twin strands of process and representation, and both these ideas need some discussion.

Representation and Description

A *representation* is a formal system for making explicit certain entities or types of information, together with a specification of how the system does this. And I shall call the result of using a representation to describe a given entity a *description* of the entity in that representation (Marr and Nishihara, 1978).

For example, the Arabic, Roman, and binary numeral systems are all formal systems for representing numbers. The Arabic representation consists of a string of symbols drawn from the set (0, 1, 2, 3, 4, 5, 6, 7, 8, 9), and the rule for constructing the description of a particular integer n is that one decomposes n into a sum of multiples of powers of 10 and unites these multiples into a string with the largest powers on the left and the smallest on the right. Thus, thirty-seven equals $3 \times 10^1 + 7 \times 10^0$, which becomes 37, the Arabic numeral system's description of the number. What this description makes explicit is the number's decomposition into powers of 10. The binary numeral system's description of the number thirty-seven is 100101, and this description makes explicit the number's decomposition into powers of 2. In the Roman numeral system, thirty-seven is represented as XXXVII.

This definition of a representation is quite general. For example, a representation for shape would be a formal scheme for describing some aspects of shape, together with rules that specify how the scheme is applied to any particular shape. A musical score provides a way of representing a symphony; the alphabet allows the construction of a written representation of words; and so forth. The phrase "formal scheme" is critical to the definition, but the reader should not be frightened by it. The reason is simply that we are dealing with information-processing machines, and the way such machines work is by using symbols to stand for things—to represent things, in our terminology. To say that something is a formal scheme means only that it is a set of symbols with rules for putting them together—no more and no less.

A representation, therefore, is not a foreign idea at all—we all use representations all the time. However, the notion that one can capture some aspect of reality by making a description of it using a symbol and that to do so can be useful seems to me a fascinating and powerful idea. But even the simple examples we have discussed introduce some rather general and important issues that arise whenever one chooses to use one particular representation. For example, if one chooses the Arabic numeral representation, it is easy to discover whether a number is a power of 10 but difficult to discover whether it is a power of 2. If one chooses the binary representation, the situation is reversed. Thus, there is a trade-off; any particular representation makes certain information explicit at the expense of information that is pushed into the background and may be quite hard to recover.

This issue is important, because how information is represented can greatly affect how easy it is to do different things with it. This is evident even from our numbers example: It is easy to add, to subtract, and even to multiply if the Arabic or binary representations are used, but it is not at all easy to do these things—especially multiplication—with Roman numerals. This is a key reason why the Roman culture failed to develop mathematics in the way the earlier Arabic cultures had.

An analogous problem faces computer engineers today. Electronic technology is much more suited to a binary number system than to the conventional base 10 system, yet humans supply their data and require the results in base 10. The design decision facing the engineer, therefore, is, Should one pay the cost of conversion into base 2, carry out the arithmetic in a binary representation, and then convert back into decimal numbers on output; or should one sacrifice efficiency of circuitry to carry out operations directly in a decimal representation? On the whole, business computers and pocket calculators take the second approach, and general purpose computers take the first. But even though one is not restricted to using just one representation system for a given type of information, the choice of which to use is important and cannot be taken lightly. It determines what information is made explicit and hence what is pushed further into the background, and it has a far-reaching effect on the ease and difficulty with which operations may subsequently be carried out on that information.

Process

The term *process* is very broad. For example, addition is a process, and so is taking a Fourier transform. But so is making a cup of tea, or going shopping. For the purposes of this book, I want to restrict our attention to the meanings associated with machines that are carrying out information-processing tasks. So let us examine in depth the notions behind one simple such device, a cash register at the checkout counter of a supermarket.

There are several levels at which one needs to understand such a device, and it is perhaps most useful to think in terms of three of them. The most abstract is the level of *what* the device does and *why*. What it does is arithmetic, so our first task is to master the theory of addition. Addition is a mapping, usually denoted by $+$, from pairs of numbers into single numbers; for example, $+$ maps the pair $(3, 4)$ to 7, and I shall write this in the form $(3 + 4) \rightarrow 7$. Addition has a number of abstract properties, however. It is commutative: both $(3 + 4)$ and $(4 + 3)$ are equal to 7; and associative: the sum of $3 + (4 + 5)$ is the same as the sum of $(3 + 4) + 5$. Then there is the unique distinguished element, zero, the adding of which has no effect: $(4 + 0) \rightarrow 4$. Also, for every number there is a unique "inverse," written (-4) in the case of 4, which when added to the number gives zero: $[4 + (-4)] \rightarrow 0$.

Notice that these properties are part of the fundamental *theory* of addition. They are true no matter how the numbers are written—whether in binary, Arabic, or Roman representation—and no matter how the addition is executed. Thus part of this first level is something that might be characterized as *what* is being computed.

The other half of this level of explanation has to do with the question of *why* the cash register performs addition and not, for instance, multiplication when combining the prices of the purchased items to arrive at a final bill. The reason is that the rules we intuitively feel to be appropriate for combining the individual prices in fact define the mathematical operation of addition. These can be formulated as *constraints* in the following way:

1. If you buy nothing, it should cost you nothing; and buying nothing and something should cost the same as buying just the something. (The rules for zero.)

2. The order in which goods are presented to the cashier should not affect the total. (Commutativity.)

3. Arranging the goods into two piles and paying for each pile separately should not affect the total amount you pay. (Associativity; the basic operation for combining prices.)

4. If you buy an item and then return it for a refund, your total expenditure should be zero. (Inverses.)

It is a mathematical theorem that these conditions define the operation of addition, which is therefore the appropriate computation to use.

This whole argument is what I call the *computational theory* of the cash register. Its important features are (1) that it contains separate arguments about what is computed and why and (2) that the resulting operation is defined uniquely by the constraints it has to satisfy. In the theory of visual processes, the underlying task is to reliably derive properties of the world from images of it; the business of isolating constraints that are both powerful enough to allow a process to be defined and generally true of the world is a central theme of our inquiry.

In order that a process shall actually run, however, one has to realize it in some way and therefore choose a representation for the entities that the process manipulates. The second level of the analysis of a process, therefore, involves choosing two things: (1) a *representation* for the input and for the output of the process and (2) an *algorithm* by which the transformation may actually be accomplished. For addition, of course, the input and output representations can both be the same, because they both consist of numbers. However this is not true in general. In the case of a Fourier transform, for example, the input representation may be the time domain, and the output, the frequency domain. If the first of our levels specifies what and why, this second level specifies *how*. For addition, we might choose Arabic numerals for the representations, and for the algorithm we could follow the usual rules about adding the least significant digits first and "carrying" if the sum exceeds 9. Cash registers, whether mechanical or electronic, usually use this type of representation and algorithm.

There are three important points here. First, there is usually a wide choice of representation. Second, the choice of algorithm often depends rather critically on the particular representation that is employed. And third, even for a given fixed representation, there are often several possible algorithms for carrying out the same process. Which one is chosen will usually depend on any particularly desirable or undesirable characteristics that the algorithms may have; for example, one algorithm may be much more efficient than another, or another may be slightly less efficient but more robust (that is, less sensitive to slight inaccuracies in the data on which it must run). Or again, one algorithm may be parallel, and another, serial. The choice, then, may depend on the type of hardware or machinery in which the algorithm is to be embodied physically.

This brings us to the third level, that of the device in which the process is to be realized physically. The important point here is that, once again, the same algorithm may be implemented in quite different technologies. The child who methodically adds two numbers from right to left, carrying a digit when necessary, may be using the same algorithm that is implemented by the wires and transistors of the cash register in the neighborhood supermarket, but the physical realization of the algorithm is quite different in these two cases. Another example: Many people have written computer programs to play tic-tac-toe, and there is a more or less standard algorithm that cannot lose. This algorithm has in fact been implemented by W. D. Hillis and B. Silverman in a quite different technology, in a computer made out of Tinkertoys, a children's wooden building set. The whole monstrously ungainly engine, which actually works, currently resides in a museum at the University of Missouri in St. Louis.

Some styles of algorithm will suit some physical substrates better than others. For example, in conventional digital computers, the number of connections is comparable to the number of gates, while in a brain, the number of connections is much larger ($\times 10^4$) than the number of nerve cells. The underlying reason is that wires are rather cheap in biological architecture, because they can grow individually and in three dimensions. In conventional technology, wire laying is more or less restricted to two dimensions, which quite severely restricts the scope for using parallel techniques and algorithms; the same operations are often better carried out serially.

The Three Levels

We can summarize our discussion in something like the manner shown in Figure 1–4, which illustrates the different levels at which an information-processing device must be understood before one can be said to have understood it completely. At one extreme, the top level, is the abstract computational theory of the device, in which the performance of the device is characterized as a mapping from one kind of information to another, the abstract properties of this mapping are defined precisely, and its appropriateness and adequacy for the task at hand are demonstrated. In the center is the choice of representation for the input and output and the

Computational theory	Representation and algorithm	Hardware implementation
What is the goal of the computation, why is it appropriate, and what is the logic of the strategy by which it can be carried out?	How can this computational theory be implemented? In particular, what is the representation for the input and output, and what is the algorithm for the transformation?	How can the representation and algorithm be realized physically?

Figure 1–4. The three levels at which any machine carrying out an information-processing task must be understood.

algorithm to be used to transform one into the other. And at the other extreme are the details of how the algorithm and representation are realized physically—the detailed computer architecture, so to speak. These three levels are coupled, but only loosely. The choice of an algorithm is influenced for example, by what it has to do and by the hardware in which it must run. But there is a wide choice available at each level, and the explication of each level involves issues that are rather independent of the other two.

Each of the three levels of description will have its place in the eventual understanding of perceptual information processing, and of course they are logically and causally related. But an important point to note is that since the three levels are only rather loosely related, some phenomena may be explained at only one or two of them. This means, for example, that a correct explanation of some psychophysical observation must be formulated at the appropriate level. In attempts to relate psychophysical problems to physiology, too often there is confusion about the level at which problems should be addressed. For instance, some are related mainly to the physical mechanisms of vision—such as afterimages (for example, the one you see after staring at a light bulb) or such as the fact that any color can be matched by a suitable mixture of the three primaries (a consequence principally of the fact that we humans have three types of cones). On the other hand, the ambiguity of the Necker cube (Figure 1–5) seems to demand a different kind of explanation. To be sure, part of the explanation of its perceptual reversal must have to do with a bistable neural network (that is, one with two distinct stable states) somewhere inside the

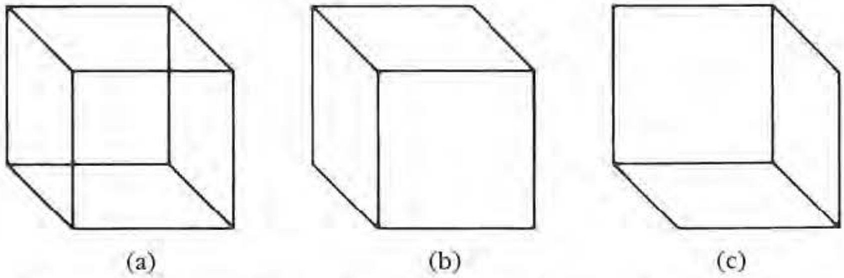

Figure 1–5. The so-called Necker illusion, named after L. A. Necker, the Swiss naturalist who developed it in 1832. The essence of the matter is that the two-dimensional representation (a) has collapsed the depth out of a cube and that a certain aspect of human vision is to recover this missing third dimension. The depth of the cube can indeed be perceived, but two interpretations are possible, (b) and (c). A person's perception characteristically flips from one to the other.

brain, but few would feel satisfied by an account that failed to mention the existence of two different but perfectly plausible three-dimensional interpretations of this two-dimensional image.

For some phenomena, the type of explanation required is fairly obvious. Neuroanatomy, for example, is clearly tied principally to the third level, the physical realization of the computation. The same holds for synaptic mechanisms, action potentials, inhibitory interactions, and so forth. Neurophysiology, too, is related mostly to this level, but it can also help us to understand the type of representations being used, particularly if one accepts something along the lines of Barlow's views that I quoted earlier. But one has to exercise extreme caution in making inferences from neurophysiological findings about the algorithms and representations being used, particularly until one has a clear idea about what information needs to be represented and what processes need to be implemented.

Psychophysics, on the other hand, is related more directly to the level of algorithm and representation. Different algorithms tend to fail in radically different ways as they are pushed to the limits of their performance or are deprived of critical information. As we shall see, primarily psychophysical evidence proved to Poggio and myself that our first stereo-matching algorithm (Marr and Poggio, 1976) was not the one that is used by the brain, and the best evidence that our second algorithm (Marr and Poggio, 1979) *is* roughly the one that is used also comes from psychophysics. Of course, the underlying computational theory remained the same in both cases, only the algorithms were different.

Psychophysics can also help to determine the nature of a representation. The work of Roger Shepard (1975), Eleanor Rosch (1978), or Elizabeth Warrington (1975) provides some interesting hints in this direction. More specifically, Stevens (1979) argued from psychophysical experiments that surface orientation is represented by the coordinates of slant and tilt, rather than (for example) the more traditional (p, q) of gradient space (see Chapter 3). He also deduced from the uniformity of the size of errors made by subjects judging surface orientation over a wide range of orientations that the representational quantities used for slant and tilt are pure angles and not, for example, their cosines, sines, or tangents.

More generally, if the idea that different phenomena need to be explained at different levels is kept clearly in mind, it often helps in the assessment of the validity of the different kinds of objections that are raised from time to time. For example, one favorite is that the brain is quite different from a computer because one is parallel and the other serial. The answer to this, of course, is that the distinction between serial and parallel is a distinction at the level of algorithm; it is not fundamental at all—anything programmed in parallel can be rewritten serially (though not necessarily vice versa). The distinction, therefore, provides no grounds for arguing that the brain operates so differently from a computer that a computer could not be programmed to perform the same tasks.

Importance of Computational Theory

Although algorithms and mechanisms are empirically more accessible, it is the top level, the level of computational theory, which is critically important from an information-processing point of view. The reason for this is that the nature of the computations that underlie perception depends more upon the computational problems that have to be solved than upon the particular hardware in which their solutions are implemented. To phrase the matter another way, an algorithm is likely to be understood more readily by understanding the nature of the problem being solved than by examining the mechanism (and the hardware) in which it is embodied.

In a similar vein, trying to understand perception by studying only neurons is like trying to understand bird flight by studying only feathers: It just cannot be done. In order to understand bird flight, we have to understand aerodynamics; only then do the structure of feathers and the different shapes of birds' wings make sense. More to the point, as we shall see, we cannot understand why retinal ganglion cells and lateral geniculate neurons have the receptive fields they do just by studying their anatomy and physiology. We can understand how these cells and neurons behave as they do by studying their wiring and interactions, but in order to understand *why* the receptive fields are as they are—why they are circularly symmetrical and why their excitatory and inhibitory regions have characteristic shapes and distributions—we have to know a little of the theory of differential operators, band-pass channels, and the mathematics of the uncertainty principle (see Chapter 2).

Perhaps it is not surprising that the very specialized empirical disciplines of the neurosciences failed to appreciate fully the absence of computational theory; but it is surprising that this level of approach did not play a more forceful role in the early development of artificial intelligence. For far too long, a heuristic program for carrying out some task was held to be a theory of that task, and the distinction between what a program did and how it did it was not taken seriously. As a result, (1) a style of explanation evolved that invoked the use of special mechanisms to solve particular problems, (2) particular data structures, such as the lists of attribute value pairs called property lists in the LISP programing language, were held to amount to theories of the representation of knowledge, and (3) there was frequently no way to determine whether a program would deal with a particular case other than by running the program.

Failure to recognize this theoretical distinction between *what* and *how* also greatly hampered communication between the fields of artificial intelligence and linguistics. Chomsky's (1965) theory of transformational grammar is a true computational theory in the sense defined earlier. It is concerned solely with specifying what the syntactic decomposition of an English sentence should be, and not at all with how that decomposition should be achieved. Chomsky himself was very clear about this—it is roughly his distinction between competence and performance, though his idea of performance did include other factors, like stopping in midutterance—but the fact that his theory was defined by transformations, which look like computations, seems to have confused many people. Winograd (1972), for example, felt able to criticize Chomsky's theory on the grounds that it cannot be inverted and so cannot be made to run on a computer; I had heard reflections of the same argument made by Chomsky's colleagues in linguistics as they turn their attention to how grammatical structure might actually be computed from a real English sentence.

The explanation is simply that finding algorithms by which Chomsky's theory may be implemented is a completely different endeavor from formulating the theory itself. In our terms, it is a study at a different level, and both tasks have to be done. This point was appreciated by Marcus (1980), who was concerned precisely with how Chomsky's theory can be realized and with the kinds of constraints on the power of the human grammatical processor that might give rise to the structural constraints in syntax that

Chomsky found. It even appears that the emerging "trace" theory of grammar (Chomsky and Lasnik, 1977) may provide a way of synthesizing the two approaches—showing that, for example, some of the rather ad hoc restrictions that form part of the computational theory may be consequences of weaknesses in the computational power that is available for implementing syntactical decoding.

The Approach of J. J. Gibson

In perception, perhaps the nearest anyone came to the level of computational theory was Gibson (1966). However, although some aspects of his thinking were on the right lines, he did not understand properly what information processing was, which led him to seriously underestimate the complexity of the information-processing problems involved in vision and the consequent subtlety that is necessary in approaching them.

Gibson's important contribution was to take the debate away from the philosophical considerations of sense-data and the affective qualities of sensation and to note instead that the important thing about the senses is that they are channels for perception of the real world outside or, in the case of vision, of the visible surfaces. He therefore asked the critically important question, How does one obtain constant perceptions in everyday life on the basis of continually changing sensations? This is exactly the right question, showing that Gibson correctly regarded the problem of perception as that of recovering from sensory information "valid" properties of the external world. His problem was that he had a much oversimplified view of how this should be done. His approach led him to consider higher-order variables—stimulus energy, ratios, proportions, and so on—as "invariants" of the movement of an observer and of changes in stimulation intensity.

"These invariants," he wrote, "correspond to permanent properties of the environment. They constitute, therefore, information about the permanent environment." This led him to a view in which the function of the brain was to "detect invariants" despite changes in "sensations" of light, pressure, or loudness of sound. Thus, he says that the "function of the brain, when looped with its perceptual organs, is not to decode signals, nor to interpret messages, nor to accept images, nor to *organize* the sensory input or to *process* the data, in modern terminology. It is to seek and extract information about the environment from the flowing array of ambient energy," and he thought of the nervous system as in some way "resonating" to these invariants. He then embarked on a broad study of animals in their environments, looking for invariants to which they might resonate. This was the basic idea behind the notion of ecological optics (Gibson, 1966, 1979).

Although one can criticize certain shortcomings in the quality of Gibson's analysis, its major and, in my view, fatal shortcoming lies at a deeper level and results from a failure to realize two things. First, the detection of physical invariants, like image surfaces, is exactly and precisely an information-processing problem, in modern terminology. And second, he vastly underrated the sheer difficulty of such detection. In discussing the recovery of three-dimensional information from the movement of an observer, he says that "in motion, perspective information alone can be used" (Gibson, 1966, p. 202). And perhaps the key to Gibson is the following:

> The detection of non-change when an object moves in the world is not as difficult as it might appear. It is only made to seem difficult when we assume that the perception of constant dimensions of the object must depend on the correcting of sensations of inconstant form and size. The information for the constant dimension of an object is normally carried by invariant relations in an optic array. Rigidity is *specified*. (emphasis added)

Yes, to be sure, but *how?* Detecting physical invariants is just as difficult as Gibson feared, but nevertheless we can do it. And the only way to understand how is to treat it as an information-processing problem.

The underlying point is that visual information processing is actually very complicated, and Gibson was not the only thinker who was misled by the apparent simplicity of the act of seeing. The whole tradition of philosophical inquiry into the nature of perception seems not to have taken seriously enough the complexity of the information processing involved. For example, Austin's (1962) *Sense and Sensibilia* entertainingly demolishes the argument, apparently favored by earlier philosophers, that since we are sometimes deluded by illusions (for example, a straight stick appears bent if it is partly submerged in water), we see sense-data rather than material things. The answer is simply that usually our perceptual processing does run correctly (it delivers a true description of what is there), but although evolution has seen to it that our processing allows for many changes (like inconstant illumination), the perturbation due to the refraction of light by water is not one of them. And incidentally, although the example of the bent stick has been discussed since Aristotle, I have seen no philosphical inquiry into the nature of the perceptions of, for instance, a heron, which is a bird that feeds by pecking up fish first seen from above the water surface. For such birds the visual correction might be present.

Anyway, my main point here is another one. Austin (1962) spends much time on the idea that perception tells one about real properties of the external world, and one thing he considers is "real shape," (p. 66), a notion which had cropped up earlier in his discussion of a coin that "looked elliptical" from some points of view. Even so,

> it had a real shape which remained unchanged. But coins in fact are rather special cases. For one thing their outlines are well defined and very highly stable, and for another they have a known and a nameable shape. But there are plenty of things of which this is not true. What is the real shape of a cloud? . . . or of a cat? Does its real shape change whenever it moves? If not, in what posture *is* its real shape on display? Furthermore, is its real shape such as to be fairly smooth outlines, or must it be finely enough serrated to take account of each hair? ***It is pretty obvious that there is no answer to these questions—no rules according to which, no procedure by which, answers are to be determined.*** (emphasis added), (p. 67)

But there *are* answers to these questions. There are ways of describing the shape of a cat to an arbitrary level of precision (see Chapter 5), and there are rules and procedures for arriving at such descriptions. That is exactly what vision is about, and precisely what makes it complicated.

1.3 A REPRESENTATIONAL FRAMEWORK FOR VISION

Vision is a process that produces from images of the external world a description that is useful to the viewer and not cluttered with irrelevant information (Marr, 1976; Marr and Nishihara, 1978). We have already seen that a process may be thought of as a mapping from one representation to another, and in the case of human vision, the initial representation is in no doubt—it consists of arrays of image intensity values as detected by the photoreceptors in the retina.

It is quite proper to think of an image as a representation; the items that are made explicit are the image intensity values at each point in the array, which we can conveniently denote by $I(x,y)$ at coordinate (x,y). In order to simplify our discussion, we shall neglect for the moment the fact that there are several different types of receptor, and imagine instead that there is just one, so that the image is black-and-white. Each value of $I(x,y)$ thus specifies a particular level of gray; we shall refer to each detector as a picture element or *pixel* and to the whole array I as an image.

But what of the output of the process of vision? We have already agreed that it must consist of a useful description of the world, but that requirement is rather nebulous. Can we not do better? Well, it is perfectly true that, unlike the input, the result of vision is much harder to discern, let alone specify precisely, and an important aspect of this new approach is that it makes quite concrete proposals about what that end is. But before we begin that discussion, let us step back a little and spend a little time formulating the more general issues that are raised by these questions.

The Purpose of Vision

The usefulness of a representation depends upon how well suited it is to the purpose for which it is used. A pigeon uses vision to help it navigate, fly, and seek out food. Many types of jumping spider use vision to tell the difference between a potential meal and a potential mate. One type, for example, has a curious retina formed of two diagonal strips arranged in a V. If it detects a red V on the back of an object lying in front of it, the spider has found a mate. Otherwise, maybe a meal. The frog, as we have seen, detects bugs with its retina; and the rabbit retina is full of special gadgets, including what is apparently a hawk detector, since it responds well to the pattern made by a preying hawk hovering overhead. Human vision, on the other hand, seems to be very much more general, although it clearly contains a variety of special-purpose mechanisms that can, for example, direct the eye toward an unexpected movement in the visual field or cause one to blink or otherwise avoid something that approaches one's head too quickly.

Vision, in short, is used in such a bewildering variety of ways that the visual systems of different animals must differ significantly from one another. Can the type of formulation that I have been advocating, in terms of representations and processes, possibly prove adequate for them all? I think so. The general point here is that because vision is used by different animals for such a wide variety of purposes, it is inconceivable that all seeing animals use the same representations; each can confidently be expected to use one or more representations that are nicely tailored to the owner's purposes.

As an example, let us consider briefly a primitive but highly efficient visual system that has the added virtue of being well understood. Werner Reichardt's group in Tübingen has spent the last 14 years patiently unraveling the visual flight-control system of the housefly, and in a famous collaboration, Reichardt and Tomaso Poggio have gone far toward solving the problem (Reichardt and Poggio, 1976, 1979; Poggio and Reichardt, 1976). Roughly speaking, the fly's visual apparatus controls its flight through a collection of about five independent, rigidly inflexible, very fast responding systems (the time from visual stimulus to change of torque is only 21 ms). For example, one of these systems is the landing system; if the visual field "explodes" fast enough (because a surface looms nearby), the fly automatically "lands" toward its center. If this center is above the fly, the fly automatically inverts to land upside down. When the feet touch, power to the wings is cut off. Conversely, to take off, the fly jumps; when the feet no longer touch the ground, power is restored to the wings, and the insect flies again.

In-flight control is achieved by independent systems controlling the fly's vertical velocity (through control of the lift generated by the wings) and horizontal direction (determined by the torque produced by the asymmetry of the horizontal thrust from the left and right wings). The visual input to the horizontal control system, for example, is completely described by the two terms

$$r(\psi)\dot{\psi} + D(\psi)$$

where r and D have the form illustrated in Figure 1–6. This input describes how the fly tracks an object that is present at angle ψ in the visual field and has angular velocity $\dot{\psi}$. This system is triggered to track objects of a certain angular dimension in the visual field, and the motor strategy is such that if the visible object was another fly a few inches away, then it would be

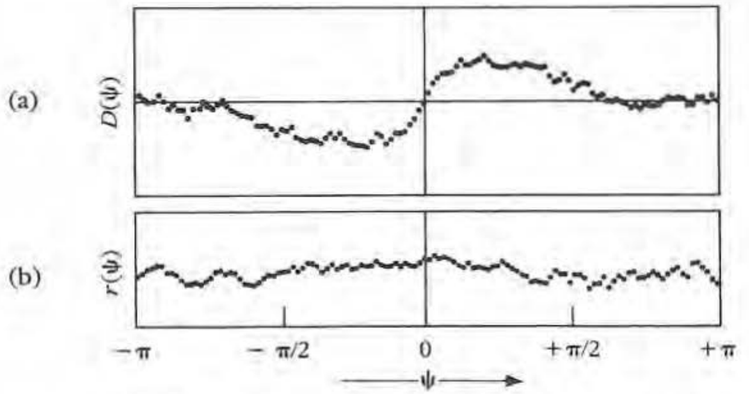

Figure 1–6. The horizontal component of the visual input R to the fly's flight system is described by the formula $R = D(\psi) - r(\psi)\,\dot{\psi}$, where ψ is the direction of the stimulus and $\dot{\psi}$ is its angular velocity in the fly's visual field. $D(\psi)$ is an odd function, as shown in (a), which has the effect of keeping the target centered in the fly's visual field; $r(\psi)$ is essentially constant as shown in (b).

intercepted successfully. If the target was an elephant 100 yd away, interception would fail because the fly's built-in parameters are for another fly nearby, not an elephant far away.

Thus, fly vision delivers a representation in which at least these three things are specified: (1) whether the visual field is looming sufficiently fast that the fly should contemplate landing; (2) whether there is a small patch—it could be a black speck or, it turns out, a textured figure in front of a textured ground—having some kind of motion relative to its background; and if there is such a patch, (3) ψ and $\dot{\psi}$ for this patch are delivered to the motor system. And that is probably about 60% of fly vision. In particular, it is extremely unlikely that the fly has any explicit representation of the visual world around him—no true conception of a surface, for example, but just a few triggers and some specifically fly-centered parameters like ψ and $\dot{\psi}$.

It is clear that human vision is much more complex than this, although it may well incorporate subsystems not unlike the fly's to help with specific and rather low-level tasks like the control of pursuit eye movements. Nevertheless, as Poggio and Reichardt have shown, even these simple systems can be understood in the same sort of way, as information-processing tasks. And one of the fascinating aspects of their work is how they have managed not only to formulate the differential equations that accurately describe the visual control system of the fly but also to express these equations, using the Volterra series expansion, in a way that gives direct information about the minimum possible complexity of connections of the underlying neuronal networks.

Advanced Vision

Visual systems like the fly's serve adequately and with speed and precision the needs of their owners, but they are not very complicated; very little objective information about the world is obtained. The information is all very much subjective—the angular size of the stimulus as the fly sees it rather than the objective size of the object out there, the angle that the object has in the fly's visual field rather than its position relative to the fly or to some external reference, and the object's angular velocity, again in the fly's visual field, rather than any assessment of its true velocity relative to the fly or to some stationary reference point.

One reason for this simplicity must be that these facts provide the fly with sufficient information for it to survive. Of course, the information is not optimal and from time to time the fly will fritter away its energy chasing a falling leaf a medium distance away or an elephant a long way away as a direct consequence of the inadequacies of its perceptual system. But this apparently does not matter very much—the fly has sufficient excess energy for it to be able to absorb these extra costs. Another reason is certainly that translating these rather subjective measurements into more objective qualities involves much more computation. How, then, should one think about more advanced visual systems—human vision, for example. What are the issues? What kind of information is vision really delivering, and what are the representational issues involved?

My approach to these problems was very much influenced by the fascinating accounts of clinical neurology, such as Critchley (1953) and Warrington and Taylor (1973). Particularly important was a lecture that Elizabeth Warrington gave at MIT in October 1973, in which she described the capacities and limitations of patients who had suffered left or right parietal lesions. For me, the most important thing that she did was to draw a distinction between the two classes of patient (see Warrington and Taylor, 1978). For those with lesions on the right side, recognition of a common object was possible *provided* that the patient's view of it was in some sense straightforward. She used the words *conventional* and *unconventional*—a water pail or a clarinet seen from the side gave "conventional" views but seen end-on gave "unconventional" views. If these patients recognized the object at all, they knew its name and its semantics—that is, its use and purpose, how big it was, how much it weighed, what it was made of, and so forth. If their view was unconventional—a pail seen from above, for example—not only would the patients fail to recognize it, but they would vehemently deny that it *could* be a view of a pail. Patients with left parietal lesions behaved completely differently. Often these patients had no language, so they were unable to name the viewed object or state its purpose and semantics. But they could convey that they correctly perceived its geometry—that is, its shape—even from the unconventional view.

Warrington's talk suggested two things. First, the representation of the shape of an object is stored in a different place and is therefore a quite different kind of thing from the representation of its use and purpose. And second, vision alone can deliver an internal description of the shape of a viewed object, even when the object was not recognized in the conventional sense of understanding its use and purpose.

This was an important moment for me for two reasons. The general trend in the computer vision community was to believe that recognition was so difficult that it required every possible kind of information. The results of this point of view duly appeared a few years later in programs like Freuder's (1974) and Tenenbaum and Barrow's (1976). In the latter program, knowledge about offices—for example, that desks have telephones on them and that telephones are black—was used to help "segment" out a black blob halfway up an image and "recognize" it as a telephone. Freuder's program used a similar approach to "segment" and "recognize" a hammer in a scene. Clearly, we do use such knowledge in real life; I once saw a brown blob quivering amongst the lettuce in my garden and correctly identified it as a rabbit, even though the visual information alone was inadequate. And yet here was this young woman calmly telling us not only that her patients could convey to her that they had grasped the shapes of things that she had shown them, even though they could not name the objects or say how they were used, but also that they could happily continue to do so even if she made the task extremely difficult visually by showing them peculiar views or by illuminating the objects in peculiar ways. It seemed clear that the intuitions of the computer vision people were completely wrong and that even in difficult circumstances shapes could be determined by vision alone.

The second important thing, I thought, was that Elizabeth Warrington had put her finger on what was somehow the quintessential fact of human vision—that it tells about shape and space and spatial arrangement. Here lay a way to formulate its purpose—building a description of the shapes and positions of things from images. Of course, that is by no means all that vision can do; it also tells about the illumination and about the reflectances of the surfaces that make the shapes—their brightnesses and colors and visual textures—and about their motion. But these things seemed secondary; they could be hung off a theory in which the main job of vision was to derive a representation of shape.

To the Desirable via the Possible

Finally, one has to come to terms with cold reality. Desirable as it may be to have vision deliver a completely invariant shape description from an image (whatever that may mean in detail), it is almost certainly impossible in only one step. We can only do what is possible and proceed from there toward what is desirable. Thus we arrived at the idea of a sequence of representations, starting with descriptions that could be obtained straight from an image but that are carefully designed to facilitate the subsequent recovery of gradually more objective, physical properties about an object's shape. The main stepping stone toward this goal is describing the geometry of the visible surfaces, since the information encoded in images, for example by stereopsis, shading, texture, contours, or visual motion, is due to a shape's local surface properties. The objective of many early visual computations is to extract this information.

However, this description of the visible surfaces turns out to be unsuitable for recognition tasks. There are several reasons why, perhaps the most prominent being that like all early visual processes, it depends critically on the vantage point. The final step therefore consists of transforming the viewer-centered surface description into a representation of the three-dimensional shape and spatial arrangement of an object that does not depend upon the direction from which the object is being viewed. This final description is object centered rather than viewer centered.

The overall framework described here therefore divides the derivation of shape information from images into three representational stages: (Table 1–1): (1) the representation of properties of the two-dimensional image,

Table 1–1. Representational framework for deriving shape information from images.

Name	Purpose	Primitives
Image(s)	Represents intensity.	Intensity value at each point in the image
Primal sketch	Makes explicit important information about the two-dimensional image, primarily the intensity changes there and their geometrical distribution and organization.	Zero-crossings Blobs Terminations and discontinuities Edge segments Virtual lines Groups Curvilinear organization Boundaries
2½-D sketch	Makes explicit the orientation and rough depth of the visible surfaces, and contours of discontinuities in these quantities in a viewer-centered coordinate frame.	Local surface orientation (the "needles" primitives) Distance from viewer Discontinuities in depth Discontinuities in surface orientation
3-D model representation	Describes shapes and their spatial organization in an object-centered coordinate frame, using a modular hierarchical representation that includes volumetric primitives (i.e., primitives that represent the volume of space that a shape occupies) as well as surface primitives.	3-D models arranged hierarchically, each one based on a spatial configuration of a few sticks or axes, to which volumetric or surface shape primitives are attached

such as intensity changes and local two-dimensional geometry; (2) the representation of properties of the visible surfaces in a viewer-centered coordinate system, such as surface orientation, distance from the viewer, and discontinuities in these quantities; surface reflectance; and some coarse description of the prevailing illumination; and (3) an object-centered representation of the three-dimensional structure and of the organization of the viewed shape, together with some description of its surface properties.

This framework is summarized in Table 1–1. Chapters 2 through 5 give a more detailed account.