The Ratio Club: A Hub of British Cybernetics[1]

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1. Introduction

Writing in his journal on the 20th September 1949, W. Ross Ashby noted that six days earlier he'd attended a meeting at the National Hospital for Nervous Diseases, London. He comments, "We have formed a cybernetics group for discussion – no professors and only young people allowed in. How I got in I don't know, unless my chronically juvenile appearance is at last proving advantageous. We intend just to talk until we can reach some understanding." (Ashby 1949a). He was referring to the inaugural meeting of what would shortly become the Ratio Club, a group of outstanding scientists who at that time formed much of the core of what can be loosely called the British cybernetics movement. The club usually gathered in a basement room below nurses' accommodation in the National Hospital, where, after a meal and sufficient beer to lubricate the vocal chords, participants would listen to a speaker or two before becoming embroiled in open discussion. The club was founded and organized by John Bates, a neurologist at the National Hospital. The other twenty carefully selected members were a mixed group of mainly young neurobiologists, engineers, mathematicians and physicists.

A few months before the club started meeting, Wiener's landmark *Cybernetics: Control and Communication in the Animal and Machine* (Wiener 1948) had been published. This certainly helped to spark widespread interest in the new field, as did Shannon's seminal papers on information theory (Shannon and Weaver 1949), and these probably acted as a spur to the formation of the club. However, as we shall see later, the first of the official membership criteria of the club was that only "… those who had Wiener's ideas before Wiener's book appeared … " (Bates 1949a) could join. This was no amateur cybernetics appreciation society; many members had already been active for years in developing the new ways of thinking about behaviour generating mechanisms and information processing in brains and machines that were now being pulled together under the term coined by Wiener. Indeed, the links and mutual influences that existed between the American and British pioneers in this area ran much deeper than is often portrayed. There was also a very strong independent British tradition in the area that had developed considerable momentum during World War II. It was from this tradition that most club members were drawn.

The other official membership criterion reflected the often strongly hierarchical nature of professional relationships at that time. In order to avoid restricting discussion and debate, Bates introduced the 'no professors' rule alluded to by Ashby. If any members should be promoted to that level, they were supposed to resign. Bates was determined to keep things as informal as possible; conventional scientific manners were to be eschewed in favour of relaxed and unfettered argument. There also appears to have

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been two further, unofficial, criteria for being invited to join. First, members had to be as smart as hell. Second, they had to be able to contribute in an interesting way to the cut and thrust of debate, or, to use the parlance of the day, *be good value*. This was a true band of Young Turks. In the atmosphere of enormous energy and optimism that pervaded post-war Britain as it began to rebuild, they were hungry to push science in new and important directions. The club met regularly from 1949-1955, with one final reunion meeting in 1958. It is of course no coincidence that this period parallels the rise of the influence of cybernetics, a rise in which several members played a major role.

There are two things that make the club extraordinary from a historical perspective. The first is the fact that many of its members went on to become extremely prominent scientists. The second is the important influence the club meetings, particularly the earlier ones, had on the development of the scientific contributions many of that remarkable group would later make. The club membership undoubtedly made up the most intellectually powerful and influential cybernetics grouping in the UK, but to date very little has been written about it – there are brief mentions in some histories of AI and cognitive science (e.g. Fleck 1982, Boden 2006, and D. Clark (2003) has a chapter on it in his PhD thesis, based on papers from the John Bates archive). This article is intended to help fill that gap. It is based on extensive research in a number of archives, interviews with surviving members of the club and access to some member's papers and records.

After introducing the membership in the next section, the birth of the club is described in some detail. The club's known meetings are then listed and discussed along with its scope and *modus operandi*. Following this some of the major themes and preoccupations of the club are described in more detail. The interdisciplinary nature of the intellectual focus of the group is highlighted before discussing the legacy of the club. Because so many rich threads run through the club and the lives and work of its members, this chapter can only act as an introduction. A fuller treatment of all these topics can be found in Husbands and Holland (forthcoming).

2. The members

Before embarking on a description of the founding of the club, it is useful at this point to sketch out some brief details of its membership as that will help to give a sense of the historical importance of the group.

The definitive list of twenty-one members, with very brief details of expertise and achievements, is given below. Of course these summaries are far too short to do justice to the careers of these scientists. They are merely intended to illustrate the range of expertise in the club and to give a flavour of the calibre of members.

W. Ross Ashby (1903-1972)**,** trained in medicine and psychiatry, is regarded as one of the most influential pioneers of cybernetics and systems science. Author of the classic books *Design for a Brain* (Ashby 1952a) and *An Introduction to Cybernetics* (Ashby 1958), some of his key ideas have recently experienced something of a renaissance in various areas of science including Artificial Life and modern AI. At the inception of the club he was director of research at Barnwood House Hospital, Gloucester. He subsequently became a professor in the Department of Biophysics and Electrical Engineering, University of Illinois.

Horace Barlow FRS (1921-), a great-grandson of Charles Darwin, is an enormously influential neuroscientist, particularly in the field of vision, and was one of the pioneers of using information theoretic ideas to understand neural mechanisms (Barlow 1953, 1959, 1961), a direct consequence of his involvement in the Ratio Club. When the club started he was a PhD student in Lord Adrian's lab at the department of physiology, Cambridge University. He later became Royal Society Research Professor of Physiology at Cambridge University.

John Bates (1918-1993) had a distinguished career in the neurological research unit at The National Hospital for Nervous Diseases, London. He studied human EEG in research into voluntary movement and became the chief electroencephalographer at the hospital. The Club was his idea and he ran it with quiet efficiency and unstinting enthusiasm.

George Dawson (1911-1983) was a clinical neurologist at the National Hospital, Queen square. At the time of the Ratio Club he was a world leader in using EEG recordings in a clinical setting. He was a specialist in ways of averaging over many readings which allowed him to gather much cleaner signals than was possible by more conventional methods (Dawson 1954). He became Professor of Physiology at UCL.

Thomas Gold FRS (1920-2004) was one of the great astrophysicists of the 20th century, being a co-author, with Bondi and Hoyle, of the steady state theory of the universe and having given the first explanation of pulsars, among countless other contributions. However, he had no time for disciplinary boundaries and at the time of the Ratio Club he was working in Cambridge University Zoology Department on a radical positive feedback theory of the working of the inner ear (Gold 1948) – a theory that was, typically for him, decades ahead of its time. He went on to become Professor of Astronomy at Harvard University and then at Cornell University.

I.J. (Jack) Good (1916- 2009) was recruited into the UK top secret code cracking operation at Bletchley Park during the second world war, where he worked as the main statistician under Alan Turing and Max Newman. Later he became a very prominent mathematician, making important contributions in Bayesian methods and early AI. During the Ratio years he worked for British Intelligence. Subsequently he became Professor of Statistics at Virginia Polytechnic Institute.

W.E. Hick (1912-1974) was a pioneer of information theoretic thinking in psychology. He is the source of the still widely quoted Hick's law which states that the time taken to make a decision is proportion to the log of the number of alternatives (Hick 1952). During the Ratio years he worked in the Psychology laboratory at Cambridge University. He went on to become a distinguished psychologist.

Victor Little (1920-1976) was a physicist at Bedford College, London, who worked in acoustics and optics before moving on to laser development.

Donald Mackay (1922-1987), trained as a physicist, was a very highly regarded pioneer of early machine intelligence and of neuropsychology. He was also the leading scientific apologist for Christianity of his day. At the birth of the club he was working on a PhD in the Physics department of King's College, London. He later became a professor at Keele University where he founded the Department of Communication and Neuroscience.

Turner McLardy (1913-1988) became an international figure in the field of clinical psychiatry. He emigrated to the USA in the late 1950s to develop therapeutic techniques centred around planned environments and communities. Later he became a pioneer of understanding the role of zinc in alcoholism and schizophrenia. At the inception of the club he worked at Maudsley Hospital, London.

Pat Merton FRS (1921-2000) was a neurophysiologist who did pioneering work on control theoretic understandings of the action of muscles (Merton 1953). Later he carried out a great deal of important early research in magnetic stimulation of the cortex for which he is justly celebrated (Merton and Morton 1980). During the Ratio years he worked in the neurological research unit at the National Hospital. He later became Professor of Human Physiology at Cambridge University.

John Pringle FRS (1912-1982) was one of the leading invertebrate neurobiologists of his day. He was the first scientist to get recordings from single neurons in insects, something that had previously been thought to be practically impossible (Pringle 1938). He did much important work in proprioception in insects, insect flight and invertebrate muscle systems. At the birth of the club he worked in the Zoological laboratory, Cambridge University. He subsequently became Professor of Zoology at Oxford University.

William Rushton FRS (1901-1980) is regarded as one of the great figures in 20th century vision science. He made enormous contributions to understanding the mechanisms of colour vision, including being the first to demonstrate the deficiencies that lead to colour blindness (Rushton 1955). Earlier he did pioneering work on the quantitative analysis of factors involved in the electrical excitation of nerve cells, helping to lay the foundations for the framework that dominates theoretical neuroscience today (e.g. Rushton 1935). He worked at Cambridge University throughout his career where he became Professor of Visual Physiology.

Harold Shipton (1920- 2007) worked with Grey Walter on the development of EEG technology at the Burden Neurological Institute, Bristol. He was the electronics wizard who was able to turn many of Walter's less than precise designs into working realities. Later he became a professor at The University of Washington at St. Louis, where he worked on biomedical applications. At the time of the Ratio meetings, his father-in-law, Clement Attlee, was prime minister of Great Britain.

D.A. Sholl (1903-1960) did classic research on describing and classifying neuron morphologies and growth patterns, introducing the use of rigorous statistical approaches (Sholl 1956). Most of the classification techniques in use today are based on his work. He also published highly influential papers on the structure and function of the visual cortex. He worked in the Anatomy department of University College, London where he became Reader in Anatomy before dying young.

Eliot Slater (1904-1983) was one of the most eminent British psychiatrists of the twentieth century. He helped to pioneer the use of properly grounded statistical methods in clinical psychiatry. Slater's work with Rudin on the genetic origins of schizophrenia, carried out in Munich in the 1930s, still underpins all respectable Anglo-American work in psychiatric genetics, a field to which Slater made many important contributions (Slater et al. 1971). He worked at the National Hospital for Nervous diseases, London.

Alan Turing FRS (1912-1954) is universally regarded as one of the fathers of both computer science and artificial intelligence. Many regard him as one of the key figures in twentieth century science and technology. He also anticipated some of the central ideas and methodologies of Artificial Life and Nouvelle AI by half a century – for instance, he proposed artificial evolutionary approaches to AI in the late 1940s (Turing 1950) and published work on reaction-diffusion models of the chemical origins of biological form in 1952 (Turing 1952). At the inception of the club he was working at Manchester University, where he was part of a team that had recently developed the world's first stored-program digital computer.

Albert Uttley (1906-1985) did important research in radar, automatic tracking and early computing during WWII. Later he became head of the pioneering Autonomics Division at the National Physical Laboratory in London where he did research on machine intelligence and brain modeling. However, he also became well know as a neuropsychologist, having made several important contributions to the field (Uttley 1979). At the birth of the club he worked at TRE, Malvern, the main British military telecommunications research establishment. Later he became Professor of Psychology at Sussex University.

W. Grey Walter (1910-1977) was a world leader in EEG research. He discovered theta and delta brain waves and, with Shipton, developed the first EEG brain topography machine (Walter and Shipton 1951). At the time of the Ratio Club he was at the Burden Neurological Institute, Bristol, where, alongside his EEG research, he developed the first ever autonomous mobile robots, referred to as tortoises, which were controlled by analogue electronic nervous systems (Walter 1950a). This was the first explicit use of mobile robots as a tool to study ideas about brain function, a style of research that has become very popular in recent times.

John Westcott FRS (1920-) made many very distinguished contributions to control engineering, including some of the earliest work on control under noisy conditions. He also worked on applications of control theory to economics which resulted in his team developing various models used by the UK Treasury. At the inception of the club he was doing a PhD in the department of Electrical Engineering, Imperial College, London, having just returned from a year in Wiener's lab at MIT. He later became Professor of Control Systems at Imperial College.

Philip M. Woodward (1919-) is a mathematician who made important contributions to information theory, particularly with reference to radar, and to early computing. His gift for clear concise explanations can be seen in his elegant and influential 1953 book on information theory (Woodward 1953). He worked at TRE, Malvern throughout his entire distinguished career (one of the buildings of the present day successor to TRE is named after him). In retirement Woodward has come to be regarded as one of the world's greatest designers and builders of mechanical clocks (Woodward 1995).

Bates' own copy of his typed club membership list of 1st January 1952 has many hand-written corrections and annotations (Bates 1952a). Among these, immediately under the main list of members, are the following letters, arranged in a neat column: 'Mc', 'P', 'S' and then a symbol that may be a 'U' or possibly a 'W'. If we assume it is a 'W', then a possible, admittedly highly speculative, interpretation of these letters is: McCulloch, Pitts, Shannon, Wiener. The first three of these great American cyberneticists attended club meetings – McCulloch appears to have taken part whenever travel to Britain allowed. Wiener was invited and intended to come on at least one occasion but travel difficulties and health problems appear to have got in the way. The 'W', if that's what it is, could also refer to Weaver, co-author with Shannon of seminal information theory papers and someone who was also well known to the club. Of course the letters may not refer to American cyberneticists at all – they may be something more prosaic such as the initials of members who owed subs – but it is just possible that Bates regarded them as honorary members.

It is clear from the membership listed above that the centre of gravity of the club was in the brain sciences. Indeed the initial impetus for starting the club came from a neurologist (Bates) who believed that emerging cybernetic ideas and ways of thinking could be very important tools in developing new insights into the operation of the nervous system. Many members had a strong interest in developing 'brain-like' devices, either as a way of formalizing and exploring theories about biological brains, or as a pioneering effort in creating machine intelligence, or both. Hence meeting tended to centre around issues relating to natural and artificial intelligence and the processes underlying the generation of adaptive behaviour – in short, the mechanisation of mind. Topics from engineering and mathematics were usually framed in terms of their potential to shed light on these issues. This scope is somewhat different to that which had emerged in America, where a group of mathematicians and engineers (Wiener, von Neumann, Bigelow, Shannon, Pitts) and brain scientists (Lorente de No, Rosenblueth, McCulloch) had formed an earlier group similar in spirit to the Ratio Club, although smaller and with a centre of gravity further towards the mathematical end of the spectrum. Their influence soon spread, via Frank, Mead, Bateson and others, into the social sciences, thereby creating a much wider enterprise (Heims 1991). This difference in scope helps to account for the distinct flavour of the British scene in the late 1940s and for its subsequent influences.

3. Genesis of the club

3.1 Founding

The idea of forming a cybernetics dining club took root in John Bates' mind in July 1949. He discussed the idea with a small number of colleagues at a Cambridge symposium on 'Animal Behaviour Mechanisms', a very cybernetics friendly topic, organised by the Society for Experimental Biology and held from the 18th to 22nd of the month. Shortly after returning to London from the meeting, he wrote the following letter to Grey Walter in which the club is formally proposed:

“National Hospital
27^{th} July 1949

Dear Grey,
I have been having a lot of ‘Cybernetic’ discussions during the past few weeks here and in Cambridge during a Symposium on Animal Behaviour Mechanisms, and it is quite clear that there is a need for the creation of an environment in which these subjects can be discussed freely. It seems that the essentials are a closed and limited membership and a post-prandial situation, in fact a dining-club in which conventional scientific criteria are eschewed. I know personally about 15 people who had Wiener’s ideas before Wiener’s book appeared and who are more or less concerned with them in their present work and who I think would come. The idea would be to hire a room where we could start with a simple meal and thence turn in our easy chairs towards a blackboard where someone would open a discussion. We might need a domestic rule to limit the opener to an essentially unprepared dissertation and another to limit the discussion at some point to this stratosphere, but in essence the gathering should evolve in its own way.

Beside yourself, Ashby and Shipton, and Dawson and Merton from here, I suggest the following:-
Mackay – computing machines, Kings. Coll. Strand
Barlow – sensory physiologist – Adrian’s lab.
Hick – Psychological lab. Cambridge
Scholl – statistical neurohistologist – UniversityCollege, Anatomy Lab.
Uttley – ex. Psychologist, radar etc TRE
Gold – ex radar zoologists at Cambridge
Pringle – ex radar zoologists at Cambridge

I could suggest others but this makes 13, I would suggest a few more non neurophysiologists communications or servo folk of the right sort to complete the party but those I know well are a little too senior and serious for the sort of gathering I have in mind.

We might meet say once a quarter and limit the inclusive cost to 5/- less drinks. Have you any reaction? I have approached all the above list save Uttley so far, and they support the general idea.

Yours sincerely,
JAV Bates” (J.Bates 1949a)

The suggested names were mainly friends and associates of Bates, known through various social networks relating to his research, who he regarded as being ‘of the right sort’. One or two were suggested by immediate colleagues, for instance Merton put forward his friend Barlow.

Walter replied by return of post enthusiastically welcoming the idea and suggesting that the first meeting should coincide with his friend Warren McCulloch's visit to England in September. Mackay furnished Bates with an important additional 'communications or servo' contact by introducing him to John Westcott who was finishing off his PhD at Imperial College, having spent the previous year in Wiener's lab at MIT as a guest of the institution. Westcott's close association with Wiener seems to have led Bates to soften his 'had Wiener's ideas before Wiener's book appeared' line in his invitation to him.

> "National Hospital
> 3rd August
>
> Dear Mr. Westcott,
>
> I have heard from Mackay that you might be interested in a dining-club that I am forming to talk 'Cybernetics' occasionally with beer and full bellies. My idea was to have a strictly limited membership between 15 and 20, half primarily physiologists and psychologists though with 'electrical leanings' and half primarily communication theory and electrical folk though with biological interests and all who I know to have been thinking 'Cybernetics' before Weiner's book appeared. I know you have all the right qualifications and we would much like you to join. The idea is to meet somewhere from 7.00pm-10.00pm at a cost of about 5/- less drinks.
>
> The second point is whether we could make McCulloch's visit in September the occasion for a first meeting. This was raised by Mackay who mentioned that you had got in touch with him already with a view to some informal talk. It has also been raised by Grey Walter from Bristol who knows him too. What do you feel? Could we get McCulloch along to an inaugural dinner after his talk for you? Could you anyway manage to get along here for lunch one day soon, we have an excellent canteen and we could talk it over?
>
> Your sincerely
> JAV Bates" (J.Bates 1949b)

Westcott was as enthusiastic as Walter. Bates wrote a succession of individual invitations to those on his list as well as to Little, who was suggested by Mackay, and Turner McLardy, a psychiatrist with a keen interest in cybernetics who was a friend of McCulloch and appears to have been hosting his immanent stay in London. The letter to Hick was typical, including the following exuberant passage: "The idea of a 'Cybernetic' dining club, which I mentioned to you in Cambridge, has caught fire in an atomic manner and we already have half a dozen biologists and engineers, all to my knowledge possessed of Wiener's notions before his book appeared and including two particularly rare birds: Mackay and Westcott who were in Wiener's lab for a year during the war…" (Bates 1949c). Bates didn't quite have his facts straight; Westcott's time with Wiener was after the war and at this stage Mackay hadn't begun his collaborations with MIT, but the implication was right – that Westcott and Mackay were both familiar with the mathematical and technical details of Wiener's oevre. All invitees accepted membership of the club. In their replies a number made general suggestions about membership – Barlow suggested considering the addition of a few more "cautiously selected psychologists" (Barlow 1949), and Pringle thought it would be a good idea to "add a mathematician to keep everyone in check and stop the discussion becoming too vague." (Pringle 1949)

During August Bates secured a room at the National Hospital that could be used for regular meetings. With Eliot Slater, a senior member of staff at the hospital, on-board, he was able to arrange provision of beer and food for club evenings. With a venue, a rough format and an initial membership list, the enterprise was starting to come into focus. The following letter from Mackay to Bates, hand written in a wild scrawl, shows that these two were starting to think about names and even emblems:

> "1st September 49
>
> Dear Bates,
> I'm afraid I've had few fresh ideas on the subject of our proposed club; but here are an odd suggestion or two that arose in my mind.
>
> I wondered (a) if we might adopt a Great Name associated with the subject and call it e.g. the Babbage Club or the Leibniz Club or the Boole Club, or the Maxwell Club – names to be suggested by all, and one selected by vote or c'ttee (Nyquist might be another). Alternatively (b) could we choose a familiar symbol of feedback theory, such as beta, and call it the Beta Club or such like? Other miscellaneous possibilities are the MR Club (machina ratiocinatrix !) and plenty of other initials, or simply the "49" Club.
>
> On emblems I've had no inspirations. I use but little beer myself and it's conceivable we might even have t-t members. But beer mugs can after all be used for other liquids and I can't think of anything better than your suggestion…
>
> Yours,
> Donald Mackay" (Mackay 1949)

Here we see Mackay sowing the seed for the name Ratio, which was adopted after the first meeting. *Machina Ratiocinatrix* is Latin for reasoning machine, a term used by Wiener in the introduction to *Cybernetics* in reference to *calculus ratiocinator*, a calculating machine constructed by Leibniz (Wiener 1948, p.12). Ratiocination is an old-fashioned word for reasoning or thinking, introduced by Thomas Aquinas to distinguish human reasoning from the supposed directly God given knowledge of the angels. After the first meeting Albert Uttley suggested using the root *ratio* giving its definition as 'computation or the faculty of mind which calculates, plans and reasons' (Bates 1949d). He pointed out that it is also the root of *rationarium*, meaning a statistical account – implicitly referring to the emerging work on statistical mechanisms underlying biological and machine intelligence, and *ratiocinatius*, meaning argumentative. Given that the name clearly came from the Latin, it seems reasonable to assume that the intended pronunciation must have been ‘rat-ee-oh’. In interviews with the authors, half the surviving club members said that this indeed is how it was always pronounced, while the other half said it was pronounced as in the ratio of two numbers! As Thomas Gold commented in 2002, “At that time many of us [in the Ratio Club] were caught up in the excitement of our thoughts and ideas and didn’t always notice the details of things like that!” (Gold 2002).

Bates’ notes for his introduction to the inaugural meeting reveal that his suggestion was to call it the Potter Club after Humphrey Potter (Bates 1949e). Legend has it that, as an eleven year old boy in 1713, Potter invented a way of automatically opening and closing the valves on an early Newcomen steam engine. Until that point the valves had to be operated by an attendant such as Potter. He decided to make his life easier by attaching a series of cords and catches such that the action of the main beam of the engine opened and closed the valves.

At the end of August 1949 Bates attended an EEG conference in Paris at which he met McCulloch for the first time. There he secured him as guest speaker for the first meeting of the club. Before describing the meetings, it will be instructive to delve a little deeper into the origins of the club.

3.2 Origins

Of course the roots of the club go back further than the Cambridge symposium of July 1949. The second world war played an important catalytic role in developing some of the attitudes and ideas that were crucial to the success of the Club and to the achievements of its members. This section explores some of these roots, shedding light on the significant British effort in what was to become known as cybernetics, as well as pointing out pre-existing relationships in the group.

The War Effort

Many of the unconventional and multi-disciplinary ideas developed by club members originated in secret war-time research on radar, gunnery control and the first digital computers. In Britain there was little explicit biological research carried out as part of the war effort, so most biologists were, following some training in electronics, drafted into the main thrust of scientific research on communications and radar. They became part of an army of thousands of technical ‘wizards’ who Churchill was to later acknowledge as being vital to the allies’ victory (Churchill 1949). Although most of the future Ratio Club biologists were naturally unconstrained and interdisciplinary thinkers, such war work exposed many of them to more explicitly mechanistic and mathematical ways of conceiving systems than they were used to. To these biologists a radar set could be thought of as a kind of artificial sense organ, and they began to see how the theoretical framework associated with it – which focused on how to best extract information from the signal – might be applied to better understanding natural senses such as vision. On the other side of the coin, several club members were deeply involved in the war-time development of early computers and their use in code cracking. This in turn brought them to ponder the possibility of building artificial brains inspired by real ones. Other engineers and theoreticians, working on such problems as automatic gun aiming alongside their biologist colleagues, began to see the importance of coordinated sensing and acting in intelligent adaptive behaviour, be it in a machine or in an animal. Many years later, in the posthumously published text of his 1986 Gifford Lectures, Donald Mackay reflected on the war-time origins of his research interests:

> "...during the war I had worked on the theory of automated and electronic computing and on the theory of information, all of which are highly relevant to such things as automatic pilots and automatic gun direction. I found myself grappling with problems in the design of artificial sense organs for naval gun-directors and with the principles on which electronic circuits could be used to simulate situations in the external world so as to provide goal-directed guidance for ships, aircraft, missiles and the like. Later in the 1940's, when I was doing my Ph.D. work, there was much talk of the brain as a computer and of the early digital computers that were just making the headlines as "electronic brains." As an analogue computer man I felt strongly convinced that the brain, whatever it was, was not a digital computer. I didn't think it was an analogue computer either in the conventional sense. But this naturally rubbed under my skin the question: well, if it is not either of these, what kind of system is it? Is there any way of following through the kind of analysis that is appropriate to there artificial automata so as to understand better the kind of system the human brain is? That was the beginning of my slippery slope into brain research." (Mackay 1991)

This coalescing of biological, engineering and mathematical frameworks would continue to great affect a few years later in the Ratio Club. As well as Mackay, of future members at least Pringle, Gold, Westcott, Woodward, Shipton, Little, Uttley and Walter were also involved in radar research. Hick and Bates both worked on the related problem of visual tracking in gunnery. Uttley also worked on a range of other problems including the development of automatic control systems, analogue computer controlled servo mechanisms and navigation computers (for this war work he was awarded the Simms Gold medal of the Royal Aeronautical Society). There is not enough space in this paper to describe any of this work in detail, instead a number of sketches are given below offering a flavour of the kinds of developments that were undertaken and the sorts of circumstances many future members found themselves thrust into.

Philip Woodward left Oxford University in 1941 with a degree in mathematics. As an able bodied young man he was whisked straight into the Army where he began basic training. However, he felt he would be much better employed at the military Telecommunications Research Establishment (TRE) nestled in the rolling hills near Malvern. It was here that thousands of scientists of all persuasions were struggling with numerous seemingly impossible radar and communications problems. Within a few days his wish was granted following a letter from his obviously persuasive father to their local MP, Lord Beaverbrook, Minister of Supply. Leaving rifle drill far behind, Woodward joined Henry Booker's theoretical group to be plunged into crucial work on antennae design and radio wave propagation. Within a few days of arriving at TRE he was summoned to see Alec Reeves, a brilliant, highly unconventional engineer and one of the senior staff in Woodward's division. A few years earlier Reeves had invented pulse code modulation, the system on which all modern digital communication is based. He firmly believed he was in direct contact with the spirits of various British scientific geniuses from bygone ages who through him were helping in the war effort. Reeves handed Woodward a file marked 'Top Secret'. Inside were numerous squiggles recorded from a cathode ray tube: his task was to analyse them and decide whether or not they came from Michael Faraday. Over the years Woodward was to face many technical challenges almost as great as this in his work at TRE. (Woodward 2002)

John Westcott was an engineering apprentice in the early years of the war. His job was little more than that of a storeman, fetching and filing orders for materials to be used in the manufacture of various military hardware. Although he didn't have a degree or much formal training, he was enormously frustrated by not being able to contribute more; he was convinced that he had design talents that could really make a difference if only he could use them (Westcott 2002). After much badgering, he finally managed to get himself transferred to TRE where his abilities were indeed soon recognised. He was teamed up with two other brilliant young engineers with whom he was given complete freedom to try and design a new type of radar set to be used by the artillery. If they were successful the device would be extremely important – by using a significantly shorter wavelength than before it would provide a much higher degree of accuracy. The other members of the team were the highly eccentric Francis Farley and, on secondment from the American Signals Corps, Charles Howard Vollum. All three were in their early twenties. At first Farley and Vollum were always at each other's throats with Westcott trying to keep the peace. Vollum became incensed at the unreliability of the oscilloscopes at their disposal and swore that after the war he'd build one that was fit for engineers to use. Despite setbacks and failures they persevered, making use of Vollum's supply of cigars to rope in extra help and procure rare supplies. Somehow they managed to combine their significant individual talents to solve the problem. This great success placed Westcott and Farley on the road to highly distinguished scientific careers while Vollum was as good as his word and after returning to Oregon co-founded the giant electronic instruments company Tektronix and became a billionaire.

Like Woodward and Westcott, Thomas Gold's route into radar research was not direct, although his entry was rather more painful. Born into a wealthy Austrian Jewish family, he was a student at an exclusive Swiss boarding school in the late 1930s when his father decided the political situation was becoming too dangerous to stay in Vienna and moved the family to London. He began an engineering degree at Cambridge University but when war broke out he was rounded up and put into an internment camp as an enemy alien. Sleeping on the same cold concrete floor as Gold was a young mathematician named Herman Bondi. The two struck up an immediate friendship and began discussing the ideas that would later make them both giants of 20th Century astrophysics. Their partnership was initially short-lived because after only a few weeks Gold was transferred to a camp in Canada. His ship survived the brutal Atlantic crossing, although others in the convey did not, being destroyed by U-boats with the loss of many hundreds of lives. Once on Canadian soil the situation did not improve. He found himself in a camp run by a brutally sadistic officer who made life hell for the interns. In order to make things bearable, he claimed he was an experienced carpenter and was put in charge of a construction gang. Ever ingenious, he built a contraption to divert steam from an outlet pipe into a water trough to allow his fellow interns to have a hot bath. He was severely beaten for his trouble. Fortunately, Bondi, who had by now been rescued from another camp by senior scientific staff who knew him at Cambridge, had been spreading word of his friend's brilliance. Gold was pulled out of internment and, like Bondi, he was assigned to work on top secret radar research. But not before he had the great pleasure one morning of joining with all other inmates in wild celebrations on hearing the unexpected news that the camp commander had died of a sudden heart attack in the night (Gold 2002).

Following a year in Princeton working with John von Neumann, Alan Turing was a research fellow at Cambridge University when the British government, fearing war was inevitable, recruited him into a secret codes and ciphers unit in 1938. As is well documented (Hodges 1983), he became an enormously important figure in the successful war-time code cracking work at Bletchley Park, and through this work was deeply involved in the development of the very first digital computers, the theoretical foundations for which he had set out in the late 1930s (Turing 1936). Once war broke out, Jack Good, who had just finished a PhD in mathematics at Cambridge under the great G.H. Hardy, was recruited into the top secret operation at Bletchley Park, where he worked as the main statistician under Turing and Max Newman in a team that included Donald Michie.

Most other Ratio Club members not mentioned above were medically trained and so worked as doctors or in medical research during the war. Most of those were based in the UK, although McLardy, who held the rank of Major, saw active service as a medical officer and was captured and put in a succession of POW camps, at least one of which he escaped from. He worked as a psychiatrist in Stalag 344 at Lamsdorf, Silesia (now Lambinowice, Poland) (BBC History 2005). In early 1945 the Germans started evacuating Lamsdorf ahead of the Russian advance. The POWs were marched west in columns of a thousand, each column under the charge of a medical officer. The conditions endured on these ‘death marches’ were appalling – bitterly cold weather, little or no food, rampant disease (Tattersall 2006). McLardy survived and eventually made it back to Britain.

Apart from plunging them into work that would help to shape their future careers, the war had a strong formative affect on the general attitudes and aspiration of many members. In a way that would just not happen in peace time, many were given huge responsibilities and the freedom to follow their own initiative in solving their assigned problems. (For a while, at barely 30 years of age, Pringle was in charge of all air-born radar development in Britain – for his war service he was awarded a MBE and the American Medal of Freedom with Bronze Palm.)

Craik

From the midst of this war-time interdisciplinary problem solving there emerged a number of publications that were to have a galvanising affect on the development of British cybernetics. These included K.J.W. Craik’s slim volume, *The Nature of Explanation,* which first appeared in 1943 (Craik 1943). Bates’ hastily scrawled notes for his introduction to the first meeting of the Ratio Club, a few lines on one side of a scrap of paper, include a handful of phrases under the heading Membership. Of these only one is underlined. In fact it is underlined three times. The phrase is ‘No Craik’.

Kenneth Craik was a Scottish psychologist of singular genius, who after many years of neglect, is remembered now as a radical philosopher, a pioneer of the study of human machine interfaces, a founder of cognitive psychology and as a father of cybernetics thinking. His story is made particularly poignant by his tragic and sudden demise at the age of 31 on the last day of the war in Europe. He was killed in a traffic accident while cycling through Cambridge. He had recently been appointed the first director of the Medical Research Council's prestigious Applied Psychology Unit. He was held in extremely high regard by Bates and the other Ratio members, so the 'No Craik' was a lament.

After studying Philosophy at Edinburgh University, in 1936 he began a PhD. in psychology and physiology at Cambridge. Here he came under the influence of pioneering head of psychology Sir Frederick Bartlett. His love of mechanical devices, and his skills as a designer of scientific apparatus, no doubt informed the radical thesis of his classic 1943 book, published in the midst of his war work on factors affecting the efficient operation and servicing of artillery machinery. Noting that 'one of the most fundamental properties of thought is its power of predicting events' (Craik, *ibid*, p.50), Craik suggests that such predictive power is 'not unique to minds'. Indeed, although the 'flexibility and versatility' of human thought is unparalleled, he saw no reason why, at least in principle, such essential properties as recognition and memory could not be emulated by a man-made device. He went even further by claiming that the human mind is a kind of machine that constructs small-scale models of reality that it uses to anticipate events. In a move that anticipated Wiener's *Cybernetics* by five years, as well as foreshadowing the much later fields of Cognitive Science and AI, he viewed the proper study of mind as an investigation of classes of mechanisms capable of generating intelligent behaviour both in biological and non-biological machines. Along with Turing, who is acknowledged in the introduction to Wiener's *Cybernetics*, and Ashby, who had begun publishing on formal theories of adaptive behaviour in 1940 (Ashby 1940), Craik was a significant, and largely forgotten, influence on American cybernetics. Both Wiener and McCulloch acknowledged his ideas, quoting him in an approving way, and the later Artificial Intelligence movement, founded by McCarthy and Minsky, was to a degree based on the idea of using digital computers to explore Craik's idea of intelligence involving the construction of small-scale models of reality (see the original proposal for the 1956 Dartmouth Summer Project on AI for an explicit statement of this: McCarthy 1955). Many members of the Ratio Club, a high proportion of whom had connections with Cambridge University, were influenced by Craik and held him in great esteem; in particular Bates and Hick, who had both worked closely with him, Walter, who cited war-time conversations with Craik as the original inspiration for the development of his tortoises (Walter 1953, p.125), and Uttley, who Bates credited with giving Craik many of his ideas (Bates 1945). Indeed, in a 1947 letter to Lord Adrian, the charismatic Nobel prize winning head of physiology at Cambridge, Grey Walter refers to the American Cybernetics movement as "… thinking on very much the same lines as Kenneth Craik did, but with much less sparkle and humour." (Walter 1947) Had he survived, there is no doubt Craik would have been a leading member of the club. In fact John Westcott's notes from the inaugural meeting of the club show that there was a proposal to call it the Craik Club in his honour (Westcott 1949-53).

Existing relationships

Although the Ratio Club was the first regular gathering of this group of like-minded individuals, certain members had interacted with each other for several years prior to its founding, often in work or discussion with a distinct cybernetic flavour. For instance, Bates and Hick had worked with Craik on war-time research related to visual tracking in gunnery and the design of control systems in tanks. In the months after Craik's untimely death, they had been involved in an attempt to edit his notes for a paper eventually published as 'Theory of the Human Operator in Control Systems' (Craik 1948).

Ashby also was familiar with Craik's ideas. In 1944 he wrote to Craik after reading *The Nature of Explanation* (Craik 1943). As intimated earlier, the central thesis of Craik's book is that 'thought models, or parallels, reality' (Craik, *ibid*, p.57). Neural mechanisms, somehow acting as 'small-scale models' of external reality, could be used to "try out various alternatives, conclude which is the best of them, react to future situations before they arise, utilise the knowledge of past event in dealing with the present and future, and in every way to react in a much fuller, safer, and more competent manner to the emergencies that face it."(Craik, *ibid*, p.61). Today this is a familiar idea, but Craik is widely acknowledged as the first thinker to articulate it in detail. Ashby wrote to Craik to suggest that he needed to use terms more precise than 'model' and 'paralleling', putting forward group theory, in particular the concept of isomorphism of groups, as a suitably exact language for discussing his theories (Ashby 1944). He went on to state rather optimistically that "I believe 'isomorphism' is destined to play the same part in psychology that, say, velocity does in physics, in the sense that one can't get anywhere without it." Craik took this suggestion seriously enough to respond with a three page letter on the nature of knowledge and mathematical description which resulted in a further exchange of letters revealing a fair amount of common ground in the two men's views on what kind of knowledge science could communicate. Craik was 'much interested to hear further' (Craik 1944) of Ashby's theories alluded to in the following paragraph in which he introduces himself:

> "Professionally I am a psychiatrist, but am much interested in mathematics, physics and the nervous system. For some years I have been working on the idea expressed so clearly on p.115: "It is possible that a brain consisting of randomly connected impressionable synapses would assume the required degree of orderliness as a result of experience …" After some years' investigation of this idea I eventually established that this is certainly so, provided that by "orderly" we understand "organised as a dynamic system so that the behaviour produced is self-preservative rather than self-destructive". The basic principle is quite simple but the statement in full mathematical rigour, which I have recently achieved, tends unfortunately to obscure this somewhat." (Ashby 1944)

In Ashby's talk of self-preservative dynamic systems we can clearly recognise the core idea he would continue to develop over the next few years and publish in *Design for a Brain* (Ashby 1952a). In that book he constructed a general theory of adaptive systems as dynamical systems in which 'essential' variables (such as heart rate and body temperature in animals) must be kept within certain bounds in the face of external and internal changes or disturbances. This work, which preoccupied Ashby for the early years of the Ratio Club, is discussed later in more detail in Section 5.

Ashby corresponded with several future members of the club in the mid 1940s. For instance, in 1946 William Hick wrote to Ashby after reading his note on equilibrium systems in the American Journal of Psychology (Ashby 1946). Hick explained that he too was "… trying to develop the principles of 'Analytical Machines' as applied to the nervous system…" (Hick 1947a) and requested copies of all Ashby's papers. The pair corresponded over the mathematical details of Ashby's theories of adaptation with Hick declaring himself "… not entirely happy with your conclusion that a sequence of breaks, if it continues long enough, will eventually, by chance, lead to a stable equilibrium configuration." (Hick 1947b). Hick was referring to an early description of what would later appear in *Design for a Brain* as postulated step mechanisms that would, following a disturbance that pushed any of the system's essential variables out of range, change the internal dynamics of an adaptive machine until a new equilibrium was established (i.e. all essential variables were back in range – see Section 5 for further details). Ashby agrees, explaining that he had no rigorous proof but "… has little doubt of its truth in a rough and ready , practical way." (Ashby 1947). A year later the Homeostat machine would provide an existence proof that these mechanisms could work. But Hick had homed in on an interesting and contentious aspect of Ashby's theory. By the time *Design for a Brain* was published, Ashby talked about step mechanisms in very general terms, stating that they could be random but did not ascribing absolute rigid properties to them, hence leaving the door open for further refinements. This correspondence foreshadows the kind of probing discussions that were to form the central activity of the Ratio Club, debates that sometimes spilled out onto the pages of journals (see Section 5 for an example of this).

During the war there had been considerable interaction between researchers at the various military sites and several members had originally met through that route. For instance, one of the things Uttley worked on at TRE was computer-aided target tracking, as well as building the first British airborne electronic navigation computer (Uttley 1982). This work on early computing devices brought him into contact with both Gold and Turing.

Several members had been friends or acquaintances at Cambridge (e.g. Pringle and Turing were contemporaries, as were Barlow and Merton who had both been tutored by Rushton). Others met at workshops and conferences in the years leading up to the founding of the club. Those involved in EEG work (Walter, Bates, Dawson, Shipton) were all well known to each other professionally (Walter and Dawson had together laid the foundations for clinical uses of EEG; a paper they wrote together in 1944 (Dawson and Walter 1944) was still used in the training of EEG practitioners in the 1980s). Ashby had interacted with Walter for some time, not least because their research institutes were nearby. So, by the time the Ratio Club started, most members had at least passing familiarity with some, but by no means all, of the other's ideas.

3.3 The way forward

For two or three years prior to the founding of the club there had been a gradual increase in activity, on both sides of the Atlantic, on new approaches to machine intelligence, as well as renewed interest in associated mechanistic views of natural intelligence. In Britain much of that activity involved future Ratio members. The phrase Bates used in his initial letters of invitation to the founders of the club, that he wished to bring together people who '… had Wiener's ideas before Wiener's book appeared …', may have been slightly gung-ho, but in a draft for an article for the British Medical Journal in 1952, Bates explained himself a little more:

> "Those who have been influenced by these ideas so far, would not acknowledge any particular indebtedness to Wiener, for although he was the first to collect them together under one cover, they had been common knowledge to many workers in biology who had contacts with various types of engineering during the war." (Bates 1952b)

It is likely that Bates was mainly thinking of chapters III, IV and V of *Cybernetics,* those on 'time series, information and communication', 'feedback and oscillation' and 'computing machines and the nervous system'. Certainly many biologist had become familiar with feedback and its mathematical treatment during the war, and some had worked on time series analysis and communication in relation to radar (some of their more mathematical colleagues would have been using some of Wiener's techniques and methods that were circulating in technical reports and draft papers – quite literally having Wiener's ideas before the book appeared). Most felt that the independent British line of research on computing machines and their relationship to the nervous system was at least as strong as the work going on in the USA (important strands of which were based on prior British work such as that of Turing) (Barlow 2001). Indeed, many were of the opinion that the central hypothesis of cybernetics was that the nervous system should be viewed as a self-correcting device chiefly relying on negative-feedback mechanisms (Wisdom 1951). This concept had first been introduced by Ashby in 1940 (Ashby 1940) and then independently by Wiener and colleagues three years later (Rosenblueth et al. 1943). The development of this idea was the central, all-consuming focus of Ashby's work until the completion of *Design for a Brain*, which set out his theories up to that point. It is interesting that Ashby's review of *Cybernetics* (Ashby 1949b) is quite critical of the way in which the core ideas of the book are presented.

Perhaps the following passage from the introduction to *Cybernetics* pricked Bates' sense of national pride and acted as a further spur:

> "In the spring of 1947 … [I] spent a total of three weeks in England, chiefly as a guest of my old friend J.B.S. Haldane. I had an excellent chance to meet most of those doing work on ultra-rapid computing machines … and above all to talk over the fundamental ideas of cybernetics with Mr. Turing … I found the interest in cybernetics about as great and well informed in England as in the United States, and the engineering work excellent, though of course limited by the smaller funds available. I found much interest and understanding of its possibility in many quarters … I did not find, however, that as much progress had been made in unifying the subject and in pulling the various threads of research together as we had made at home in the States." (Wiener 1948, p.23)

However, whatever the views on Wiener's influence – and the more mathematical members will surely have recognised his significant technical contributions – it is clear that all those associated with the Ratio Club agreed that Shannon's newly published formulation of information theory, partly built on foundations laid by

Wiener, was very exciting and important. The time was ripe for a regular gathering to develop these ideas further.

4. Club meetings

The London district of Bloomsbury often conjures up images of free thinking intellectuals, dissolute artists and neurotic writers – early twentieth century bohemians who, as Dorothy Parker once said, 'lived in squares and loved in triangles'. But it is also the birth place of neurology, for it was here, in 1860, that the first hospital in the world dedicated to the study and treatment of diseases of the nervous system was established. By the late 1940s The National Hospital for Nervous Diseases was globally influential and had expanded to take up most of one side of Queen Square. It was about to become regular host to the newly formed group of brilliant and unconventional thinkers.

In 1949 London witnessed the hottest September on record up to that point, with temperatures well above 90F. In fact the entire summer had been a mixture of scorching sunshine and wild thunderstorms. So it was an unseasonably balmy evening on the 14th of that month when a gang of scientists, from Cambridge in the east and Bristol in the west, descended on the grimy bombed-out capital, a city slowly recovering from a war that had financially crippled Britain. They converged on the leafy Queen Square and assembled in a basement room of the hospital at 6.30 in the evening. After sherries, the meeting started at 7:00. Bates' notes for his introduction to this inaugural gathering of the club show that he spoke about how the club membership was drawn from a network based around his friends, and so was somewhat arbitrary, but that there had been an attempt to strike a balance between biologists and non-biologists (Bates 1949e). He then went on to make it clear that the club was for people who were actively using cybernetic ideas in their work. At that point there were 17 members but he felt there was room for a few more. (The initial membership comprised: Ashby, Barlow, Bates, Dawson, Gold, Hick, Little, Mackay, McLardy, Merton, Pringle, Shipton, Sholl, Slater, Uttley, Walter, Westcott). He pointed out that there were no sociologists, no northerners (for example from Manchester University) and no professors. Possible names for the club were discussed (see Section 3.1) before Bates sketched out how he thought meetings should be conducted. In this matter he stressed the informality of the club – that members should not try and impose 'direction' or employ 'personal weight'. All agreed with this sentiment and endorsed his 'no professors' rule – scientists who were regarded to be senior enough to inhibit free discussion were not eligible for membership.

Warren McCulloch then gave his presentation, *Finality and Form in Nervous Activity*, a popular talk that he had first given in 1946 – perhaps not the best choice for such a demanding audience. Correspondence between members reveals almost unanimous disappointment in the talk. Bates set out his own reaction to its content (and style) in a letter to Grey Walter:

> "Dear Grey,

> Many thanks for your letter. I had led myself to expect too much of McCulloch and I was a little disappointed; partly for the reason that I find all Americans less clever than they appear to think themselves; partly because I discovered by hearing him talk on 6 occasions and by drinking with him in private on several more, that he had chunks of his purple stuff stored parrot-wise. By and large however, I found him good value." (Bates 1949f)

Walter replied to Bates apologizing for not being present at the meeting (he was the only founding member unable to attend). This was due to the birth of a son, or as he put it "owing to the delivery of a male homeostat which I was anxious to get into commission as soon as possible." (Walter 1949) He went on to tell Bates that he has had 'an amusing time' with McCulloch who had travelled on to Bristol to visit him at the Burden Institute. In reference to Bates' view on McCulloch's talk, he comments "… his reasoning has reached a plateau … flowers that bloom on this alp are worth gathering but one should keep one's eyes on the heights."

A buffet dinner with beer followed the talk and then there was an extended discussion session. The whole meeting lasted about three hours. Before the gathering broke up, with some rushing off to catch last trains out of London and others joining McCulloch in search of a night cap, John Pringle proposed an additional member. Echoing the suggestion made in his written reply to Bates' original invitation to join the club, Pringle put forward the idea that a mathematician or two should be invited to join to give a different perspective and to 'keep the biologists in order'. He and Gold proposed Alan Turing, a suggestion that was unanimously supported. Turing gladly accepted and shortly afterwards was joined by fellow mathematician Philip Woodward, who worked with Uttley. At the same time leading Cambridge neurobiologist William Rushton, who was well know to many members, was added to the list. The following passage from a circular Bates sent to all members shortly after the first meeting shows that the format for the next few sessions had also been discussed and agreed:

> "It seems to be accepted that the next few meetings shall be given over to a few personal introductory comments from each member in turn. Assuming we can allow two and a half hours per meeting, eighteen members can occupy an average of not more than 25 minutes each. The contributions should thus clearly be in the nature of an aperitif or an hors d'oeuvres – the fish, meat and sweet to follow at later meetings." (Bates 1949g)

Regardless of reactions to the opening talk, there was great enthusiasm for the venture. The club was well and truly born.

Following the inaugural meeting in September 1949, the club convened regularly until the end of 1954. There was a further two day meeting and a single evening session in 1955 and a final gathering in 1958 after the now classic *Mechanization of Thought Processes* symposium organized by Uttley at the National Physical Laboratory in Teddington (Blake and Uttley 1959).

Table 1 shows the full list of known Ratio Club meetings. This has been compiled from a combination of individual meeting notices found in the Bates archive, surviving members' personal records and a list of meetings made by Bates in the mid

1980s. There are inconsistencies between these sources, but through cross-referencing with notes made at meetings and correspondence between members this list is believed to be accurate. It is possible that it is incomplete – if so, only a very small number of additional meetings could have occurred.

Figure 1: The main entrance to the National Hospital, Queen Square in 2002. Ratio Club meetings were held in a room in the basement.

Meeting	Date	Speakers, title
1	14 Sep. 1949	Warren McCulloch, *Finality and Form in Nervous Activity*
2	18 Oct. 1949	Introductory talks from Sholl, Dawson, Mackay, Uttley.
3	17 Nov. 1949	Introductory talks from Gold, Bates, McLardy
4	15 Dec. 1949	Introductory talks from Pringle, Merton, Little, Hick; Grey Walter
5	19 Jan. 1950	E. Slater, *Paradoxes are Hogwash*; D. Mackay, *Why is the Visual World Stable?*
6	16 Feb. 1950	Introductory talks from Shipton, Slater, Woodward
7	16 Mar. 1950	Introductory talks from Ashby, Barlow
8	21 Apr. 1950	Introductory talks from Wescott, Turing
9	18 May 1950	*Pattern Recognition*, Walter, Uttley, Mackay, Barlow, Gold
10	22 June 1950	*Elementary basis of Information Theory*, Woodward
11	18 July 1950	*Concept of Probability*, Gold, Mackay, Sholl
12	21 Sep. 1950	*Noise in the Nervous System*, Pringle
13	2 Oct. 1950	Meeting at London Symposium on Information Theory
14	7 Dec. 1950	*Educating a Digital Computer*, Turing
15	22 Feb. 1951	*Adaptive Behaviour*, Walter
16	5 Apr. 1951	*Shape and Size of Nerve Fibres*, Rushton
17	31 May 1951	*Statistical Machinery*, Ashby
18	26 July 1951	*Telepathy*, Bates
19	1 Nov. 1951	*On Popper: What is happening to the universe?*, Gold
20	21 Dec. 1951	Future Policy; discussion on *The possibility of a scientific basis of ethics* opened by Slater; discussion on *A quantitative approach to brain cell counts* opened by Sholl.
21	8 Feb. 1952	*The Chemical Origin of Biological Form*, Turing; *The Theory of Observation*, Woodward
22	20 Mar. 1952	*Pattern Recognition*, Uttley; *Meaning in Information. Theory*, Mackay
23	2-3 May 1952	Special Meeting at Cambridge (organised by Pringle).
24	19 June 1952	*Memory*, Bates; *The Logic of Discrimination*, Westcott
25	31 July 1952	*The Size of Eyes*, Barlow; *American Interests in Brain Structure*, Sholl
26	24-25 Oct. 1952	Special Bristol Meeting at Burden Inst. (cancelled)
27	6 Nov. 1952	*Design of Randomizing Devices*, Hick; *On Ashby's Design for a Brain*, Walter
28	11 Dec. 1952	*Perils of Self-awareness in Machines*, Mackay; *Sorting Afferent from Efferent Messages in Nerves*, Merton
29	19 Feb. 1953	*Pattern Discrimination in the Visual Cortex*, Uttley and Sholl
30	7 May 1953	*Absorption of Radio Frequencies by Ionic Materials*, Little; *The Signal to Noise Problem*, Dawson
31	2 July 1953	Warren McCulloch: Discussion of topics raised in longer lectures given by McCulloch at UCL in previous week
32	22 Oct. 1953	*Demonstration and Discussion of the Toposcope*, Shipton; *Principles of Rational Judgement*, Good
33	11 Feb. 1954	Discussion on *How does the nervous system carry information?*; Guest talk: *Observations on Hearing Mechanisms*, Whitfield and Allanson
34	17 June 1954	*Servo control of Muscular Movements*, Merton; *Introduction to Group Theory*, Woodward,
35	25 Nov. 1954	*Negative Information*, Slater and Woodward; Guest talk: *Development as a Cybernetic Process*, Waddington
36	6-7 May 1955	Special meeting West Country (TRE, Barnwood, Burden)
37	15th Sep. 1955	Discussion meeting after 3rd London Symposium on Information Theory. Many guests from USA.
38	27 Nov. 1958	Final reunion meeting after NPL *Mechanisation of Thought Processes* Symposium

Table 1: Known Ratio Club Meetings

The order of member's introductory talks was assigned by Bates using a table of random numbers. Due to overruns and some people being unable to attend certain meetings, the actual order in which they were given may have been slightly different from that shown in the table. However, they did occupy the dates indicated.

The format of the opening meeting – drinks, session, buffet and beer, session, coffee – seems to have been adopted for subsequent meetings. Member's introductory talks, which highlighted their expertise and interests, typically focused on some aspect of their current research. John Westcott's notebook reveals that a wide range of topics were discussed (Westcott 1949-53): Scholl talked about the need to construct an appropriate mathematics to shed light on the physiology of the nervous systems; Dawson described on-going work on eliminating noise from EEG readings; Mackay argued for a more complex description of information, both philosophically and mathematically, claiming that it cannot be adequately defined as a single number; Uttley sketched out the design for a digital computer he was working on at TRE; Gold illustrated his more general interest in the role of servomechanism in physiology by describing his work on a radical new theory of the functioning of the ear, which postulated a central role for feedback (Gold (2002) later remembered that at the time the Ratio Club was the only group that understood his theory); Bates talked about various levels of description of the nervous system; McLardy described recent research in invasive surgical procedures in psychiatry; Merton outlined his work on using cybernetic ideas to gain a better understanding of how muscles work; Walter described his newly constructed robotic tortoises, sketching out the aims of the research and early results obtained (see Section 5 for further discussion of this work); Woodward talked about information in noisy environments; Little discussed the scientific method and the difficulty of recognizing a perfect theory; Hick outlined his research on reaction times in the face of multiple choices – the foundations of what would later become known as Hick's law (Hick 1952) which makes use of information theory to describe the time taken to make a decision as a function of the number of alternatives available (see Section 2 for a brief statement of the law); Ashby talked about his theories of adaptive behaviour and how they were illustrated by his just-finished Homeostat device (see Section 5 for further discussion of this work); Barlow outlined the research on the role of eye movement in generating visual responses that he was conducting at this early stage of his career (see the interview with Barlow later in this volume for further details of this work), and Westcott talked a little about his background in radar and his work with Wiener at MIT before outlining his mathematical work on analysing servomechanisms, emphasising the importance of Wiener's theory of feedback systems on which he was building. After each of the presentations discussion from the floor took over.

After the series of introductory talks, the format of meetings changed to focus on a single topic, sometimes introduced by one person, sometimes by several. Prior to this, Ashby circulated two lists of suggested topics for discussion; an initial one on 18th February 1950 (Ashby 1950a) and a refined version dated 15th May 1950 (Ashby 1950b). They make fascinating reading; giving an insight into Ashby's preoccupations at the time. The refined list is reproduced here. Many of the questions are still highly pertinent today.

1. What is known of 'machines' that are defined only statistically? To what extent is this knowledge applicable to the brain?
2. What evidence is there that 'noise' (a) does, (b) does not, play a part in brain function?
3. To what extent can the abnormalities of brains and machines be reduced to common terms?
4. The brain shows some indifference to the exact localisation of some of its processes: to what extent can this indifference be paralleled in physical systems? Can any general principle be deduced from them, suitable for application to the brain?
5. From what is known about present-day mechanical memories can any principle be deduced to which the brain must be subject?
6. To what extent do the sense-organs' known properties illustrate the principles of information-theory?
7. Consider the various well known optical illusions: what can information-theory deduce from them?
8. What are the general effects, in machines and brains of delay in the transmission of information?
9. Can the members agree on definitions, applicable equally to all systems - biological, physiological, physical, sociological - cf: feedback, stability, servo-mechanism.
10. The physiologist observing the brain and the physicist observing an atomic system are each observing a system only partly accessible to observation: to what extent can they use common principles?
11. The two observers of 10, above, are also alike in that each can observe his system only by interfering with it: to what extent can they use common principles?
12. Is 'mind' a physical 'unobservable'? If so, what corollaries may be drawn?
13. What are the applications, to cerebral processes, of the thermodynamics of open systems?
14. To what extent can the phenomena of life be imitated by present-day machines?
15. To what extent have mechanisms been successful in imitating the conditioned reflex? What features of the C.R. have conspicuously not yet been imitated?
16. What principles must govern the design of a machine which, like the brain, has to work out its own formulae for prediction?
17. What cerebral processes are recognisably (a) analogical, (b) digital, in nature?
18. What conditions are necessary and sufficient that a machine built of many integrated parts should be able, like the brain, to perform an action either quickly or slowly without becoming uncoordinated?
19. Steady states in economic systems.
20. What general methods are available for making systems stable, and what are their applications to physiology?
21. To what extent can information-theory be applied to communication in insect and similar communities?
22. To what extent are the principles of discontinuous servo-mechanisms applicable to the brain?
23. What re-organisation of the Civil Service would improve it cybernetically?
24. What economic 'vicious circles' can be explained cybernetically?
25. What re-organisation of the present economic system would improve it cybernetically?
26. To what extent can information-theory be applied to the control exerted genetically by one generation over the next?

27. Can the members agree on a conclusion about extra-sensory perception?
28. What would be the properties of a machine whose 'time -was not a real but a complex variable? Has such a system any application to certain obscure, i.e. spiritualistic, properties of the brain?

The last topic on the initial list is missing from the more detailed second list: ‘If all else fails: The effect of alcohol on control and communication, with practical work.’ This suggestion was certainly taken up, as it appears were several others: shortly after the lists appeared Pringle gave a talk on the topic of suggestion 2 (meeting 12), as did Walter on 14 and 15 (meeting 15). Topic 27 came up in talks by Bates and Good (meetings 18 and 32). Issues relating to many of the other suggestions often arose in group discussions, being in areas of great interest to many members (e.g. topics 6-13, 16-18 and 26). In particular, Barlow recalls much discussion of topic 17 (Barlow 2007).While Ashby’s publications and notebooks make it clear that some of the suggestions are based on the central research questions he was grappling with at the time (e.g. 1, 18, 20, 22), it is very likely that some of the others arose from issues brought up by other members in their introductory talks. In the mid 1980s Bates made some notes for a planned article on the Ratio Club (Bates 1985), a plan which unfortunately did not come to fruition. However, among these scant jottings is mention of Ashby’s lists, which further suggests that they did play a role in shaping the scope of topics discussed.

Members often volunteered to give talks, but when he felt it was necessary Bates actively controlled the balance of topics by persuading particular members to give presentations. Sometimes there were requests from members for particular subjects to be discussed or particular people to give talks on certain topics. Looking through the list of subjects discussed, many are still extremely interesting today; at the time they must have been positively mouth watering.

At the end of 1950, after meeting him at the first London Symposium on Information Theory, Bates invited I.J. ‘Jack’ Good along to the next meeting as his guest. The speaker was Turing, Good’s friend and war-time colleague. This was a particularly lively meeting and after it Good wrote to Bates expressing how much he had enjoyed the evening and apologizing for being too vociferous. He wondered “would there be any serious objection to my becoming a member?” (Good 1950a). Bates replied that “the club has been going for a year, and is entirely without any formal procedures. New members join by invitation, but I think personally you would be a great asset, and hope you will be able to come as my guest to some future meetings, so that perhaps my view will become consensus!”(Bates 1950a). Bates’ view obviously did hold sway as Good became the twenty-first member of the Club. Perhaps it was thought a third mathematician was needed to help the other two keep the biologists in order. Partly because of the size of the room used for meetings, and partly because Bates had firm ideas on the kind of atmosphere he wanted to create and who were the ‘right sorts’ to maintain it, the membership list remained closed from that point.

For the first year meetings were monthly and were all held at the National Hospital in Queen Square. From mid 1950 until the end of 1951 the frequency of meetings dropped slightly and in the second half of 1951 attendance started to fall. This was mainly due to the not inconsiderable time and expense incurred by members who were based outside London every time they came to a meeting. In October 1951

Woodward had written to Bates explaining that he had to take part of his annual leave to attend meetings (Woodward 1951); the following month Walter wrote to explain that he had difficulty in covering the expenses of the trips to London necessary for Ratio gatherings. He suggested holding some meetings outside London in member's labs, pointing out that this would also allow practical demonstrations as background for discussion (Walter 1951).

Indeed the return journey to Bristol could be quite a hike. Janet Shipton remembers waiting up to greet her husband, Harold, on his return from Ratio meetings, "He would get back in the dead of night, the smell of train smoke on his clothes." (Shipton 2002)

At the December 1951 meeting of the club Bates called a special session to discuss future policy. Beforehand he circulated a document in which he put down his thoughts on the state of the club. Headed 'The Ratio Club', the document opened by stating that "looked at in one way, the Club is thriving - in another way it is not. It is thriving as judged by the suggestions for future activities." (Bates 1951). These suggestions are listed as requests for specific talks by Woodward (on the theory of observation) and Hick (on the rate of gain of information), an offer of a talk on morphogenesis by Turing, as well as various suggestions for discussion topics (all of these suggestions, offers and requests were taken up in subsequent meetings). Bates goes on: "In addition to this, we have in pigeon-holes a long list sent in by Ashby of suitable topics; various suggestions for outside speakers; and a further suggestion that members should collaborate in writing different chapters to a book on the lines of 'Cybernetics', but somewhat tidier." Sadly, this intriguing book idea never came to fruition. He then explains the cause for concern:

> "Looked at in another way, the Club is ailing. For the past three meetings, half or more of the members have been absent. This half have been mostly those who live out of London - the most reasonable inference clearly is that a single evening's meeting does not promise to be a sufficient reward for the inconvenience and expense of getting to it. In addition one member has pointed out that if expenses cannot be claimed the night's absence is counted against the period of his annual leave! The whole point of the Club is to facilitate contacts between people who may have something to contribute to each other, and who might not otherwise come together, and it would seem that some change in its habits may be indicated." (Bates 1951)

Bates then list some suggested courses of action for discussion at the next meeting. These ranged from having far fewer, but longer, meetings to doubling the membership.

It was decided that there would be six or seven meeting a year, four or five in London and two elsewhere. The meetings would start earlier to allow two papers. A novel suggestion by Philip Woodward was also taken up: to start a postal portfolio – a circulating package of ideas – 'to be totally informal and colloquial'. Bates prepared a randomized order of members to dictate the route the portfolio would travel.

This new regime was followed from the first meeting of 1952 until the club disbanded, and seemed to go a good way towards solving the problems that prompted its instigation. The typical meeting pattern was now to gather at 4:30 for tea followed by the first talk and discussion, then a meal and drinks followed by the second talk and discussion.

Most Ratio talks were based on current research and were often early outings for highly significant work, sometimes opening up new areas of enquiry that are still active today. For instance, Turing's talk on *Educating a Digital Computer*, in December 1950, was on the topics covered by his seminal *Mind* paper of that year (Turing 1950) which introduced The Turing Test and is regarded as one of the key foundational works of machine intelligence. As the title suggests, that talk focused on how an intelligent machine might be developed – Turing advocated using adaptive machines that might learn over their lifetime and also over generations by employing a form of artificial evolution. This meeting is remembered as being particularly good with Turing on top form stimulating a scintillating extended discussion (Bates 1950b). Turing's 1952 talk on biological form was another gem, describing his as yet unpublished work on reaction-diffusion models of morphogenesis (Turing 1952) which showed how pattern and form could emerge from reaction-diffusion systems if they are appropriately parameterized (a role he hypothesized might be taken on by genes). Apart from launching new directions in theoretical biology, this work was pioneering in its use of computer modeling and was to prove extremely influential. There is not enough space to describe all the important work discussed at club meetings but further summaries will be scattered at appropriate places throughout the rest of this paper.

As well as research talks, there were also various 'educational' presentations, usually requested by the biologists. For instance, Woodward gave several on information theory, which gave the biologists very early access to important new ways of thinking. By all accounts Woodward was an extremely good lecturer, blessed with a gift for insightful exposition (this is evident in his 1953 book *Probability and Information Theory, with Applications to Radar* – Woodward 1953, still regarded by some theorists as one of the most profound works in the area since Shannon's original papers.) Barlow was particularly influenced by these exciting new ideas and became a pioneer of the use of information theory as a theoretical framework to understand the operation of neural systems, particularly those associated with vision. This theoretical framework either directly or indirectly underpinned many of Barlow's very important contributions to neuroscience. He regards the Ratio Club as one of the most important formative influences on his work and sees "much of what I have done since as flowing from those evening meetings" (Barlow 2001) (see the interview with Barlow later in this volume for further discussion of this point). In a similar spirit there were lectures on probability theory from Gold and Mackay and on the emerging field of control theory from Westcott.

In 1952 two extended out-of-London meetings were planned, one in Cambridge in May and one in Bristol in October. The Cambridge meeting was organized by Pringle and was held from Friday afternoon to Saturday morning in his college, Peterhouse. After drinks and dinner Pringle led a session on *Processes Involved in the Origin of Life.* Correspondence after the meeting mentions that this session was captured on a tape recorder, although the recording has not yet come to light. The next day visits were arranged to various labs including: Cavendish (physics), led by Gold; Zoology; Physiology, led by Rushton; Psychology, led by Hick; and Mathematics. The photograph shown in Figure 2 was taken at this meeting, quite possibly after the pre-dinner sherries mentioned on the invitation sent out to Club members. Not everyone was able to attend and several of those in the photograph are guests. A limited number of guests were allowed at most meetings and over the years various distinguished visitors took part in club gatherings. As well as McCulloch, Pitts and Shannon, these included J.Z. Young, the leading anatomist and neurologist, who attended several meetings, C.H. Waddington, the pioneering theoretical biologist and geneticist, and Giles Brindley who became a distinguished neuroscientist and was David Marr's PhD supervisor. Jack Good once brought along the director of the NSA, home to the USA's code breakers and makers, who he knew through his work for British Intelligence. That particular meeting was on probability and included prolonged discussions of experiments claiming to give evidence for ESP. Following the 1955 London Symposium on Information Theory, a special club meeting involved a host of leading lights from the world of information theory and cybernetics, many from overseas. These included: Peter Elias, JCR Licklider, Warren McCulloch, Oliver Selfridge, Benoît Mandelbrot and Colin Cherry. Records are sketchy on this matter, but it is likely that many other luminaries of the day took part in other meetings.

Figure 2: Some members of The Ratio Club with guests outside Peterhouse, University of Cambridge, May 1952. The photograph was organized by Donald Mackay. *Back row (partly obscured)*: H. Shipton, J. Bates, W. Hicks, J. Pringle, D. Scholl, J. Westcott, D. Mackay. *Middle row*: G. Brindley, T. McLardy. W.R. Ashby, T. Gold, A. Uttley. *Front row*: A. Turing, G. Sutton, W. Rushton, G. Dawson, H. Barlow. Courtesy Wellcome Library for the History and Understanding of Medicine.

The Bristol meeting was to be held at the Burden Neurological Institute starting at noon on Friday 24th October 1952 and running into the next day but it seems to have been cancelled at the last minute dues to heavy teaching commitments preventing a substantial number of members from attending. The talks and demonstrations planned for this meeting were moved into later club meetings. These included Grey Walter opening a discussion on *Mechanisms for Adaptive Behaviour* which focused on simulation of learning by man-made devices, and in particular on the issues raised in Ashby's recently published book *Design for a Brain*, and a presentation by Shipton on the Toposcope, the world's first multi-channel EEG recording device. The machine, developed by Shipton and Walter, was capable of building and displaying bidimensional maps of the EEG activity over the brain surface and included frequency and phase information.

From mid 1953 meeting became less frequent, with only three in 1954 and two in 1955. In 1955 the extended West-Country event finally happened, starting at TRE Malvern on May 6th and then going the next day to the Burden Institute in Bristol via Ashby's Barnwood House lab. The meeting was primarily devoted to demonstrations and discussions of work in progress at these locations. At TRE various devices from Uttley's group were on show. These included a 'tracking simulator', a novel apparatus designed to provide a versatile means of setting up and studying problems relating to a human operator working in a closed loop system. The device used a two gun cathode ray tube and required the operator to track a moving dot by controlling a second dot with a joystick. Also on show were Uttley's systems for automatically classifying spatial and temporal patterns and pioneering electronic and hydraulic systems capable of inference using principles from conditional probability.

To reach the next leg of the multi-site meeting, Philip Woodward recalls travelling across country in a Rolls Royce Barlow had borrowed from his brother. As they hurtled towards 'Ashby's lunatic asylum', Rushton diagnosed the exact form of Woodward's colour blindness by getting him to describe the spring flowers he could see on the verges (Woodward 2002).

At Barnwood House, Ashby demonstrated his Dispersive and Multi-stable System (DAMS), and the Homeostat was available for those who were not already familiar with it. As mentioned earlier, the Homeostat demonstrated the theories of adaptation developed in *Design for a Brain*, where it is described in some detail. Although Ashby had talked at earlier club meetings about the DAMS machine, this would have been the first time that most members had seen it first hand. The DAMS device, which is much less well know than the Homeostat – mainly because Ashby was not able to develop it sufficiently to fully demonstrate his theories – was intended to explore possible learning behaviours of randomly connected non-linear components. The motivation for this was the intriguing possibility that parts of the brain, particularly the cortex, might be (at least partially) randomly wired. Ashby had been developing the machine for some years and demonstrated the current version which by then illustrated some interesting properties of 'statistical machinery'. The theoretical line started in this work resurfaced many years later in Gardner and Ashby's computational study of the stability of large interconnected systems (Gardner and Ashby 1970). There is a nice anecdote about the machine which originates from this 1955 meeting. Philip Woodward remembers being told, possibly apocryphally, that when Ashby asked a local engineering firm to construct part of the device, specifying random connections, they were so bemused, particularly since the order was coming from Barnwood House Psychiatric Hospital, that they rang up to check that Dr. Ashby was not in fact a patient (Woodward 2002).

The club was full of lively and strong personalities. Mackay, Turing and Walter were, in their very different ways, brilliant speakers who each made scientific broadcasts for BBC radio. Grey Walter, in particular, was something of a media personality with appearances on popular radio quiz shows and early television programmes. He was a larger than life character who liked to cultivate a certain image – that of a swashbuckling man of the world. He was, as Harold Shipton noted in 2002, ‘a bugger for the women’ (Shipton 2002). This reputation did him no favours with many in the scientific establishment. Walter stood in marked contrast to Mackay, a fiery lay preacher who had been brought up attending the Evangelical Free Church of Scotland (one of the radical breakaway ‘wee free’ churches). Many who knew him have remarked on a certain tension between his often radical scientific ideas about the nature of intelligence and his strait-laced religiosity. Horace Barlow, a great friend of Mackay and an admirer of his ideas, has noted that “his conviction that he had a special direct line to a Higher Place, somehow slightly marred his work and prevented him from becoming as well regarded as he should have been.” (Barlow 2002) According to Barlow’s biographical memoir of Rushton (Barlow 1986), his former tutor “cut a striking and influential figure .. was argumentative, and often an enormously successful showman .. he valued the human intellect and its skilful use above everything else.” Giles Brindley, a guest at several meetings, remembers that Barlow and Gold were very active in discussions and that when occasionally a debate got out of hand, Pringle would gently refocus the conversation (Brindley 2002).

Members came from a rich mix of social and educational backgrounds ranging from privileged upbringings to the humblest of origins. Harold Shipton’s story is particularly remarkable. In the years before World War II he was plucked from the life of an impoverished farm labourer by RAF talent scouts who were looking for bright young men to train as radar operators. During training it quickly became apparent that he had a natural gift for electronics which was duly exploited. After the war, before he had been demobbed, he was sent to the Burden Institute to find out what Grey Walter was doing with the suspiciously large amounts of surplus military electronic equipment he was buying. He and Walter immediately hit it off and he stayed. At the institute he met his future wife – Clement Attlee’s daughter Janet. (Attlee was leader of the Labour party, Churchill’s deputy during the war and Prime Minister of Britain from 1945-51.) Hence, at the west-country meeting, members of the all-male Ratio Club were served tea by the Labour Prime Minister’s daughter.

Bates had created a powerful mix of individuals and ideas with just the right degree of volatility. The result was that meetings were extremely stimulating and greatly enjoyed by all. All the surviving members interviewed recalled the club with great enthusiasm (Gold (2002) described meetings as “always interesting, often exciting.”). Even those, such as Woodward and Westcott, who felt that they were net givers, in terms of the direct intellectual influence of the club on members’ work, found meetings a pleasure and were annoyed when they had to miss one.

The social atmosphere of the club was sometimes carried on in after-meeting parties. Philip Woodward remembers that on one occasion some of the group reconvened on the enormous Dutch sailing barge Pat Merton kept in St. Catherine's docks on the Thames. Merton had arranged for a pianist and oboeist to play on deck. St. Catherine's was a working dock in those days with a large sugar refinery that belched out pungent fumes. As the night wore on and the drink flowed, the sugar strewn route up the dock side steps to the toilet became more and more treacherous. (Woodward 2002)

The list in Table 1 shows that a number of external speakers were invited to give presentations. Despite reactions to his talk in 1949, Warren McCulloch was asked back in 1953 to open a discussion on his work, and attended other meetings as a guest – members grew to appreciate his style.

As the club developed Ashby was keen to see it transform into a formal scientific society – 'The Biophysical Society' or 'The Cybernetics Society' – with a more open membership. His proposals for this were resisted. It seems that, for many members, the informal atmosphere of the club, exactly as Bates had conceived it, was the most important factor. When Ashby proposed that Professor J.Z. Young should be admitted as a member, Sholl wrote to Bates in protest:

> "I consider membership of the Club not only as one of my more pleasant activities but as one of the most important factors in the development of my work. I have stressed before how valuable I find the informality and spontaneity of our discussion and the fact that one does not have to be on ones guard when any issue is being argued. At the present time we have a group of workers, each with some specialised knowledge and I believe that the free interchange of ideas which has been so happily achieved and which, indeed, was the basis for the founding of the Club, largely results from the fact that questions of academic status do not arise." (Sholl 1952)

Young was the head of the Department of Anatomy at University College where Sholl worked and although Sholl collaborated with him and continued to do so after the club disbanded, in those days academic relations and the processes of career advancement were such that he would have felt very uncomfortable with his boss as a member. In any event the 'no professors' rule prevailed.

By the end of the summer of 1955 the club had run its course; many important intellectual cross-fertilisations had occurred, and all had learnt much from each other. In 1954 Turing had died in tragic and disturbing circumstances that have been well documented (Hodges 1983). By now several member's research had become very well known internationally (e.g. Ashby and Walter in cybernetics, with Uttley not far behind, and Rushton and Pringle in neurophysiology) and others were on the cusp of major recognition. As careers advanced and families grew, many found it increasingly difficult to justify the time needed for meetings. Another factor that may have played a part in the club's demise was that cybernetics had become respectable. Lord Adrian had endorsed it in one of his Royal Society presidential addresses and talk of its application in every conceivable branch of biology was rife. The frisson of anti-establishmentarianism that seeped into the early meetings was all but gone. The September 1955 meeting, tacked on to the end of the London Symposium on

Information Theory, turned out to be the last. A reunion was held in November 1958 after Uttley's *Mechanization of Thought Processes* symposium at the National Physical Laboratory. Nine members turned up (Bates, Barlow, Dawson, Sholl, Slater, Uttley, MacKay, Woodward, Hick); of the rest, Bates' note of the meeting reads:

> "Absent: with expressed regret: Grey Walter, Merton, Westcott; with expressed lack of interest: Ashby, McLardy; without expression: Rushton, Pringle, Little, Good; emigrated: Gold, Shipton." (Bates 1958)

At the meeting, suggestions were put forward for possible new and younger members. The first name recorded is Richard Gregory, then a young psychologist who had just made his first professional presentation at the symposium; clearly, Bates had not lost his ability to spot talent, as Gregory later became an extremely distinguished vision scientist and Fellow of the Royal Society. However, the initiative came to nothing, and the club did not meet again.

5. Themes

Although a very wide range of topics were discussed at club meetings, a number of important themes dominated. These included: information theory, probabilistic and statistical processes and techniques, pattern recognition, and digital versus analogue models of the brain (Barlow 2002, 2007, Bates 1985). The themes usually surfaced in the context of their application to understanding the nervous system and/or developing machine intelligence.

5.1 Information theory

By far the greatest proportion of British war-time scientific effort had gone into radar and communications, so it is perhaps unsurprising that there was huge interest in information theory in the club. Many of the brain scientists realised very early on that here was something that might be an important new tool in understanding the nervous system. Shannon's technical reports and papers were not easy to get hold of in Britain in the late 1940s and so the first time Barlow came across them was when Bates sent him copies – with a note to the effect that this was important stuff – along with his invitation to join the club. Barlow agreed with Bates, immediately grasping the fact that information theory provided a new, potentially measurable quantity that might help to give a stronger theoretical underpinning to neurophysiology. Over the next few years, as he learned more about the subject at club meetings – particularly from Woodward – he developed a theoretical framework that shaped his research and helped to propel him to the forefront of his field. Barlow used information theoretic ideas in an implicit way in his now classic 1953 paper on the frog's retina (Barlow 1953). This paper gives the first suggestion that the retina acts as a filter passing on useful information, developing the idea that certain types of cells act as specialised 'fly detectors' – that the visual system has evolved to efficiently extract pertinent information from the environment, an idea that was to become very influential. Later, in a series of very important theoretical papers, he argued that the nervous system may be transforming 'sensory messages' through a succession of recoding operations which reduce redundancy in order to make the barrage of sensory information reaching it manageable (Barlow 1959, 1961). (Reducing the amount of redundancy in a message's coding is one way to compress it and thereby make its transmission more efficient.) This line of reasoning fed into the later development of his equally influential 'neuron doctrine for perceptual psychology' which postulated that the brain makes use of highly sparse neural 'representations' (Barlow 1972). As more neurophysiological data became available, the notion of redundancy reduction became difficult to sustain and Barlow began to argue for the principle of redundancy exploitation in the nervous system. In work that has become very influential in machine learning and computational neuroscience, Barlow and co-workers have demonstrated how learning can be more efficient with increased redundancy as this reduces 'overlap' between distributed patterns of activity (Gardner-Medwin and Barlow 2001). For further discussion of these matters see the interview with Barlow later in this volume (Barlow 2007).

Information and its role in biology was at the heart of many club debates. Mackay believed the Shannon formulation was too restrictive and during the Ratio years he developed his own set of ideas, allied with Gabor's version of information theory (Gabor 1946), that took account of context and meaning (Mackay 1952a,b). In the early period of the club's existence Ashby was working hard on the final version of *Design for a Brain* and his habit of quizzing members on specific topics that would help him refine the ideas in the book left several members with the impression that that he was exclusively preoccupied with his own ideas and not open to new influences. However, his journals indicate that he was becoming convinced of the importance of information theory. He records a conversation with Gold and Pringle at one meeting in 1950 on how much information was needed to specify a particular machine, and by extension how much information must be encoded in the genes of an animal. His arguments were demolished by Gold who pointed out that "complexity doesn't necessarily need any number of genes for its production: the most complicated organisation can be produced as a result of a single bit of information once the producing machinery has been set up" (Ashby 1950c). As usual Gold was decades ahead in stressing the importance of genotype to phenotype mappings and the role of development. This theme resurfaced in Ashby's 1952 paper *Can a mechanical chess-player out play its designer* (Ashby 1952b) in which he used information theory to try and show how it might be possible to construct a machine whose behaviour goes beyond the bounds of the specifications described by its designer. This paper caused debate within the club, with Hick in particular disagreeing with Ashby's claim that random processes (such as mutations in evolution) can be a source of information. This resulted in Hick joining in the discussion of Ashby's paper on the pages of *The British Journal for the Philosophy of Science*, where the original work had appeared (Ashby 1952b), a debate which also included a contribution from J.B.S. Haldane (Haldane 1952).

A striking example of the degree of enthusiasm for information theoretic ideas within the club is given by the contents page of the first ever issue of the IEEE Transactions on Information Theory, the field's premier journal. This issue was based on the proceedings of the 1st London Symposium on Information Theory, held in September 1950, and as such was completely dominated by Ratio Club members (*IEEE Transactions on Information Theory* **1:**1, Feb. 1953). Of the 22 full papers that were published in it, 14 were from Club members. Of the remaining eight, three were by Shannon and two by Gabor.

5.2 Probability and statistics

Probabilistic and statistical methods and processes were also of central concern to many members in areas other than information. Good was a leading statistician who pioneered various Bayesian 'weight of evidence' approaches (Good 1950b), something that partly stemmed from his war-time code cracking work with Turing, who also had a keen interest in the subject. Good led a number of club discussions and debates on related topics which may have influenced Uttley's ground-breaking work on conditional probability machines for learning and reasoning (Uttley 1956). In recent years similar approaches to those pioneered by these two have become very prominent in machine learning. Woodward was very knowledgeable on probability theory and gave, by request, at least one lecture to the club on the subject. Slater was one of the first psychiatrists to use well grounded statistical techniques and did much to try and make psychiatry, and medicine in general, more rigorously scientific. Likewise, Sholl, whose first degree had been in statistics, introduced statistical methods to the study of the anatomy of the nervous system. Many of the brain scientists in the club were concerned with signal to noise problems in their practical work and Barlow remembers that this was a regular topic of discussion (Barlow 2006). He recalls that Gold had deep and useful engineering intuitions on the subject. As has been mentioned, Dawson was the leading expert on extracting clean EEG signals in a clinical setting.

A related area that prompted much discussion was that of the possible roles of random processes and structures in the nervous system. It has already been noted that Pringle and Ashby gave presentations in this area, but Barlow remembers that many other members, including Turing, were intrigued by the topic (Barlow 2002).

5.3 Philosophy

A quick glance at the meeting titles shown in Table 1, and the topics of introductory talks listed in Section 4, make it obvious that many club discussions had a distinctly philosophical flavour. Mackay was particularly keen to turn the conversation in that direction, prompting Hodges to refer to him as 'a philosophical physicist' in a mention of a Ratio meeting in his biography of Turing (Hodges 1983 p.411), and Woodward recalls that it was a good idea to keep him off the subject of Wittgenstein! (Woodward 2002)

5.4 Pattern recognition

Pattern recognition was another hot topic, in relation to both natural and machine intelligence. The ninth meeting of the club on the 18th May 1950 was dedicated to this subject, then very much in its infancy; the perspectives of a number of members were followed by a general free-for-all discussion. Ashby provided a hand-out in which he tried to define 'recognition' and 'pattern', concluding that a large part of pattern recognition is classification or categorisation, and wondering "can class-recognition profitably be treated as a dissection of the total information into two parts – a part that identifies the inputs' class, and a part that identifies the details within the class?" (Ashby 1950d). Grey Walter also provided a hand-out – a set of condensed and hasty notes in which he concentrated on a brief survey of types of pattern recognition problems and techniques. He noted that "recognition of pattern correlates well with 'intelligence'; only highest wits can detect patterns in top Raven Matrices where the symmetry is abstract not graphic. Likewise in 'good' music, odours (not so much in man)." (Walter 1950b) We can be sure that a vigorous debate ensued!

There is no room to discuss the other equally interesting themes that arose in club discussions, such as motor control mechanisms in humans and animals (Merton and Pringle were particularly expert in this area, with Westcott providing the engineering perspective), analogue versus digital models of the functioning of the nervous system (see the interview with Barlow later in this book for discussion of this in relation to the Ratio Club) and the relationship between evolution and learning (Pringle wrote an important paper on this at the time of the club (Pringle 1951) which, as Cowan has pointed out (Cowan 2003), laid the foundations for what later became known as competitive learning).

5.5 Artefacts and the synthetic method

However, there is one last implicit theme that is important enough to deserve some brief discussion: the use of artefacts within the synthetic method. In addition to the engineers, several other members were adept at designing and constructing experimental equipment (often built from surplus military components left over from the war). This tendency was naturally transferred to an approach referred to by Craik as the 'synthetic method' – the use of physical models to test and probe neurological or psychological hypotheses. In this spirit Ashby and Walter developed devices that were to become the most famous of all cybernetic machines: Ashby's Homeostat and Walter's Tortoises. Both machines made headlines around the world, in particular the tortoises which featured in newsreels and television broadcasts, and were exhibited at the Festival of Britain (Holland 2003).

The Homeostat was an electromechanical device intended to demonstrate Ashby's theory of ultrastable systems – adaptive systems making use of a double feedback mechanisms in order to keep certain significant quantities within permissible ranges. As mentioned earlier, these essential variables represented such things as blood pressure or body temperature in an animal. According to Ashby, ultrastable systems were at the heart of the generation of adaptive behaviour in biological systems. Part of the device is shown in Figure 3.

The machine consisted of four units. On top of each was a pivoted magnet. The angular deviation of the four magnets represented the main variables of the system. The units were joined together so that each sent its output to the other three. The torque on each magnet was proportional to the total input current to the unit. The units were constructed such that their output was proportional to the deviation of their magnet from the central position. The values of various commutators and potentiometers acted as parameters to the system: they determined its subsequent behaviour. The electrical interactions between the units modelled the primary feedback mechanisms of an ultrastable system. A secondary feedback mechanism was implemented via switching circuitry to make pseudo-random (step) changes to the parameters of the system by changing potentiometer and commutator values. This mechanism was triggered when one of the essential variables (proportional to the magnet's deviation) went out of bounds. The system continued to reset parameters until a stable configuration was reached whereby no essential variable were out of range and the secondary feedback mechanisms became inoperative. The units could

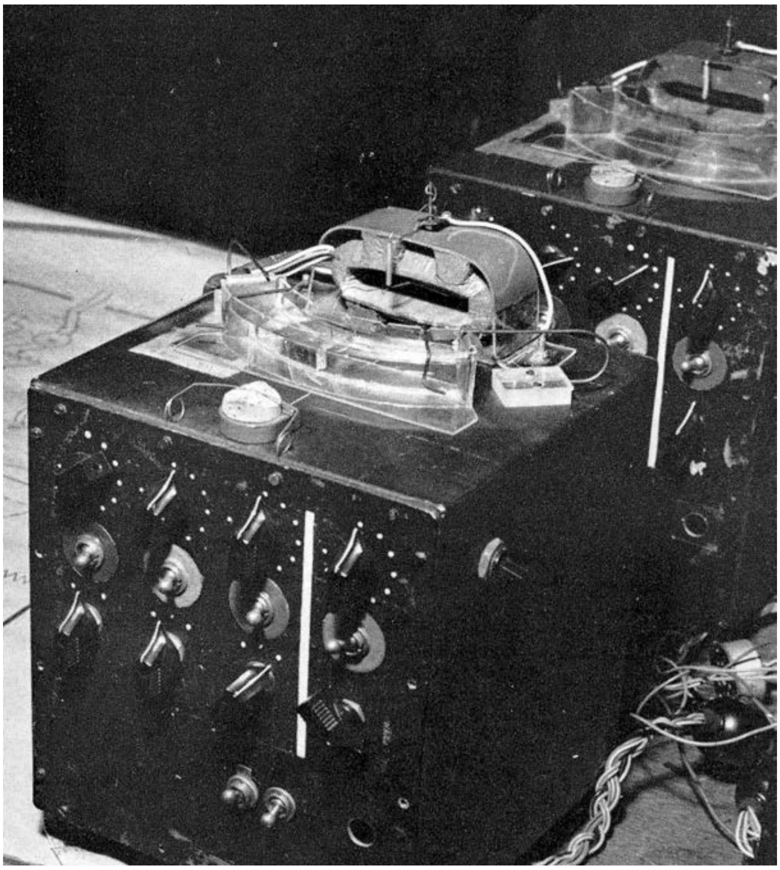

Figure 3: The Homeostat. Two of the four units can be seen.

be viewed as abstract representations of an organism interacting with its environment. Ultrastability was demonstrated by first taking control of one of the units by reversing the commutator by hand, thereby causing an instability, and then observing how the system adapted its configuration until it found a stable state once more (for full details see Ashby 1952a).

On November the 20th 1946 Turing had written to Ashby after being passed a letter from Ashby to Sir Charles Darwin, director of the National Physical Laboratory, distinguished mathematician and grandson of *the* Charles Darwin (and therefore Horace Barlow's uncle). Ashby had enquired about the future suitability of the planned ACE digital computer, which was being designed at the NPL by Turing and others, for modelling brain-like mechanisms. We can assume he was thinking of the possibility of using the computer to develop a programmed equivalent of what was to become his famous Homeostat. In his reply, Turing enthusiastically endorsed such an idea, telling Ashby that "..in working on the ACE I am more interested in the possibility of producing models of the action of the brain than in the practical applications of computing." (Turing 1946). Turing explained that in theory it would be possible to use the ACE to model adaptive processes by making use of the fact that it would be, in all reasonable cases, a universal machine. He went on to suggest that ".. you would be well advised to take advantage of this principle, and do your experiments on the ACE, instead of building a special machine. I should be very glad to help you over this." Unfortunately this collaboration never materialised. Turing withdrew from the ACE project following the NPL management's inability, or unwillingness, to properly mange the construction of the machine (Hodges, 1983). Although the ACE project stalled, Ashby's notebooks from 1948 show that he was still musing over the possibility of using a computer to demonstrate his theories and was able to convinces himself that the ACE could do the job (Ashby 1948a). A pilot ACE digital computer was finally finished in mid 1950, but in the meantime a physical Homeostat had been finished in 1948 (Ashby 1948b). The Manchester Mark1, often regarded as the world's first full-scale stored-programme digital computer and the project with which Turing was by then associated, was finished a few months after this. It is very interesting to note that Ashby was considering using a general purpose programmable digital computer to demonstrate and explore his theories before any such machine even existed. It would be many years before computational modelling became commonplace in science.

Grey Walter's tortoises were probably the first ever wheeled mobile autonomous robots. The devices were three-wheeled and turtle-like, sporting a protective 'shell' (see Figure 4). These vehicles had a light sensor, touch sensor, propulsion motor, steering motor, and an electronic valve based analogue 'nervous system'. Walter's intention was to show that even in a very simple nervous system (the tortoises had two artificial neurons) complexity could arise out of the interactions between its units. By studying whole embodied sensorimotor systems, he was pioneering a style of research that was to become very prominent in AI many years later, and remains so today (Brooks 1999, PRS 2003). Between Easter 1948 and Christmas 1949, he built the first tortoises, Elmer and Elsie. They had similar circuits and electronics, but their shells and motors were a little different. They were rather unreliable and required frequent attention. . The robots were capable of phototaxis, by which they could find their way to a recharging station when they ran low on battery power. In 1951, his technician, Mr. W.J. 'Bunny' Warren, designed and built six new tortoises for him to a high professional standard. Three of these tortoises were exhibited at the Festival of Britain in 1951; others were demonstrated in public regularly throughout the fifties. He referred to the devices as *Machina Speculatrix* after their apparent tendency to speculatively explore their environment.

Walter was able to demonstrate a variety of interesting behaviours as the robots interacted with their environment and each other (Walter 1950a, 1953). In one experiment he watched as the robot moved in front of a mirror, responding to its own reflection. "It began flickering," he wrote. "Twittering, and jigging like a clumsy Narcissus." Walter argued that if this behaviour was observed in an animal it "might be accepted as evidence of some degree of self-awareness."

One or other of the machines was demonstrated at at least one Ratio meeting. Tommy Gold recalled being fascinated by it and wondering if the kind of principle underlying its behaviour could be adapted to develop autonomous lawnmowers (Gold 2002), something that came to pass many decades later.

There was much discussion in meetings of what kind of intelligent behaviour might be possible in artefacts and, more specifically, how the new general purpose computers might exhibit mind-like behaviour. Mackay was quick to point out that "the comparison of *contemporary* calculating machines with human brains appears to have little merit, and has done much to befog the real issue, as to how far an artefact *could* in principle be made to show behaviour of the type which we normally regard as characteristic of a human mind." (Mackay 1951)

Figure 4: Grey Walter watches one of his tortoises push aside some wooden blocks on its way back to its recharging hutch. Circa 1952.

6. Interdisciplinarity

From what we have seen of its founding and membership, to comment that the Ratio Club was an interdisciplinary organization is stating the obvious. However, what is interesting is that is was a successful interdisciplinary venture. This was partly a function of the time, when recent war-time work and experiences encouraged the breaking down of barriers, and partly a function of Bates' keen eye for the right people. Even when war work was factored out, many of the members had very broad backgrounds. To give a few examples: Sholl had moved from mathematical sciences to anatomy following earlier studies in theology, zoology and physiology, Uttley had degrees in mathematics and psychology, Merton was a brilliant natural engineer (he and Dawson were later instrumental in the adoption of digital computing techniques in experimental neurophysiology). All the brain scientists had strong interests, usually going back many years, in the use of mathematical and quantitative techniques. There was a similar, if less marked, story among the engineers and mathematicians: we have already commented on Gold's disregard for disciplinary boundaries, Turing was working on biological modelling and Mackay had started his conversion into a neuropsychologist. Most members were open-minded, with wide-ranging interests outside science. This mix allowed important issues to be discussed from genuinely different perspectives, sparking off new insight.

Most members carried this spirit with them throughout their careers and many were involved in an extraordinarily wide range of research, even if this was within a single field. This lack of narrowness meant that most had other string to their bows (several were very good musicians and a number were involved with other areas of the arts), sometimes starting whole new careers in retirement (Woodward's enormous success in clock making has been mentioned, and in later life Slater became an expert on the use of statistical evidence in analysing the authorship of Shakespearean texts).

Figure 5: Jack Good at home in 2002. The sculpture above his head, 'Jack Good's Dream', was made in glass by an artist friend and exhibited at the famous 1968 Cybernetic Serendipity show at the ICA, London. It is based on a geometric construction of intersecting cylinders and spheres – the formation came to Jack in a dream.

Figure 6: Philip Woodward at home in 2002. In the background is one of the mechanical clocks he has designed and built. His W5 clock is one of the most accurate pendulum controlled clocks ever made. It has won a number of international awards and helped to make Woodward one of the most celebrated horologists of our times.

A key ingredient in the club's success was its informal, relaxed character, which encouraged unconstrained contributions and made meetings fun, and the fact that it had a fairly strong focus right from the start: new ways of looking at mechanisms underlying intelligent behaviour, particularly from a biological perspective.

7. The legacy of the club

In the USA the cybernetics movement organised the Macy Foundation conferences whose published proceedings were made available a year or so after each meeting; they consisted of verbatim transcripts, lightly edited by Heinz von Foerster, and so the substance of all the presentations and discussions was readily available to the academic community and the public, where they had considerable influence. In contrast, no detailed records of the Ratio Club's meetings were made, let alone circulated or published, and so, in assessing the influence of the Ratio Club, it is clear that it can only have been of two kinds: the influence of its members on one another, and the consequences of that influence for their own work.

Unravelling such influences is non-trivial, but we have already seen testaments from several members on how important the club was to the development of their research. In 1981, after coming across some long forgotten Ratio material, Pringle was prompted to write to Bates:

> "Dear John,
> Going through some drawers of papers today in the lab, I came across a photograph of 17 members of the Ratio Club … It occurs to me that someone ought to write up the history of the club, since it was in the old 17th century tradition and, to me at any rate, was a most valuable stimulus at a time when I was only just getting back into biology after the war." (Pringle 1981)

He also wrote to Mackay, who agreed on the importance of the club and sent his Ratio papers to help with the history Pringle and Bates planned to put together. Unfortunately this venture stalled.

Pringle's response to the club was typical of its effect on many members, particularly the biologists: it acted as an inspiration and a spur. Much subsequent work of members had its origins, at least partially, in club discussions. The important influence on Barlow has already been explained; given his major impact on neuroscience, if all the club had done was to put Barlow on the road he travelled, it would be of significance. Clearly it did much more than that. As a mark of his debt to the Ratio Club, Uttley included the photograph of it shown in Figure 2 in his 1979 book, *Information Transmission in the Nervous System* (Uttley 1979). The influence of the biologist in the club appears to have played an important role in Mackay's transformation from physicist to prominent neuropsychologist. The pages of Ashby's private journals, in which he meticulously recorded his scientific ideas as they developed, show that the club had some influence on him, although how much is hard to judge – before becoming very well known, he had worked on his theories in isolation for years, and there was always something of the outsider about him. His grandson, John, has pointed out that Ashby's most prolific years, as far as scientific journal writing was concerned, exactly coincide with the Ratio years (Ashby, J. 2004). In all events he was an active member who rarely missed a meeting.

Most members went on to pursue highly distinguished careers. Many gained professorships at prestigious universities. Between them club members were awarded a host of prizes and honours, including seven Fellowships of the Royal Society and a CBE to Slater for services to psychiatry. Four members (Barlow, Rushton, Gold, Walter) came within striking distance of a Nobel Prize (many feel that at least Rushton and Barlow should have received one) and Turing's work is likely to be remembered for centuries. Many papers and books written by members of the group, including those produced during the Ratio years, are still widely cited, with many ideas and techniques emanating from members very much in currency today.

Uttley and Mackay went on to set up and run successful interdisciplinary groups, at the NPL and Keele University respectively; it is likely that their experience of the extraordinary club influenced them in these ventures.

So how should we assess the club's contribution? It seems to have served a number of purposes during a narrow and very specific window in time. It influenced a relatively small group of British scientists in their post-war careers; but given the degree of eminence many of them reached, and their influence on subsequent generations, this turned out to be highly significant. It certainly concentrated and channeled the cybernetic currents that had developed independently in the UK during the war. It also provided a conduit for the new ideas from the US to be integrated into work in the UK. It stimulated the introduction into biology of cybernetic ideas, and in particular of information theory. And, perhaps appropriately for a cybernetic organisation, it stopped meeting when these purposes had been achieved.

Despite its length, this paper can only act as an introduction to the life and times of the club and its members. We were forced to leave out a huge amount of fascinating material; there is still much to tell.

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