Richard L. Gregory (March 7th 2000)

ADVENTURES OF A MAVERICK

In the Beginning--School and war

My father was a scientist--an astronomer--being the first director of the University of London Observatory. So I was brought up with optical instruments and also with the importance of making observations. My father measured the distances of the nearer stars for most of his life--using parallax from camera-positions separated by the 186,000,000 miles diameter of the Earthís orbit around the Sun. These measurements are crucial for scaling the universe. Is it an accident that years later I tried to scale and explain distortions of visual space?

At school I learned simple electronics in our Radio Club, as we built our own short wave receivers, and then more in the RAF at Number One Signal School at Cranwell. Cranwell had excellent teaching, and was highly civilised, with its drama and music societies.

I should have been posted to the Gold Coast, but a telegram recalling me from Christmas leave did not arrive in time so I was posted to Training Command in Canada. This was a year flying around the Bay of Fundy and St Johns, sometimes testing radio communications and radar; then six months with the Fleet Air Arm at Kingston, Ontario, where I had my own boat and sailed among the Thousand Islands. During almost six years in the RAF I had time to read and think on physics and biology, and wrote a science column in a local RAF magazine. I also read C.G. Jung (developing a permanent allergy) and William James (who remains a hero.) No doubt I absorbed some useful concepts from the technologies of radio and radar.

After the German war was over, and the Japanese war near its end, the RAF offered me an unusual and useful experience. I was posted (mysteriously) directly by the Air Ministry, to explain radar and communication systems to the public. This was at the John Lewis bomb site in Oxford Street, with an ambitious exhibition: the first jet plane (I actually saw its first flight, at Cranwell) and secrets were revealed. We explained how things worked, to all manner of people, especially factory workers who made bits and pieces throughout the war without for secrecy reasons knowing what they were for. We had four million visitors in six months. Is this a record? The experience was invaluable and great fun. Perhaps it led, thirty years later, to the founding of the Exploratory, the first Hands-on Science Centre in Britain.

Cambridge and the influence of Kenneth Craik's engineering ideas

I was lucky to get a scholarship to Cambridge; by the imaginative scheme of the time, the Forces Preliminary Examination. Although Cambridge was a family tradition on both sides, my father thought it would be a waste of time and probably would not have made it possible without the scholarship. So the war did me a lot of good. Getting to Cambridge was truly an impossible dream come true. I read Moral Sciences (for the first two years philosophy, ethics, logic and psychology.) I just missed Wittgenstein, but was taught by Richard Braithwaite, C.D. Broad, Alfred Ewing and John Wisdom. Some of us spent an hour a week with Bertrand Russell (then 76) which was wonderful, though he was rather bored with mathematical logic and mainly concerned with the politics of immediately post-war Europe.

The third year was entirely psychology. This was 1949-50, so I was among the last of Sir Frederic Bartlett's students. He was a major life-influence, remaining in memory the revered intellectual grandfather. (Something of this is described in my Bartlett Lecture (1999)). On the special occasions of his weekly lectures, he would share ideas that were interesting him, at that time the importance of prediction in skills. His favourite example was from cricket, which he loved; the batsman reading the present from his past knowledge, to predict the immediate future within his "range of anticipation". Such ideas countered the stimulus-response accounts of Behaviourism that still dominated American psychology. Bartlett had the ability to ignore whole chunks of subjects, concentrating on what mattered. His judgement was remarkably right, though perhaps he was too cavalier over statistics. I followed him in trying to save my students from the more idiotic ideas--especially the fashionable mathematical models of learning--though no doubt without his wisdom.

Cambridge psychology was deeply affected by Kenneth Craik's ideas, as presented in his short book The Nature of Explanation (1943). Craik's premature death by an accident in 1945, when he was knocked off his bicycle outside his Cambridge college, was a major tragedy. His ideas inspired British psychology at that time and remain with us today. Much came from applying war-time technologies for considering the brain and the nature of skills. It is well known that Bartlett was greatly influenced by Craik's engineering concepts, and these were dominating ideas in the Department following Craik’s death. In general, the concepts of servo-control and predicting, which were developed for anti-aircraft guns, led to cybernetic accounts of the nervous system, and to Craik's crucially important notion of the brain representing perceptions, ideas and so on, by physical states.

For those of us who are guided by engineering principles, it seems somewhat paradoxical that engineering does not have a clue how to describe or explain consciousness--yet we look to engineering concepts for the answer. Perhaps most of us suppose that a sufficiently complicated artificial brain would be conscious; but we don’t know how, or why, this would be. How we would recognise that it has sensations (or qualia) remains a puzzle; but this is also a puzzle for brain-based minds, other than our own.

A positive gain of the neuronal account of mind was active positing and testing of engineering-type accounts of intelligence, perception and learning. These were not, however, generally related to specific neural events. Indeed, many psychologists simply ignored the brain. The burgeoning fields of cybernetics and Artificial Intelligence were based on the assumption of brain-based mind, but so little was known of cortical function, that this was generally recognised only implicitly. Exceptions were the very different ideas of Donald Hebb in Canada and Warren McCulloch at MIT, (McCulloch and Pitts (1943). The great British exception was Kenneth Craik, with his Internal Models. Surely the most important single idea in cognitive psychology is brain representation in physical terms. Craik (1943) wrote:

“By a model I do not mean some obscure non-physical entity which attends the model, but the fact that it is a physical working model which works in the same way as the process it parallels . . . Thus the model need not resemble the real object pictorially; Kelvin's tide-predictor, which consists of a number of pulleys on levers, does not resemble a tide in appearance, but it works in the same way in certain essential respects--it combines oscillations of various frequencies so as to produce an oscillation which closely resembles in amplitude at each moment the variation in tide level at any place.”

This is the basis of AI--except that computers came to be more symbolic, as they became digital. No doubt the rise of computer technology has profoundly affected how we think of brain and mind; but now, there is a considerable swing back to more analogue accounts of brain function. These had their start especially in the ideas of Donald Hebb (1949), which stressed the importance of slowly-won inductive generalisations for learning and perception--with a specific neural hypothesis.

Internal models

I took Craik's Internal Models idea literally with the disturbance-rejecting telescope camera. From a working simulator, built with my technician Bill Matthews, this was developed and tested with the brilliant engineer Steven Salter. Salter built the final version and we tested it on mountains in New Mexico and Arizona. This was supported by the Royal Society and the US Air Force. It started with a simulation, made in my room in the Cambridge Department of Psychology (Figure xx), using a randomly disturbed fish tank, to play the part of the atmosphere. In a simple form, it embodied Craikian Internal Models. Perhaps it was an early example of Artificial Intelligence, as it used knowledge from the past to improve perception of the present.

The method was, first, to take a long photographic exposure of for example the moon, through the image-disturbing turbulence of the atmosphere. The positions of the contours were correctly placed, though blurred, in this photographic "Internal Model". After rapid processing, the photographic plate was replaced in precisely the same position in the camera; so the dynamically disturbed image fell on to its own time-averaged negative picture. When the image most nearly matched the negative, the least light passed through - so providing an auto-correlation signal from a photo-multiplier which received the integrated light passing through the negative. This signal opened the shutter of a second camera, which received the moon's image directly; but only at moments when the disturbance was minimal, when the auto-correlation was high. This is when the disturbed present matches the average past (Gregory 1964a, 1974a). So disturbances were rejected. A picture, better than the original, was built up in the second camera by many successive exposures selected in this way to have minimal disturbance (Figure xx).

Figure xx
Craikian Internal Model basis of an image-improving system

A long exposure photograph of astronomical objects, taken through atmospheric turbulence, gives statistically correct positions of contours, though it suffers blurring, As a photographic negative, it was used as a Craikian Internal Model to give a running autocorrelation signal, for selecting moments of best "seeing", for a second camera to integrate photic energy with multiple exposures. The initial negative is at the top of the first tube. The photo-multiplier is in the second tube. The sampling shutter for the second camera (to the right, not shown) is below the photo-multiplier.

It might seem somewhat bizarre that a psychologist should spend time, and be allowed to spend time, on such a project. But it was possible to do just about anything in the Cambridge of that period. I hope this is still true!

It was also the influence of engineering ideas that made me question interpretations of experiments for localising brain functions. This was sometimes taken as a general criticism of what came to be called Neuropsychology--inferring local functions from clinical symptoms of brain damage--but this is not quite right. I never claimed that it is impossible, only that there are severe logical difficulties to be considered, especially when adequate theoretical models of brain functions are not available. This “attack” upset some friends, but they were generally tolerant of this eccentricity and did not simply dismiss it. These issues extend to how to interpret images from the wonderful PET and functional NMI techniques. Perhaps these difficulties are not considered sufficiently now.

Having done some electronics, I saw the issues in engineering terms (indeed, a paper was called “The brain as an engineering problem” (1961)). My first paper on this was Models and the localisation of function in the central nervous system, presented at the Mechanisation of Thought Symposium Processes (1958). Electronic systems have processes going on inside which are not in their outputs or "behaviour", such as oscillators in radios or TV's. When detected, with a probing oscilloscope, they are mysterious if the principles of the circuits are not understood. To interpret EEG brain waves, we must know what processes they reflect and how this works. For problems of interpreting ablation experiments, I suggested an analogy from radio engineering which has often been quoted since, as a potential trap (Gregory 1958, Vol II page 678):

“Suppose that when [a] condenser breaks down, the set emits howls. Do we argue that the normal function of the condenser is to inhibit howling? Surely not. The condenser’s abnormally low resistance has changed the system as a whole, and the system may exhibit new properties, in this case howling.”

Neurophysiologists, when faced with comparable situations, have sometimes postulated suppresser regions, but this may be entirely wrong. The problem is to analyse the new circuit - which may require very special, indeed unique understanding. What happens when bits go wrong, or are missing, can be unexpected and exceedingly hard to explain. In serial, or closely coupled parallel systems, there is no simple one-to-one relation between internal component losses or failures, and output behaviour or symptoms. It was just this kind of problem of interpretation from inadequate theoretical understanding that defeated the engineers of the Three Mile Island atomic power station, when the coolant failed.

Research and teaching at Cambridge (1950-67)

I was fortunate in being kept on at Cambridge after graduating, by Sir Frederic Bartlett, first at the MRC Applied Psychology Unit, which was then in the same building as the University Department of psychology. Bartlett seconded me to the Royal Navy for a year to work on escaping from submarines, following the disaster of the Affray, when two crews were tragically lost. This was at the Royal Naval Physiological Laboratory at Portsmouth, using a large pressure tank with controlled Oxygen and CO_2 levels, with dummy escape gear. I designed and built a printing recorder (named Thoth, after the Egyptian god of writing) for recording what happened over ten or more hours. It was fun going on trips in the subs., and indeed this was a great experience with considerable responsibility There is much to be said for combining practical and theoretical problems to work on.

After three years, I was appointed as Demonstrator and shortly after Lecturer in the Psychology Department of the University, under Professor Oliver Zangwill. The Department expanded with a new wing, and I was very fortunate to be given the whole of the top floor, which I designed as the Special Senses Laboratory. This I ran with numerous British and American foundation grants and wonderful students. We worked on fascinating projects on vision and hearing, problems for astronauts for the forthcoming moon landing of 1969 (Gregory & Ross 1964a and b), and designing various instruments, with our own workshop. This was a great time. There was a strong emphasis on engineering ideas, no doubt following Craik's lead; but this did not entirely dominate the Department. Notably, Alice Heim was developing her intelligence tests (AH4 and AH5), and there were strong links with animal behaviour in the Zoology Department, as well as clinical work with the local Fulbourn hospital and the National Institute of Neurology at Queen Square in London.

We were in the same building as Physiology. Following the heritage of Lord Adrian (I attended some of his demonstrations), we saw the physiologists as superior beings, with us rather low in the pecking order. The present effective sharing of ideas and co-operation over experiments came later, especially by the founding of the Craik Laboratory. Especially important were William Rushton, Horace Barlow (who very much combined physiology with psychology and still does) John Robson and Fergus Campbell. Fergus was the leader of grating experiments for investigating Fourier accounts of vision. Although this was important, I confess to finding its domination over a decade of visual research annoying as it distracted from more cognitive concepts, which seemed fundamental--though at that time generally regarded as dubious science with little or no explanatory power. It is a great pleasure that this is now how much of perceptual research is developing. Of course the discoveries of physiology are incredibly exciting; but it is the linking of physiology with processes carried out by the machinery of the brain that gives deep insights into sight, and so much more. A great insight came from Claud Shannon's theory of information as surprise. What might happen affects perception and behaviour (Shannon and Weaver 1949) Hick (1952), so stimuli are not all-important.

Recovery from blindness: The case of SB

A chance changed the way I came to think of perception: a rare case of recovery from blindness. This was the case of SB, studied with my colleague Jean Wallace (Gregory and Wallace 1963). Jean saw in a Midlands local paper that a man blind all his life, was to receive corneal transplants at the age of 52. This presented a quandary: immediate action was essential; but we were very busy at the time, with teaching and other duties. Within an hour we packed every imaginable visual experiment into the car and set off for the hospital. It was the best decision I ever made.

Whatever we may have found of benefit to others, it was this investigation that set my path to vision, and phenomena of illusions, as among the most fascinating topics in psychology. In fact, this set up how I would think about perception and the mind. We found that SB could see almost immediately objects already familiar to him, especially through touch, though he remained blind for a long time to unknown objects. Most striking: he could read upper case letters, he had been taught to read by touch in the blind school, but not lower case letters which were not taught in the school. Further, he could tell the time visually, without any help or practice. Here the touch experience was from a large pocket watch, with no glass. He could unhesitatingly tell the time by touch from its hands. The conclusion was, that object vision depends on knowledge derived from active exploration, giving meaning to the eyes' images. It showed, also, the importance of cross-modal transfer - knowledge from one sense being available to other senses. Our findings, especially extensive cross-modal transfer from touch to vision, were very surprising at that time. There is now confirming evidence from other cases, (Valvo 1971) in Italy, and recently from Japan (Cf. Gregory 1997).

Considering how eyes and other senses--together with brains--give knowledge of the world of objects, is the central question. There are only two theories of perception: Direct, and Indirect. Both go back to the Greeks; especially Direct theories, which seemed plausible before the retinal image and the complexity of neural processing were discovered. These (it seems to me) make J.J. Gibson’s Direct pick-up theory impossible to accept literally. This was my conclusion shortly after graduating, but I greatly respected Gibson’s experiments, and much liked Jimmy and his wife Eleanor personally. With his puckish grin he looked like a Walt Disney character, while Eleanor was stately and dignified. Jimmy would argue for hours on end. We stayed up all night in his house comparing insect with human vision. Jimmy allowed that insects had retinal image, but not humans. (Is this the sometimes misleading power of top-down belief on observation?)

As a student I was saturated in his Direct theory of vision (in his Perception of the Visual World (1950)) but came to disagree with it--though I greatly appreciated his experimental work. In fact, though, I spent too much time writing objections to direct theories of vision, as I came to hold a very different, essentially Hemholtzian, view of perception as being creatively intelligent. (Hence the title of my Royal Institution Christmas Lectures The Intelligent Eye 1967, published 1970). I couldn’t then, and still do not, believe that perception of objects as solid things with causal properties can be given without a major contribution of knowledge of the world, gained from interactive experience, especially by handling objects. This is very different from Gibson's view that the information is simply out there, to be "picked up". Undoubtedly perception is adapted to the environment, through Evolution, so his ecological optics makes a lot of sense; but it is striking that we can come to see and cope with new kinds of objects and situations, as in driving and flying. It amazed me that Gibson could deny retinal images (which with the associated complex physiological processing clearly makes vision indirect), and also that he could deny phenomena of illusions. They are, indeed, a traditional embarrassment for "direct" theories. Gibson says that there are no illusions, except occasionally in special laboratory situations; but surely this is not the truth of illusions. They are all-pervasive, though not always recognised as distortions or whatever in normal situations. They become obvious in situations such as hill walking, or golf, where there are clearly noticeable errors. (A golf pro wrote to me, saying he would misjudge the length of a drive after tall trees near the green had been cut down.) For me, illusions are both strong evidence of the indirectness of perception and they are useful for investigating many physiological and cognitive processes, (Cf page xx).

To accept an indirect account of perception, and spend time on illusions, was quite "way-out" at the time, though I was certainly not the first to take illusions seriously: Hermann Helmholtz, W.H.R. Rivers, Adelbert Ames, Jerome Bruner and Donald Mackay, as well as Lionel and Roger Penrose with their wonderful impossible objects, made important discoveries by playing with illusions seriously. In Britain, Donald Mackay (at Kings College in London and then at Keele) deserves special credit for seeing their importance and investigating and discovering a wide variety of illusory phenomena.

My approach was from epistemology. Indeed, this is an experimental epistemology, for the phenomena suggest answers to ancient philosophical questions of how we experience and know. Direct theories promise certainty (which has great appeal), while indirect theories give no such promise (which can be unsettling). John Locke's distinction between Primary and Secondary characteristics is germane here. It is disturbing to think that colours are created in the brain and projected into the world of objects - which themselves have no colour - though this is their most striking reality.

Helmholtz's perceptions as Unconscious Inferences was a far more appealing approach for me than Gibson's Direct pick-up. Essentially following Helmholtz, it seemed to me that perceptions are predictive hypotheses. I have always found this a useful notion, especially as it suggests links between processes of perception to the methods by which science gains knowledge, (Cf. Mind in Science 1981). The notion that perceptions are hypotheses gives a status for perceptions that seems right. It also gives phenomena of illusions a rational place - though they have to be played down or denied by Direct accounts, which seems quite wrong. (These ideas are developed throughout my papers and books, especially: Gregory 1963, 1968a, 1968b, 1970, 1980, 1997).

Seeking truths through illusions

Illusions can be seen as trivial, as irritating, as dangerous, as amusing - or as significant phenomena for discovering perceptual processes, leading to the very nature of perception itself. Any of these can be true; though it is the last that justifies the remarkable attention they have now earned. Interest in illusions goes in fashions. They were deeply unfashionable when I was a student, though popular a century before.

It was the abnormality and general lack of illusions experienced by SB, following his recovery of sight, that attracted my attention to these phenomena and made me think of many of them as being more cognitive than physiological. Of course physiology is always involved: the question is where the action is that generates the phenomenon. Generally cognition seemed too vague a notion at the time: a view I did not share, but tried to make the ideas more concrete. This was greatly helped by the prevalence of computer errors due to inappropriate software though the hardware was working normally. This is the key concept, and not at all vague, for cognitive illusions.

The Muller-Lyer was my chosen example, for it is very robust and readily measured. The idea came when I suddenly realised that the Muller-Lyer arrows are flat projections of corners--as inside rooms, or outside buildings, respectively. Then the Ponzo figure is obviously a perspective of receding lines. In all cases, features signalled as more distant are expanded. This is opposite to the optical shrinking of retinal images with distance. This suggested that the modus operandi of the distortions is Size Scaling, which normally compensates image shrinking with distance. But in a picture there is no shrinking, as it is flat, and so depth cues distort. What had probably hidden this before was the fact that the illusory figures generally <u>appear</u> flat. So the suggestion was: Size Scaling can be set directly by perspective or other depth cues even though depth is not seen, as when countered by the texture of the picture surface (Gregory 1963). This led to experiments where the texture was removed, and to observations with luminous depth-ambiguous objects, such as wire cubes--which change shape as they flip in depth--the apparently further face appearing larger--though the retinal image is unchanged. This made it possible to separate bottom-up from top-down Constancy Scaling. (This was originally described neutrally as "Primary" and "Secondary" scaling. The key was using ambiguous figures or objects, to separate bottom-up signals from top-down knowledge.

Edinburgh: The Department of Machine Intelligence and Perception

I left Cambridge in 1967, to help to found the Department of Machine Intelligence and Perception at Edinburgh with Donald Michie and Christopher Longuet-Higgins FRS. Christopher and I joined Donald, whose idea it was, and who was already at Edinburgh. This was a brave dream which was generously supported by the University of Edinburgh, especially by its Vice-Chancellor Sir Michael Swann, with funding by the Science Research Council (SRC).

It came at a bad time for me, as my Cambridge projects were going great guns. Edinburgh was a long way away from Cambridge, and I had to prepare apparatus for the six Royal Institution Christmas Lectures I was to give in London (the first on colour television), yet we did not even have electricity in the embryo workshop. This was the first of many snags. The first problem, however, was more amusing than tragic. Initially we were offered a de-consecrated Church of Scotland church for our laboratory and offices; but when they heard we were going to build a robot, it was withdrawn! The notion of minds in machines may indeed have theological implications, and can still be frightening.

The Department undertook a lot of theoretical work on vision and learning and problem-solving, so the dream was in fair part fulfilled (Cf. Michie 1974. For general background, discussion and references, see Boden 1977). I never became at all expert at programming, and I made very little contribution beyond something of the philosophy. This was pushing the importance of internal representations for prediction and gap-filling, rather than direct control from the inputs. Christopher Longuet-Higgens made a neat, very simple, wheeled model to show this. More important, Christopher developed fundamental ideas for neural nets.

Unlike myself, Christopher learned to program. Jim Howe (who had been my graduate student at Cambridge) went on to run the Department for many years.

We, the founders, were criticised for over-optimism - expecting general intelligence by the end of the century - though without optimism we would never have started. What the early history of AI showed, is that the brain is far more complicated and more subtle than had been realised. This was useful knowledge. It made the problems and aims of brain research more realistic and suggested some specific goals. The goal for me now (as then) is to give machines rich and rapidly available knowledge of the world. When a machine can draw interesting analogies, and make puns, it will have arrived.

Christopher and I left the Department after just a few years. This is regrettable. A major reason was the infamous Lighthill Report. The highly distinguished mathematician and fluid dynamicist Sir James Lighthill was commissioned by the SRC to write a report on the current state and likely future of AI. For right or wrong, this report had drastic effects on the new science of Artificial Intelligence. Perhaps Lighthill was being wisely cautious, but it held up progress in one of the main activities that, surely, will mark out this century.

Bristol: The Brain and Perception Laboratory

I moved from Edinburgh to the University of Bristol, to start a new MRC funded mini-department in the Department of Anatomy in the Medical School. It was a great privilege to be able to run my own show, in prime laboratory space, with our own Workshop and darkroom. Brain and Perception really started from an essay - an attempt to spell out an experimental philosophy of perception with a plan for progress. The emphasis was to be phenomena-based, for testing cognitive concepts with an eye on clinical implications. Running for twenty years, it was great fun and it achieved much that was intended, together with several surprises.

One of the initial aims of Brain and Perception was to look at eye-hand tracking in Parkinson's Disease, with gaps in the track, to see whether they have abnormal limited powers of prediction. This was taken up and studied over many years by Ken Flowers, who is now an established authority on Parkinson's Disease. Robert Williams studied object recognition in dynamically noise-degenerated displays, measuring “central” temporal integration. We studied many phenomena of illusions, including the Cafe Wall (Gregory and Heard 1979, 1982) and illusory contours (Gregory and Harris 1975, Harris and Gregory 1975)

We also devised several instruments, including a Heterochromatic Photometer, a 3-D Drawing Machine, and a Speech Processing Hearing Aid. The latter should be useful, but was never manufactured, probably because at that time it could not be made sufficiently small to be inconspicuous. At any rate, this brought home the difficulty of developing techniques in the laboratory to a stage where industry might become involved.

Hands-on Science--the Exploratory

Upon retiring I was fortunate to be able to continue as an Emeritus Professor, with various small grants, especially Gatsby, with the position of Senior Research Fellow. The University and the Department have been and still are generous, giving me space and time beyond my sell-by-date. I am most grateful. This has allowed me to write papers and books (including revising my best-known book Eye and Brain) and to collaborate with colleagues in this country and abroad, as well as giving umpteen lectures.

Presenting one’s special knowledge and ideas more widely, can be worthwhile and may repay the dept to society that supports research beyond its immediate pay-back. But for the public to be interested, there must be mutual learning for how to present and understand ideas and discoveries. So Public Understanding of Science is important - but also scientific understanding of the public. The adventure of science should be a major basis of general culture. Psychology and perception are relatively easy to put across.

It was demonstrating RAF technologies to the public, at the end of the war that initiated me into communicating with the public. This was confirmed by meeting Frank Oppenheimer in 1970 in San Francisco, when giving the first Smith-Kettlewell lecture, at the opening of the Eye Institute of this name. Frank had just opened the Exploratorium, hands-on science centre. It was - it is - a revelation. Here, anyone can experiment and try out ideas. I helped him design perception experiments, and we spent many hours on the aims and philosophy of hands-on science.

The Exploratory was founded eighteen years ago. Something of its philosophy is given in A History of the Bristol Exploratory (1987) and in the last chapter of Mirrors in Mind (1997). It put into practice the theoretical insight from SB's recovery of sight: that effective sight is immediately available when there is knowledge from touch - the idea being, that infant learning by hand-eye experimenting may be continued, to enrich experience and understanding throughout life. The Exploratory attracted over two million visitors, and became an accepted Bristol institution with a wide reputation. Curiously and sadly, it has been killed by the massively funded Millennium project. Money can destroy as well as create.

The Great Enigma: Consciousness.

Although eliciting experiences is what art, and most of living is all about until recently consciousness has been a taboo topic for science. So Francis Crick's book The Astonishing Hypothesis (1994) is astonishing, and most welcome. There are two major questions: How can a physical system, the brain, generate consciousness? What, if anything, does consciousness do? No plausible answer has so far been suggested for the first, except that consciousness is an emergent property of certain kinds of complexity. Emergent properties, such as those of water from combining Oxygen and Hydrogen atoms, (and note how very different water is from these gasses) remain mysterious before we have a wide embracing theory. Presumably this is what we seek for consciousness, to remove the mystery from this emergence from brain (or computer?), physical functions to subjective sensations.

The second question, seems to me to point to the special significance of the present moment. It is occasionally pointed out (Cf. Humphrey 1992) that consciousness is in the now. Surely this is right. It is interesting to compare the vividness of perception with the shadowy, quite dim awareness of memory and imagination. (Try looking at something, then shut the eyes and compare with the memory. The difference, surely, is dramatically striking.) If indeed perception depends to a very large extent on knowledge from the past, there must be a problem for distinguishing present from past. Yet this is essential for survival in the present. To confuse a green traffic light with a memory of a traffic light could be disastrous.

The present moment is signalled by real-time afferent inputs. This should be adequate for primitive stimulus-response behaviour - but for cognitive brains, such as ours, surely the real-time inputs could easily be confused or lost in the wealth of memory. So, perhaps the vivid sensations of perception flag the present, to avoid confusion with the past. What of the relatively dim consciousness of memory and imagination? These, also, are in the present; but they lack afferent inputs - which alone can signal the present. In both cases, the present moment seems to be a key to consciousness (Gregory 1998). There is a hint here, but perhaps no more. Philosophy raises questions, and can help to formulate questions; but here, as elsewhere, we expect answers to come from experimental science.

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