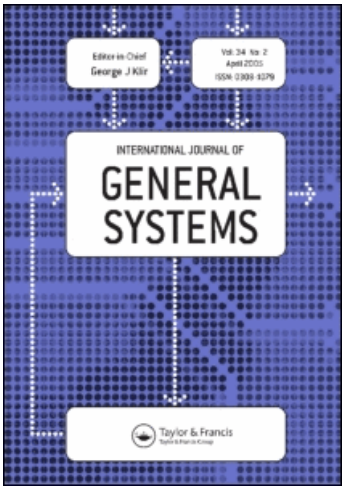

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Autobiographical retrospectives: Some reminiscences of cybernetics and systems

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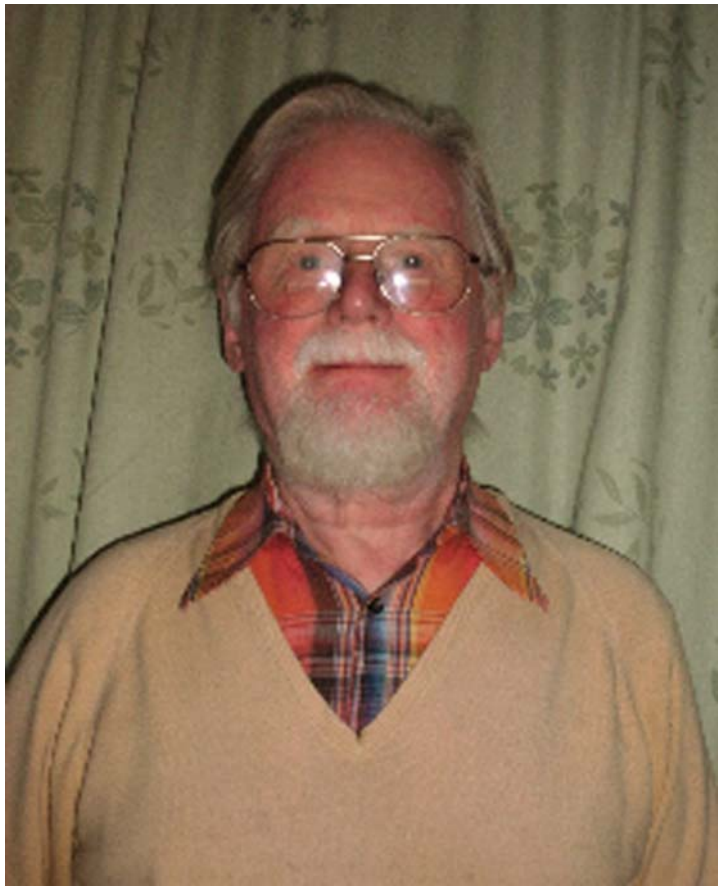

ALEX M. ANDREW

Systems movement: autobiographical retrospectives is a special section of this Journal, the purpose of which is to produce, via invited autobiographical articles, historical information, and insights regarding the thought processes and individual motivations of leading figures in the systems movement. This valuable information is normally not included in regular publications, which tend to focus on results rather than the creative process leading to those results. The autobiographical articles are likely to help us to improve our understanding of how, and why, the systems movement has progressed since its emergence in the mid-20th century. Each article in this section is published strictly by invitation extended to individual authors by the Editor, and is based on the recognition that these individuals have made major contributions to the systems movement.

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AUTOBIOGRAPHICAL RETROSPECTIVES

Some reminiscences of cybernetics and systems

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Introduction

An invitation to write an autobiography is an honour, but at the same time quite disconcerting and requiring difficult decisions about what to include and how much detail is warranted. I have included quite a lot of personal data that does not seem entirely appropriate in a scientific journal. One reason is that it is difficult to write a biography without a fair amount of such material to bind it together and another is that standpoints on scientific and technical topics do not grow from a vacuum, and background personal data may be more relevant than at first appears, especially in providing reasons for questioning the soundness of the said standpoints. Anyhow, I will start with some details about parents and early upbringing, but will keep them brief and hope they are not unduly tedious.

Origins

I was born in central Scotland, of Scottish parents who married shortly after the end of the First World War in which my father had served in France and Belgium. I am not quite old enough to be counted as belonging to the baby boom that followed that war because there was a boy before me who did not survive and then a few years passed before I arrived. After me they had two girls. My father had not had the benefit of formal education beyond the age of 12, and yet he became a leader in his community in a way that I have certainly never emulated, partly in helping to organise annual treats for widows and children of men who were less fortunate than him and had not survived the war, and partly in setting up and running a Boy Scout troop, initially at the suggestion of the local Church of Scotland minister.

My paternal great-grandfather was a rigger of square-rigged ships in one of the shipyards on the Clyde, and when the demand for that skill dwindled, the family had moved to the vicinity of the famous Carron Iron Works and my grandfather worked there, but my father avoided that claustrophobic environment and, for the time I can remember up to the beginning of the Second World War, he worked as a commercial traveller for a manufacturer of confectionery. My mother had worked in an office and gave valuable help with the bookkeeping that was part of the travelling job.

These details may have influenced my later thinking by encouraging a pragmatic viewpoint that for example makes it easy to sympathize with Ashby's view of the brain as a specialized organ of survival, with the evolution of intelligence accounted for in terms of utility to survival at every stage. This places the emphasis differently from that of second-order cybernetics with its attention to observation and of fuzzy theory with its basis in the evolutionarily-late development of language. I think I can claim to have absorbed something of what was termed the 'protestant work ethic' of Scotland at that time, though not sufficiently to make me consistently diligent, since I can easily think of points at which

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I missed opportunities through insufficient application. I did not absorb protestantism either, nor any other religion.

Glasgow and Farnborough

I was still at school at the start of the Second World War. My father realised that the peddling of confectionery was not work of national importance and despite his age he enlisted again in the army. He was not sent abroad but served with anti-aircraft units in some quite dangerous places including Scapa Flow in the Orkneys. Meanwhile I had shown aptitude for mathematics and science, and when I completed my school course in 1942, I obtained a state bursary to study 'Physics with Radio' at a university. Scientists were needed for the war effort, both in the forces and in industry and government research, and this was one of the subject combinations offered, another being 'Chemistry with Metallurgy'. I embarked on a 4-year course at Glasgow University, with principal subjects Mathematics and Natural Philosophy and subsidiary subjects Chemistry and Radio.

The Radio course and laboratory were set up and run by Dr Walter McFarlane who did a superb job and inspired enthusiasm in the students. It is startling, though, to reflect that one of the textbooks recommended was the *Admiralty Handbook of Wireless Telegraphy*, which includes a brief mention of spark transmitters! Nevertheless, the course brought us fully up to date and included experiments with microwaves. It is strange to reflect that polythene and polystyrene, now common as packaging materials and as litter, were introduced as recent discoveries that were vital for their low dielectric losses at microwave frequencies. Military training in the university Senior Training Corps was compulsory for male students, with a camp in the summer between the first and second years of the course.

At the end of the second year, in 1944, almost the entire class was directed to various forms of national service. I asked for government research and was sent to the Royal Aircraft Establishment in Farnborough, where I was assigned to a section concerned with type approval of components. The work was undoubtedly important since it affected the reliability of electronic equipment used by the armed forces, and at that time there were special challenges raised by the need to operate in humid tropical conditions in which temperature cycling would introduce water into any cavity not hermetically sealed. Most of the development work, however, was done by manufacturers and the main function of the section I had joined was subjecting submitted sample components to a battery of tests, and recording results.

I managed to transfer to a small group concerned with speech secrecy devices, or scramblers, a topic that certainly offered scope for ingenuity. The group had completed work on a particular scrambler system before I joined and was exploring new ideas. Speech has so much redundancy that scrambling to give total privacy is extremely difficult and perhaps impossible. It was acknowledged that the coding in the system just completed by the group could be 'cracked' by an enemy examining the scrambled output, but it was thought that the complexity was such that in many applications this would not be done in time for the information to be of strategic value. No better solution was found during my association with the group, but it was an interesting time and much thought was given to the nature of speech communication and, in particular, to the elimination of much redundancy in 'vocoder' schemes.

I was at Farnborough for about 18 months, when the war ended and I was made redundant along with many others who were similarly recruited. The situation was complicated because conscription to the armed forces was still in force and as the laws

stood we were still liable to be called up for an extended period. This was so, even though at the earlier stage we were told that the needs of government research and industry for our skills were more pressing than those of the forces. Fortunately the rules were relaxed and it was agreed that we could continue in employment that was officially approved as being of a scientific nature, but resumption of the university course was not allowed until a total of 3 years had passed. I spent the remaining 18 months with a manufacturer of electronic components and developed electronic devices with application in the production line, and then returned to Glasgow University. I was treated by the government like an ex-service person and given financial support, and successfully completed the course and graduated in 1949. I do not think I fully realized at the time just how lucky I was to have come through the war period without ever being in serious danger.

Post-Grad in Glasgow and MIT

After graduation I joined a company specializing in radar systems, but I had begun to feel that I should be altruistic and give attention to research with a bearing on medical practice, reinforced by a realization that interesting unexplored territory lay in the overlap between the physical and biological sciences. I responded to an advertisement for an electronics technician to be shared between the departments of Physiology and Psychological Medicine of Glasgow University. It was a post that would not normally be filled by a graduate, and it was agreed that I should be accepted, but with the revised status of Research Assistant and with the stipulation that I enrol for a PhD in a biological topic.

This was a time of what the Professor of Physiology termed a 'pestilential encroachment of electronics into physiology'. A great many people, including some whose primary interest was biology, had returned from war service acquainted with electronics. Government surplus electronic equipment and components could be bought cheaply and used to make amplifiers and stimulators and other gadgetry that was especially useful in neurophysiology. Various ways of making microelectrodes were devised and used for stimulation and for single-unit recording, and it looked as though the working of the nervous system could be analysed as though it too was a piece of electronics.

In Glasgow, interest in the new topic of cybernetics was stimulated by two talks by Warren McCulloch. His initial contact was due to the circumstance that in these days London's Heathrow airport was often closed by fog and a more certain way of arriving from America was to fly to Prestwick, near Glasgow, and to go south by train. He formed links with the Glasgow area and was proud of his own descent from a Scot who had sailed from Kirkudbright four generations before. He invited me to spend some time with his group in the Massachusetts Institute of Technology and I was there for the whole of 1954 and half of 1955.

In Glasgow, I had certainly been encouraged to apply myself to physiological research that would count towards my thesis. It was necessarily on small animals, with frogs as the obvious choice, since I was not qualified to be licensed to operate on mammals, and did not wish to do so. Despite the encouragement, it was difficult to settle to do biological work with awareness that I was doing it clumsily, while surrounded by people with much more skill for that kind of manipulation, but short of electronic gadgetry that I could provide. In MIT the situation was nicely reversed, since I was a member of a small group doing biological research within the Research Laboratory of Electronics. It is, however, very difficult to be a true polymath and I must admit to falling short.

In Massachusetts, there was no law to stop me from operating on mammals and I did perform one experiment on a cat, helped only by the girl who was secretary to the group.

It was an 'acute' experiment, meaning that the animal was anaesthetized at the start and never regained consciousness. I am embarrassed to recall that I failed to obtain useful data because of a mathematical error, namely a misapprehension about the origin of the set of axes used in specifying a location for a recording electrode. Although I would not have made the same mistake again, I thereafter participated in work on mammals only as an assistant, but I pursued a project on vision in the frog, with much guidance from permanent group members Jerry Lettvin and Pat Wall. Interesting results on frog vision were obtained at the time, and its study was resumed later by Jerry Lettvin in collaboration with Humberto Maturana (Lettvin *et al.* 1959, 1961). This later work has aroused much interest, to the extent that it is a claim to fame that it was for a project for me that the group first concerned itself with frogs and their vision.

Autobiographies exist for both Lettvin (1998) and Wall (2001), and obituaries for the latter (McMahon *et al.* 2001, Andrew 2002a), but I must mention that at some points Jerry Lettvin's memory has been unreliable and he is not correct in attributing total opposition to animal experimentation to me and to my then future wife Joyce. We both believed that such work should be closely regulated, but both of us had chosen to be involved in it.

Warren McCulloch and his wife Rook were wonderfully hospitable and like other members of the group I spent many weekends on their farm in Old Lyme, Connecticut. I had a privileged insight into an early source of the ideas of cybernetics, when Warren invited me to accompany him on a visit to Orange, New Jersey where he had to attend to the handover of a house that he and Rook had finally decided to sell, as recounted in Andrew (2005). The house was on a magnificent scale and they had planned to run it as a research centre to which they would invite workers from anywhere in the world whose work interested them. The research theme was to be the one that Warren epitomized by his question: 'What is a number, that a man may know it, and a man, that he may know a number?' (McCulloch 1960) and he clearly hoped for a breakthrough between neurophysiology on the one hand and overt conscious mental behaviour on the other. Both Warren and Rook had come from wealthy families and expected to support this ambitious plan from their own resources, but the depression of the thirties changed their situation drastically. The handing-over of the house was an emotional 'end of a dream'.

Back in Glasgow I made some studies of nerve excitation and prepared a PhD thesis (Andrew 1956) with sections as follows:

(1) *Electrical excitation of nerve*. This was treated both experimentally and theoretically. A study by Offner (1946) based on a model due to Hill (1936) had suggested that the way to stimulate nerves with minimum electrical power was by use of an exponentially-rising waveform, and I was able to show that a triangular waveform should be almost as good. The finding has potential application to clinical electro-convulsive therapy, a technique that was much more widely used then than now. Another contribution was an extension of Hill's theory to take account of aspects of the phenomenon of accommodation in nerve fibres. To evaluate parameters of accommodation, a stimulator was developed (Andrew 1952) delivering current with a particular exponential waveform. Changes in parameters can reflect pathological conditions in humans, and the stimulator was used in clinical studies, by Simpson (1955) and others.
(2) *Frog vision*. Studies carried out in MIT (Andrew 1955) were reported. Recordings were made from a microelectrode inserted in an optic lobe of the frog brain, in response to visual stimuli. Responses to movement of a spot of light were recorded

and it was shown that they could not be accounted for as summations of on- and off-responses from points traversed. One motivation was consideration of the results of Sperry (1944) on regeneration of the optic nerve, which appeared to demonstrate neural specificity that did not fit with current ideas on spontaneous self-organization. No alternative to the postulated specificity was found, but the study laid foundations for the later work of Lettvin and Maturana.

(3) *Information theory in neurophysiology*. This was considered in general terms in a review (Andrew 1953) and later with specific attention to neural transmission, supplementing a treatment by D.M. MacKay. The topic is discussed later in Andrew (1984).

(4) *Electric field of a current dipole in a homogeneous conducting sphere*. This was analysed using mathematical spherical harmonics theory and a solution found for any location and orientation of the dipole. The aim was to aid the interpretation of electroencephalograph (EEG) recordings. After the solution was found, an equivalent treatment prompted by electrocardiography was found to have been published elsewhere. In Glasgow I gave technical assistance with clinical EEG, and because of its non-invasive nature, I was keen to find means of exploiting it as fully as possible, both clinically and for research.

(5) *Indicating impulse-frequency in neurophysiological experiments*. An electronic device for indicating discharge frequency was developed (Andrew and Roberts 1954) and used in studies by Boyd and Roberts (1953). It measured pulse intervals and was shown to respond to changes in frequency as rapidly as theoretically possible. Three further sections of the thesis also dealt with electronic apparatus requirements for physiology, grouped under the three headings of amplifiers, stimulators, and auxiliary apparatus. They included a number of original contributions that were significant at the time though without much relevance to modern solid-state electronics technology.

As well as the interests indicated by this review of thesis topics, I was involved in some other noteworthy projects. The work of my supervisor Dr Tristan Roberts on feedback control of voluntary action was not given the recognition it deserved, due to reluctance at the time of the Physiological Society to accept the teleological explanations implicit in feedback theory. I believe that the experimental findings of Boyd and Roberts (1953) could usefully be re-examined. While at MIT I had some involvement in the study of cat spinal cord (Pitts 1953), which led to demonstration of fibre-to-fibre interactions (Howland *et al.* 1955). The connection formed with Donald MacKay, then at King's College in London, had great influence on my thinking. He was a member of the Ratio Club, a British focus for cybernetic thinking, and also a close friend of Warren McCulloch.

Some time after graduating with my doctorate, I heard about a new division being set up in the National Physical Laboratory (NPL) in Teddington, a short way west of London, under the direction of Dr Albert Uttley and undertaking research in the broad field denoted by cybernetics. I was somewhat dissatisfied with my situation and prospects in Glasgow (though in retrospect I can see I should perhaps have been more patient) and I resigned and was able to join the team in Teddington.

Teddington and Moscow

The NPL is the oldest of British government laboratories and was among other things a centre for development of digital computers, originally under the leadership of Alan Turing. I now had access to a digital computer and quickly appreciated the ease with which

fairly complex systems could be modelled as programs rather than by special-purpose circuitry. The computer was an English Electric DEUCE (Digital Electronic Universal Computing Engine), a commercial version of an ACE pilot model (Automatic Computing Engine) that was developed in NPL. It used vacuum tubes and acoustic delay-line storage and was programmed in binary machine language on punched cards.

I was also involved with the Conditional Probability Computer, a learning automaton devised earlier by Uttley. This modelled features of conditioned reflex behaviour and lent itself to a number of demonstrations of learned behaviour. Application of the principle to industrial control was considered with specific attention to a distillation column in an oil refinery. I had disagreement with Uttley over whether effective control could be achieved on the basis of a small number of discrete yes/no signals and argued for some kind of continuous or smeared-over learning. I do not think it occurred to either of us at the time that the biological conditioned reflex might be considered to have continuous nature, though described and studied in terms of discrete events because humans are good at thinking in such terms.

About a year after I joined NPL there was a very significant conference there on Mechanization of Thought Processes. Participants came from many countries and one important result was a consolidation of streams of thought that had previously been pursued fairly independently on the two sides of the Atlantic. In the UK, the ideas were promoted by the Ratio Club, of which Albert Uttley and Donald MacKay were members, and in the USA, the topic of Artificial Intelligence had emerged, encouraged by Warren McCulloch.

I gave a paper that I called 'Learning Machines' (Andrew 1959) in which I indicated how continuous devices could self-organize, in ways that were also indicated by the term 'learning filter'. The importance of continuous measures in intellect was also argued by MacKay (1959) and by Minsky (1959, 1963), who referred to subtle forms of continuity as 'heuristic connection' between problems. A review is by Andrew (1982) and discussion of the related topic of succinct representation is in Andrew (1978a).

I also had the privilege of attending the first Congress of the International Federation of Automatic Control (IFAC) in Moscow in 1960 and gave a paper (Andrew 1961) in which I reported results of computer simulation of a continuous controller that was self-optimizing with features that could warrant description as self-organizing. As a means of system analysis, the simulation approach was much less powerful than mathematical techniques advanced by Wiener and others, but can be argued to correspond more closely to biological evolution of means of control. The paper seemed to be well received and fitted well with studies in the very active Institute of Automation and Remote Control in the host country. The latter were widely seen as narrowing the gap between artificial intelligence (AI) and control theory.

The IFAC Congress was attended and addressed by Norbert Wiener and the only time I ever spoke to him was when I happened to enter a lift in the Moscow University building along with him and his wife. He asked whether I had felt offended to be introduced as 'English' by the session chairman, when my accent clearly indicated Scottish origin. I had seen Wiener many times in the corridors of MIT, but it was at the time of the unfortunate rift between him and McCulloch. He probably had not noticed me as a temporary member of the McCulloch group, and if he had, the facial hair I had grown in the meantime would be enough to disguise me at the later encounter.

The Congress in Moscow was a wonderful experience, with a welcoming atmosphere that made it difficult to imagine that international tensions were not on their way out. A useful and congenial contact, among others, was with Anatoli Viktorovich Napalkov of

the Department of Higher Neural Activity in the Biology Faculty of Moscow University, who had a novel approach to the representation of learning in terms of linked conditioned reflexes.

Another useful and congenial contact made around this time was with the department headed by Prof. Karl Steinbuch in the Technical University of Karlsruhe, the main focus of cybernetics in Germany with particular focus on a learning device termed as lernmatrix. A postgraduate student, Peter Müller (later Müller-Stoy) spent the year 1960 with me in NPL and was sufficiently impressed by the emphasis on continuity to let his doctoral thesis (Müller 1964) be on a non-binary version of the lernmatrix.

When I joined NPL, it was expected that I would initiate specifically biological research. Studies of machine learning have obvious biological connotations, but are removed from hands-on biological experimentation. At the time many people had the idea that better understanding of neural processing, and better performance by artificial neural nets, would come from closer examination of the properties of synapses, and some experimenters who were primarily zoologists, notably Horridge (1968), examined synaptic transmission in a variety of primitive organisms. I had some ideas about examining the synapses connecting with the giant axon of the earthworm, but did not go beyond a few inconclusive behavioural observations on intact worms. In retrospect, the emphasis seems misguided since it is difficult to see how studies of single synapses could possibly reveal mechanisms allowing purposeful self-organization of networks. Later studies under the heading of back-propagation postulate influences passing through multiple synaptic connections.

I am sorry to say that I left NPL after some acrimonious exchanges. I paid a visit to USA for a meeting, then yielded to urging by Warren McCulloch to stay on for another relevant meeting, although I did not have approval. I also entered into discussion with the group in Karlsruhe about a possible scholarship visit there, again without clearing the plan in Teddington. The laboratory followed the civil-service rule, whereby correspondence could be opened centrally, although addressed to an individual, and when I learned that a letter from Karlsruhe was answered before I knew of its existence I decided to resign.

It is easy to see ways in which an establishment with civil-service rules is not an ideal home for innovative research and, for example, I found the requirement for strict hours of attendance to be irksome. On the other hand, it was a great privilege to have an unfettered research opportunity without the teaching duties of a university post and I should have been more ready to accept the restrictions, some of which were needed to impose order and to persuade taxpayers that their money was not being squandered. A year or two after I left the laboratory, there was a change of policy that required that projects had to relate rather directly to the needs of industry and that the more imaginative research was better done by universities.

SIGMA, Illinois and Reading

In 1962, I joined the consultancy firm Science in General Management (SIGMA) Ltd, Croydon, UK, headed by Stafford Beer. I had foolishly imagined that there would be some sort of backroom research facility, decoupled from the ‘coal-face’ of client interaction, but of course the small company could not afford such a luxury and also client interaction was a basic component of the activity. It was necessary to appear confident when talking to managers and directors and company chairmen, even when without a clue as to what to do next. I have to admit that after two years I ‘chickened out’ and decided this was not for me.

No doubt I would have become more confident if I had stayed with the company and gained experience. There were of course some projects undertaken by the company that

clearly failed to bring much benefit to the clients, and the charging of a substantial consultancy fee in these cases was held to be justified by the observation that a doctor expects to be paid even if his patient dies, and similarly for a lawyer who loses a case. A writer at the time described management consultancy as: 'an expensive way of being insulted by men half your age and with a tenth of your experience' and Stafford Beer actually used this in touting for business, to make managers feel they were 'standing on dignity' if they refused to allow their operations to be scanned critically by a relative amateur. It does, however, require that the relative amateur be thick-skinned to the point of brashness.

At a later date, when I had joined the Cybernetics Department of Reading University, it was said in a biographical note on Stafford Beer in, I think *The Times*, that he was a cybernetics consultant, which the writer said he interpreted as meaning that he 'lived by his very considerable wits'. I think the interpretation would not have displeased Stafford, even though it ignores the large amount of coherent theory he brought to bear, but it greatly annoyed Peter Fellgett, by then Professor of Cybernetics and head of the department of that name in Reading University, who wrote to the paper complaining about misrepresentation of what he claimed to be an established academic topic. The truth is of course somewhere between, with cybernetics sufficiently well defined to warrant books, journals, and courses though less firmly circumscribed than, say, chemistry. Peter Fellgett's vigorous response was undoubtedly partly triggered by the difficulties he experienced in the university Senate in arguing for resources for his department in competition with others teaching traditional subjects. Academia has its coal-faces, too!

My time in SIGMA was not without some achievement, especially in adapting the optimization technique of Rosenbrock (1960) to determine an optimal location for a manufacturing facility so as to minimize distribution cost for its product, given the locations and requirements of a set of consumers, and possibly taking advantage of lower-cost bulk shipment to distribution depots. For reasons that are not clear to me (and that certainly were not precipitated by my leaving), things started to go wrong in SIGMA some time after I left and eventually it was much reduced and merged with an associated larger firm. Some of my former colleagues congratulated me on having had the foresight to leave when I did, but in fact I had no inkling when I left that SIGMA would not continue to prosper. I was pretty sure that, as far as my financial state was concerned, my best plan would have been to stay where I was.

I had talked to Heinz von Foerster during an earlier visit to the USA about the possibility of spending a year with his group in Illinois and he had responded favourably. I contacted him and he obtained US Air Force funding and my wife Joyce and I spent the academic year 1964–5 in Illinois. I had realised that self-organization in neural nets, or in nets in general including social systems, had to depend on communication pathways in the net, and I discussed this under the heading of 'significance feedback' (Andrew 1965, 1993). One version of this was essentially the 'backpropagation of error' that was later developed by others including Werbos (1974) [reprinted later in Werbos (1994)] and Rumelhart *et al.* (1986), and that is the basis of most of the many practical applications of artificial neural nets in recent decades.

On my return from Illinois I was able to join the teaching staff of a new department of Cybernetics in the University of Reading, set up following an initiative by Prof. R.W. Ditchburn at the time of his retirement as the head of the Physics Department. A major and fruitful research effort he had headed was a study of visual perception as influenced by stabilizing the retinal image, which is normally rapidly changing due to involuntary eye movements (Ditchburn 1973, 1987).

The new department was headed by Prof. Peter Fellgett and despite the historical connection to the study of visual perception, the emphasis was not strongly on biology. A course leading to a degree in Cybernetics was being offered and a range of topics was chosen to give a firm foundation. These included Information Theory, taught with particular reference to measurement and instrumentation, mathematical control theory, electronics, computer technology, and computer programming.

I was able to fit into this by teaching matters related to programming, especially since there was initially no Computer Science department in Reading. I covered basic aspects of compiler construction including syntax-directed translation, and Artificial Intelligence including programming techniques using recursion and list processing. I also covered a range of topics related to Operations Research including linear programming in its various manifestations, dynamic programming, discrete and continuous computer simulation, optimisation, and path planning.

As the basis for a first degree, the curriculum was necessarily rather down-to-earth compared to some views of cybernetics, but many students made good use of the grounding it provided. A connection was formed with biological topics when James Lovelock, of 'Gaia' fame (Lovelock 1979, Andrew 1988), was appointed a visiting Professor and led a research project to study the dinosaurs as well as supervising studies directly related to the Gaia hypothesis. Another initiative connected with perception and, therefore, biology was the work of Peter Fellgett and a colleague on quadraphonic sound reproduction.

In June 1968, fairly early in my period with Reading, I was one of the guests at the dinner in London at which Stafford Beer announced the formation of an Institute of Cybernetics in Brunel University, funded by a grant he had obtained from the International Publishing Corporation. He invited applications for posts in the new venture and I felt tempted, but after a few near-sleepless nights of indecision I decided to stay in Reading. I stayed in touch with Prof. Frank George who headed the Brunel unit, and with several other members of it, and as far as I know I was the only person who ever set foot both there and in the Reading department.

Another development around this time was the founding by Prof. John Rose of the World Organisation of Systems and Cybernetics (WOSC), intended to be a federation of societies in the field, and with a Congress every 3 years. I attended a number of the Congresses and by chance (after Frank George had to withdraw because of a medical emergency in his family) was a member of a delegation organized by John Rose in 1978 to present a gold medal to the ill-fated President of Romania, Nicolai Ceauşescu. In the following years, I became increasingly involved with WOSC, especially after I retired from Reading and found that reviewing books and making other contributions to *Kybernetes* and *Robotica* was a good way of staying in touch and making some contribution to the intellectual scene. These two journals were founded by John Rose with the first linked to WOSC. I am currently Director-General of WOSC and hope to attend the next Congress in Nanjing in September 2011.

In 1982, I took early retirement from Reading University, taking advantage of a policy at the time of encouraging senior members to leave so as to allow replacement by more junior staff on lower salaries. Retirement allowed Joyce and me to move to an attractive location in Devon and I had time to concentrate on my ideas about continuity and AI. We also had ideas about supplementing our income by commercial activity and formed ourselves into a company that we called 'Viable Systems'. This was before the virtual monopolization of the term by Stafford Beer and our use of it arose independently. I had ideas about writing commercial software for the small computers that were coming on the market, but this was a difficult area to get into, especially since the available technology

was changing rapidly. We finally made some profit by doing typesetting of mathematical material for a major publisher, but only at the cost of a great deal of tedious effort.

One of the advantages expected from retirement was freedom to accept temporary positions abroad and we spent the academic year 1987–8 at the University of the Aegean in Bornova, a suburb of Izmir in Turkey. The opportunity arose from a contact made by Joyce when she took a Master's course in Applied Statistics in Reading, when one of her fellow-students was Fikret Ikiz who later became a Professor and for a spell Vice-Chancellor of the University of the Aegean. I taught programming and Artificial Intelligence and Joyce assisted in classes on report writing in English.

We liked Turkey and paid a number of visits, but when we were there in 1993 Joyce had a heart attack and died. She loved Turkey and is buried there. A few years later, I married Ludmila ('Mila') Michaelovna Levkina, the former wife of Anatoli Napalkov. Anatoli had died in 1989 and Mila was soon to retire from her position in the Department of Mycology and Algology of the Biology Faculty of Moscow University. I also found the rural retreat in Devon to be just too remote and we have moved back to Reading. The proximity of Reading to Heathrow is convenient, especially since Mila's son, Dmitriy Anatolevich Napalkov and his wife Patritsia visit frequently from Moscow to check on our welfare besides enjoying the attractions of Reading and London. They are both active staff members of the Department of Higher Neural Activity of Moscow University.

Aspirations

I have written six books (Andrew 1963, 1983, 1985, 1989, 1990, 2009), and the last three defend a particular approach to cybernetics with, I hope, increasing clarity with passage of time. I do not intend to add another book, but of course there are aspects that invite further investigation. In the 1990 work I tried to demonstrate a connection between continuous and discrete operation that would be plausible as a feature of biological evolution by considering path-planning for mobile robots. Path planning to avoid obstacles can depend on continuous processing, for example by letting the path match lines of force of a hypothesized electric field, but changes in the position of obstacles can produce discontinuous changes of optimal path. I have to admit that computer simulations meant to illustrate the point did not work out as I hoped and require further attention.

I think I may also be able to shed light on another matter that is touched on in the 2009 book. In recent decades, studies using artificial neural nets have proved useful and their discussion is no longer heretical in AI, with most applications depending on some variant of the backpropagation principle. There is, however, no evidence that real neurons provide the necessary two-way processing and serious doubt has been expressed, notably by Hinton and McClelland (1988). I think that a small difference in my presentation of 'significance feedback' may go some way towards reconciliation of the biological and computational studies and this is another area that I hope to develop.

I also want to look into some other areas such as a re-examination of the results of Boyd and Roberts (1953) on knee-joint receptors, as mentioned earlier. There is certainly no shortage of interesting research directions to explore. I have attached some references to miscellaneous topics (Andrew 1970, 1975, 1978b, 1979a,b,c, 2002b, 2006, 2007) to confirm this. Also, a regular feature I prepare for the journal *Kybernetes* gives me incentive to stay in touch with developments accessible on the Internet.

The fields of cybernetics and systems theory do not have precisely defined boundaries and would lose much of their power and interest if they did. However, I have to admit to considerable disquiet about recent developments, especially under the heading of

second-order cybernetics, that divert attention from the challenges of earlier work and McCulloch's famous question. I hope that what I have put forward here and through other channels will help to direct attention to these earlier issues. A thoughtful review of the issues is in a recent book by Pickering (2010).

Preparing an autobiography is a strange experience and I hope I have not been unduly tedious or trivial or otherwise inappropriate. I now feel embarrassed because I can think of people who influenced and guided me at various times, but whom I have not mentioned. To try to mention all of them would certainly make my account extremely tedious and I can only apologize and acknowledge that there are many people to whom such apology, and gratitude, are due.

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