Enclosing the Field

from 'Mechanisation of Thought Processes' to 'Autonomics'

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Abstract

It is always difficult to perceive something new except upon the model of what is already known. The result is a partial and incomplete understanding that can only be improved by experience. Our present perception of computing remains strongly influenced by its origin as a mathematicians' machine. A notion of mathematics as the arbiter of what constitutes a 'science' has long been ascendant. As a result, even when viewed from a more empirical standpoint, computing has been influenced by the aspirations of engineering disciplines to acquire the status accorded to a science. Yet there always has been an interest in computing machinery from a different perspective: that of biology—autonomic mechanisms, understood through observation rather than the proving of theorems. But this runs counter to a prevailing trend in twentieth century biology toward a discrete and computational molecular model.

The story of the Autonomics Division of the National Physical Laboratory between 1957 and 1966 provides a particular instance of these tensions. It coincides with a critical point in the history of computing: the time when a distinct discipline of 'computing science' took form. The field thus enclosed represented the dominant mathematical and engineering interests, the biological view surviving only in cognitive science.

But these models have not proved adequate for the study of what is distinct and unique to this new science: software—an amorphous concept, at once both intangible and yet, it seems, an engineered product. To resolve this paradox it is suggested that a model of software as a form of literature needs to be developed.

Abbreviations

ACE	Automatic Computing Engine. (at NPL)
Add8353	Cambridge University Library, Manuscripts Collection: Letters and Papers of Sir Gordon Sutherland
AVIA	PRO: Records of the Ministry of Aviation etc.
BT	PRO: Records of the Board of Trade etc.
CME	NPL: Control Mechanisms and Electronics Division (renamed in 1960 the Autonomics Division)
DSIR	PRO: Records created or inherited by the *Department of Scientific and Industrial Research*, and of related bodies
GC/179	The Wellcome Library for the History and Understanding of Medicine, Archive: Bates, John AV, and the Ratio Club
MIT	Massachusetts Institute of Technology
MoTP58	Symposium on the Mechanisation of Thought Processes, NPL Teddington. November 1958
MT	Machine Translation
NAHC	National Archive for the History of Computing (Manchester)
NPL	National Physical Laboratory
NRDC	National Research and Development Corporation
PRO	Public Record Office
RAE	Royal Aircraft Establishment (at Farnborough)
RRE	Radar Research Establishment (at Malvern, formerly TRE)
TRE	Telecommunications Research Establishment (at Malvern, later RRE)

[Photograph, landscape 25x17cm]

'MacKay's Photo' [Cambridge 2/3 May 1952 ?] *From left to right; standing*: Giles Brindley, Harold Shipton, Tom McClardy, John Bates, Ross Ashby, Edmund Hick, Thomas Gold, John Pringle, Donald Sholl, Albert Uttley, John Westcott, Donald MacKay; *sitting:* Alan Turing, Gurney Sutton, William Rushton, George Dawson, Horace Barlow. (Wellcome Library, London)

Chapter 0. Enclosure

Some of the history of a subject may be revealed in its bibliography. The frequent reference to certain authors or publications, whether by way of relevance or reverence, has something to say about what is significant in that particular field. The bibliography of Artificial Intelligence does not lack references to the published proceedings of the Symposium on Mechanisation of Thought Processes that was held at the National Physical Laboratory in Teddington, SW of London, in the autumn of 1958. The particular papers referenced will vary according to preferred flavour: McCarthy's 'Programs with Common Sense', Selfridge's 'Pandemonium' are but two of the most frequently cited; there were also papers from Marvin Minsky, John Backus and others—some less familiar to the student of computing's history. Yet, while histories of computing never fail to mention the meeting at Dartmouth in the summer of 1956 at which 'Artificial Intelligence' is said to have been founded, nor do they neglect the volume *Automata Studies*, edited by McCarthy and Shannon and published shortly before, the 1958 Symposium is rarely mentioned. The same contributors appear—and more besides. Such gatherings were as rare in 1958 as in the years before, yet history and the bibliographical record seem to differ. A curiosity—this research began as an attempt to discover why.

But this inquiry led to a broader question: what is computing science? Or rather why, when the evidence of that 1958 symposium attests to a very broad interest in the intellectual challenge of computing, should computing have developed to be for the most part an amalgam of mathematics and engineering? To point to the origin of computing machinery in the mid-twentieth century, as a tool for applied mathematicians and as an engineered artefact, will not suffice. The new machinery, both its potential uses and the philosophical questions raised, attracted wide interest; and not all from an ill-informed public dazzled by 'giant electronic brains'.

A new phenomenon is perceived, explained, and assimilated by analogy with what is already known. A restricted range of analogues, a poverty of models, may constrain the understanding that is eventually achieved. When, through familiarity, the computer ceases to be 'a sort of...' and is just an everyday object, the place of that object in our world may depend on the route by which we have come to understand it.

In the 1940s and 50s there were other models of computing and automated machinery, analogies that took their inspiration not from engineering, physics, or mathematics, but from biology; from empirical observation rather than *a priori* reasoning; from a study of creatures in the world, doing and being done by, rather than commanded by instructions in code. This approach retains a following, indeed it shows signs of renewed interest, but it is peripheral to what presently constitutes computing science; cognitive science is, perhaps, more a sub-field of psychology than computing. And new fields such as bio-informatics are certainly not in the tradition of biology:

rather, they show the adoption (some might say usurpation) of the reductive and discrete traditions of physics by the life sciences.

If we talk of an enclosure of computing science, of its assimilation into a predominant scientism—reductive, mechanical, the purity of its science assayed by the density of its mathematical content—then the traditions of natural history are not the only exclusion. There was, for example, from the beginning, a significant interest in computing applied to natural language. This has had less lasting influence; unlike genetics and molecular biology the enthusiasm for machine translation did not lead to a notable success for a mathematico-logical approach. But what of a counter influence? At the time of the Teddington symposium, there were significant developments in programming languages, Fortran, Cobol, Algol, and Lisp, all represent significant strands in the move toward high-level languages and conceptions of a virtual machine. That is, a machine instantiated by an effort of imagination. Yet though we talk of programming *languages*, the analogy of software-as-fiction appears to have attracted little attention. By and large, we study the language of programs in a manner which the experience of machine translation might have led us to distrust. Though programs are *written*, and the text enacted, the analogy of a *literature* of programs seems to have attracted only passing attention.

In 1958 then, there was an open field: computing as an intellectual challenge knew no disciplinary boundaries. Within ten years, there was a distinct discipline of computing science, yet one that seems to lack a 'cognitive content' of its own. A study of computing machinery is engineering, a study of algorithms, mathematics: what is unique to computer science? The answer I suggest is software, but not as the coding of an algorithm, nor the specification of a virtual machine; outside the field enclosed by engineering and mathematics, there is not only a natural history but also a humanist perspective: a study of the virtual worlds conjured up by programming.

In chapter one we will consider the problem of what we see when we encounter a thing for the first time. In chapter two, the origin of the deep cultural bias that favours the abstract, the 'useless' and mathematical knowledge. The third chapter looks at the computing and automation in the biological tradition. Chapter four is an account of Autonomics at NPL, a research programme that owes much to that biological influence. Examining the context of one project in that programme, machine translation, the dominance of a mathematico-logical model is evident. Finally, in a contrasting view of the nature of language, chapter six attempts to trace the gradual coming into consciousness of programming as a literary activity.

This work thus falls into three parts. The first two chapters are concerned with theory, a history of ways of seeing. The second and major part, which extends from chapter three into chapter five, is a history of a particular perception of computing and computers. The final part, taking chapter five forward into six, retraces that history of computing to detect the emergence of yet another perception, one whose status—mainstream or meander—is as yet undecided.

Part I

Guildenstern: A man breaking his journey between one place and another at a third place of no name, character, population or significance, sees a unicorn cross his path and disappear. That in itself is startling, but there are precedents for mystical encounters of various kinds, or to be less extreme, a choice of persuasions to put it down to fancy; until—"My God," says a second man, "I must be dreaming, I thought I saw a unicorn". At which point, a dimension is added that makes the experience as alarming as it will ever be. A third witness, you understand, adds no further dimension but only spreads it thinner, and a fourth thinner still, and the more witnesses there are the thinner it gets and the more reasonable it becomes until it is as thin as reality, the name we give to common experience. … "Look, look!" recites the crowd, "A horse with an arrow in its forehead! It must have been mistaken for a deer."

Tom Stoppard *Rosencrantz and Guildenstern are Dead*

Chapter 1. *Prolepsis* or, What is a Programme?

Commencing a lecture on *Faraday's Lines of Force* in 1873, James Clerk Maxwell invited his audience to look at a phenomenon at once both familiar and yet mysterious in the singular, and at that time still unorthodox, mode devised by Faraday. He forewarned his listeners:

> In order to do so I must have the indispensable help of your own best powers of thought. If I had merely to describe to you some new discovery in science, I should be able to avail myself of your previous knowledge as a foundation, and to erect thereon a representation of the new fact which you were to place beside those old ones which you knew before. The greater your previous knowledge, the easier would be my task. But what I have to do is something quite different. I have to shew you the facts with which you are already acquainted in a light of a different character from that which the most illustrious philosophers have shed upon them—a light which the wisest among them would probably have avoided as deceptive and misleading had it been presented to him in his own time, because the slow yet steady progress of science has only in more recent times prepared us for its reception.[1]

Maxwell reminds his audience of the counter-intuitive notions of Newtonian theory: objects do not move only if pushed, our motion is not always accompanied by the sensation of motion. "It is impossible to overestimate the influence which the experience of smooth sailing has had on the minds of men in enabling them to get rid of these habits of thought."[2] In this way understanding depends on general concepts and experience that may not be universal. Faraday, self taught, had a limited knowledge of mathematics and thus insulated from the mathematical treatises of Poisson and Ampère "was obliged to explain the phenomena to himself by a symbolism which he could understand, instead of adopting what had hitherto been the tongue of the learned."[3] Maxwell's influence and contribution to the mathematicising and quantifying tradition of physical science—that worldview in which a computing machine is a desideratum—will be considered in a later chapter, here my concern is the problem of the understanding or even merely the perception, of phenomena encountered for the first time.

Another instance is presented by Umberto Eco:

> Often, when faced with an unknown phenomenon, we react by approximation: we seek that scrap of content, already present in our encyclopaedia, which for better or worse seems to account for the new fact. A classic example of this process is to be found in Marco Polo, who saw what we now realise were rhinoceroses on Java. Although he had never seen such animals before, by analogy with other known animals he was able to distinguish the body, the four feet, and the horn. Since his

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[1] *The Scientific Letters and Papers of James Clerk Maxwell vol. II 1862-1873*. (ed. PM Harman) Cambridge 1995 p792

[2] *ibid.* p793

[3] *ibid.* p802

> culture provided him with the notion of a unicorn—a quadruped with a horn on its forehead, to be precise—he designated those animals as unicorns. Then, as he was an honest and meticulous chronicler, he hastened to tell us that these unicorns were rather strange—not very good examples of the species, we might say—given that they were not white and slender but had "the hair of the buffalo" and feet "like the feet of an elephant". [4]

Rather than add to a catalogue of the vivid, Marco Polo modified the bestiary that he and his audience already held in common: he "corrected the contemporary description of unicorns, so that if they existed, they would be as he saw them and not as the legend described them."[5] Eco develops his theme by considering the duck-billed platypus; a 'water mole' to the first Australian settlers it took over eighty years to create a secure place for it in a taxonomy that had never envisioned possibilities other than mammal, bird or reptile. Such technical disputations contribute to the shaping of a discipline. The platypus "had set itself athwart the path of taxonomy to prove its fallaciousness."[6]

Now, is this just a problem of composing a description or illustration in terms that will make sense to someone who was not there, or is it the perception of the witness that is constrained by experience? Did they actually *see* differently? Vision is only the plainest example of something far broader as Maxwell's example shows. And what is the relation between the name of a thing and the object itself, is it just another associated quality? Or should the label attached to an object have a special status? Does it 'belong' to the object or the thinker? Again, what is the relation between language and thinking: are they mutually dependent, an autocorrelation, or a trick of our perceptions?[7]

What events command the most attention in a history is dependent on the culture of a particular society, and the resulting history, by reworking and contributing to that culture, feeds forward. It is the result of complex interactions of not always conscious motives—what follows may have unintended consequences. The world we know constantly changes and our understanding with it, made in our own image. In appropriating the ideas of the past for our own purposes we may refashion them to present purposes, but

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[4] Umberto Eco. *Kant and the Platypus*. Secker & Warburg 1999 p57

[5] *ibid.* p58. Albrecht Dürer's famous illustration of *The Rhinoceros* (1515) might also be cited: it is not a deception but an effort of imagination, fabricating from forms that were known and thereby possible to imagine.

[6] *ibid.* p250 A quotation of a remark made by René-Primevère Lesson in 1839.

[7] According to Aristotle: "Spoken words are the symbols of mental experience and written words are the symbols of spoken words. Just as all men have not the same writing, so all men have not the same speech sounds, but the mental experiences, which these directly symbolise, are the same for all, as also are those things of which our experiences are the images." [quoted in LS Stebbing 'Sounds, Shapes , and Words' Aristotelian Society *Supplementary Volume XIV* 1935] p1

a history must needs account for a double relativity: how things appeared to their own time and now, and how that time appears now.

This concern with what is made by the mind of man rather than nature, is not new. Anticipations can be found in Giambista Vico's (1668–1744) *Scienza Nuova*, published in 1725. But the influence is indirect, with later thinkers picking from fragments of his sometimes confused ideas. For the most part the 'New Science' is "unreadable and unread"[8]. In the history I will present here—of a time when there were computers but not yet a 'computing science'—the process of understanding a wholly new thing, yet always in terms of what is known, will assume an especial prominence. Thus it will be helpful to first examine some variations of this 'social constructivist' theme; in particular to define what I mean by *prolepsis*.

Prolepsis occurs in early English as a term of rhetoric: an introductory summary, in particular the anticipation and answering of potential objections to an argument. In the nineteenth century it also became a grammatical term for the anticipatory use of adjectives. Renaissance classicism had recovered another sense, more appropriate to the present purpose: an anticipation, the representation of a future state as something already existing, a presupposed idea[9]. But for the sense I intend here, the word's Greek origin, in particular its place in the philosophy of Epicurus, is worth recovering. Perhaps the clearest exposition is that of Cyril Bailey:[10]

> This general concept Epicurus named the 'anticipation' (προλήψιζ)[11], because it enables us to anticipate the appearance of anything for which we may be looking, or which we desire to construct. The 'anticipation' is not, like the sensation, 'true', because it does not correspond to any existing reality, but it has derived truth, because it is founded on a series of sensations. It is the 'anticipation' which enables the mind to perform

[8] Isaiah Berlin. *Vico and Herder*. Hogarth Press 1976 p95

"...he conceived of mathematics as the invention of fictions, as an art or game like chess, not as a descriptive procedure, or system of tautologies." *op cit* p88

[9] See the various editions of the *Oxford English Dictionary.*

[10] Bailey writing in 1928 notes with regard to Epicurus' attitude to the soul, in particular its perishing with the body: "it is almost impossible for us to approach the whole question in a sufficiently materialistic mood". Bailey's attitude may be taken as a useful indication of the difficulty a materialist conception of the mind would pose to an educated, albeit non-scientific, audience of the early twentieth-century. (See Cyril Bailey. The Greek Atomists and Epicurus. Oxford 1928 p384)

It may also be noted that Julia Annas in her conclusion to *Hellenistic Philosophy of Mind* (California 1992), also indicates a circumspect view of materialist philosophies of mind "If this [eliminative materialism] is a lasting trend, then it may be that the modern context for discussing the mind turns out not to be so very dissimilar from the ancient one." (p210)

[11] *prolepsis* [pi rho omicron lambda eta psi iota sigma]

> the act of cognition by referring the sensation to its appropriate concept and to reason by means of the combination of concepts.[12]
>
> [...]
>
> Lucretius does not translate προλήψιζ but uses *notities* as an exact equivalent in passages such as:-
>
> *pratera si non alii quoque vocibus usi*
>
> *inter se fuerant, unde insita notities est*
>
> *utilitatis et unde data est huic prima potestas,*
>
> *quid vellet facere ut sciret animoque videret?*[13]
>
> elsewhere it has the more general sense of concept.[14]

Any detailed discussion of what Epicurus had in mind is doubly difficult. Not only are there the usual problems in determining the thought of any thinker: differences of language, cultural and historical reference, revisions and inconsistencies; in the case of Epicurus very little survives. Though the work of Epicurus was found among the badly damaged scrolls discovered at the ruins of Herculaneum in the eighteenth-century, much was destroyed by impatient and unsuccessful efforts of recovery. The long poem of Lucretius, *de rerum natura*, which is believed to follow closely the *On Nature* of Epicurus, is the most extensive evidence we have.[15]

Some of his ideas were developments of the earlier Greek atomists, in particular Democritus to whom what was known to the mind were the sensations given by phenomena. These phenomena derived from a material world of atoms and void, so that all knowledge was ultimately, but only indirectly, that of the atoms and a void. This, it would seem, led Democritus to attach particular importance to the sensation of touch, it alone provided contact with the real qualities of matter. Sight was indirect: colour, and even size and shape, were only indirect and imperfect apprehensions. This distrust of sight is atypical of Greek thought, and the tendency to cast metaphors of knowing and understanding in terms of *seeing* persists to this day.

> ...Democritus was neither a sceptic nor a rationalist, nor a phenomenalist, he does not fit into any of the modern categories; he neither denied nor affirmed the truth of all sensation nor of all thought;

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[12] Cyril Bailey. *Prolegomena* to Lucretius. *de rerum natura*. OUP 1947 p53

[13] In translation: "how could any one man have had a *prolepsis* of language and so invented it, if he had not already heard men speaking?" Lucretius. *de rerum natura* V 1046-9

[14] Bailey *op cit* p54

[15] For an account of the discovery and attempted recovery of the scrolls found at Herculaneum and discussion of the fidelity of *de rerum natura* to Epicurus see D.Sedley, *Lucretius and the transformation of Greek Wisdom*. Cambridge 1998. Also see DH.Roberts *et al. Classical Closure*. Princeton 1997.

> but built up for himself a 'theory of knowledge', subtle and almost paradoxical, but based directly on his atomic conception of the world.[16]

In the natural world Democritus established 'necessity' as a controlling force, both divine intervention and chance were thereby excluded. But in the world of human action a place is left for chance. It would seem that just as sensation formed a bridge between the fundamental nature of the atomic world and human knowledge so, implicit in what we know of Democritus, was a disassociation of human thought and consequent actions from an absolute material determinism. This apparent paradox was, it appears, not confronted by Democritus; it was however of concern to Epicurus.

To Epicurus the world appeared a *concilium*, an organism of interdependent parts. It is not the totality of atoms and void but an 'enclosure (*περιοχή*) of sky'.[17] Thus the world we inhabit, the only one we know, is not unique. It has qualities that suggest self-organisation and a certain independence from the universe of atoms and void; qualities by which, locally at least, an account may be given of free will. Local self-organisation permits the formation of a *nomos* that may be more complex than the *physis* that governs the atomic world. Yet at the same time subject to chance in the sense of perturbation from outside the 'enclosure'.

To read the Hellenistic philosophers in this light is perhaps to over-anticipate not only present theories but the premonitions of the early cyberneticists[18]. There is the beginning of a concept of causality that avoids the 'view from nowhere'; not only do the gods take no interest in the world, but the human view must avoid the supposition of a possible 'gods' eye view'. The Epicurean distinction between rational and irrational parts of the soul stresses, in a way that may seem counter-intuitive to the modern mind, the irrational part. There is a sense that Epicurus was groping uncertainly toward a concept of organisation and coherent form as a thing in itself. Like the cyberneticists (of whom we will have much to say in a latter chapter) there is a willingness to consider human behaviour and feeling as arising out of the body without involving a deliberative consciousness.[19]

> For Epicurus, deliberation or selection at the moment of action is not stressed. Rather, action is the product of the world's impinging on an agent who already has a character and reactive capacities of a certain

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[16] Cyril Bailey. *The Greek Atomists and Epicurus*. Oxford 1928 p360

[17] *ibid.*

[18] On the relation of the cyberneticists of the 1940s to more recent thought, and the absence of phenomenology in the development of their ideas see J-P Dupuy. *The Mechanization of the Mind*. Princeton 2000

[19] Though a cell-like world within an atomic universe can provide the environment that reconciles chance, necessity and free-will, there remains a problem of the 'first mover'. The Epicurean solution is the 'swerve' or *clinamen*—it is unclear if this is to be a single initiating event or a permanent condition among atoms and void, but ultimately it does not matter; one kick start is sufficient.

> kind. As with the Stoics, the answer to 'Why did you do that' will be given by reference to two factors. One is the way the world appeared to me at the time; the other is my overall state at that time, a state resulting from past choices and present endorsement of past development.[20]

Besides the sense of historical process to be found in Epicurean *prolepsis* there is another theme for which Lucretius, as quoted above, may serve as introduction: the relation of thought to language, both the necessity and impossibility of setting one apart from the other in any discussion of the nature of ideas. Once again, Bailey offers a clear exposition:

> Epicurus wished his philosophy to represent the common sense of the 'plain man' ; he therefore eschewed all metaphysical subtleties and had contempt for the dialectic or logic of Plato and Aristotle. But nevertheless he found it necessary to have certain rules of procedure in investigation, which he summed up under the head of Canonice. As a preliminary Epicurus insists that care must be taken in the use of words, which should always be employed in their first and natural meaning, that is, in accordance with the first visual image which the word suggests to the mind, so that no further explanation is necessary.[21]

Thus it is difficult to describe something, or at least to hold it securely in mind, even to oneself, without having a word for it. But words, as labels for objects, can distort and mislead; "the first and natural meaning" is not universal. Hence the dilemma: to identify something securely by giving it a new name or risk misunderstanding by using existing words in a new context. And how to convey the meaning of the new word, even more so a new idea, except in terms of what is already known. A vocabulary is necessary, not merely to describe, but to manipulate ideas. [22]

It is this sense of *prolepsis* to which I wish to direct attention: the idea that a thing can only be recognised in terms of what is already known. It is a development of concepts—of mental imagery—by something like a Markov Process; the next step is always built upon the present state and every state interdependent. The Markov principle is something of an idealisation, it is possible to consider more elaborate systems in which the historical reference is more than one level deep. But its essence is that nothing is added or taken away, a system of self-referential development. This is not quite the *feedback* of the cyberneticists, the whole process is better conceived as occurring in a continuous present, the teleological effect inheres in pre-existing probabilities rather than a corrective flow of information. It is a process that is simultaneously reactive and adaptive.[23]

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[20] Julia E Annas. *Hellenistic Philosophy of Mind*. California 1992 p181

[21] Cyril Bailey. *Prolegomena to* Lucretius. *de rerum natura*. OUP 1947

[22] I would make a distinction however between a language of thought that is necessary in order to think consciously about and communicate ideas and the further assumption that thought processes must be a processing of language.

[23] To invoke the shade of James Clerk Maxwell again, we might compare his dynamical explanation of the mechanism of the Watt governor with a naïve

But, if all ideas are old ideas, what justification is there to revive an old word? Would not a present one do? *Anticipation* is unsatisfactory, it implies not only the future already exists but in general use has also connotations of excitement and a teleological activism. *Preconception* carries suggestions of a "false consciousness" or prejudice; the implication that, with hindsight, the incompleteness and error of current thinking will be clear. *Notion* is just too scanty and implies the ready availability of alternatives.

If the plain man's words seem somehow inadequate will sophistry improve things: Bacon's *idola* of cave and forum, or that overworked *paradigm* of Kuhn?

Francis Bacon's 'Great Renewal' can be seen as marking a transition from a Renaissance to Modern sensibility. The title of the first stage of this ambitious project, *Novum Organon* (1620)[24], refers back to the philosophy of Aristotle that dominated the middle ages, yet it is also a move forward from renaissance neo-Platonism, toward a new synthesis of experiment and reason. Bacon felt a need for a re-foundation of understanding; the first notions of things[25], accumulated over ages, had become confused, to apply logical reasoning or innate wit to such material could only build error upon error. Received wisdom was no longer a satisfactory basis for knowledge; true to his training as a lawyer, he insisted that the evidence must be tested.[26] Nature remains personified in Bacon's writing, the reasoning to be applied is forensic rather than an abstract logic. In introducing his plan for a recasting of *science*—that is human knowledge in general rather than just an empirical and natural philosophy—Bacon observes that "the division of the sciences which we employ include not only things which have been noticed and discovered but also things that until now have been missed but should be there."[27]

Bacon rejects the syllogism as the basis of reasoning because its apparent mathematical certainty is ill-founded. It is based on words which are "counters and signs of notions."[28] If these words are not clear and precise then the whole rationalising edifice crumbles. The alternative offered is an inductive method, to progressively refine general concepts, and implicitly the meaning of words, from experience. So, in contrast to the Epicurean method,

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explanation in terms of cycles of measurement and correction. See Timothy van Gelder. 'Dynamics and Cognition' in *Mind Design II* (ed. John Haugeland) MIT 1996

[24] Bacon *Novum Organon* London: 1620; quotations and references are to the translation by Michael Silverthorne. *The New Organon*. Cambridge 2000

[25] "the first notions of things which the mind accepts, keeps and accumulates (and which are the source of everything else), are faulty and confused and abstracted from things without care" *New Organon* p2

[26] John Aubrey's *Brief Lives* attributes to William Harvey the much quoted remark that Bacon wrote philosophy like a Lord Chancellor.

[27] Bacon. *Novum Organon* p15

[28] Bacon *op cit* p16

the first meaning of a word evoked by a sense impression cannot be trusted.[29] Like a legal process: a contract must first define the meaning of key terms, a testimony must be questioned.

So it is that the process of discovering the real nature of things must first rid the mind of *idola*, those illusions that distort or obscure a clear understanding. The intellect is considered an unreliable interpreter of raw sensations which, if not themselves wholly reliable, are nonetheless the only portal we possess onto the natural world.[30] In addition to the deficiencies of the individual mind as a conduit of sensation further *idola* are generated by its function as an instrument of reason. Thought processes can be erroneous and, distributed as a common culture and language, become of themselves obstacles to truth. Thus Bacon proposes a threefold refutation: of philosophies, of proofs and of natural human reason.[31] It is necessary to "take in images exactly as they are". The causal chain of nature is absolute, but our reason is wanting an exact image of its links.

For Bacon there is a distinction to be made between argumentation *about* nature and the discovery and perception *of* nature. The first is the way of the sophist to whom logic and reason are but a mental exercise, they build illusion upon illusion. He ascribes to the earlier Greek philosophers a moderate scepticism and curiosity about nature, yet they failed to engage directly with nature preferring speculation and imagination. Tools and machines will be necessary for a proper investigation of nature. Thus we might attribute to Bacon an early move toward the 'two cultures': between demonstrators and disputants, between the adornment of discourses and the conquest of nature. "Let there be two sources of learning therefore, and two means of dissemination... let there be one method for cultivating the sciences and a different method for discovering them."[32]

Bacon's new method of *induction* is to be based on experience. This new philosophy will not leap from particular instance to generalisation but gradually work up to abstractions derived from observation. It will not anticipate nature but interpret it. Consequently before observation can begin the old anticipations, false generalisations that derive their credibility from familiar and commonplace observations, must be set aside. These *idola* are of four categories, which may be identified in more modern terms as physiological, psychological, social and philosophical.[33]

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[29] As Epicurus did not hold logic in such high regard, the dangers of proceeding from immediate sensation to verbal reasoning was not at issue in his system.

[30] Bacon *op cit* p18

[31] *ibid.* p19

[32] *ibid.* p30

[33] *Novum Organum* Bk1 Aphorism XXXIX.

The first (*idola* of the tribe) are rooted in the limitations of the human species itself. Bacon, like Protagoras two millennia before,[34] believes perceptions are relative to man not the universe.[35] Protagoras was ambiguous in his pronouncement, it remains unclear if he meant the individual or the species. Bacon however is clear; in addition to the limitations of our senses as a mirror of nature there are also the psychological limitations of the individual man — the *idola* of the cave. In these both nature and nurture have a part, individual disposition, education and the events of life, make the preoccupations and prejudices of each person unique. With a reference to Heraclitus he notes that people prefer to hold to their own opinion rather than accept a universal knowledge[36].

Yet if there is no universal knowledge nonetheless people do share (often erroneous) opinions. Belief can be founded upon hearsay: a poor choice of words, or the pedantry of the learned, both lead to the *idola* of the marketplace. Social constructivism is no new thing—“Plainly words do violence to the understanding, and confuse everything; and betray men into countless empty disputes and fictions.”[37] Such ideas when traded become abstract entities, objects of study in their own right; philosophies are likened to plays, creating false and fictitious worlds—the *idola* of the theatre.

Human understanding , say Bacon, craves to find order even if it has to be invented, gives greater credence to the positive than the negative instance, is most impressed by what is most apparent, and cannot comprehend infinity or lack of cause. All these contribute not only to the creation of philosophies but also the axioms and principles of sciences. There are minds too ready to discern differences and others inclined to see similarities where there are none, both are grasping at imaginary shadows. But the greatest of errors are the consequence of the use of words. “For men believe that their reason controls words.”[38] Sophistry, an over-refined use of words, sets ordinary meaning in opposition to precision. Learned disputes degenerate into arguments about words rather than matters of real substance. Bacon sees the desirability, as later Leibniz did, to give words the precision of mathematics, but it is not possible, ‘words beget words’ in an endless regress. There is no direct relation between words and objects. Words may be given to non-

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[34] Man is the measure of all things—of things that are, that they are, and of things that are not, that they are not. (Protagoras, DK 80B1)

[35] Bacon *op cit* Bk1 Aphorism XLI

[36] The fragment from Heraclitus is presumably DK22B1 “...For though all things happen in accordance with this *logos*, people are like the inexperienced when they experience such words and deeds as I set out, distinguishing each in accordance with its nature and saying how it is.” or DK22B2: “But although the *logos* is common, most people live as if they had their own private understanding.” [translation: McKirahan (1994)]

[37] Bacon *op cit* Bk1 Aphorism XLIII

[38] Bacon *op cit* Bk1 Aphorism LIX

existent things which thereby become real objects in fantasy. Or, applied in an undiscriminating way, confound unrelated concepts.

This 'Sophistic' error is attributed to Aristotle; unlike the earlier Greek philosophers who had an interest in nature, Aristotle was too much concerned with dialectic. "He was always more concerned with how one might explain oneself in replying, and to giving some positive response in words, than of the internal truth of things."[39] He had made up his mind first and distorted experience to suit his opinion. But if sophistry is to be distrusted so too must too great a reliance on empiricism. Bacon has the alchemists in mind when he warns of the dogmas and distorted view of the world that comes from concentrating all attention on but a few experiments and narrow objectives. Commonplace notions even if vague and ill-founded do at least have a common relevance. Overspecialisation was predicted.

> We already conceive and foresee that, if ever men take heed of our advice and seriously devote themselves to experience (having said goodbye to the sophistic doctrines), then this philosophy will at last be generally dangerous, because of the mind's premature and precipitate haste, and its leaping or flying to general statements and principles of things; even now we should be facing this problem.[40]

The third error of philosophy began with Pythagoras, and was advanced by Plato and all his followers; it is the folly of idealism, of founding fantastic theories upon abstractions. High mindedness and intellectual ambition can be as corrosive as a desire for worldly power. If Aristotle was guilty of being dogmatic, of being a model for the vicious disputation of his Scholastic successors, Plato's fault was rather to have set truth as something unobtainable and thus turned men's minds to unproductive and tentative speculation. And thereafter came the neo-Platonists who corrupted philosophy with mathematics. A systematic science, as it is refined by observation, may approach the precision of mathematical axioms but it should not presume mathematics as the foundation of things.[41] The teachers of science have erred in presenting systems as complete, unable to admit the provisional nature of their understanding.[42]

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[39] Bacon *op cit* Bk1 Aphorism LXIII

[40] Bacon *op cit* Bk1 Aphorism LXIV

[41] See Bacon *op cit* Book 2 Aphorism VIII. Thomas Fowler in his edition of 1884 regards Bacon as having, albeit imperfectly, anticipated the idea of the perfection of a science as being a consequence of its closeness to mathematics. However this appears to me to ignore Bacon's strictures against the Platonists and his concern to balance the empirical and ideal. We will refer to this remark of Fowler further in chapter 3 in the context of the character of nineteenth century science and education.

[42] See Book 1 Aphorism LXXXVI, Bacon praises the pre-Socratics for setting forth their knowledge in the form of aphorisms and not claiming completeness. This may of course be an illusion based upon the very fragmentary survival of their ideas.

There must therefore be a more balanced way. After the pre-Socratic era philosophy took another turn, the interest in natural phenomena gave way to rhetoric and the self promotion of schools. Ideas, unlike knowledge derived from nature, do not increase but merely change. By contrast mechanical arts are constantly refined. But there is a prejudice against these mechanical arts

> ...a certain opinion or judgement, which is deeply ingrained but arrogant and harmful, namely that the majesty of the human mind is diminished if it is too long and too deeply involved with experiments, and with particular things which are subject to the senses and bounded by matter: especially as such things tend to be laborious to investigate, ignoble to think about, crude to speak of, illiberal in practice, infinite in number and short on subtlety.[43]

A new set of axioms is required, based upon experiment and nature rather than verbal reasoning, the truth of these axioms must be demonstrable. Yet there is a problem in this new foundation: "For men are accustomed to divine the new by the example of the old, and by imagination schooled and stained by the old".[44] Examples of the recent technologies are cited: the invention of cannon, the discovery of silk, the compass, printing. These things have, in Bacon's view, no analogue in things already known, no prior conception (*praenotio*) could have led to them. But in Bacon's new method, 'written experience', a systematic recording and sorting of facts and observations may, it is hoped, reveal such inventions which appear so obvious with hindsight. [45] In finding new knowledge false preconceptions of new things may be as misleading as old prejudices. In the second part of *Novum Organum* Bacon set out his system for 'knowledge engineering', a method of generating and analysing the data which will (with an optimism still to be found today) lead to knowledge without preconception.

In conclusion, Bacon's *idola* are an instance of what I will term *prolepsis* but I take a more positive (or at least inescapable) view of preconception and prior notion. The computer has made possible a greater accumulation and sifting of data than Bacon could imagine, but it has also shown how limited is its usefulness of itself. It will be the argument here that *idola* are a necessary part of knowledge rather than a fog that obscures it.

Though Thomas Kuhn's *Structure of Scientific Revolutions* must be held responsible for the contemporary excess of 'paradigms' *Kuhn's* work is perhaps less original than is sometimes supposed. In 1969 Quentin Skinner, writing in

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[43] Bacon *op cit* Bk1 Aphorism LXXXIII

[44] Bacon *op cit* Bk1 Aphorism CIX

[45] Thus, like Leibniz's 'let us calculate', another example of what seems a good idea until computers make it a possibility, Bacon's *organum* is a proposal with promises not unlike those of data-warehousing, if only enough information can be accumulated and processed we may 'drill down' to original discoveries.

History and Theory[46] mentions Kuhn only in a footnote, "the vocabulary of 'paradigms'" is credited to EH Gombrich's *Art and Illusion* (1960). In this work, Gombrich turns to psychology (in particular the Gestalt school) to expand the observation first made in *The Story of Art* (1950) that the Egyptians painted "what they knew" whereas the impressionists painted "what they saw". Gombrich, stresses the importance of sorting and categorising over association. Not logical but exploratory; "the activity of a living organism that never ceases probing and testing its environment."[47]

In the same paper Skinner identifies a 'myth of prolepsis': a form of anachronism in which an account of a past action or expression of ideas is described in terms which could not have been applied at the time.[48] Prolepsis as used here has only a restricted sense yet the term could, I believe, apply more widely to his argument. Thus "We must classify in order to understand, and we can only classify the unfamiliar in terms of the familiar."[49] and "crediting a writer with a meaning he could not have intended to convey, since that meaning was not available to him"[50]. In the worst case, as with any teleological explanation, "the action has to await the future to await its meaning." A political historian, concerned with anachronism in the history of ideas, his discussion concerns the respective merits of 'text' or 'context', in interpreting predominantly documentary evidence. Are there such things as 'timeless elements' and what are the errors attendant when they are "hypostatised into an entity"?[51] Both philological and contextual methods are found to be unsatisfactory—history and philosophy pose perennial questions but there are not perennial answers. It is the translator's dilemma, cultural, historical and linguistic; the incompatibility of accuracy and intelligibility when evidence has to be presented; even individual authors may hold inconsistent beliefs from time to time.

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[46] Quentin Skinner. 'Meaning and Understanding in the History of Ideas', *History and Theory* vol.VIII 1969 (p7, footnote 17)

[47] EH Gombrich. *Art and Illusion*. Phaidon (3e 1968) p23

[48] *ibid*. p22 "...the mythology of prolepsis is the conflation of the necessary asymmetry between the significance an observer may justifiably claim to find in a given statement or other action, and the meaning of that act itself." A review and extension of the debate instituted by Skinner can be found in Nick Jardine. 'Uses and Abuses of Anachronism in the History of the Sciences', *History of Science* xxxviii(2000) p251-270

[49] *ibid*. p5

[50] *ibid*. p9 and on p18 "The history thus written becomes a history not of ideas at all, but of abstractions: a history of thoughts which no one ever actually succeeded in thinking, at a level of coherence which no one ever actually attained."

[51] *ibid* p9 also termed "the reification of doctrines"

An influence acknowledged by Kuhn, Ludwik Fleck's *Genesis and Development of a Scientific Fact* (1935),[52] also has roots in Gestalt psychology. Regardless of the extent of its indirect influence,[53] Fleck's presentation of the process of scientific advance is of some intrinsic interest to our present argument. Ostensibly a history of the recognition, classification, diagnosis and treatment of syphilis, the substantive thesis is sociological: how consensus and individual opinion influence the formation of concepts. A 'disease' is not a physical state nor an agent, but a classification, a constellation of observations and supposed causes. The key idea is of a *Denkstil*, (thought style) and *Denkkollektiv*, which, with some reservations, may be rendered in English as 'thought collective':

> a community of persons mutually exchanging ideas or maintaining intellectual interaction, we will find by implication that it also provides the special 'carrier' for the historical development of any field of thought, as well as for the given stock of knowledge and level of culture.[54]

It carries a stronger sense of inescapable commonality and acceptance of ideas than 'school of thought' or 'scientific community' might suggest. The association of ideas, explanation in terms of causation, "can survive and develop within a given society only if this explanation is stylised in conformity with the prevailing thought style."[55] This *Denkstil* has a historical dimension, ideas have precursors, misconceptions descend to the present. Concepts, the presentation of problems, the syllabus of formal education, the tacit knowledge of apprenticeship, language and institutions: all assist in maintaining this continuity of thought. The direction of research is constrained by a propensity to retain established concepts: "what does not fit into the system remains unseen".[56] There is no formal relation between concept and evidence, they are not, despite the aspiration of the positivists, a logical system. They are like social structures "every age has its own dominant conceptions, as well as remnants of past ones and rudiments of those of the future."[57]

Fleck notes that the aspirations of the Vienna Circle for an ideal construal of human thinking—as something fixed and absolute—is ill founded. There is no objective thinking free from emotions. What is graced by the name of 'rational' is not emotionless, but impersonal in the sense that it conforms to

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[52] Ludwik Fleck *Entstehung und Entwicklung einer wissenshaftlichen Tatsache* Basel: Schwabe & Co. 1935. The first English translation by TJ Trenn and F Bradley (University of Chicago 1979) contains a foreword by Kuhn.

[53] The original edition was of 640 copies of which only 200 appear to have been sold (*op cit* preface xviii). The English translation of 1979 which appeared in paperback in 1981 and is still in print may be presumed to have reached a wider audience.

[54] Fleck *op cit* p39

[55] Fleck *op cit* p2

[56] Fleck *op cit* p27

[57] Fleck *op cit* p28

the 'average mood' of the *Denkkollektiv*.[58] It has an excessive and pious reverence for logic. In a passage that anticipates Kuhn's notion of 'incomensurability' Fleck says that truth is not relative or subjective in the usual sense but is determined by *Denkstil*. Within the same *Denkkollektiv* there is a consistent view of what is true, but between one *Denkstil* and another there is no equivalent thought. Truth is not a convention but either "an event in the history of thought" or, in context, a "stylised thought constraint".[59]

The Gestalt influence can be seen clearly in Fleck's conception of the function of language. Words are not names of objects but '*ideophones*', phonetic and mental equivalents of the experiences that coincide with them. Thus what comes into the mind with words is not a logical assignment but a "transference of experience". In this version of the source of language what is prime is not symbolism and the conventions of logic but the retention and manipulation of experience. Experience captured in language may be more readily remoulded to its environment than the 'real' events that gave rise to it. In this way human thought is more adaptable, subject to a more rapid evolution, than a physical environment. A 'mutation' in *Denkstil*, such as the transformation of physics in the 1920s by relativity and quantum mechanics, is rapid. "What just a few years ago was regarded as a natural event appears to us today as a complex of artefacts."[60]

For an idea to prosper it must remain close enough to accepted opinion.

> Only a classical theory with associated ideas which are plausible (rooted in a given era), closed (limited), and suitable for publication (stylistically relevant) has the strength to advance.[61]

Understanding is dependent upon what is already known, but a developing knowledge will also rework and renew the stock of knowledge. Knowledge is inescapably diexical, relative not only to known 'fact' but the cultural environment. As thoughts pass from one person to another they mutate, they feed back to the originators who see their own transmissions in a new light. By this social process ideas come to be 'in the air', not truly attributable to an isolated individual but collective. And research does not always lead to the intended goal, the route may be roundabout; in the case of Columbus, a search for a known 'India' found a new 'America'. New discoveries are not the result (*pace* Bacon) of tabulation and mechanical reason. The 'trained eye', the product of inculcation into a prevailing thought style, will find no contradiction; discovery requires a perturbation and eventual transformation of the *Denkstil*.

This holistic view of the world of thought is at one with his conception of the biological organism. It depends upon the purpose of the investigation whether

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[58] Fleck *op cit* p50

[59] Fleck *op cit* p100

[60] Fleck *op cit* p26

[61] Fleck *op cit* p30

it is appropriate to view a cell, the symbiosis of fungus and alga in the lichen, the ant, the ant colony, or the forest as a biological individual. A materialistic notion of a "self contained, independent unit with fixed boundaries" is a fiction. It is central to Fleck's thesis re 'disease' that there is no such individual agent, the concept is a remnant of mediæval notions of 'demons' as causative agents. In Fleck's view the body is not invaded by an infection but undergoes a complex revolution within.

An individual may belong to more than one *Denkkollektiv* though rarely in fields that adjoin. There are inner circles of specialists, experts, an outfield of educated amateurs and beyond that general knowledge. Specialised sciences build their concepts upon a common ground of popular science, it is a source of metaphor and analogy, characterised by a vivid simplification. Training in special sciences begins with 'textbook science' in which all hint of controversy or uncertainty is suppressed. For the practitioner a '*vademecum* science' organises knowledge into a system, it is an inscription of the *Denkstil*. By contrast 'journal science' is personal and tentative, a probing of the boundaries of the *Denkkollektiv*. Fleck provides a vivid and engaging metaphor of the progress of a developing scientific field.

> The relation between journal science and vademecum science shows up in modern progressive science as a characteristic structure of the esoteric circle. It resembles a column of troops on the march. Every discipline, in fact almost every problem, has its own vanguard, the group of research scientists working practically on a given problem. This is followed by the main body, the official community. Then come the somewhat disorganised stragglers. This structure becomes the more conspicuous the greater the progress in the field of investigation. Journal science, which comprises the latest work, becomes more or less removed from vademecum science, which always lags behind. The vanguard does not occupy a fixed position. It changes its quarters from day to day and even from hour to hour. The main body advances more slowly, changing its stand—often spasmodically—only after years or even decades. Its path does not closely follow that of any one of the vanguards. The main body adjusts its advance according to reports received from the vanguard, but maintains a certain independence. The direction that the main body chooses from the many suggested by the vanguards is always unpredictable. Paths must first be widened into roads, and the ground levelled, so that the terrain undergoes considerable change before it can become the garrison of the main body.[62]

Thus the marauding army of academia lives off the land. When the grove has been pillaged, the field reduced to a scholastic quagmire, the academy moves on. Ripe fields may be abandoned, new knowledge annexed, but always at the disputed boundary of the known world.

[62] Fleck *op cit* p124

Kuhn's 1962 preface acknowledges the influence of Fleck, "an essay that anticipates many of my own ideas"[63], though some 14 years later, contributing an introduction to the English translation he claims to be "almost totally uncertain"[64] what he took from Fleck's work. In contrast to Fleck, Kuhn's examples are drawn mainly from physics. He talks of the 'mature sciences', a term that suggests an adherence to a notion of progressive refinement and improvement of knowledge in sciences. Thus it would seem Kuhn is justified in rejecting the charges of relativism pressed by some of his critics; his 'science' is essentially the classical mathematical physics that achieved its full glory at the close of the nineteenth century. A 'revolution' in Kuhn's terms is more an instrument of progress than a cycle; an Enlightenment worldview, not—as the seventeenth century saw it—the world turned upside down.

In a 1969 postscript Kuhn admits that the term 'paradigm' had caused difficulty, a consequence of the multiple senses he had heaped upon it, and accepted it had two distinct variants[65]. One was the sociological dimension, a community of like minded people with a common indoctrination through education and training; sharing a worldview that would tend to constrain their interpretation of both theory and observation. This is certainly 'commensurable' to use another of Kuhn's terms, with *Denkkollektiv*, the *idola* of cave and forum, and *prolepsis* as set out above. The second sense of paradigm admitted by Kuhn is closer to the grammatical terminology, an example used as a pattern, a foundation for tacit learning by which a student acquires facility in a standard technique. It may be argued that this too can be included within the senses of *Denkkollektiv*, *idola* (of tribe and theatre?) and *prolepsis*. But what is of greater interest is how this sense seems to imply a commitment to the non-relativistic notion of science as being a matter of 'Laws', a 'mature science' progressing to ever closer union with the axiomatic certainties of mathematics. "Imagine what would happen in the sciences if consistency ceased to be a primary value."[66] What Kuhn seems to imply here is that although scientists may dispute interpretations and results a commitment to consistency is the 'essential tension' that makes science what

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[63] Thomas Kuhn. The Structure of Scientific Revolutions. Chicago 1962/1970 p vi

[64] Fleck *op cit* p ix

[65] Kuhn refers to Margaret Masterman's (in Lakatos & Musgrave 1970) identification of 21 (22 according to Kuhn) meanings of 'paradigm'. This dissecting of separate interpretations is somewhat unfair. We can employ the concept of prolepsis to elucidate this. If we suppose that in writing TSSR Kuhn concept of paradigm was, if not necessarily clear and distinct, nonetheless a heterogeneous constellation of notions then we may say he had a prolepsis of 'paradigm'. He knew what he wanted to say/describe and finding no existing term adequate, assigned a special and semi-private meaning to 'paradigm'. Then each of those 21/22 instances are not to be taken as distinct usages or individually as definitions. They are references to the same concept, mutually interdependent, they are synthetic rather than analytic, serving to bind the special meaning of 'paradigm' into a context of ordinary usage.

[66] Thomas Kuhn. *The Structure of Scientific Revolutions*. Chicago 1970 p186

it is. Without the arguments there could be no revolutions and consequently no progress, but periods of consolidation into consistency are the mark of a 'mature science'. Before, say, the time of Copernicus, there was no consistency only perpetual dispute between schools, a situation that did not bring progress.

There is therefore a difficulty, despite Kuhn's statement of interest in the sociological dimension and the popularity of his work outside the physical sciences, in applying his insights beyond the model of physics. The only biological science, mentioned in passing by Kuhn, is biochemistry; that is biology in a form that comes closest to the mathematics and physics model. The traditional biology of observation, classification, and interpretation is ignored. Implicitly it is not a 'mature science'. And this problem applies in even greater measure to the 'social sciences', where there is no boundary imposed by the constraints of the physical world. As both RH Coase and Herbert Simon have noted economists can create ideal worlds of mathematical certainties, but their models often do not fit the behaviour of real people.[67] Most notably such social models cannot hope to account for changes of behaviour brought about by knowledge of the models themselves.

In mid-twentieth century discussion of the philosophy and history of science the alternative most often countered to Kuhn's sociological view has probably been that of Karl Popper. To talk of two opposing schools is inaccurate, but both thinkers have been enormously influential, and their ideas have been the starting point both for elaboration and reappraisals. Much of that debate is presented in *Criticism and the Growth of Knowledge*, a series of essays, including contributions by Kuhn and Popper, that expands on presentations originally made in 1965.[68] In that volume Imre Lakatos succinctly summarises both the agreement and difference between them before proceeding to present a reworking of Popper's falsification principle that accommodates the view of science as a social activity that is characteristic of Kuhn. As Lakatos argues, there are problems with a naive view of falsification. It requires a clear distinction of theory and observation, and assumes that observations may have validity as logical terms. Only then can a speculative (i.e. theoretical) proposition be disproved by actual experiment. Both conditions require a *tabula rasa*, for the observer to have sensation free from expectation. If sensation and theory are inseparable to the human mind then these conditions fail. No experiment will ever be free from expectations prompted by the theory it is designed to refute. Fallible and theory laden, observations from experiment are not equivalent to logical propositions. Consequently, claims

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[67] See RH Coase. *The Firm, the Market, and the Law*. Chicago 1988, Herbert A Simon. *Models of Man: social and rational*. Wiley 1957

[68] Imre Lakatos. Alan Musgrave (eds) *Criticism and the Growth of Knowledge* (proceedings of the International Colloquium in the Philosophy of Science, London, 1965,vol 4). Cambridge 1970. "...it is a rational reconstruction and expansion rather than a faithful report of the actual discussion" p vii

Lakatos, a theory, taken as a set of logical propositions, cannot be refuted by experiment.[69]

Whereas Popper is concerned with the fate of theories Lakatos looks to 'research programmes' as the unit of scientific endeavour. A research programme is sustainable while there is still a yield of useful new knowledge. That is if confidence can be maintained that despite setbacks the core of theories and paradigms (in a sense later emphasised by Kuhn—a recipe or accepted method[70]) continue to work. A failure of a particular theory or experiment, or difficulties at a particular time, will not be decisive. "There are no such things as crucial experiments."[71] A research programme is maintained so long as it generates new content—it must explain new facts not merely offer reinterpretations of what is already known. The research programme of a mature science is described by Lakatos as having "heuristic power", it is not merely a collection of trial and error results but has a unity and consistency by which additional theories are generated from within. This process, unlike that posited by Kuhn is not primarily psychological, the programme develops according to an internal and rational standard. "'Crisis' is a psychological concept [...] in Kuhn's view scientific revolution is irrational, a matter for mob psychology."

A research programme preserves the rationality of science by supposing the progress of science to be driven by an independent and internal commitment to rationality and logic[72]. But this presupposes an abstract conception of scientific research. Research programmes occur not in isolation—as pure science—but in a social context, scientists are people too, they have ambitions and aspirations, their research programmes require funding. Hence the research programme is subject to a wider practical social and psychological constraint than that posed by experiment and theory. It may be abandoned if it fails but the criteria of failure are a matter of opinion rather than strict logic or experimental refutation.

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[69] As with much philosophy of science of this era, there is an implicit assumption of physics as the exemplar of science.

[70] See Kuhn 'Second Thoughts on Paradigms' (in *The Essential Tension*. Chicago 1977) where the terms 'disciplinary matrix' and paradigm are preferred. The first, which is global and includes the latter, may be considered at least a near relation of *Denkstil*, *idola*, or *habitus*. The second is subdivided into symbolic generalisation, models and exemplars. Paradigm in this restricted sense implies something more wilfully acquired and modified than *prolepsis*.

[71] Lakatos *op cit* p173

[72] Kuhn refers to this contrast between internal and external shaping in his essay 'The Relation between History and the History of Science' (in Kuhn. *The Essential Tension*. Chicago 1977). History of science, like the history of art or literature can be studied from without or within. Art is built upon past art. "Like scientists, philosophers, writers and musicians, they live and work both within a larger culture and within a quasi-independent disciplinary tradition of their own." p152

As Lakatos made clear in his introduction to *Falsification and the Methodology of Scientific Research Programmes*[73] both philosophies are a response to the profound changes in the science of physics in the past century—the 'falsification' or the revolutionary overthrow of the Newtonian 'paradigm'. "For centuries knowledge meant proven knowledge." Philosophical scepticism could not prevail against the palpable worldly success of the natural philosophy that became physics. But in the past century this absolute notion of truth has become less tenable. In its place notions of truth have been proposed based on probability, inference to best explanation, or sociological consensus. Popper's solution, as presented by Lakatos, is to be "bold in conjectures", that is accept as provisionally valid speculative and unproven theories, but to match this with an openness to reject theories when there is evidence that they are mistaken.

Both then, see science as progressing, albeit in an irregular manner, subject to revision and revolution, but in the case of Kuhn there is a more disjoint 'punctuated equilibrium'; normal science only admits its mistakes and errors reluctantly. In their focus on physics as the model for science and the emphasis on discontinuity as a salient feature of progress, these expositions are products of their time; in the twentieth century changes in the style and status of physics have had an influence beyond the physics journal. Relativity has become 'textbook science' and quantum mechanics qualified for the 'vademecum'.

An interesting and more recent variation on this theme is given by James McAllister in his *Beauty and Revolution in Science*.[74] The rationalist image of science, that of Popper and Lakatos, is challenged not only by a social cohesion among scientists that at various times may impede or accelerate the adoption of theories, there is also evidence of the influence of aesthetic judgements. McAllister argues that neither of these apparently irrational influences need be inconsistent with a rational conception of science.

Whereas Kuhn sees a revolution motivated by dissatisfaction with a theory grown ugly by too many piecemeal accommodations to awkward fact McAllister takes a contrary view; it is the established theory that has the aesthetic advantage. New theories are adopted reluctantly because although considered inelegant they yield better results. Only with familiarity does a new theory come to appear aesthetically pleasing.

The starting point is an account of the rationality of science. In both realist and instrumentalist conceptions theories are judged by their 'empirical adequacy'. That is the capacity of a theory to accord with observation, predict, be consistent and provide explanation without tautology. But the historical evidence contradicts this rationalist image.

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[73] *op cit* pp91-195

[74] James McAllister. *Beauty and Revolution in Science*. Cornell 1996

One response to this was provided by the logical positivists; a 'context of discovery' in which intuition and other irrational factors might play a part, was not to be confused with a 'context of justification' in which theory is validated by the application of logic and observation. Another is to define science narrowly; any aesthetic influence is a perturbation upon the true and objective path of science. But there is ample testimony from scientist's themselves that the beauty of theory is important and the division into two contexts but a fiction.

The dismissal of aesthetics—of beauty as a criterion—may be attributed to a tendency to regard beauty as a property of objects rather than perception. The contrary view is possible if beauty is regarded as projected onto objects by the observer—it is in the eye of the beholder. In the case of ideas certain qualities have, historically, been considered a mark of beauty: clarity, simplicity, symmetry, uniformity etc.

Scientific theories, being abstract entities, require representation if they are to be communicated and perceived. The properties of the representation should be considered distinct from those of the theory represented. How are abstract qualities to be perceived apart from their representation?

> What we have been calling the representation of an abstract entity such as a scientific theory should be regarded not as a depiction of the entity but as an algorithm for creating a mental replica of it: consulting a representation of an abstract entity enables me to replicate it in my mind. A person's knowledge of an abstract entity is thus gained by examining a mental replica of it rather than the concrete representation.[75]

It is, argues McAllister, the intrinsic properties of this replica that provoke an aesthetic response.

Aesthetic criteria form a canon largely shared by a community, in which each criterion can be considered to have a weighting. The evolution of a canon corresponds to a change in these weightings. McAllister identifies five classes of aesthetic qualities that have "profoundly influenced theory choice at least since the Renaissance."[76] These are: simplicity, symmetry, invocation of models, visualisation/abstraction, and metaphysical allegiance.

Working within a tradition applies to scientists just as to artists and poets. Models are subject to fashion, the mechanistic models of the nineteenth century no longer find favour with physicists. Biological models, as Norbert Wiener observed, have often followed the development of advanced machinery from clockwork to electronics.[77] There is a tendency to choose models that have been associated with empirical success in other fields. In forming and reforming a canon "scientific communities choose those [criteria]

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[75] McAllister. *op cit* p29

[76] *ibid.* p41

[77] Wiener. *Cybernetics.* 1948 p[?]

that would have been satisfied by their empirically most successful theories of the recent past."[78] Thus continuity and entrenchment is encouraged. Aesthetic judgements are a form of habituation, the continued success of a new but displeasing theory will lead over time to a re-weighting of the canon. Conversely unsuccessful theories may be maintained because they accord with a prevailing canon.

This model of science, in contrast to that of Kuhn, has not one but two mechanisms that moderate change.

> One is the set of empirical criteria, which are formulated by goal analysis and show little change in time. The other is the set of aesthetic criteria, which originate in inductive projection and alter in response to the perceived performance of past scientific theories.[79]

It is the tension between these principles that can lead to a period of sudden and revolutionary change. In such a case there is first the suspension or rejection of aesthetic criteria as standing in the way of empirical success. Then a new canon emerges re-establishing an aesthetic in accordance with a new measure of success. The judgement of empirical adequacy remains a constant.

In Kuhn's model aesthetic factors play a part only in revolutionary episodes; new theories are proposed to purify an accumulation of anomalies. For McAllister, aesthetics is a conservative force, theory change is not irrational but forced on empirical grounds. Science is essentially constant in its purpose and rational.[80]

The sociology of Pierre Bourdieu, in particular the concept of *habitus*, provides yet another variant on this theme of *prolepsis*. Bourdieu's writing also provides many other observations and conceptualisations pertinent to this thesis, namely; 'the scholastic point of view', 'fields', *illusio*, *ordination*, and 'symbolic capital'.[81]

Bourdieu sees the etymological derivation of scholar from *skholé*, meaning leisure, as significant. A scholastic view of the world requires detachment, a freedom from the limits imposed by the inescapable necessities of existence. It follows that scholarship and 'practical reason' are separated. Furthermore the scholar's point of view is often not one of total detachment and disinterest but a commitment to a particular field. (Though the 'field' features frequently in Bourdieu's writings and its meaning would appear largely self evident it is a

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[78] McAllister *op cit* p55

[79] *ibid.* p128

[80] Thus Copernicus is not in this view a revolutionary. His system better preserves the 'beauty' of circular motion at uniform velocity. The true revolution is in the work of Kepler, prompted by Tycho's planetary data.

[81] References in the following account are largely taken from the English translations of the following works *Méditations pascaliennes* (1997) and *Raisons Practiques* (1994) which in style and content are relatively accessible to the English reader.

concept he promises to expand upon in some forthcoming work.) A 'field' is a self-constituted social group whose common interest is integral to each member. There is an essentially involuntary quality to membership, a matter of assimilation through education and acculturation.

To be involved in a field is to have a commitment to shared, but to the wider world uncommon, assumptions and habits of thought. Thus participation in a field involves an *illusio*, taking seriously matters which are of little or no concern to the world at large. This sense of commitment to a particular interest—scientific, literary, philosophical etc.—that really does not matter gives this activity the appearance, from the outside, of a game. It requires a certain kind of leisure. Commitment to the game is imperceptible and largely implicit. "The original investment has no origin , because it always precedes itself and, when we deliberate on entry into the game, the die is already more or less cast."[82]

We might say that no one really decides to become a computer scientist or programmer it is an imperceptible process of being drawn into the game, it becomes a way of life, of imperatives and values that appear self evident. "To speak of a decision to 'commit oneself' to a scientific or artistic life (as in any other of the fundamental investments of life – vocation, passion, devotion) is, [...], almost as absurd as evoking a decision to believe..."[83]

This does not constitute a born–not–made, nature–nurture, apposition, the interplay between individual disposition, opportunity and enabling context, is subtle and complex. The 'scholastic point of view' is one that disregards the importance of custom and habit, of tacit knowledge, in favour of those things which can be deliberatively enumerated. Deliberative reason, with its presumption of leisure and detachment, has consequently an association with nobility, an enhanced social status. Plato is cited as an example of this opposition between the philosophers who "talk at their leisure in peace" and the rhetors speaking in the courts, always in a hurry "for the water flowing through the water clock urges them on."[84] Even today, the academic and intellectual inhabit a cloistered world distinct from everyday life.[85]

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[82] Pierre Bourdieu. *Pascalian Meditations*. Polity Press 2000 p11

[83] *ibid*. Bourdieu refers to Pascal's argument of the wager: such a rationalist argument is ineffective; *le coer a ses raisons que la raison ne connaît point*.

[84] Plato *Theatetus* 172-176c quoted by Bourdieu in *Pascalian Meditations* p 13.

[85] Bourdieu observes with particular reference to American universities

> "How could we not believe that capitalism has dissolved in a 'flux of signifiers detached from their signifieds', that the world is populated by 'cyborgs', 'cybernetic organisms', and we have entered the age of the 'informatics of domination', when one lives in a little social and electronic paradise from which all trace of work and exploitation has been effaced. / The effects of scholastic enclosure, reinforced by those of academic elitism and prolonged coexistence of a socially very homogeneous group, inevitably favour an intellectualocentric distance

Bourdieu presents this detachment of the scholar from ordinary life as partaking of three forms of fallacy. There is , first, the distance from 'practical reason', an academic view of the world that favours universal principle (on paper, in theory) rather than the pragmatic and particular. The "often magical, syncretic, in a word, prelogical" knowledge that predominates in everyday life is discounted. In its place theory is projected on to the actual world. As has been shown above the idea that all perception involves some projection of a *prolepsis* onto the world is of ancient lineage. But Bourdieu argues that the academic view is not one formed by feedback or involvement within the world, it is not holistic in its relation of observer and observed. The actions of real people are explained as if they had the thoughts and concepts that the theoretician assumes his 'agents' to have.

People are thereby supposed to act in accordance with canons of rationality that they do not in fact possess. It is an error to which economists are particularly prone, "it is not sufficient to tinker with an inadequate paradigm by speaking, as Herbert Simon does, of 'bounded rationality'."[86] To take into account the limits of available information and the calculating capacity of the mind, to look for 'satisficing' solutions is still to assume that the cognitive process is one that is essentially computational. So also the theory of 'rational expectations', though it provides for a correspondence between anticipations and probable occurrences, remains wedded to an assumption of deliberative decision making as the foundation of human behaviour.

This is a theme that has been expounded by the economist R.H. Coase[87]. Economics can be seen as the study of choice; as such it can be independent of a study of prices, consumers and goods in real markets. It may then be applied to other forms of choice such as those considered by law or politics. A hypothetical price theory may be extended to the non-human context—the maximising of utilities. As such a 'price theory' can be applied to understanding animal behaviour. But such abstraction provides economics, applied in the narrower sense, with a poor grasp of actual markets. "We have consumers without humanity, firms without organisation, and even exchange without markets."[88]

> from the world. The social and mental separation is, paradoxically, never clearer than in the attempts – often pathetic and ephemeral – to rejoin the real world, particularly through political commitments whose irresponsible utopianism and unrealistic radicality bear witness that they are still a way of denying the realities of the social world." (ibid. p41)

[86] Bourdieu. *Practical Reason*. Polity Press 1998 p221

[87] RH Coase. *The Firm, the Market and the Law*. Chicago 1988. (Which includes 'The Nature of the Firm', which first appeared in 1937 and 'The Marginal Cost Controversy' of 1946.)

[88] Coase *op cit* p3

Price theory can show a relation between relative prices and preferences but it does not explain *why* people choose as they do nor does it require an assumption that people *are* rational utility maximisers.[89]

> It would not seem worthwhile to spend much time investigating the properties of such a world. What my argument does suggest is the need to introduce positive transaction costs explicitly into economic analysis so that we can study the world that exists. This has not been the effect of my article. [...] The world of zero transaction costs, to which the Coase Theorem applies, is the world of modern economic analysis, and economists therefore feel quite comfortable handling the intellectual problems it poses, remote from the real world though they may be.[90]

In 'blackboard economics' all the information is present and the teacher plays all the parts. The reality is different, it is not possible to be both an 'agent' within and an external manipulator of equations.

To return to the sociology of Bourdieu... "the social scientist credits agents with his own vision and in particular an interest in pure knowledge and pure understanding which is normally alien to them."[91] In effect all languages become dead languages to be interpreted rather than used as an instrument. The reasoning of a scientist *qua* scientist is not used by that same scientist in everyday life. Observed behaviour is explained in terms of causes—the physical causes of the scientific world view—what people may think or feel they are doing is assumed to conform to and reduced to the cause and effect model.[92]

Bourdieu has said of *habitus*, "the past remains present and active in the dispositions it has produced", and stresses that it is practical sense rather than rational calculation that is the underlying principle of human action.

Like Kuhn, Bourdieu identifies fields as clusters around a 'disciplinary matrix'. Each field has its *nomos*, a viewpoint or thesis that is constitutive of the field, one that is so inherent that neither its explicit statement nor a detached appraisal is possible from within. Thus the field is separate from common sense, "a stock of self-evidence shared by all", a "primordial consensus". Interaction between specialists in different fields requires this shared common sense. This is reminiscent of Fleck's view of popular science as the common ground between scientific specialists. Bourdieu's concept is however broader,

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[89] "Whether men are rational or not in deciding to walk across a dangerous thoroughfare to reach a certain restaurant, we can be sure that fewer will do so the more dangerous it becomes. [...] Why a man will take a risk of being killed in order to obtain a sandwich is hidden from us even though we know that, if the risk is increased sufficiently, he will forgo seeking that pleasure." *ibid.* p5

[90] Coase *op cit* p15

[91] Bourdieu *Practical Reason* Polity Press 1998 p53

[92] Bourdieu writes of "The canonical distinction between explanations based on causes and explanations based on reasons." (Practical Reason p134)

in particular he notes a national character to this common sense, inculcated by education. "The existence of transitional fields (scientific ones, in particular) creates specific common senses which call the national common sense into question and it favours the emergence of a scholastic view of the world that is (more or less) common to *scholars* of all countries."[93]

A tacit adherence to the particular system of beliefs, that is the *nomos* of the field, requires *illusio*; "the fundamental belief in the value of the stakes of the dispute and in the presuppositions inscribed in the very fact of disputing"[94]. To share the *illusio* is also to have a common 'symbolic capital': knowledge and reputation, technical competence, which is valued and acknowledged only within the field.[95]

A field emerges in a historical context, its *habitus* is a system of dispositions. To those within a field the space of possibilities is provided by the *nomos* and *illusio* that gives the field its distinct identity. The field is constructed and defined from within. The process is neither entirely logical (i.e. a chain of deliberations) nor a matter of chance and accidents. There is an inner rationality and necessity to the historical development which from the outside may appear wilful and intentional but not necessarily inevitable and determined.[96] Thus the field is like the body, it has a tacit dimension. The scholastic point of view is of "body-as-thing, known from the outside as mechanism", it is a "spectators relation to the world.[97]

Illusio links an agent to the world not physically but in a space of associations: "that way of *being in* the world, which means that an agent can be affected by something very distant, even absent, if it participates in the game in which he is engaged."[98] Like a well practised team those who play a game react together: there is no need for symbolic communication, it is a shared response to a common environment, "sense of the game is adjustment to the forthcoming of the game."[99] The field (which to those within it constitutes a world) is seen in a particular way because to those who share its particular *illusio* there is a distinct set and hierarchy of significant values. A person who

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[93] Bourdieu *Pascalian Meditations* Polity Press 2000 p98

[94] *ibid.* p102

[95] We see this clearly in the case of computing, where despite 'skills shortages' the competencies valued among, say programmers, rarely command sincere regard in the common sense world.

[96] Bourdieu likens the process to a composer at a keyboard: "like a composer at her piano, which offers apparently unlimited possibilities to invention in writing—and in performance—but at the same time imposes the constraints and limits inherent in its structure [...] constraints which are also present in the dispositions of the artist, themselves dependent on the possibilities of the instrument" *Pascalian Meditations* p116

[97] *Pascalian Meditations* p133

[98] *Pascalian Meditations* p135

[99] *Pascalian Meditations* p208

belongs to a field is attuned to its values, has a position within it, from which is derived symbolic capital. The definition of this is self referential: "Symbolic capital is any property (any form of capital whether physical, economic, cultural or social) when it is perceived by social agents endowed with categories of perception which cause them to know it and to recognise it, to give it value."[100] Or, "Symbolic capital is capital with a cognitive base, which rests on cognition and recognition"[101] A *habitus* is an anticipation of what is required, like a well trained team or orchestra every member is spontaneously attuned to a collective mentality. It is a garment (*un habit*) or a familiar habitat. "He feels at home in the world because the world is also in him..."[102] The notion of *habitus* avoids the scholastic trap of seeing people as 'agents', that is acting according to abstract criteria of rationality that are creations of those on the outside, unthinkable to those 'agents'. It is the *illusio* of utilitarianism to suppose that agents are moved by conscious reasons.

Of particular interest in the context of computing is the observation by Bourdieu that

> The dialectic between dispositions and positions is seen most clearly in the case of positions situated in zones of uncertainty in social space, such as still ill-defined occupations, as regards both the conditions of access and the conditions of exercise [...] their future depends on what is made of them by their occupants, or at least those of them who, in the struggles within the 'profession' and in confrontations with neighbouring and rival professions, manage to impose the definition of the profession most favourable to what they are.

Those who form a new profession bring to it their own "inclinations inscribed in habitus", there is, at first, no *habitus* demanded of those who aspire to the post, it has, in the informal and tacit mode, no definition.

Scholastic classification is *ordination*, in French meaning both a difference of rank and a consecration. Thus education not only selects but also assigns a place. It preserves a social structure by regeneration of *habitus*. In the nineteenth-century it was the educational institutions that fostered the creation of bureaucratic castes, a 'state nobility'. (Bourdieu cites particularly the cases of France, Germany, Japan.) In France that class had its origins in the '*noblesse de robe*' a legal profession, originally concerned with canon (i.e. ecclesiastic) law, whose status was given by the king rather than inherited. By contrast, in Germany and Japan the bureaucratic elite was formed around a scholastic culture with an emphasis on noble origin. In all these cases a technical and scientific education maintains its status by emphasising a scholarly component, knowledge *of* science rather than *doing* science.

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[100] *Pascalian Meditations* p47

[101] *Pascalian Meditations* p85

[102] *Pascalian Meditations* p143

Prolepsis, *idola*, *Denkstil*, *habitus & illusio*, a field of enquiry, an enclosure of knowledge, profession and discipline.[103] By prolepsis I wish to emphasise not disjunction but continuity. Unlike a paradigm, prolepsis is not a state of mind that can be wilfully discarded or acquired. It is unique to an individual mind yet for the most part a common heritage of all. A theory or research programme is but one component of an individual and collective 'state of mind'; its replacement does not constitute a total overthrow of a process of thought—for the most part the outcome of a revolution is that, after the dust has settled, things are pretty much the same.

A distinction can be drawn between metaphor and prolepsis. Metaphor, broadly conceived, is to speak or write about one thing whilst having something to say about another. Sometimes, when the metaphor is a figure of thought that is endemic to a culture, not an individual insight but a shared theme, it may be characterised as a myth rather than metaphor.[104] "A myth, of course, is not a fairy story. It is the presentation of facts belonging to one category in the idioms appropriate to another."[105] Prolepsis, on the other hand, is latent metaphor; a potential pattern match for something as yet unencountered. Its activation is involuntary, it may subsequently be rejected as mistaken or misleading, but the impulse to make the connection is innate. A history of ideas, of encounters with new concepts, therefore needs to consider *prolepsis*, the 'lines of force' that channel a developing perception.

But that perhaps is a misrepresentation of what is to follow. Because this is not so much the story of the foundation of computing as of its enclosure. Enclosure not being a matter of containment but of definition; without boundaries there is no 'field'. What lies about the boundary, left out as much as kept in, is of particular significance.

In looking for the origins of a field of computing, the answer is not to be found in the formal agreements and proceedings, the official foundation of institutions, the granting of certificates and charters, curricular and publications. The field of computing is largely an informal one, computing is still largely free of institutional ossification.[106] Referring to the history of

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[103] "...discovering a new sort of phenomenon is necessarily a complex process which involves recognising both *that* something is and *what* it is. [...] that process extends over time and may often involve a number of people." Kuhn. The 'Historical Structure of Scientific Discovery'. (in *The Essential Tension*, Chicago 1977) p 171

[104] "It is a figure of thought used by an entire civilisation, repeated, modified, and resurrected over time without ever becoming fixed or dead." John Scheid & Jesper Svenbro. *The Craft of Zeus: myths of weaving and fabric*. Harvard 1996.

[105] Gilbert Ryle. *The Concept of Mind*. (1949) Penguin Books 1990 p10

[106] The greatest contribution of formal educational institutions to computing has arguably been the provision, of free access to computers, and latterly networks, for a generation of innovators.

biology, Joseph A Caron[107] identifies three criteria that mark the formation of a distinct discipline: the founding of institutes and learned societies, debates about curricula and education, and the formation of a distinct cognitive content.[108] All these elements can be identified in the computing world of the late 50s and early 60s but it is 'distinct content' that is of particular concern. I will , ultimately, suggest that 'programs', viewed as textual, literate artefacts, form that content. But in order to do so it will help to counter a prolepsis derived from the historic origin of computing science: the program as encoding, as the representation of an ideal form, as implementing an algorithm. "I have to shew you the facts with which you are already acquainted in a light of a different character..."

[107] Joseph A Caron. 'Biology' in the Life Sciences: a historiographical contribution. *History of Science* xxvi (1988) p244

[108] Alternative readings of Bourdieu, emphasising the importance of educational institutions, applied to Caron's criteria may lead to different conclusions.
See Nick Jardine. 'Uses and Abuses of Anachronism in the History of the Sciences' *History of Science* xxxviii (2000) p261 where he writes: "[educational institutions], rather than the domain of ideas and representations, is, I suggest, the most basic level of presupposition of the disciplinary categories of the sciences"

Chapter 2. *Bildung*

Counting is the religion of this generation it is its hope and its solution.

Gertrude Stein *Everybody's Autobiography* 1938

Social status, material wealth, and education, the three are interdependent: wealth buys education, education brings status, status assists the acquisition of wealth. Education is this context being as much a matter of socialisation (*Bildung*) as of training or the development of innate talent and intelligence. But there is a tension provided by the various aims of education: as an induction into a higher status group or to learn a 'trade' and thus the means to an income. Thus it is that often the education that confers the highest status is that which has least practical value; the wealthy and those most assured of their social position can afford an education that has no immediate value.[109]

This tension is a significant background to the development of science in mid-twentieth-century Britain. There is an obvious association of science with education, training and industry. There is also the uncertain position of the scientist in the social and administrative elite. In the chapters that follow we will be taking a close interest in developments at the National Physical Laboratory, a government laboratory with, in the late 1950s, both an industrial and academic research mission. And, as it happens, a unique though somewhat anomalous place in the history of computing. It is therefore useful to consider those intertwined reforms of academia and government administration initiated in the nineteenth century that shaped the environment in which scientists and their institutions were to operate in the late 1950s. Finally, the institutional structure at NPL highlights clearly the character of computer science's main line of development as a confluence of mathematical and engineering interests. We may better understand how mathematics has tended to be the senior partner in this alliance by considering the cultural background.

With mediæval and monastic foundations, the universities of Europe were created to provide training appropriate to rôles assumed by the church. As clerical institutions they were bound to decline with the rise of secular power. In England, in particular, the reformation saw their role confined to the

[109] The Robbins report of 1963 lists four objectives of higher education. "We begin with instruction in skills suitable to play a part in the general division of labour. We put this first, not because we regard it as the most important, but because we think it sometimes ignored or undervalued." Thus the committee acknowledged the case only to dismiss it. The real interest of the committee is clearly elsewhere: "to promote the general powers of the mind [...] not mere specialists but rather cultivated men and women." "the advancement of learning." "Finally there is a function that is more difficult to describe concisely, but that is none the less fundamental: the transmission of a common culture and common standards of citizenship."

qualification of Anglican clergy. Elsewhere in Europe, including Scotland, they retained their mediæval rôle of training not only for the church but for medicine and the law.[110]

It should also be noted that both a centralised civil service and state supported universal education came much later to Britain than France and Germany where they preceded industrialisation.[111] This may be attributed to the accident of geography; less threatened by invasion than countries with extensive land borders, England's administrative system developed differently. In Norman and Tudor times it had, relatively speaking, an extensive and centralised administrative mechanism. In the seventeenth and eighteenth centuries by contrast, administration became more localised.[112] In nineteenth-century Britain centralisation was more the consequence of improved communications and industrialisation than a deliberate extension of state powers.

France has a long tradition of central government and its technocratic tradition has roots in the 1650s when a school to train military engineers (*Ecole Royal de Génie*) was established at Mézières. It is notable that its founders were determined not only to train competent engineers but also to ensure their status both by attracting aristocratic recruits and, failing that, to ensure its graduates were socialised as members of an élite social class.[113] In 1744 the *Ecole des Ponts et Chaussées* was founded in Paris. It is not surprising that these institutions, free from the clerical associations of the universities, should survive the Revolution, and the founding of the *Ecole Polytechnique* under Napoleon was a continuation of this tradition.

It was French military success that gave encouragement to the educational innovations of von Humboldt in Prussia. A notable feature of this new education was its Hellenism; a conscious affirmation of incipient nationhood in opposition to the 'Latin' style of an imperial and catholic France. Throughout Europe in the nineteenth century the study of Greek could be seen as "a legitimating exemplar for romantic nationalism."[114]

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[110] This appears to be a consequence of Henry VIII's 'nationalisation' of the church in England. Professional training moved to the London teaching hospitals and Inns of Court.

[111] See JA Armstrong. *The European Administrative Elite*. Princeton 1973. Armstrong defines a 'take-off' period for industrialisation of 1783–1840 for Britain, 1830–70 for France, and 1840–70 for Prussia.

[112] "Especially significant was the lack of a territorial officer dependent on the centre. Instead, the justice of the peace system, dominant well into the nineteenth century, represented particularistic local interests." Armstrong *op cit*. p34

[113] Armstrong *op cit*. p177 *et seq*.

[114] Christopher Stray. *Classics Transformed: schools, universities and society in England 1830–1960*. OUP 1998 p15

Though defeat by France was a spur to the reform of German universities and the growth of an administrative class, the changes had begun earlier. The creation of a civil service in various German states from the 1750s boosted the law faculties of the universities and made them attractive to an aristocratic elite. A Prussian civil service with entrance by examination had been established by 1771. At the same time the introduction of a system of state elementary education not only laid foundations for further educational development but transferred the traditions of the seminary to the classroom. Thus began the notable philological tradition of German scholarship, of accurate recovery and recension of text and archaeological artefact, which was so influential throughout the nineteenth century. The research degree was another innovation in the philological tradition, stressing discovery and accumulation of knowledge, by a system of tutelage in an exclusive academic frame rather than the public lectures that continued to dominate instruction in the French universities. The German universities, as developed through the nineteenth century, had two characteristics: the provision of an elite and essentially aristocratic training for higher administration based on law, and, in parallel, an academic culture 'a general glorification of pure knowledge based on research.'[115]

Technical education in Germany developed separately, though strongly influenced by the prestige that attached to the research culture of the universities. In the early nineteenth century military engineering did not have the status of law nor the academic attractions of the Greek Revival, however the quality of elementary education in Prussia was high and this formed a sound basis for the establishment of the *Technische Hochschule*, a system of higher education distinct from the universities. If the status of engineers did not equal that of legally trained aristocratic administrators, their rare theoretical training was nonetheless valued by industrial entrepreneurs. As the twentieth century opened engineers were coming to predominate on the executive boards of German companies. A table compiled by Armstrong gives an indication of the comparative importance of various disciplines in businesses in the 1950s.

Business Executives with Higher Education in the 1950s (per cent)[116]

	All Technical and Science	Engineering only	Law	Economics, Business
France	55%	(51%)	9%	10%
Germany	36–57	(45)	19–21	17–21
Great Britain	42	(20)	9	35
USA	46	—	15	31

[115] Armstrong *op cit.* p 164

[116] Armstrong *op cit* p181. Armstrong's figure for the number for British executives with 'economics and business' education seems improbably high, possibly this figure was obtained by placing accountants in this category ?

In England industrialisation had preceded both the development of a universal education system and the creation of a centralising administrative class. It is these relative timings: of industrialisation, state education, the creation of a civil service and, in the case of Britain, a significant colonial economy, that may explain national differences in the status of education and scientific research.

Given such context it is no surprise that in the 1830s Charles Babbage should appeal to continental models when finding the Royal Society culpable in the *Decline of Science in England.*[117] A tradition of looking to Germany for models of technical and scientific education continued to 1914. But it was not criticism of the Royal Society but of the Civil Service that served to promote a revitalisation of the universities of England—that is to say Oxford and Cambridge, there were no others until the origination of University College in 1826. The new universities struggled to survive, Kings College in London was merely a prelude to Oxbridge for many of its students. It was not until the 1870s that the civic universities began to be significant institutions.

By most accounts there is a strong correlation between the reform and revival of the universities in England during the nineteenth century and the expansion and reform of the Civil Service during the same period. Commenting on the Northcote-Trevelyan report that had been presented to Parliament in 1854, Lord Robert Cecil (later, as Lord Salisbury, Prime Minister) said it was "neither more nor less, from beginning to end, than a schoolmaster's scheme."[118] But this report, though significant for its statement of certain principles—an end to appointment by patronage, entry by open and competitive written examination, separation of intellectual and mechanical labour, promotion by merit and unified conditions of employment across departments—was not the first nor the last of many gradual changes. Pressure to reduce appointment by patronage—"our high Aristocracy has been accustomed to employ the Civil Establishments as a means of providing for the Waifs and Strays of their Families"[119]—had begun much earlier. Both the American, and later Napoleonic, wars had put constraints on expenditure in general and brought exceptional emoluments to holders of offices that handled military supplies. By the 1850s these opportunities had been much reduced, indeed the report (which many considered far from thorough or impartial) probably exaggerates this aspect of corruption. The majority of

[117] Charles Babbage *Reflections on the Decline of Science in England and on some of its causes* London: 1830 [ed Martin Campbell-Kelly, Pickering & Chatto 1989]

[118] Edward Hughes. 'Civil Service Reform 1853-5' *Public Administration* XXXII (1954). An earlier version appeared in *History* (June 1942).

[119] Sir Charles Trevelyan. letter to Mr Delane 6. February 1854 (quoted in Hughes, 'Sir Charles Trevelyan and Civil Service Reform' *English Historical Review LXIV* 1949)

public officers[120] were engaged in Customs and Excise duties for which physical fitness rather than literacy was a prime qualification, or were Post Office clerks and copyists.[121]

Reforms of the Civil Service had been presaged by a far more comprehensive reform of appointment and training to the Indian Civil Service. The East India College had been founded in 1806, and two years attendance there was a precondition of appointment to the Indian Service. Sir Charles Trevelyan was himself trained there. The curriculum stressed matters of practical importance, not only Asiatic languages (it was Oxford's failure to provide any education in these subjects that had prompted its foundation), but also accountancy and law. Anticipating the assumption of direct control over India by the Crown, there was a campaign to eliminate the system of patronage that had been rife in the East India Company before such spoils might become a tool of the government at home. In this context, educational reformers at Oxford, in particular Benjamin Jowett of Balliol, were able to advance their case for competitive examinations that favoured the existing university curriculum. By 1855 the College had closed, recruitment was nominally open, but the benefits of an education attuned to the requirements of the Service were lost.[122]

The Northcote-Trevelyan report, which when presented to parliament in 1854 was accompanied by a letter from Jowett, setting out a proposed curriculum for a Civil Service examination, met with strong opposition, not only from vested interests but also more principled objections to the proposed system of selection. It had the support of William Gladstone, who as MP for Oxford University had both an interest in university reform and close contact with the universities. The proposals for open competitive examination met with particular opposition; not only did it threaten to fill the service with 'crammers' and 'literary men' unsuited to the requirements of the departments, there was also the fear, allegedly expressed by the Queen herself, that the public service would be open to "low people without the breeding or feelings of gentlemen."[123]

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[120] The total number employed in Public Offices in 1797 was 15,818 of which Customs 6,004 Excise 6,580 and the Post Office 957. *Parliamentary Papers* 1828 xvi (quoted in EM Cohen *The Growth of the British Civil Service 1780–1939* George Allen and Unwin 1941.)

[121] The scope for political patronage was limited, and was probably no more extensive or 'corrupt' than that legitimately afforded politicians today by placement of jobs and grants in 'development areas'.

[122] See RJ Moore. 'The Abolition of Patronage in the Indian Civil Service and the Closure of Haileybury College' *The Historical Journal* VII 2 (1964), pp246–57, and also the introductory chapter of: Anthony Farrington. *The Records of the East India College Haileybury & Other Institutions*. HMSO 1976

[123] Quoted in Hughes (1942/1954) *op cit.*[the provenance of the citation is unclear]

Such was the opposition, it was immediately obvious that no parliamentary approval could be obtained. Though the disorder and manifest inefficiencies revealed by the Crimean Campaign gave some additional weight to the proponents of reform, it was only by the device of an Order in Council, which avoided debate in parliament, that a Civil Service Commission was established in 1855 and limited qualifying examinations introduced. The examinations slowly became established, largely because by the Superannuation Act of 1859 a certificate of examination by the Commissioners was made a necessary prerequisite for a pension. A further Order in Council of 1870, when Gladstone was Prime Minister, established open and competitive examinations throughout the Civil Service. The examinations were at two levels: 'regulation I', was intended for graduate level recruitment, 'regulation II' for clerks. There was also a third class of employment that of the 'writers'—hourly paid, ostensibly temporary, copying clerks.

It was as a result of the operation of these further reforms that Sir Stafford Northcote, by then Chancellor of the Exchequer, appointed Lyon Playfair to a Civil Service Inquiry Commission that reported in 1875. They found that implementation, which varied among departments, had had some unintended and undesired consequences. The best examinee had the choice of all the available posts, the remaining positions were offered to those who had passed in order of merit. Thus the departments had no control over the particular suitability of a candidate for the work on offer, nor could a candidate low on the list with few choices remaining defer entry and await a more suitable vacancy. Under the old system the Head of the Office had a choice, and that in turn encouraged loyalty to the office or department. As a consequence departments had found various methods to 'work around' the regulations; setting up weak token candidates, or grading posts at a higher (regulation I) level, or employing the more lowly writers to do the same work as established clerks.

The examinations had improved the basic literacy and numeracy of the service and had provided encouragement for elementary education, but there was no test of suitability for a particular post and "It may well be doubted if any examination can effectively test a man's real and permanent capacity for the practical business of life." The system had proved impractical in its assumption "that comparative success in a literary examination confers not merely qualification for service, but an absolute title to a comparatively high rank in the service."[124]

The Playfair Report recommended a two stage examination: an initial qualifying examination of 'handwriting, arithmetic, English composition, geography, and English history; to be followed by a competitive examination in several subjects selected from a list set by the Commissioners. The offices

[124] First Report of the Civil Service Enquiry Commission *Parliamentary Papers* 1875 XXIII c1113 [Playfair]

would be able to require examination passes in particular subjects, candidates might reject positions on offer yet remain on the list of candidates for future vacancies.

There was also the problem of the writers who, numerically significant, demanded equal pay and conditions for equal work. This was solved gradually by a variety of measures, involving regarding and reserving positions for writers who passed the qualifying examinations. But the fundamental problem caused by the division into categories of mechanical and mental labour remained; it was not so much solved as rendered obsolete by the introduction of the typewriter. (Though this would, a generation later, bring a recurrence of the problem when the women employed as 'typewriters' began to demand equal status.) There was another undesirable aspect to 'mechanical labour':

> The routine work in which they have been so long engaged, and with which they are so familiar, appears to them to be the end instead of being merely the means to the end. They cannot distinguish and separate the substance from the form in which it has always been presented to their minds, and the result is that a large number of the general body of the Clerks are not qualified to fill efficiently the higher posts in the service.[125]

As we have seen the Northcote-Trevelyan reforms were introduced by a series of indirect administrative measures, often with adjustments to correct unintended (albeit predicted) effects. Yet, by the end of the nineteenth century, the general intent of those who supported the Northcote-Trevelyan proposals had been realised: entry to the Civil Service was by competitive examination, political and personal patronage had been eliminated, the employment was of high status attracting those of high academic attainment from the country's most revered universities. This achievement was not without it critics at any time and in the twentieth century that criticism was increasingly focused on the detachment of the administrative class from the practice and technology of a 'modernist' science led world. In the 1950s and 60s there were several enquiries into the administration of the Civil Service of which the most significant was perhaps the Fulton Report of 1968. This, in principle at least, removed the essentially class based stratification of the civil service and was also intended to remove the divide between specialists (such as scientists) and a generalist administration.

This debate over the composition and function of both the Administrative and Scientific Civil Service was current throughout the 1950s and 60s. It is therefore of some importance for an understanding of developments at the National Physical Laboratory in this period to have in mind contemporary perceptions of both the Service itself and its founding tradition. There was first of all Northcote-Trevelyan as a foundation myth of the Civil Service[126], it

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[125] First Report of the Civil Service Inquiry Commission. [Playfair] C1113 Parliamentary Papers 1875. XXIII

[126] For a more recent version in a similar vein see Michael Duggett 'The Evolution of the UK Civil Service 1848-1997' a paper prepared for the International Institute of

suffuses the account of Emmeline Cohen. Published in 1942 and re-issued, with a new publisher and without revision in 1965, she presents an essentially favourable view of the development of the Service.[127] It had evolved in three phases, in the first the 'Public Officers' became Civil Servants subject to parliamentary control. "New standards of financial integrity gained recognition, and successful efforts were made to put an end to the peculations which had been common in earlier times."[128] The second, to 1890, was dominated by the Northcote–Trevelyan proposals: "The main concern in this period was to make the Service more efficient by improving the quality of its personnel."[129] Finally, in the period up to 1939 an expanding Service was welded into a consistent whole with common grading and tighter Treasury control[130]. In conclusion "the Service has shown itself capable of adaptation to changing circumstances." It had continued to make notable improvements and "remains one of the most characteristic of British constitutional and political institutions."[131] Its tradition was against violent change; "it is from the recruits of 1939 that the Permanent Secretaries of 1970 will be chosen."[132] Yet growth of administrative activity had brought many specialists into the service and by the 1930s the problems of the barrier between technical and administrative staff were apparent.[133]

> There can be no doubt that at one time the 'specialist' officer in the Civil Service was looked on as a subordinate by his administrative colleagues, and his opinions insufficiently consulted. Sir Arthur Newsholme noted

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Administrative Sciences Quebec Conference, July 1997' [http://www.britishcouncil.org/governance/manag/civil/index.htm 02/09/2001]

[127] Emmeline W Cohen. *The Growth of the British Civil Service 1780–1939.* George Allen & Unwin 1941, second impression Frank Cass & Co. 1965

[128] *ibid.* p21

[129] *ibid.* p21

[130] Employment in Public Offices totalled 15,879 in 1797, of which 12,584 were employed in the Customs and Excise, and 957 in the post office. There were 107,782 employed (excluding industrial staff) in 1902, 135,721 in 1911. The number had swollen to 368,910 in 1920 before falling back to 306,154 by 1929. In 1938 the figure was 376,491. *ibid.* pp 34, 164 Cohen cites the Parliamentary Papers as her source. These figures differ from those of more recent writers although Dugget (1997 *op cit*) appears to confirm the 1929 figure of 424,000 including 122,000 'industrial'. GK Fry (1997 *op cit*) on the other hand quotes non-industrial totals of 50,000 in 1902, 161,000 in 1920, 111,000 on 1930 and 163,000 in 1939. For later years Fry puts the numbers at 433,000 (1950), 380,000 (1960), 493,000 (1970), 547,000 (1980) 495,000 (1990) and 458,000 in 1995.

[131] *ibid.* p195

[132] *ibid.* p205

[133] In 1931 it was estimated that there were ten thousand Civil Servants with scientific, professional or technical qualifications and a further eleven thousand undertaking technical roles at a lower level. It was claimed only one in twenty successful candidates for the Administrative Class had a degree in science or economics.

> an 'honest belief, common to many Government Departments, that technical advice is advice not to be given until called for by the secretariat who, it is assumed, are entirely competent to decide whether such advice is needed,' and that such advice once recorded, it was assumed that it could be re-applied in what were regarded by the secretariat as analogous circumstances.[134]

Cohen appears on the whole sympathetic to the *status quo* of the Service.[135] She concedes that changes to selection, "still largely dominated by the educational principles of Macaulay and Jowett", would be necessary but comes to no firm conclusion on the best way to proceed. Another aspect of the extended functions of government draws her attention: the increasing use of "public service organizations which are semi-independent, although their governing bodies are appointed by the Government" Such organisations (the BBC, the Central Electricity Board are cited) had their own recruitment procedures and paid salaries far exceeding those of civil servants.

> ...in the future it cannot be a matter of indifference to the public how these great new organizations are staffed, the scales of pay they adopt, etc. A complex situation is arising which some consider may lead to grave difficulties. Certainly from the point of view of the future of the Service, it is highly desirable that a proper relationship should be established between public and the semi-public services, and that the high prizes offered in newer bodies should not be allowed to detract from the prestige of the Service nor discourage recruitment.[136]

The journal *Public Administration* reprinted the Northcote–Trevelyan Report in 1954. Accompanying it was an essay by Edward Hughes, *Civil Service Reform 1853–5* which had first appeared in *History*, June 1942. Hughes, Professor of History at Durham University was more critical of the Service, concluding: "It is perhaps time to ask whether these prognostications of 'evils and mischief' have not been partially realised."

Making extensive use of the papers of Sir Robert Peel who became Prime Minister in 1841, Hughes argues that the problems of patronage and inefficiency were exaggerated. Few appointments were available to offer as political favours, most being managed by the departments according to their own well established, albeit particular, customs. Those appointments that were subject to political consideration might well be in the gift of the opposition; local MPs had independent influence over purely local appointments. Most jobs were not clerical, but manual, the Customs and Excise, being by far the largest employer. "The qualities absolutely required are physical strength, sound health, honesty, sobriety and a docile and

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[134] *ibid.* p206 The quotation is from: Sir Arthur Newsholme (Chief Medical Officer to the Local Government Board), *The Last Thirty Years in Public Health* 1936

[135] The tone of much of her writing suggest the author was herself either a civil servant or closely identified with it.

[136] *ibid.* p210–211 [Note the echo of Jowett's 'valuable prizes' (See below) in the phrase 'high prizes'.]

contented disposition, with only as much education as will suffice for keeping tallies and books of the simplest." What was required was not the middle class schoolboy but "a hardy mariner or labourer."[137] At this lowest level—nine-tenths of appointments—a basic educational test was effective both in improving the standards of the public service and giving encouragement to elementary education. (An education it should be noted that was at that time neither compulsory nor free.) But the introduction of 'a schoolmasters scheme' did not guarantee the candidate would be suitable for any particular position. Systems of patronage were not incorrigibly corrupt; those commending a candidate were under some moral obligation not abuse their connection by attempting to place someone wholly unsuitable. As many critics saw at the time an open competition for 'crammers' at all levels would have undesirable consequences.

A final perspective on the status and influence of the Civil Service is provided by John Armstrong an American social scientist whose study *The European Administrative Elite* was published in 1973.

Armstrong argues that the civil service reforms were not driven, so much by a demand for competence in administration, nor by pressure for more open entry by a rising middle class (the number of administrative posts on offer was small), but was the result of a move by the universities to gain status. Established professions, of medicine and law were not university based, and the Anglican clergy were of declining influence. University education as an essential prerequisite for entrance to higher administration provided a justification for the university.

> For a century the administrative elite provided a prestige goal for their charges, demonstrating that commitment to the educational elite's values provided the surest avenue to the societal elite. As a 'retreatist' administrative elite became somewhat less prestigious and its methods of recruitment more subject to universalistic challenges, the universities have turned to alternative channels to legitimize their role.[138]

As we have already noted the Northcote-Trevelyan report of 1854 was accompanied by a letter from Benjamin Jowett, setting out a proposed curriculum for a Civil Service examination. In it Jowett stated "men of attainments are also men of character"[139] and, just in case studious pursuits did not wholly prevent a dissolute life, that enquiries might be made and references sought from a headmasters, medical men, magistrate and clergyman before the candidate sat the examination. The examinations should

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[137] Hughes *op cit* p40 quoting evidence of Sir Francis Baring to the Committee.

[138] John A Armstrong. *The European Administrative Elite*. Princeton 1973 p156

[139] Parliamentary Papers 1854 XXVII Reports of Committees of Inquiry paper 1713 Organisation of the Civil Service. Letter from the Rev. B. Jowett.

be supervised by a 'College of Examiners' headed by a "some eminent person [...] of the rank of Privy Councillor."

Having limited the number of candidates by preliminary examination in English and arithmetic in the next stage "We must test a young man's ability by what he knows, not what we wish him to know."[140] Thus, in principle, Jowett appears to countenance a broad selection of knowledge.

> The knowledge of Latin and Greek is, perhaps, upon the whole, the best test of regular previous education. Mathematics are the predominant study of one of our universities. Moral philosophy is a principle subject at Oxford, no less than at Edinburgh and Glasgow. An increasing class of persons receive a foreign or an English, in contradistinction to what may be termed a classical education. Some of the candidates again may be entered at Inns of Court. Lastly, it may be remarked that there are subjects, such as physical science and civil engineering, which notwithstanding their immense growth in the last few years, have scarcely yet found their way down into education, and in reference to which the proposed examination may be made to operate usefully. These and similar considerations should enter into our scheme, which, supported as it is by valuable prizes, must exercise a great influence on the higher education of the country.

As the final sentence shows, in this scheme there was a clear intent to enhance the rewards of higher education as much as the quality of public employees.

The Civil Service reform initiated in the mid-nineteenth century, though beneficial to the established universities need not necessarily have restricted the curriculum and might have even encouraged its extension. But in practice this was not the outcome; the fears of those who predicted 'cramming' and questioned the relevance of examinations to the practical business of life were realised, between the 1850s and1870s there was a notable 'exam mania'. The testing of what was known became, inevitably, a test of what had been taught. A test of general education tended to reinforce the predominant position of the classics. Only in the case of elementary education, did the preliminary qualifying test of literacy and numeracy have the desired effect of raising the value of, and thereby encouraging, elementary education. By 1870 it was possible to make elementary education compulsory. By 1900 the need for secondary education of a non-academic kind, that is technical education, had become the subject of attention; a debate that was inseparable from a concern over both political and commercial rivalry with Germany, and the status of science.

But any reform of education had to overcome the dominance of classics, with Latin and Greek essential for entry to Oxford and Cambridge the influence filtered down to secondary curriculum. Even when university entrance was irrelevant as an objective, classics defined a scale of educational values. Discipline not knowledge was promoted, and thus a pecking order: classics,

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[140] *ibid.* p27

mathematics (as an art not a science), science (largely chemistry) as pre-medical training, and modern languages a subject for girls and others not intended for university.

After the collapse of Roman Empire there was almost complete ignorance of Greek in the west until the fifteenth-century. The monopoly of Latin as the language of Europe's learned was challenged by the growth of printing in vernacular languages. From the end of the seventeenth century the learning of Latin was increasingly a 'discipline', that is, not something of practical use in giving access to knowledge. "This 'discipline', was further defined as a superior alternative to the mere 'furniture' of the mind: strengthening mental powers, rather than simply filling the mind with facts."[141]

> This correlation of classics and class—Latin discipline for the commercial classes, (Greek) culture 'for its own sake' for their superiors – can also be seen in England. Here the Greek revival of the late eighteenth and nineteenth centuries coincided with the industrial revolution and the growth of a class society. During the first generation after industrial take-off, between the two extensions of the franchise in 1832 and 1867, the old noble elite and the upper layers of the new bourgeoisie merged to form a new, assimilated social elite; and at the heart of this process lay the public schools. Within their secluded rural settings, nationally accessible through the expanded railway network of the 1840s, financial capital was transmuted into cultural capital. 'Godliness and good learning' inculcated the style and manners of a gentleman; and this learning was almost entirely classical.[142]

The Taunton Commission of 1868 recommended the establishment of county education authorities; this was successfully opposed by the public schools, which, spurred by this threat, established the Headmasters' Conference in 1869.[143] By 1900 the public schools were at the height of their influence. Most Oxbridge scholarships were tied to classics, the only counterweight was the sponsorship of science in Grammar Schools and Civic Universities in years up to 1902 by the Department of Science and Art. Thus emerged a three tier system; public schools teaching Greek to age 18, Grammar schools teaching Latin to age 16, and elementary education that by 1918 extended to age 14.

In Scotland the retention of medical teaching within universities provided a foundation for the teaching of chemistry. In England, science in secondary schools, being largely provided for those intending a medical career, included little physics. So it was chemistry rather that physics that was the foundation of science teaching in the nineteenth century and, the influence permeating

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[141] Christopher Stray. 'Culture or Discipline? the redefinition of classical education' in Michael H Price (ed). *The Development of the Secondary Curriculum.* Croom Helm 1986. See also Christopher Stray. *Classics Transformed: schools universities and society in England 1830-1960*. Oxford 1998

[142] Stray (1986) *op cit* pp 13-14

[143] Debates on the pronunciation of Latin (each school had its own) were a perennial feature of the conference from its foundation up to 1926.

down into the school curriculum, chemistry continued to be a major component of the General Science taught in the first half of the twentieth century.

The academic development of physics by contrast was grounded in mathematics—abstraction rather than observation. The case of electromagnetism provides an example. Faraday, whose experimental career began with chemistry as an assistant to Humphrey Davy, based his physical theories of upon observation. But taken up by Maxwell at Cambridge these observations became mental models; the prelude to a purely mathematical theory. Physics gained academic respectability by association with a traditional arts subject.[144]

The traditions of classics teaching spread to the teaching of modern languages. Beyond the teaching of reading and writing at elementary level English was not established as a significant component of the school curriculum until the interwar years, and even tended to be seen as a 'girls subject' or, as with the 'modern side' in general, best suited to those judged less academically able. The academic outlook of the universities pressed down upon secondary education, which was for the most part focused on the grammar school and the requirements for university entrance. At the universities the academically respectable study of modern languages was at the philological level, in schools it was taught on the model of Latin as a mental discipline. Ability to converse in a foreign language was an optional part of the Cambridge Tripos examination in Modern Languages until 1917. Though the teaching of FR Leavis in the 1930s represented an innovation it was still in the traditions of classics teaching, the discipline model of Latin replaced with the high culture model of Greek. "The university view of language as a discipline triumphed with ease over the radical view of *language as communication*."[145] But it would be unfair to attribute this 'mentalist' view of language learning to "dusty nineteenth-century pedagogues teaching form because they had no content to teach,"[146] it had its origin in the 'Age of Reason' and the search for universal natural laws; as such it drew on much the same currents of thought as fed the positivist and mathematical roots of the sciences.

The period 1870 to 1940 saw many changes in the curriculum of secondary education, in fact the creation of a mass system of secondary education. The School Boards created by the education act of 1870 had local powers to compel attendance from ages five to ten. But universal education (to age twelve) was not achieved until 1900. Before the raising of the school leaving

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[144] Mathematics being part of the mediæval curriculum predates the modern conception of a 'science'.

[145] William Rowlinson. 'Modern Languages: the retreat from reform' in Price *op cit* p79 [my italics]

[146] *ibid.*

age to fourteen in 1918 state secondary education consisted largely of county grammar schools established by the education act of 1902.[147] This Act had established an Education Department which swallowed the Department of Science and Art which had done much to promote the teaching of science since the 1860s. The new schools were not totally free but public funding was conditional on at least twenty-five per cent of places being awarded by free scholarships. The policy of establishing the grammar schools was supported by the senior civil servant at the new department, Sir Robert Morant. The assessment of Morant's influence remains controversial.[148] He believed in the value of elites, but also in promoting social mobility; he curtailed the emphasis on science that had been encouraged by grants from the Department of Science and Art; a regulation required that if two languages were studied one had to be Latin. The provision of Latin can be said to have opened the door for scholarship pupils to enter Oxford and Cambridge (but compulsory Greek remained a bar until the 1920s).

In sum, the path through secondary education to university in the inter-war years, that is the general educational background of the senior scientists and administrators of a government laboratory in post-war Britain, was still strongly influenced by a curriculum familiar to Jowett a century before. The mental discipline, if less so the humanism, of the classics still had a powerful influence, the art of mathematics reigned over the sciences. Though Jowett's school of political economy, law and moral philosophy (to 'supply the requirements of the Treasury, Board of Trade &c') may be said to have been realised in Oxford's Modern Greats (introduced 1921), the modern languages and history he saw as meeting the requirements of the Foreign Office had progressed slowly. And still after a hundred years "there are subjects, such as physical science and civil engineering, which, notwithstanding their immense growth in the last few years, have scarcely yet found their way down into education."[149]

When Charles Babbage went to Cambridge in 1810 it is said he was dismayed to find he knew more of algebra than his tutor. As a consequence Babbage, together with John Herschel and George Peacock, founded the Analytical Society "to do their best to leave the world wiser than they found it."[150] It was not only the decline of mathematics that concerned Babbage, the Royal Society was subjected to particular scorn as an institution needing rescue

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[147] Although some local school boards had established higher grade schools to teach beyond the leaving age this expenditure was found to be illegal in a judgement of 1899, which necessitated the 1902 Act to regulate the position.

[148] For a summary see Michael Sanderson. *Education and Economic Decline in Britain 1870 to the 1990s*. CUP 1999.

[149] Parliamentary Papers 1854 XXVII Reports of Committees of Inquiry, paper 1713 Organisation of the Civil Service. Letter from the Rev. B. Jowett.

[150] Quoted in BV Bowden. *Faster Than Thought*. Pitman 1953 p7

"from contempt in our own country, from ridicule in others."[151] Much of Babbage's attention is diverted by personal animosity toward the Society's officers, but yet he does note some fundamental weaknesses compared to similar learned societies in France, Italy, and—growing in ambition—Prussia. The restricted membership of the learned societies abroad: in Italy, forty; in Prussia, thirty-eight; seventy-five in France, made membership a distinction and honour. By contrast the membership of the Royal Society, at nearly seven-hundred, was far less distinguished; not only as scientists (most were clerical or medical men), but also in terms of social status; Babbage notes the far more numerous titles of nobility in learned societies elsewhere.[152]

But, though confined to a single chapter, the more fundamental of the deficiencies noted by Babbage concerns education. Though reforms had begun at Cambridge by the 1830s Babbage could still write,

> A young man passes from our public schools to the universities, ignorant almost of the elements of every branch of useful knowledge; and at these latter establishments, formed originally for instructing those who are intended for the clerical profession, classical and mathematical pursuits are nearly the sole objects proposed to the students ambition.[153]

His proposed reform did not question an assumption that such education would be restricted to an elite (and that not founded upon intellect)—"our system of academical education ought to be adapted to nearly the whole of the aristocracy of the country."[154] But the idleness of this aristocracy was to be discouraged: a degree should indicate the possession of "a certain quantity of knowledge." This knowledge should be of greater variety, and appeal to more varied tastes, than hitherto, but attendance at lectures and examination should be a precondition of graduation. The new subjects proposed might be grouped: History and Law, Political Economy and applied science, Chemistry and Geology, Zoology and Botany. (Note that this time, there was no 'physics'; that aspect of natural philosophy was, as mechanics and astronomy, subsumed into the 'mathematical pursuits'.)

Something like these reforms was adopted, first at Cambridge and later at Oxford, but it did not generally lead to a greater status for the sciences. In England the two learned professions, medicine and law, were studied independently of the universities at teaching hospitals and inns of court.

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[151] Charles Babbage. *Reflections on the Decline of Science in England and On Some of its Causes.* London [1830] (ed Martin Campbell-Kelly) Pickering & Chatto 1989 p23

[152] It may of course be that there is an auto correlation between the distinction conferred by restricted membership and the presence of noble titles. Even so Babbage's claim that intellectual activity has a lesser status in England stands. "Had an English editor wished to particularize that nobleman, he would undoubtedly have employed the term *wealthy*, or some other of the epithets characteristic of that quality most esteemed among his countrymen." *ibid.* p18

[153] *ibid.* p2

[154] *ibid.* p2

Though a limited knowledge of science might be asked of a military engineer, that was hardly a sure and certain route to status. In England a university degree was not, of itself, an essential part of the path to advancement. When in the 1870s there was political pressure to ensure greater competence and probity in administration by introducing entry by examination into the Civil Service it was the traditional subjects, Classics and, to a lesser extent, Mathematics that prospered. For the Civil Service examinations were based upon, and therefore favoured, those of Oxford and Cambridge, which in turn favoured those who had, by dint of a public school education, had Latin and Greek beaten into them (literally) from an early age. Thus the social composition of Government was preserved, the governing elite of Britain was never an intellectual elite.

Stray (1998) in his account of classics education in the nineteenth and twentieth centuries argues that both the balance of emphasis between Latin and Greek and the interpretation of these cultures reflected contemporary interests. Moreover there were differences among European nations in their appropriation of the classical heritage. From late eighteenth-century, in both Germany and England, Hellenism was ascendant; Latin, associated both with republican Rome and the mediæval church fell out of favour as a model and ideal of social organisation—it had become associated with republican France. Hellenism was thus in Germany associated with an emerging nationalism.

> The upper-middle class groups in Germany who carried the ideology of romantic Hellenism were politically weak, widely scattered members of a society which was not yet a nation. Against the Roman classicizing of the Gallic tradition they constructed a Hellenic ideal whose utopian nature reflected their own distance from the sites of power and the political fragmentation of Germany.[155]

The re-foundation of universities in Prussia adapted those institutions to produce schoolteachers rather than priests. Where once academic careers were founded on Biblical scholarship they were now turned to classical philology.

In England however, a reviving Hellenism was associated not with nationalism but social distinction; it was part of a transmutation of capital into culture. It was not, like Latin, associated with discipline but with a transcendent and independent culture: "while some parents demanded useful knowledge for their sons, those in search of status looked precisely for the useless: an education which both in its results and by being undertaken at all, would symbolise a gentlemanly independence from the need to work."[156]

Though never as influential upon the educational curriculum as classics, and always a minority interest, mathematics nonetheless had an established place

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[155] Christopher Stray. *Classics Transformed; schools universities and society in England 1830–1960.* Oxford 1998 p25

[156] *ibid.* p21

in the universities. At Cambridge in particular, the reflected glory of Newton's achievement gave it a particular prestige.

Newton had transformed Kepler's model of planetary motion, harmonious but empirical, and given it a purely mathematical foundation.[157] But Newton was conscious of the absence of a material mechanism that would explain gravity, the force at the heart of his *Mathematical Principles of Natural Philosophy*. There was a tension between the experimental empiricism initiated by Bacon and the demands of the new, 'mechanical philosophy' that explanation should be in terms of matter and motion alone. Descartes' vortex model of planetary motion would not do, the motion might be observed but not the matter.

> What is taught in metaphysics, if it is derived from divine revelation, is religion; if it is derived from phenomena through the five external senses, it pertains to physics; if it is derived from knowledge of the internal actions of our mind through the sense of reflection, it is only philosophy about the human mind and its ideas as internal phenomena likewise pertain to physics. To dispute about the objects except insofar as they are phenomena is dreaming. In all philosophy we must begin from phenomena and admit no principles of things, no causes, no explanations, except those which are established through phenomena. And although the whole of philosophy is not immediately evident, still it is better to add something to our knowledge day by day than to fill up men's minds with the preconceptions of hypotheses.[158]

There is evidence in the unpublished drafts of the *Principia*, that Newton had explored possible analogies with magnetism, (static) electricity, light, heat and bodily sensation.[159] But finally Newton's solution was simply to leave the question open:

> I have not as yet been able to deduce from the phenomena the reason for these properties of gravity, and I do not feign hypotheses. For whatever is not deduced from the phenomena must be called a hypothesis; and hypotheses, whether metaphysical or physical, or based on occult qualities, or mechanical, have no place in experimental philosophy. The impenetrability, mobility, and impetus of bodies, and the laws of motion and the law of gravity have been found by this method. And it is enough that gravity really exists and acts according to the laws that we have set

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[157] It would be too much of a digression to consider here the many explanations and interpretations of the achievements and significance of the work of Galileo, Descartes, Newton etc. For a extensive overview of the historiography see: H Floris Cohen. *The Scientific Revolution*. Chicago. 1994

[158] Isaac Newton The *Principia*, translated by I Bernard Cohen and Anne Whitman. University of California Press. 1999. p54 [the preliminary draft of Newton's unpublished preface to the *Principia*]

[159] See I Bernard Cohen. A Guide to Newton's *Principia*, University of California Press. 1999. Chapter 9.

> forth and is sufficient to explain the motions of the heavenly bodies and of our sea.[160]

Thus an important aspect of Newton's contribution was this model of the 'scientific' mode of writing. The reader was not to know from reading the *Principia* that the system of 'rational mechanics' and the dynamics of his 'system of the world' "were not ends in themselves but only parts of a unified general ordering of the universe that encompassed alchemy, the theory of matter, 'vegative' and other 'spirits', prophecy, the wisdom of the ancients, and God's providence."[161]

It is in the acceptance and development of Newton's system that the divergence in British and Continental tradition becomes apparent. In Britain, it appears, the principle of '*hypotheses non fingo*' was observed; it was a purely mathematical exposition of Nature's laws to which a physical explanation was as yet to be appended. Elsewhere it seems there was a greater tendency to accept these mathematical laws in a Platonist spirit; as being the essence of reality. Perhaps this was aided by the greater facility for mathematical working afforded by Leibniz's version of the calculus. (Or perhaps the answer is to be sought in a Catholic rather than Anglican approach to the ineffable?) Whatever the reason, by the close of the eighteenth century, mathematics at Cambridge had fallen into decline.

The reform of mathematics in Britain, in which Babbage took a significant part led to the introduction of methods of analysis from continent. At that time mathematics, like Latin, was intended as a 'training of the mind'. It was not taught primarily for any intrinsic worth —the concept of a 'liberal education' did not countenance learning a useful trade or skill—but rather that the supposed accomplishment of thinking rationally could be applied to the more tangled and intractable features of real life. Thus in the mid-nineteenth century those Cambridge wranglers who were not destined for the church (as more than two thirds were) were as likely to follow a career in the law as to remain in academic life. But those few who did were set to dominate British physics as it developed in the nineteenth century, there were few professors of physics in any British university at the close of the century who had not studied at Cambridge.

In Cambridge it was the tradition of 'mixed mathematics' (i.e. applied to physical systems) that led to the mathematical physics which was to rise to particular eminence with Thomson and Maxwell in the 1850s and lead to the founding of the Cavendish laboratory. Cambridge mathematics, still in the shadow of Newton, was strong on astronomical calculations, optics, and fluid

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[160] Newton *op cit*. p943

[161] I Bernard Cohen. A Guide to Newton's *Principia*, p60. [It should be kept in mind that this is a very recent interpretation. Certainly, during the period covered by this history, and among the scientists who are our subject, understanding of Newton's ideas was confined to what he had actually published.]

mechanics. There was however little interest in heat, magnetism and electricity. These had developed outside Cambridge, initially by Faraday and Joule, and then at Glasgow and Edinburgh where the tradition of Natural Philosophy favoured experiment and Baconian induction. By contrast at Cambridge, though not theoretical and abstract in the continental way, physics was fundamentally mathematics applied to physical systems (mechanics) rather than stemming from an interest in physical systems *per se*. It was the merging of these traditions, particularly by Thomson and Maxwell both Scots who built upon the insights of Faraday, that formed what has been called the 'Cambridge School' of mathematical physics of the mid– to late–nineteenth century. This physics had the prestige of established learning, not only the medieval pedigree of mathematics but also the traditions of Newton and Bacon. It kept close company, if not with actual experiment, then at least to analogy with, and observation of, nature. It was wary of 'framing hypotheses', of purely abstract theorising. Thomson was an innovator in analogy—prepared to link disparate physical systems e.g. heat and fluid mechanics, not by any similarity of process but solely by mathematical equivalence. Maxwell also was prepared to use physical analogy as a stepping stone to a mathematical model that went beyond anything that was perceptible as a mechanism.

Maxwell wrote in *On Faraday's Lines of Force* "The aim of exact science is to reduce the problems of nature to the determinants of quantities by operations with numbers." In the course of the nineteenth century the study of electricity and heat became the province of physics rather than chemistry—it passed from Joule and Faraday to Thomson and Maxwell. Energy rather than Force became the key concept. The result was that by the close of the nineteenth century the art of mathematics had become the foundation of 'science', and physics, adding new phenomena—heat, electricity and magnetism—to a domain that already claimed mechanics and optics, was established as pre-eminent among the sciences. Perfection in science was equated with the ideal of mathematical form. Thus the completeness of physics at the close of the nineteenth century can be seen not just as the tendency for an old guard to see their life's work as being finished, it is also 'complete' in the sense of being fully mathematicised. Moving on to 'complete' biology as many physicists would do in the following century (not to mention similar ambitions of social sciences, particularly economists) can be seen as having an always implicit and often overt intention of mathematicisation.

In an edition of Bacon's Novum Organum, published in 1889 the editor, Thomas Fowler, Wykeham Professor of Logic at the University of Oxford opined in a footnote to the ninety sixth aphorism of book one:

> "I am inclined to think that Bacon had conceived, though perhaps not very clearly, the true relation between Physics and Mathematics. The most general axioms of Natural Philosophy were to be carefully established by induction; then, when they had been stated in a quantitative form, they were to be combined with each other, and

> worked out deductively, by means of mathematical calculations, into all their ramifications and applications. It is needless to add that the magnificent achievements of modern science (witness, for instance, the Principia or Optics of Newton, or the Mécanique Analytique of Lagrange) have been due at least as much to mathematical calculation as to inductive generalisation. The more advanced sciences, in fact, become, in their later stages, mainly mathematical. But, while a science remains in its earlier, or inductive, stage, it is only capable of mathematical treatment, if at all, to a very slight degree. This for instance, is still the case with the sciences of chemistry and physiology."

It should be noted that this gloss upon Bacon is that of a mathematician and logician, not an experimental scientist, certainly not an engineer. The passage of Bacon referred to does not elevate Mathematics above all, rather it refers to Natural Philosophy as currently impure and corrupted: by logic of the Aristotelians, the natural theology of Plato, and the mathematics of neo-Platonists. In the preceding aphorism[162] in which Bacon calls for a hitherto un-attempted alliance of experiment and pure reasoning, Fowler's commentary stresses "how far Bacon was from recommending a merely empirical philosophy".

Of course physics was not "complete", but nonetheless there was, a closure; the physics that came after was different. It was not deterministic, it was not an analogue of concepts tacit to the mechanic or engineer. In a sense physics had become more than complete, it had become pure mathematics. One consequence of this was a growing divide between the teaching of mathematics as a necessary adjunct to other arts such as engineering and an academically pure mathematics. In the 1880s, John Perry, Professor of Mechanical Engineering at Finsbury Technical College, had pioneered the teaching of "practical mathematics". A syllabus based on this teaching was adopted by the newly created Board of Education (successor to the Department of Science and Art) in 1899. It was a deliberate departure. Perry, disparaging the high-status academic mathematicians of Cambridge, described his methods as "exceedingly different from what used to be the study of the mere mathematician of the same subjects".[163]

This engineering-centric approach preferred decimals and approximations to fractions and introduced logarithms and slide rules into the teaching of arithmetic. The plotting of graphs on squared paper was a significant innovation in the teaching of algebraic formulae. For geometry, the idealised axioms and deductions of Euclid's geometry were less favoured than methods of drawing and measurement. Trigonometry, solid geometry, calculus and vector methods, all traditionally considered "advanced" were included. Overall, the approach was empirical. Contemporary writers and publishers referred to it as 'learning by doing', 'heuristic' and 'observational'. The

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[162] See quotation preceding chapter 3.

[163] Perry quoted in Price *op cit*

influence of the practical work that was a feature of teaching in school science laboratories was acknowledged.

Though science teaching had featured in the curriculum since the 1870s a sharp divide remained between ‘classical’ and ‘modern’ sides. The demands of the new sciences were not met by the traditional courses in which mathematics was considered an art. Yet this arts based mathematics was the abstract mathematics favoured by the universities. A consequence was to reinforce a differentiation between mathematics taught as part of a technical education and as an academic subject. The public schools of England, with university entrance examinations in mind, naturally concentrated on the latter. Nonetheless reforms were made in the early 1900s, the Universities following the lead begun by the Civil Service Commission ten years before when drawing and measurement were introduced in army entrance examinations.

These attitudes towards mathematics were part of a wider ‘declinist’ debate at the time. The reform of mathematics teaching was supported by the British Association for the Advancement of Science. The *Engineer* of February 1902 carried an article in favour of reform:

> In England almost alone has there been solid refusal to budge with the times on the part of University mathematical examiners and public school headmasters. They have stuck to the literal inspiration of their Euclid, and by their influential stolidity have retarded for more than a full generation the intellectual development of the British race.[164]

The “Perry Movement” suffered from over-enthusiasm in some cases (“graphomania” was a frequent criticism) and in the expansion of secondary education following the Education Act of 1902 a shortage of suitably qualified teachers was notable. By the time of Perry’s death in 1920, a reaction was discernible, with calls for a “return to the Classics” (i.e. Euclid), for “intellectual discipline” and less “pandering to the spirit of mere utility in education.”[165] The separation, reinforced by the 1902 Education Act, between technical and academic education was thus maintained. In Universities also, the establishment of Engineering as a subject distinct from both Mathematics and Natural Science led, paradoxically, to a move away from ‘practical mathematics’. Status required detachment from connotations of manual labour and a “broader and deeper theoretical basis”. Further extension of secondary education notably after 1944 brought a renewal of interest in the ‘useful and less academic’. As Price (1986) observes “deep rooted issues of social class, status and control in education were implicit”.[166] In this context we might note the remarks of Alan Turing’s headmaster:

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[164] RH Smith ‘Reform in Mathematical Education’ *Engineer*, (93, 7 February 1902 pp129-30) quoted in Price (1986) *op cit*

[165] See Price *op cit*

[166] MH Price *The Development of the Secondary Curriculum*. Croom-Helm, 1986

> I hope he will not fall between two stools. If he is to stay at a Public School, he must aim at becoming educated. If he is to be solely a Scientific Specialist, he is wasting his time at a Public School.[167]

The "Perry Movement" can be considered a world-wide phenomenon. His innovations had been influenced by his teaching of electrical engineers in Tokyo between 1875-9, and his methods were taken up enthusiastically in America. There it was believed that this 'tangible' mathematics would give the student "a feeling toward his mathematics extremely different from that which is met with only too frequently—a feeling that mathematics is indeed itself a fundamental reality of the domain of thought, and not merely a matter of symbols and arbitrary rules and conventions."[168] The influence of this and its ready assimilation into a favoured mode of expression ("the penchant of engineers for the graphic idiom") has been posited as an important influence on the analogue computing machines of Vannevar Bush.[169] But, whereas in America this practical influence could extend into elite engineering schools such as MIT, in England there remained a deep social divide between the 'practical' and 'theoretical'.

The prejudice against anything resembling manual labour, found sciences *infecta et corrupta* in proportion to lack of mathematics. Laboratories were long resisted by the ancient English universities.[170] Note also how to Thomas Fowler in 1889,[171] chemistry was in "its earlier, or inductive stage", this at a moment when the German chemical industry supported by chemists trained in its research universities, founded on a technology, aniline dyes —invented thirty years earlier in England—had already established its dominance in the British market.

By the end of the nineteenth century, at least in some eyes, physics could be considered 'complete'. The future would be one of refinement and consolidation. Other sciences however, particularly biology, were open to further advances, to approach the determinism and 'lawful' predictable behaviour that exemplified physics. It is notable that in the advocacy leading to the founding of the National Physical Laboratory in 1900, the promise of startling new discoveries, is absent. The need to advance industrial research, improve technical education, determine fundamental physical constants and define accurate standards of measurement for science, industry and

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[167] Quoted in A Hodges. *Alan Turing, the Enigma*. Hutchinson 1983 p26

[168] EH Moore, presidential address to American Mathematical Association 1903, quoted in Owens (1986) *op cit*

[169] See L Owens. 'Vannevar Bush and the Differential Analyzer: the text and context of an early computer' *Technology and Culture* (27.1) 1986 reprinted in Nyce & Kahn. *From Memex to Hypertext*. Academic Press1991

[170] In the 1960s plans for a new biological science building at Oxford met with particularly strong opposition. It is a subject that dominates the papers of John Pringle. [See bibliography]

[171] *qv*. quotation above.

commerce, are all advanced for the case. But not a promise of innovation and invention by way of fundamental discovery.

The National Physical laboratory was founded in 1900 after a prolonged campaign led by the British Association for the Advancement of Science and supported by the Royal Society to create in Britain an institution comparable to the *Physikalische-Technische Reichanstalt* established in Berlin in 1883. As we have already shown there were notable differences between England and her continental rivals in the development of education, state institutions and the status of science. The debate surrounding the foundation of NPL also confronted the question of whether such a research institute should follow the French or German model. In France elite schools had been established independent of ancient universities thus separating secular education from religious foundations. Teaching and research were also assigned to separate institutions. The German model, less driven by anti-clerical considerations, created state universities and combined teaching and research. In England the established universities, (which remained Anglican institutions) prevailed, and for the most part government, when it supported research at all, favoured universities rather than the establishment of research institutes. In this context the creation of a National Physical Laboratory is something of an anomaly.

The development of physics in the nineteenth century and the advance of mathematical and quantitative explanations demanded the reliable determination of physical constants. Coupled with this primarily scientific interest, new industries, particularly those associated with electricity, required the establishment of legal standard measures of hitherto un-conceptualised quantities. The NPL was thus the result of a confluence of interests: the *parvenu* professional science lobby, represented by the British Association for the Advancement of Science, wanted a state funded research laboratory; the more established Royal Society accepted that modern science would require resources beyond the pocket of amateurs; the Government, as always swayed by the Treasury, accepted the mercantilist case. The link with the Royal Society lent scientific *gravitas* to an institution whose Treasury funding was justified by the pragmatic interests of industry and the Board of Trade.

This characterisation of the NPL at genesis should also give weight to the perception of physics at this time. From the time of Newton, physics had been triumphant, not only was it successful in predicting the behaviour of the material world, but in approaching the perfection of mathematics it linked empirical and idealist world views. To some, at the close of the nineteenth century, it appeared that physics was nearly "complete", it only remained to fill in the details. For the present discussion the question of how widespread this (ironic with the benefit of hindsight) belief was around 1900 need not be at issue.[172] What may be taken as significant, however, is that whilst the

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[172] See Arthur M Silverstein '"The End is Near!" The Phenomenon of the Declaration of Closure in a Discipline' *History of Science* xxxvii (1999).

proponents of a National Physical Laboratory argued for the establishment on various grounds: national competitiveness, technical education, trade standards, industrial innovation etc., it does not appear to have been argued that a NPL was necessary for, nor would lead to, new discoveries or innovations in fundamental scientific knowledge. The NPL was a *physical* laboratory, its foundational purpose was not the advancement of physics but the determination of physical constants.

The Great War of 1914-18 changed the perception of science in Britain and exposed the weakness of those British industries dependent on scientific research. The industrial strength of German industry and the determined support of scientific education and research in that country was often invoked as reason enough to improve British science. Though it has since been questioned how valid such 'declinist' arguments were there is no doubt that such views were and continue to be widely held. Thus in 1974, the historian DSL Cardwell, speaking to a Royal Society audience, noted that the market in aniline dyes derived from coal-tar, discovered in Britain in 1856, was dominated by German companies by the 1880s. "[by 1914] about 80% of the dyes used in Britain, the great textile country with abundant coal, were imported from Germany."[173] Similar problems affected other industries based on the new technologies founded on chemistry and electricity.

One result was government funding to encourage research in universities. As this was intended to be in support of industry direction by the Board of Education was thought to be inappropriate. At the same time government efforts to establish indigenous chemical and optical industries had been hampered by the difficulty of establishing an administrative structure with scientific and technical credibility. The creation of the Department of Scientific and Industrial Research, part of the Board of Trade, in 1915 was a consequence. It assumed responsibility for financing scientific research in all fields except medicine (for which there was a separate Medical Research Council). A separate Scientific Civil Service was created to supply scientific advice and assistance to various branches of government. The NPL which though grant aided by government had been in the care of the Royal Society was drawn into this scheme. In 1917 the National Physical Laboratory was, in effect nationalised, it became a research station of the DSIR. But although funding rested with the DSIR, a notional authority over NPL resided with a General Board composed of nominees of the Royal Society and representatives of industry. Actual oversight of research programmes was left to an Executive Committee, which was dominated by the Royal Society.[174]

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[173] DSL Cardwell. 'Science and World War I' *Proc. Roy. Soc. Lond. A.* 342 447-456 (1975)

[174] Of the sixteen members of the Executive Committee, four were *ex officio* members of the Royal Society, six direct nominees of the Royal Society and the remaining six chosen by the Royal Society from a panel of eighteen; nominally representatives of

This position, of Government paying the bills but the Royal Society calling the tune, persisted until 1966 when the NPL finally came under the full control of the newly created Ministry of Technology. In these circumstances there was always some tension in defining the role of NPL; was it to provide a standards and measurement service, R&D for industry or be a government funded institute of purely academic research?

Because the NPL existed before the creation of DSIR its purpose and status was not equivalent to other research stations. A committee of enquiry into the DSIR noted in 1956 that the heads of its Divisions could be regarded as equivalent in status to Directors of other laboratories.[175] It was not funded by industry research associations, but neither was its work clearly the province of government as was the case with military establishments such as RAE or TRE, or facilities with obvious strategic importance such as Harwell. Post-war the expansion of universities had increased the research funding dispensed through the University Grants Committee. The role of the DSIR in funding research and postgraduate education was thus called into question. It was stated "the Department's industrial effort is not made because it is an inherent function of government [...] the aim should be to make industry research minded" But neither the committee of enquiry nor the subsequent DSIR Act of 1956 seem to have resolved this. The provisions in that Act to reduce Royal Society influence at NPL were of limited effect, with negotiations over implementation continuing up to the creation of the Ministry of Technology in 1964.

There were also anomalies in the position of scientists within the government service. Industry paid better, universities offered greater status, defence establishments could more often command greater resources. Until 1966 the pensions of scientists within the government service were provided through the universities scheme, this was intended to facilitate an interchange of personnel between research establishments and universities. But this also highlighted the divisions within the civil service: administrators, often less highly qualified, were paid more and were promoted to higher grades.

Thus in 1956, the Superintendent of NPL's Mathematics Division, ET Goodwin, speaking to the NPL's Executive Committee:

> He pointed out that really first class men do not, in general, get into the Scientific Civil Service; the fact that the division has a number of senior staff of such calibre is an accident due to the war.[176]

"industry" but in fact the nominees of nine professional engineering institutions. In practice these appear to have also been FRS.

[175] A copy of the report was sent to Gordon Sutherland (then still at Michigan) by HJ Hadow (UKSM Washington) on 30 April 1956. In the accompanying letter he *states "I think you know Cmd9734 is a much abridged and bowdlerised version of the full Committee report. I understand that the full report was given severely limited circulation"* [Sutherland Papers, Cambridge Add 8353 C36]

[176] PRO DSIR 10 409. NPL Executive Committee Papers 1956, 21 February 1956.

Much of the work at NPL was routine testing, funded by fees paid by industry. Other research, prompted by the current concerns of government, could be transferred to other establishments as the perceptions of its importance changed. A case of particular interest here is the Radio Division. This joined the NPL in 1933, having previously been a separate Radio Research Station of the DSIR. In early 1935 its Superintendent, Robert Watson-Watt instigated the research program that led to the development of radar—"the whole technique already being worked out for ionospheric work at Radio Research Station."[177] But with its military implications realised Watson Watt and his team moved to a new establishment at Orfordness. By 1941, after several moves to apparently safer ground, this had become the Telecommunications Research Establishment at Malvern. A consequence for NPL was that in 1945 the NPL's Radio Division had no expertise in the new pulse coded electronic techniques needed to build the ACE. Electronics research and expertise had been built up elsewhere in the civilian sector: at the Post Office, the BBC and EMI's pre-war work on television had been preserved and enhanced by the development of radar—all this had passed NPL by. To DH Whiffen, who had joined the NPL's Basic Physics Division in 1960, it appeared that

> The laboratory was demoralised and had not really recovered from its wartime decline at the expense of TRE, Malvern, Harwell, Chalk River, Los Alamos and many other establishments which took the best of British Physicists. No one started lively important research in London within range of a German attack, the exception being those efforts which required ship tanks, notably the Barnes Wallis block busting bombs.[178]

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[177] Watson Watt. *Three Steps to Victory* (1959) p430 quoted in Robert Buderi. *The Invention That Changed the World.* Abacus 1996 p57

[178] DH Whiffen to N Sheppard, 17 November 1980. This is part of the material assembled by Shepard for his biography of Sir Gordon Sutherland (*Biographical Memoirs of Fellows of the Royal Society*. 28. 1982 pp589–626) and deposited with the Sutherland Archive. Cambridge Add8353 A53

Part II

Qui tractaverunt scientias aut empirici aut dogmatici fuerunt. Empirici, formicae more, congerunt tantum, et utuntur: rationales, aranearum more, telas ex se conficiunt: apis vero ratio media est, quae materiam ex floribus horti et agri elicit; sed tamen eam propria facultate vertit et digerit. Neque absimile philosophiae verum opificium est; quod nec mentis viribus tantum aut praecipue nititur, neque ex historia naturali et mechanicis experimentis praebitam materiam, in memoria integram, sed in intellectu mutatam et subactam, reponit. Itaque ex harum facultatum (experimentalis scilicet et rationalis) arctiore et sanctiore foedere (quod adhuc factum non est) bene sperandum est.[179]

Francis Bacon *Novum Organum*

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[179] Those who have treated of the sciences have been either empiricists or dogmatists. Empiricists, like ants, merely accumulate and use; Rationalists, like spiders, fabricate webs of themselves; the bee takes a well considered mean: its materials are gathered from the flowers of garden and field; but it has the ability to change and rework them. This is not unlike the true craft of philosophy; which does not rely solely or principally upon mental powers, nor record the matter of natural history and mechanical experiment unremarked, but transformed and refashioned by the intellect. Therefore to bind these faculties (experimental and rational) in closer and purer agreement than has yet been made promises well. *Novum Organum* liber I.xcv

Chapter 3 *Ratio*

Nineteen Forty-three was a notable year: though yet unnamed, 'cybernetics' was certainly in mind. Warren McCulloch and Walter Pitts presented *A Logical Calculus of the Ideas Immanent in Nervous Activity*,[180] Norbert Wiener, Arturo Rosenblueth and Julian Bigelow published *Behaviour, Purpose and Teleology.*[181] In England, Kenneth Craik, who had studied philosophy at Edinburgh before joining the Psychological Laboratory at Cambridge, published *The Nature of Explanation*.

Warren McCulloch, once characterised his life's work by a characteristically flamboyant question: "What is a number that a man may know it, what is a man than he may know a number."[182] Kenneth Craik posed equally large questions but took a more pragmatic approach: why and how was the neural mechanism able to model so much of the world? And what was the significance of measurement and calculation—considered as a product of the neural mechanism—to this process?

> Our question, to emphasise it once again, is not to ask what kind of thing a number is, but to think what kind of mechanism could represent so many physically possible or impossible, and yet self consistent, processes as number does.[183]

McCulloch and Pitts introduced their hypothesis, "Because of the 'all or none' character of nervous activity, neural events and the relations among them can be treated by propositional logic" with a note of caution: it was a "theoretical neurophysiology [...] neither of us conceives the formal equivalence to be a factual explanation." But, in 1943, intellectual fashion favoured a mode of explanation that sought to bring together statements of observations and the rigours of logic. It is perhaps not entirely a coincidence that the advent of the computer should coincide with the high point of logical positivism; it requires a certain commitment to the value of computation to make the difficulties of its mechanisation worthwhile.[184] But the ultra-rationalist temper of the age was cast upon computing: digits and logic, symbolic abstraction and precision have come to be seen as of its essence. But there were other ways of seeing computation: as analogy and model, mutually, yet imperfectly, bonded to a biological organism.

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[180] WS McCulloch, WA Pitts. 'A Logical Calculus of Ideas Immanent in Nervous Activity' *Bulletin of Mathematical Biophysics*, vol. 5 1943 [This issue also included a review of the second, greatly enlarged, edition of D'Arcy Thompson's *On Growth and Form*]

[181] A Rosenblueth, N Wiener, J Bigelow. 'Behaviour, Purpose and Teleology' *Philosophy of Science* 10 1943

[182] W McCulloch. *The Embodiment of Mind*. MIT 1965. p2

[183] Kenneth Craik. *The Nature of Explanation*. Cambridge 1943. p55

[184] I use 'logical positivism' here in a broad sense i.e. a particular school of school of philosophy whose *denkstil* had a significant influence on society at large.

Craik sought an accommodation between theory and practice. The advance of experimental science has made *a priorism* less acceptable, "no one would now dare to draw up a list of self-evident but not tautologous propositions."[185] The "strict formalism" of Russell and Whitehead and the attempts of the logical positivists to resolve all difficulties by the more exact use of words and unambiguous definitions obliged them to adopt a circumscribed view of the world that bears little relation to the "untidy tangle of experience."

> But in a theoretical study such as philosophy the new facts which present themselves are determined more by one's mental make-up than by an impartial sampling of reality; for it is association rather than experience that presents them. So even this 'coherence test of truth' may indicate, as perhaps it does in the case of Kant's 'architectonic', not so much the objective validity of the theory as the groove in which the author's mind runs. Instead of his theory being as wide as reality, his perception of reality may be as narrow as his theory.[186]

The methodology of the 'positive sciences' does not demand 'complete exactness and finality' but an extension of the capacity to predict the behaviour of the empirical world. Philosophy should learn from physics, says Craik. But the physics he has in mind is self-verifying, affirmed by experiment and observation. At a time when physics was tending toward an increasingly abstract and 'philosophical' view of the foundations of nature, Craik seems to hold a rather Baconian view, a study of nature rather than abstraction. In short it is in the tradition of biology rather than the new physics.

An obsession with exactitude, of supposing that any observation or experiment might be definitive, is a fundamental error. There is no justification in supposing 'that the nature of the world must accord with our exact definition',[187] Paradox, an unavoidable consequence of the limitation of human senses and perception is to be accepted. The problem "should be corrected in the way of greater extensiveness and denotative power, rather than greater analytical, intensive or connotative exactitude."[188]

Avoiding abstract *a priori* theory, the philosopher and psychologist should seek the correlation of prediction and experiment. The justification of a theory is that it 'works' as a prediction—"the power to explain involves the power of insight and anticipation."[189]

This anticipation is innate, insight a "distance-receptor in time". An organism, if it is to survive, must anticipate. Thought can be viewed as a

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[185] Craik *op cit.* p1

[186] Craik *op cit.* Preface

[187] Craik *op cit.* p17

[188] Craik *op cit.* p4

[189] Craik *op cit.* p7

mechanism for prediction and it is not surprising that "our thought processes are frustrated by the unique, the unexplained and contradictory." Philosophical and psychological language that emphasises the noun rather than the verb has hindered our understanding. Physical reality itself is no longer conceived in terms of immutable substances. "A mass of iron ore is so unlike an engine that the uninitiated might disbelieve that an engine could ever be made from it". Thus in the case of consciousness, a search for a fundamental substance leads nowhere, an understanding of process and mutability is essential.

> Maybe there is more organisation, and a new kind of simplicity and unity in the living organism; but this possibility seems to me no reason for handicapping ourselves by trying to explain living processes in language that could not describe non-living ones.[190]

Reality has a 'fuzziness' that leads to paradox and contradiction, the result of nominative language, of assuming our nouns to have an impossible precision.

> In general, the language of causality seems the most fruitful and successful and the language of substance, or existence and immutability, the least.[191]

But if Craik finds an excess of precision and nominalism unsatisfactory he is equally dissatisfied with relativity and purely descriptive theories. The statistical physicists—he refers explicitly to Born—are unjustified in the "substitution of *indeterminism* for *uncertainty*". "The physicists seem to have a curious belief that they have constructed things rather than discovered them." There is no justification for "imposing on reality the burden of supporting the shortcomings of our own intellects and instruments. [...] In the absence of the assumption of causality the whole notion of probability seems meaningless."[192]

For Craik then, our concepts are not constructed *a priori* but are derived from, and made to fit, experience. When our concepts "fit the facts" that is because they indicate the nature of a pre-existing, objective reality. Causality, physical mechanism, is central.

> Somewhere, consciousness enters and has its function; but the thought that this is so should not deter us from applying physical explanations as long as they reduce the anomalies. Indeed, we should find an increase in anomalies, or an increase in the number of facts which cannot be met, as soon as we apply physical explanations in spheres where they do not apply.[193]

In this, we may see an echo of Craik's own transition from philosopher to psychologist; a recapitulation of psychology's own emergence at the close of the nineteenth century as a discipline distinct from philosophy. A study of the

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[190] Craik *op cit*. p21

[191] Craik *op cit*. p22

[192] Craik *op cit*. p33

[193] Craik *op cit*. 49

mental world that was not only empirical but material rather than metaphysical. The external world exists and it is by a causal process that mind is made from matter. In this view thinking is modelling: an external event or process is observed, translated into symbols, 'reasoned' with, and finally, retranslated into either external action or "recognition of the correspondence between these symbols and external events."[194]

Though Craik speaks of translation into symbols the examples Craik uses have more of the flavour of a mimetic mechanism than abstraction: Kelvin's tidal predictor, an anti-aircraft 'predictor'. The gear teeth of a calculating machine are seen as representing by position, but do not betoken numbers.

> By a model we thus mean any physical or chemical system which has a similar relation-structure to that of the process it imitates. By 'relation-structure' I do not mean some obscure non-physical entity which attends the model, but the fact that it is a physical working model which works in the same way as the process it parallels, in the aspects under consideration at any moment.[195]

Thus Craik's 'symbols' are certainly not Platonic objects of pure disembodied reasoning.

> Any kind of working model of a process is, in a sense, an analogy. Being different is bound somewhere to break down by showing properties not found in the process it imitates or by not possessing properties possessed by the process it imitates. Perhaps the extraordinary pervasiveness of number, and the multiplicity of operations which can be performed on number without leading to inconsistency, is not a proof of the 'real existence' of numbers as such but a proof of the extreme flexibility of the neural model or calculating machine. This flexibility renders a far greater number of operations possible for it than any other single process or model.[196]

This is perhaps the moment to consider the distinction between 'digital' and 'analogue/analog' computing. There are two versions of this distinction, which the dominance of a particular form of computing in the years since Craik wrote has served to occlude. Firstly there is a distinction to be made between computation by modelling, and calculating by the formal manipulation of tokens and symbols. Secondly, is the distinction drawn between representing quantities by the measure of some analogous substance or physical state, and representation by number symbols. The electronic digital computer favours both numerical (digital) representation and token manipulation. In the 1940s this combination was not self evident. Calculators were mechanical, the analogous position rather that the symbolic state of components was their most obvious feature.

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[194] Craik *op cit*. p50

[195] Craik *op cit*. p53

[196] Craik *op cit*. p53

It is modelling rather than symbolism that is central. Words and other tokens, spoken or written, are only one means of thinking. The *process* of relating tokens, is also a symbolism, but once again what is important is not the symbol itself but "the ability of processes to parallel or imitate each other." Thus, in the case of a calculating machine, it is the whole machine that is taken as symbolising a process. In today's terms Craik's view is perhaps consonant with the idea of a particular program, a piece of software, being a model of a process.

The model need not precede nor be a successor to, the external object. Which is the model and that which is modelled, is but a matter of convenience. The model is but one side of an equation. It is whichever counterpart may most conveniently permit the trial of alternatives—cheaper, smaller, possibly faster, than the real thing.

The particular advantage of the nervous system is that the mimesis is one of pattern rather than substance: the nervous system only has "to produce combinations of excited arcs, not physical objects".

> My hypothesis then is that thought models, or parallels, reality —that its essential feature is not 'the mind', 'the self', 'sense-data', nor propositions but symbolism, and that this symbolism is largely of the same kind as that which is familiar to us in mechanical devices which aid thought and calculation.[197]

In this view there is representation but not hypostatisation of ideas. There are components within the machine that may be identified as analogues, and these, to an observer, may appear symbols of external objects; but these are not manipulated by the machine as tokens of external objects.

> Yet it is clear that, when any calculating machine is in operation, there are objects or events —the number of teeth projecting from an Odner wheel or the radial distance of the friction wheel on an integrating disc— which represent numbers, and have a greater degree of 'thinghood' and conceptual definiteness than the interconnecting parts and the continuous transmission of motion from one part to another. In the same way in a neural calculating machine there may well be patterns of excitation in the cortex, temporal and spatial groupings of impulses, and so forth which, to a physiologist sufficiently skilled, would 'represent' concepts or sensations of objects.[198]

Is the whole greater than the sum of its parts? Observation of the apparent movement of the planets can lead to the conceptualisation of the solar system — even though "no one has ever stood at some distant point in space [...] from which he might see that system as an obvious unit or pattern". It is not therefore futile to hope that an understanding at the small scale of neural processes might lead to an understanding of the larger scale process of

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[197] Craik *op cit*. p57

[198] Craik *op cit*. p76

thinking. A mind can hope to know its own nature, synthesis from the bottom up is possible.

Yet this position is not exactly reductionist, it does not expect to find the whole explanation in the details. The intention is to create a hypothesis from partial observation, a hypothesis that may then be matched to observation. In Craik's view: if theory matches expectation, if the model is near enough exact, that is because the theory matches an independent, external, objective reality.

Complexity does not always mean that something is hard to understand, structure and organisation may make for easier comprehension. The atomic complexity of components acquires the simplicity of purpose and co-ordination when assembled.

The dangers of introspection must be recognised, there is no hard and fast distinction to be made between conscious and unconscious thought. The difficulty on a particular occasion of a familiar manual task may bring it into conscious consideration. And, equally, a task requiring close attention may be learnt and become automatic. What is, and what is not conscious, whether it is possible to think of two things at once, these are questions that often depend on how a task is defined. A far more interesting problem is the nature of the mechanism.

Thought and feeling are not separable in Craik's view; even in scientific work unarticulated ideas and emotions have a part to play. The difference between the scientist and the artist is that the former must test ideas against experimental results, whereas the measure of artistic worth is wholly a matter of human judgement. In both cases however the mind is not to be considered in isolation from its environment which is both physical and social. Despite this recognition of what we might now describe as the embedded nature of cognition, science remains objective and ahistorically cumulative. Theories may be poor and inaccurate but, being aligned to reality, they are assumed to converge on truth.

Both the classical moralist, seeking the origin of action in the idea of the good, and the hedonist, emphasising the purely selfish motive are equally in error.

> On our view any man is part of a causally connected universe, and his actions are part of the continuous interaction taking place in it.[199]

There is no "hypothetical common causative factor in all acts". We must ask instead "how large a part of reality—of the external world and of other men's thoughts and wishes—is influential in any given act of a particular man."

The mind is thus "a highly complex machine, built of parts having dimensions where the classical laws of mechanics are still very nearly true, and having dimensions where space is, to all intents and purposes, Euclidean."[200] Like a

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[199] Craik *op cit*. p90

[200] Craik *op cit*. p95

calculating machine that can parallel phenomena such as the stress and strain in a bridge, the brain can model and predict phenomena. Possibly, it cannot model all phenomena, only those features of the world whose mechanism resembles itself.

> ...the powers of one mechanism to imitate another are remarkable, and by suitable conventions the most evasive and paradoxical phenomena may be represented in terms of a rigid mechanism. But even if such thought *does* finally fail to describe the facts consistently I do not see that it will mean that its causal interpretation of the nature of the world, based on its representation of the larger objects in that world which it is more fitter to represent is wrong.[201]

Thus we are of the world, and in the world, and our limitations when theorising about the world that is beyond the range of our senses does not diminish the value of our sensations.

> Our thought then , has objective validity because it is not fundamentally different from objective reality but is specially suited to imitating it.[202]

Behaviourism is seen to have limitations, the mechanism of the conditional reflex is important because an understanding of a modifiable reflex would explain a much wider range of intelligent response than mere reflexes. But that does not mean that all such response is in essence only a reflex. It is the concept of the reflex, and concepts that may be derived from the detailed mechanism, that is important.

> The emphasis is on the modifiable mechanism which sometimes shows itself in the modification of a reflex—not on the phenomenon of reflex modification.[203]

The holism of the Gestalt approach is also to be criticised: it is not enough to describe what happens, a causal mechanism should be found. To do otherwise is “like explaining a railway collision by saying the two trains are drawn together by a force”.[204]

The problem of understanding a mechanism, even one we have made ourselves, is appreciated. The problems of debugging predate the programming of computers.

> Further, it is not true that a calculating machine gives out no more than its designer put into it; he usually designs it in order that it *shall* do something he cannot— *vide* the Bush differential analyser. Even in a wireless set it is possible, by making a mistake in the wiring, to obtain effects which it may require the best brains to diagnose, if they are required simply to think out the cause of the malfunctioning.[205]

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[201] Craik *op cit*. p95

[202] Craik *op cit*. p99

[203] Craik *op cit*. p114

[204] Craik *op cit*. p114

[205] Craik *op cit*. p117

So what, in summary does Craik have to say? Firstly, explanation is giving the cause of a thing or event. Though we may be mistaken in particular cases, on the whole, our perceptions are to be trusted. Secondly, Humean scepticism is self contradictory; "it assumes the ability of words to symbolise events, an assumption no better and no worse than that of causation". The relativity of modern statistical physics is equally contradictory, for (according to Craik) the theory of probability is based on the assumption of rigid causality. Finally, the old *a priori* philosophies are seen to be fallacious in the light of experimental science, and the verbal and formal precision of symbolic logic and logical positivism are but the old *a priorism*.

> Assuming then the existence of the external world I have outlined a symbolic theory of thought in which the nervous system is viewed as a calculating machine capable of modelling or paralleling external events, and have suggested this process of paralleling is the basic feature of thought and of explanation.

This has been a detailed account of *The Nature of Explanation*, why? It is necessary to recover a view which has been obscured by subsequent developments. Craik was writing before the advent of computers in the form we now take as archetypal – electronic, digital, logical, tokenising. His computer is analogue. Though he is aware of the digital/analog mode of calculation there is no conception of a computer of the Turing Machine form. Indeed, considering his self-confessed unease with the advanced mathematics of quantum theory, and the logic of Russell & Whitehead, it seems unlikely that he would have had much interest in questions such as the *Entscheidungsproblem*. His interest is not in formal reasoning. He is concerned with the physiological basis of *thinking* not *intelligence*. Thus there is no interest in building intelligent machines. Thinking machines, however, already exist. The differential analyser and the desk calculator are thinking machines according to Craik's hypothesis – the 'modelling' theory of thought. As such these point the way to understanding thought as a physiological process. A 'computor' is always, whatever the mechanism, an analogy machine. And, being equivalent, the choice is one of convenience only which—mind or mechanism—is taken as a model of the other.

In a postscript, on the evidence for a physiological basis for thought, he refers to several works on neuro-physiology. The one exception is about electronics, but concerns neither symbolic reasoning nor calculating machines. If "Hoag's *Basic Radio*" is analogue electronics it certainly isn't an analogue of arithmetical or logical reckoning. It is, like the brain, a complex machine having effects that bear no obvious relation to its physical composition or 'mechanism'. We should note also that Craik does not single out 'higher thought processes' for special attention: human, animal, or machine, thought as modelling is a unity. He concludes in a challenge to his critics: "at what level in the animal kingdom do you consider that life, feeling, thought, purpose, will, and consciousness originate?"

Craik hoped to test his theory by experiment and in 1947 a paper *Theory of the Human Operator in Control Systems* was published in the *British Journal of Psychology*. But this was a posthumous work; Craik had died, age 31, following a road accident the previous day, on 8th May 1945.[206] As Maurice Wilkes recalls: "Like many men of outstanding promise who die young, he had become something of a legend; but in his case the reputation was not undeserved, and he would undoubtedly have gone far."[207]

Craik's notes for *Theory of the Human Operator in Control Systems* and other work were tidied up by his co-workers at the Medical Research Council, John Bates (working at the National Hospital, London) and WE Hick (at Cambridge). Both seem to have found the task daunting, Bates suggesting that further assistance be sought:

> Uttley, Tustin , Campbell and others gave Ken many of his ideas and have shown a lot of interest in the pure physiology and psychology of the game (especially the first two). I agree that it should all be written up in one part, and suggest that they might be asked to come in.[208]

But Bates also saw an opportunity to promote their approach, urging that publication be under the auspices of the Medical Research Council rather than the Ministry of Supply.

> The idea that the M.R.C. are employing folk with an interest in servos hasn't got home to Keppel Street. [...] I think that a lot of good could come by installing now our goods firmly in the physiologist's shop instead of hawking them on the pavement, and an M.R.C. publication is a way to do it.[209]

This plan met difficulties, publication had to be by the Ministry of Supply:

> ...the purpose is stated to be the provision of permanent records of research carried out for the Ministry during the war, both in its own establishments and extra-murally.[210]

There were also "loose ends" to the research:

> Many of Ken's unfinished investigations seem to be intended to "refute Uttley's theory," but it doesn't seem to be known here what the theory is.[211]

Bates replied a week later:

> Uttley had two "theories" with which Ken disagreed: 1) that the operator was incapable of absolute quantitative measurement; 2) that there was "visual feed back" in some of Ken's experiments.[212]

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[206] Obituary by FC Bartlett: Nature June 1945.

[207] M Wilkes. Memoirs of a Computer Pioneer. MIT 1985. p22–23

[208] Wellcome Library, Archive: Bates, John AV, and the Ratio Club. GC/179 B.31, Bates to Hick 30 May 1945.

[209] *ibid*

[210] GC/179 B.31, Hick to Bates 31 May 1945.

[211] *ibid*

It is unclear to what extent Bates and Hick did prepare the paper. *Theory of the Human Operator in Control Systems* appeared, in two parts, in the British Journal of Psychology in December 1947 and May 1948. A footnote credits the preparation for publication to a Miss MA Vince.

The first part, *The Operator as an Engineering System*, describes the operator as "an intermittent correction servo". The second part, *Man as an Element in a Control System*, is less technical. The system is described as a chain of sensory devices, a conceptual system, an amplifying system and mechanical linkages. The intent is "to discover in detail the characteristics of this human chain" [...] "with a view to showing the various advantages and disadvantages of the human operator as compared with an automatic system."

The editor of the Journal, Frederick Bartlett, in an introductory footnote suggested

> [It] was intended, not as a final form for publication, but as a preliminary draft for discussion. In view, however, of the very lively and widespread interest that has developed in the problems discussed in these two papers, and of the attractive and original way in which Craik handles them, it seems highly desirable that they should be made public.[213]

On 23rd February 1948 Bates gave a lecture to the Cambridge Psychological Society: *Man's Design and Performance—speculation before and after Sherrington*. As his notes make clear the talk was "a reiteration of [Craik's] thesis"

> He would have [a] broader basis than gadgets [...]
>
> Can we conceive [of a] total explanation of human behaviour in all its aspects in terms of existing physical [notions] – in particular in terms of a spacial [*sic*] array of electric potentials as a function of time.[214]

This reference to "electric potentials" is that of the nervous system, but by 1948 a new form of machine had come into existence. The computer was to change the terms of inquiry, thereafter the human operator would be modelled less as a servo mechanism more a symbol processing logician.

Craik appears in this light as the lost leader of a group of British physiologists and psychologists, who as a consequence of their wartime redeployment, developed an interest in electronics and control mechanisms. The interest in electrical mechanisms in the nervous system was not new, it had been established before the war. But the redirection of research imposed by necessity, moved it toward engineering technology rather than pure science, something that, but for this wartime demand, might not have had such comprehensive influence.

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[212] GC/179 B.31, Bates to Hick 8 June 1945.

[213] British Journal of Psychology vol. XXXVIII (2) December 1947

[214] GC/179 A.6, Bates. notebook L33.

In 1948, servo mechanisms were brought to the attention of a wider audience with the publication in both America and Britain of Norbert Wiener's *Cybernetics–or control and communication in the animal and the machine*. To Bates and others, his ideas, though controversial, revived an interest that might otherwise have faded as they resumed their careers as physicians and biologists. It was Wiener's publication that prompted Bates to found the Ratio Club. It was, from the start, an intentionally informal gathering of like-minded "cyberneticists". That term must be used with caution. It is a convenient label for ideas that were always vaguely defined but it masks a diversity of views and ambitions. If it had not been for Norbert Wiener's cooption of the word, the congeries of ideas that had emerged from pragmatic psychological and physiological studies during the war years may never have gained a label. In Britain in particular, without the spur of Wiener's publication, those who regarded themselves as having 'had Wiener's ideas before Wiener' might never have gathered at all. Writing to Grey Walter, a neuro-physiologist at the Burden Neurological Institute in Bristol, Bates explained :

> I have been hearing a lot of 'Cybernetic' discussions during the past few weeks here and at 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 at say 7.0pm 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 discussion at some point in this stratesphere[*sic*] but in essence the gathering should evolve in its own way.[215]

Bates proposed membership included the associates of Walter: Ross Ashby and Harold Shipton; two colleagues of Bates at the National Hospital in London: George Dawson and Pat Merton; several friends at Cambridge: Horace Barlow, Edmund Hick, Thomas Gold and John Pringle; plus Donald Sholl a "statistical neurohistologist" at University College, and Donald MacKay who was working on "computing machines" at Kings College, both in London; and Albert Uttley, identified by Bates as "ex psychologist, radar etc. TRE". Bates letter continues:

> I could suggest others but that 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.

[215] GC/179 B1, Bates to Grey Walter 27 July 1949..

Thus, from its instigation, the club was never intended to be a vehicle toward gaining formal credence for 'cybernetics', but rather an opportunity to discuss a 'mechanistic' interpretation of the nervous system without fear of ridicule. An atmosphere neither of undue reverence for the "ghost in the machine" nor wildly uninformed speculation on the future of "electronic brains". Looking back much later, John Pringle wrote:

> The club was unique and in the 17th century tradition; to me it was invaluable in getting me back into biological research after the war.[216]

During August, Bates corresponded with the prospective membership and a few more names emerged. MacKay suggested John Westcott, an electrical engineer at Imperial College, who, like MacKay had worked with Norbert Wiener for a year at MIT. Both Westcott and Grey Walter were in touch with Warren McCulloch in America.

> 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: Could we get McCulloch along to an inaugural dinner after his talk for you?[217]

The membership was to be "half primarily physiologists and psychologists though with 'electrical leanings'" and half communication theory and ex-radar folk with biological leanings".[218] Pringle suggested some mathematical strengthening was necessary:

> I hope we shall find a mathematician to join the group as the whole discussion could be somewhat vague if we were not kept in check by someone with a quick appreciation of Mathematical formulation.[219]

Horace Barlow also thought that "a cautiously selected psychologist or two would be a good thing" as he felt he knew "none of the psychological literature".

> ... What one needs is someone who knows what is psychologically nonsense, what is psychologically common sense.[220]

This need to contain speculation though not inhibit imagination was critical, Bates did not want a large gathering of people who were merely interested, but as he wrote to Scholl, only those who "may use the concepts in their everyday work". The membership should not exclude people who were good, but too much seniority might be inhibiting. Warren McCulloch, a "high

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[216] GC/179 B26, Prof. JWS Pringle, Oxford to Prof. DM MacKay, Keele. 17 January 1981

[217] GC/179 B1, Bates to Westcott. 3 August 1949,

[218] these phrases recur with variations in several letters

[219] *ibid.* Pringle to Bates 5 August 1949

[220] *ibid.* Barlow to Bates 25 August 1949 [A letter in which Barlow apologises for a delay in answering "caused by my thesis which I've been fighting hard these last few weeks".]

priest", "full of stimulating phrases and half finished experiments" would be an attraction for the inaugural meeting but this was not to be the format thereafter. Writing to Grey Walter after the first meeting which was held on 14th September 1949, Bates admitted:

> I had led myself to expect too much of McCulloch and 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 six occasions and by drinking with him in private on several more, that he had chunks of his purple stuff stored parrot wise.[221] By and large however I found him good value.[222]

That initial meeting brought together seventeen members to hear McCulloch, who as Grey Walter observed "seems to like fancy titles", declaim upon *Finality and Form in Nervous Activity.* Those who attended , in addition to Bates original proposed membership, were V Little of the Physics Department at Bedford College (introduced by MacKay), Tom McClardy of the Maudsley Hospital, Eliott Slater another colleague of Bates at the National Hospital, and Westcott. Grey Walter, "detained by domestic responsibilities," was unable to attend.[223] A few more members were proposed:

> I have got together a dining club. It consists at the moment of 17 members, half biologists - (mostly neurophysiologists) and half engineers and mathematicians. We all have a common interest in the working out of recent advances in communications and computing theory.
>
> We had our first meeting last week and two members who knew you personally ([Tom] Gold & Pringle), and those who did not, were unanimous in wishing that you would join the company. The next dinner has been fixed for Tuesday Oct. 18th in London and we do hope you will feel able to come. Gold said that he would write to you about it too.
>
> The club has no defined 'purpose' and it has been decided to occupy the first few meetings in requesting each member in turn to make a few personal introductory comments.[224]

Turing replied positively though he noted that, because of the difficulty of travelling from Manchester he would be unable to attend many meetings in term time. Bates' notes, appended to a typescript list of the initial

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[221] Donald Davies (interviewed 11 February 1998) made a similar observation regarding Wiener: "inspiring but an illusion ... heard him give a lecture five years later [*c*1952] —nothing had changed."

[222] GC/179 B3, Bates to Grey Walter 4 October 1949.

[223]GC/179 B3, Grey Walter to Bates 14 August 1949. As he explained later "I was sorry not to get to the first meeting, owing to the delivery of a male homeostat which I was anxious to get into commission as soon as possible" [GC/179 B1,Grey Walter to Bates 29 September 1949]

[224] GC/179 B1, Bates to Turing 22 September 1949. There is also a ms draft of this letter in Bates' notebook at GC/179 A10. The typescript version has 'John Gold' probably a misreading of Bates' handwriting which has Tom Gold in the notebook ms.

membership, remark cryptically, "incomplete - no sociologists, northerners, professors. No Craik, No Daniel".

So, four years after his death, Craik's absence was noted; we may reasonably suppose that the ideas outlined in *The Nature of Explanation* remained influential among this group. And probably to a wider audience; Craik's only publication in book form was reprinted by Cambridge University Press in 1952, and again in 1967. Wiener's publication of *Cybernetics* stimulated debate, but to members of Bates' dining club, his ideas were familiar and he was not their sole source. But did this group share all of Wiener's ideas —what were Wiener's ideas?

A child prodigy, Norbert Wiener gained a PhD in Philosophy from Harvard University in 1913. He was then aged 18. Then studied with Bertrand Russell in Cambridge, before travelling to Göttingen to study with Hilbert. From philosophy his attention turned to mathematics. The advance of quantum theory in physics brought new attention to statistical theories. Wiener's interest in the Brownian Motion and harmonic analysis led to his publication of *The Fourier Integral and Certain of its Applications* in 1933[225]. He went to MIT on his return from Europe in 1923, and it was there that he worked on control devices during the war.

As the introduction to *Cybernetics* makes clear, its genesis was philosophical "a monthly series of discussion meetings on scientific method" conducted at Harvard Medical School by Arturo Rosenblueth. They took an interdisciplinary view, believing that "boundary regions of science [...] offer the richest opportunity." It was such a boundary—observing the similarity between the wild oscillation known to engineers of servo mechanisms as 'hunting' and the uncontrollable tremors of nervous disorders such as Parkinson's Disease, that gave rise to the publication, with Julian Bigelow and Arturo Rosenblueth, of *Behaviour, Purpose and Teleology* in1943, the same year as Craik's *Nature of Explanation*.

Making a distinction between behavioural and functional method, *Behaviour, Purpose and Teleology* presents behavioural study as an opportunity to observe mechanical and neural feedback as mutually analogous. Behaviour is divided into the random and the purposeful. The presence or absence of feedback further categorises purposeful behaviour and purposeful behaviour with feedback is to be denoted teleological. The classification continues with a predictive form of teleology. "All purposeful behaviour may be considered to require negative feedback."

This distinction based on the purposefulness of machines is achieved by ignoring the intention of the maker. A roulette wheel, having random behaviour is described as "designed precisely for purposelessness". And a

[225] Cambridge 1933

clock, though its maker's purpose is conceded, is nonetheless declared to be, of itself, without purpose: "there is no specific final condition toward which the movement of the clock strives." Thus purpose is identified with autonomous goal directed activity. To attain a goal it is, by implication, necessary to have the potential to both over and under-estimate the goal.

With this identification of the predictive form of teleological behaviour the similarity with the thinking of Craik is clear. A view of both animal and machine as predictive calculators is common. *Behaviour, Purpose and Teleology* perhaps goes further in its equivalence of animal and machine by positing the possibility of robots: "The ultimate model of a cat is another cat, whether it be born of still another cat or synthesis in a laboratory."

But by identifying animal behaviour with that of predictive machinery Rosenblueth, Wiener and Bigelow are breaking away from strict behaviourism. Teleology "rather discredited at present" is identified with the mechanism of feedback, thus the behaviourist might properly consider purpose, not as an occult 'metaphysical' occurrence within a black box but afferent and observable.

The final paragraph of *Behaviour, Purpose and Teleology* makes one further claim:

> According to this limited definition, teleology is not opposed to determinism, but to non-teleology. Both teleological and non-teleological systems are deterministic when the behaviour considered belongs to the realm where determinism applies. The concept of teleology shares only one thing with the concept of causality: a time axis. But causality implies a one way, relatively irreversible functional relationship, whereas teleology is concerned with behaviour, not with functional relationships.

Thus the apposition of the views of Craik and Wiener concerns nature of causality.

The concepts of causality and time, Newtonian and Bergsonian, underpin Wiener's argument in *Cybernetics*. Newtonian time is reversible, the analogous pattern is that of the wheel, it is a palindrome; "the book of astronomy reads the same backward as forward". The laws of mechanics apply if the variable denoting time in its mathematical formulation has a negative value. But these Newtonian systems are isolated, we can presume to possess to a sufficient approximation, all the necessary data.

This is not the case with complex systems. To predict, say, the weather we must make do with estimates and probabilities. Statistical methods are asymmetric, the prediction becomes less precise the longer it runs, reversing the process will not converge on the initial assumption.

For Wiener there is a connection between the twentieth century challenge to Newtonian physics, evolutionary processes and the philosophy of Bergson. "About the year 1900, it became apparent that there was something seriously wrong with thermodynamics...” The result was the statistical theory proposed by Heisenberg in 1925 whereby:

> ...Newtonian physics has become a picture of the average results of a statistical situation, and hence an account of an evolutionary process. [...] Bergson emphasised the difference between the reversible time of physics, in which nothing new happens, and the irreversible time of evolution and biology, in which there is always something new.[226]

Thus in Wiener’s view the old antagonism between vitalism and mechanism is resolved on new ground. That “Newtonian physics was not the proper frame for Biology” can now be conceded, there is no wall between matter and life; matter can now be seen to be as complex and irreversible a process as life.

“The thought of every age is reflected in its technique.”[227] From the watch as the model of a Newtonian cosmos to the statistical thermodynamics of the steam engine. The mark of the modern age is the separation of power and communication engineering. The power to move machinery and the signal that controls it are, to Wiener, separate entities. There is no longer the ‘kick-back’ of a mechanical linkage to be suppressed. On the contrary it is now important for ‘feed-back’ to be deliberately engineered into the system. The aim of the communications engineer is “not economy of energy but the accurate reproduction of a signal”.

When Wiener coined the term *cybernetics* he was unaware that it had been used by the physicist Ampère in 1843 to mean the art of government. According to Ampère this was a sense, by analogy with the art of steering a ship, quite commonly invoked by the ancient Greek writers. Wiener’s neologism may have contributed to a certain suspicion of a general theory of control and government. (Wiener notes that *governor* is derived from a Latin corruption of the Greek.) A *science* of control would rob not just biology of occult vitalism, those who practised the art of politics might also distrust a claim to an exact science and theory of government. To a classically educated elite the original Greek sense—the political, the rhetorical—if not Ampère’s *cybernetique* would have more resonance than the new and specialist sense intended by Wiener. An enthusiasm for planning and normative control was part of the, but, certainly in Britain, enthusiasm for science and an implicate modernity was tempered by suspicion. Scientists were perhaps seen as a new and rising elite, and thus a threat to those without scientific training.

What is new in Wiener’s concept of the function of a governor is the role of communication. Wiener acknowledges James Clerk Maxwell’s 1868 paper on the Watt governor as significant in the history of feedback: it gave an abstract mathematical description of the mechanical device patented by Watt in 1788. Watt’s device for regulating a steam engine had precursors in similar devices

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[226] Norbert Wiener. *Cybernetics*. (2. edn) MIT 1961 [p37]

[227] *ibid.*

developed for windmills.[228] Wiener, in abstracting the concept of communication, takes the theoretical generalisation a stage further. Mechanical feedback is no longer limited in application to a single device. Connections between devices or between animal and machine can now be analysed as if instances of mechanical linkage. All government can be treated as a matter of communication, and a theory of mechanism applied. This theory is not, in Wiener's formulation, a deterministic causality, but stochastic and non-reversible in time. None the less, this treatment is fundamentally mathematical—an idealisation of real objects—it is not an intuitive, tacit theory of government.

Ross Ashby wrote:

> Cybernetics stands to the real machine – electronic, mechanical, neural or economic – much as geometry stands to a real object in our terrestrial space.[229]

The cybernetic view replaces the simple unidirectional causal chain from intention to action, and the simple reflex arc of behaviourist theory, with a less deterministic, circular model. The neurologists distinction between afferent and efferent nerve impulses, the binary simplification of the McCulloch and Pitts model of the neuron, do not fit easily into Wiener's theories of mechanism. As well shall see, Wiener's ideas did not decompose to simple calculations, and as symbolic calculation became possible using computers, attention turned to ideas and problems that could be readily answered with this new machine.

Wiener's earlier mathematical work on Brownian motion, time series and ergodic theory is united with a Bergsonian vitalism. It is one more step toward a virtual rather than physical world view, concerned with forms rather than substances. Wiener wrote in *The Human Use of Human Beings* in 1950:

> Our tissues change the pattern remains [...] we are but whirlpools in a river of everflowing water. We are not stuff that abide but patterns that perpetuate themselves.

In the later nineteenth century the heat engine replaced clockwork as the technological model of choice. Thermodynamics was applied to models of the living organism: "It is the metabolic balance which is the centre of attention."[230]

It is not the steam engine but the electronic valve which is the model for cybernetics. A low energy device in which noise rather than loss of power is the engineer's first concern. Automata are no longer entirely autonomous, they are "effectively coupled to the extended world". The rules of operation are subject to modification from within and without. Thus automata can be

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[228] See: T van Gelder. 'Dynamics and Cognition' in J Haugeland. *Mind Design II.* MIT 1996

[229] Quoted in PR Masani. *Norbert Wiener 1894–1964*. Basel: Birkhäuser 1990. p258

[230] Wiener *op cit.* p41

"subsumed under one theory with the mechanism of physiology". The theory is statistical.

> Thus the modern automata exists in the same sort of Bergsonian time as the living organism [...] Vitalism has won to the extent that even mechanism corresponds to the time structure of vitalism.[231]

The central chapters of *Cybernetics* are technical.

> We are dealing precisely with those matters for which the symbolism of mathematics is the appropriate language, and we can avoid it only by long periphrases which are scarcely intelligible to the layman, and which are intelligible only to the reader acquainted with mathematical symbolism by virtue of his ability to translate them into this symbolism.[232]

Donald Mackay, a member of the Ratio Club who had worked with Wiener at MIT in the1940s, reviewing these chapters of *Cybernetics* noted:

> They include the fruits of some years of war work on filter design and prediction but make difficult reading for the non-specialist.

Writing for the readership of *Electronic Engineering*, Mackay rated this material as important but overly condensed. The space devoted to an overlong and 'chatty' introduction would, MacKay suggests, better have served its audience if it had been given over to a more detailed exposition.

In these chapters the heritage of Wiener's work on ergodic theory and Brownian motion is apparent, as is the influence of Shannon's communication theory. Unlike Craik, Wiener fully accepts the indeterminacy and stochastic basis of quantum mechanical explanation. The observations, even of "classical physics are incomplete and approximate" being

> ...sufficiently precise for the needs of classical physics over the range of precision where it has been shown experimentally to be applicable.

Contrast this with the similar observation of Craik, where he makes a reluctant admission of the limits of the humanly observable world. For Craik the position, essentially Kantian, is that there is a limit to observation but an implicit faith in the causality of what cannot be observed.

> If it fails, in the end, to represent in itself microscopic phenomena obeying different laws this will merely mean that the mind –itself an instrument like the microscope– is faced with a situation to which it is inapplicable, as a microscope is inapplicable to the resolution of points nearer than half the wave-length of the light employed. [...] But it will not mean that interdependence is unreal, and that causal explanations are a misrepresentation of statistical results; the interdependence of things will remain, as before, proved by the macroscopic effects.[233]

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[231] Wiener *op cit.* p44

[232] Wiener *op cit.* p98

[233] Craik *op cit* p95

To Craik observations are necessarily anthropocentric, and causality is, despite his avowed rejection of *a priorism*, a fundamental commitment. For Wiener however, the indeterminacy of the future is accepted, "the great contribution of Heisenberg" was to replace a Newtonian causal physics with "one in which the time series can in no way be reduced to an assembly of determinate threads of development in time".[234]

Wiener distinguishes "the analogy[*sic*] machine [...] where the data are represented on some continuous scale" whose accuracy is determined by the precision of its scale, from a numerical machine where the accuracy is a matter of "the sharpness with which the contingencies are distinguished". The binary form of numerical machine is to be preferred as the most cost effective.

It is of interest that no clear distinction is drawn between the choice of representation of quantities—by analogous scaling or discrete tokens, and the method of problem representation and computation—by analogous model or symbolic manipulation. It is here perhaps, that we can discern the influence of the simplified McCulloch and Pitts neuronal model. The neurons are "ideally suited to act as relays". Wiener mentions only briefly "non-neuronic influences, perhaps of a humoral nature" may affect the function of the brain".[235] But, although the electronic control systems with which Wiener was familiar were analogue in both senses, a combination of digital neuronal model and the engineering advantage of binary numerical computation tended toward a view of the brain as a digital computer.

If the brain is not quite a computer to Wiener, it is because the brain has only one program which must run continuously throughout life.

> Thus the brain under normal circumstances is not the complete analogue of the computing machine but rather the analogue of a single run on such a machine.[236]

In the context of "brain as a logical machine" Wiener defines logic:-

> The science of today is operational; that is, it considers every statement as essentially concerned with possible experiments or observing processes.[237]

Logic is not to be taken as *a priori*, pre-existing and independent of human thought however. Thought is broader than logic, "Psychology contains much that is foreign to logic", but a logic that went beyond our capacity to understand would mean nothing to us.

> All logic is limited by the limitations of the human mind when it is engaged in that activity known as logical thinking.

[234] Wiener *op cit* [p93]

[235] Wiener appears here unaware of the autonomic nervous system

[236] Wiener *op cit*. p121

[237] Wiener *op cit*. p124

The logic of the machine thus throws light on human logic, but it cannot extend what we understand by logic. It is a human trait, as is learning. Learning conceived as a dynamic process – an idea that has "filtered through from physics to the biological and psychological sciences." If it is mental content rather than process which has dominated psychology in the past

> This may well have been a survival of the scholastic emphasis on substances, in a world in which the noun was hypostatised and the verb carried little or no weight.[238]

The new computing machine is an analogy, an opportunity to consider new problems. The computer enables the solution of partial differential equations because of a capacity to perform the iterative process of successive approximation at high speed. But in the case of non-linear equations the mathematical theory was inadequate. The phenomena of turbulence can be explored experimentally in a wind tunnel (i.e. by analogy/model) but the possibility of fast and highly accurate computation carried a promise of developing the mathematical theory.

> It is already becoming clear in the use of these new machines that they demand purely mathematical techniques of their own, quite different from those in use in manual computation or in the use of machines of smaller capacity.[239]

Wiener concludes his treatment of the computing machine by remarking on the power consumption of the device; the heat generated is considerable yet insignificant relative to the number of individual operations performed. The appropriate measure of performance is not energy but information. "Information is information, not matter or energy. No materialism which does not admit this can survive in the present day."[240]

Wiener stresses that in both brain and computer the physical structure itself is insignificant, for the essence of the system is process. This concern with dynamic process is characteristic of Wiener's work, it applies to the early mathematical work, to cybernetics, and carries through into the social concerns that come to the fore in much of his later writing.

In discussing language, Wiener is well aware that this is not just a passive exchange of symbols but an activity in which both parties participate. He supposes himself encountering someone "who cannot speak my language and whose language I cannot speak". This does not make communication impossible, nor does it require pre-arranged codes or signs. Merely being alert to signs of emotion or interest in others is sufficient to learn a great deal about each other. "It will not be long before I discover the things which seem important to him, not because he has communicated them to me by language,

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[238] Wiener *op cit.* p127

[239] Wiener *op cit.* p131

[240] Wiener *op cit.* p132

but because I myself have observed them."[241] A "signal without intrinsic content" may acquire meaning by being related to observation. Thus communication precedes language, it is more than an exchange of tokens.

The growth of society brings a breakdown in communication. The increasing specialisation of science limits the potential insight to be gained from bringing diverse ideas together. There is a hint of the Epicurean *prolepsis* in Wiener's remark *apropos* Vannavar Bush's proposal for automated library searches, "they are limited by the impossibility of classifying a book under an unfamiliar heading unless some particular person has already recognised the relevance of that heading for the particular book."[242]

Wiener's world is necessarily dynamic, but it is an organism that to cohere and prosper requires stability, a dynamic coherence – a homeostatic process. Observing the contemporary interest in game theory and noting that free competition is "elevated to the rank of an official article of faith in the United States"[243] Wiener does not think it a recipe for social stability.

Von Neumann's picture of the player "as a completely intelligent, completely ruthless person is an abstraction and a perversion of the facts"[244] The world contains both "knaves and fools" the former exploit and the latter are exploited. In a small community the situation can be saved, the average person can be considered intelligent in matters which are of direct concern, and even altruistic when touched directly. In such circumstances a homeostasis of society is possible.

> It is only in the large community, where the Lords of Things as They Are protect themselves from hunger by wealth, from public opinion by privacy and anonymity, from private criticism by the laws of libel and the possession of the means of communication, that ruthlessness can reach its most sublime levels. Of all these anti-homeostatic factors in society, the control of the means of communication is the most effective and most important.[245]
>
> In a society like ours, avowedly based on buying and selling, in which all natural and human resources are regarded as the absolute property of the first business man enterprising enough to exploit them, these secondary aspects of the means of communication tend to encroach further and further on the primary ones.[246]

Communication should contribute to the stability of a dynamic society, but if communication is restricted to what is profitable, controlled by the consequently wealthy, motivated by ambition and power the "the state is stupider than most of its components". Cybernetics may contribute to an

[241] Wiener *op cit.* p157

[242] Wiener *op cit.* p158

[243]Wiener *op cit.* p158

[244] Wiener *op cit.* p159

[245] Wiener *op cit.* p160

[246] Wiener *op cit.* 161

understanding of these processes, but Wiener warns that does not mean it offers a means of management. His friends in the social sciences are mistaken in seeking to apply natural science methods to social problems, “From believing this necessary they come to believing it possible.”

The methods of natural science require a “high degree of isolation of the phenomena from the observer”. This is obviously so in the case of astronomy, and on the small scale of particle physics statistical aggregation rather than the fate of individual particles is the human scale view of the problem:

> we are too small to influence the stars in their courses, and too large to care about anything but the mass effect of molecules, atoms and electrons.[247]

This cannot apply to the social sciences: as would be the case with a machine logic that exceeded our understanding: a transcendental realism, even if possible, would not interest us. In the social sciences, by contrast, the objects of study are customs, people and events on the same scale as our own. The statistical runs on that scale are short and what we observe are artefacts of our own creation. “There is much we must leave, whether we like it or not, to the un-‘scientific’ method of the professional historian.”

Social concerns are increasingly visible in Wiener’s writing from the time of *Cybernetics* (1947), it was followed in 1950 by *The Human Use of Human Beings*, a more populist work, which takes up the themes of the final chapters of *Cybernetics*. Wiener added two new chapters to *Cybernetics* when it was republished in 1961 (the main text appears not to have been revised). The social themes are expanded by concerns about nuclear war. The scientific content is expanded by an account of non-linear feedback, electro-encephalography and self-organising systems. Thus he anticipates current work on dynamical theories of cognition:

> We thus see that a non-linear interaction causing the attraction of frequency can generate a self-organising system, as it does, for example, in the case of the brain waves we have discussed and in the case of the [electrical power generating alternator] network.[248]

Wiener was planning to visit Europe in January 1950 and John Bates wrote to him in November 1949 inviting him to speak to the Ratio Club. In this letter the club is described as “a near resemblance to your Macy Group” with a membership “stretching from psychiatrists to pure mathematicians”. Though Bates says that “the intervening specialities are adequately represented” the obvious difference compared to the Macy Group is the absence of social scientists. There was no equivalent of Gregory Bateson and Margaret Mead. Nor were there any philosophers. But some members of the club—Ross Ashby, Donald MacKay, Grey Walter—did attend meeting of the Macy

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[247] Wiener *op cit.* p163

[248] Wiener *op cit.* p202

Group.[249] Wiener did not address the club. He did not make his regular trip to Europe that year and though, writing to Bates in December 1949, he expressed a wish to meet the next year (1951) no meeting with the Ratio Club is recorded.[250]

After that inaugural meeting addressed by Warren McCulloch how did the club proceed?

The club held thirteen meetings between September 1949 and October 1950 in accordance with Bates' intention to hold frequent meetings initially. The first few meeting were introductory. At each two or three members talked on matters related to their own research interest. The name of the club was proposed by Uttley, who submitted an extract from a Latin dictionary.

> RATIO reckoning, account, computation, calculation;
>
> (whence) a register, list, catalogue; sum, number; a relation with; plan, mode of procedure, method, manner; the faculty of mind which calculates and plans; the reason; theory, doctrine, knowledge.
>
> RATIOCINARIUM A statistical account
>
> RATIOCINOR to compute, calculate; to argue, infer conclude
>
> RATIOCINATIUS Argumentative[251]

The place of logic and mathematics as a species of rationality in the conspectus of the society is interesting. As previously noted, the need for a formalism was recognised, but the membership was, despite Bates intention of an even balance, composed of more biologists than engineers and mathematicians. Uttley whose first degree was in mathematics, is conspicuously more 'rationalist' in relation to the group as a whole. After the second meeting of the club (18 October 1949), at which Sholl, Dawson, MacKay and Uttley had given presentations, Eliot Slater, in a letter addressed to Uttley and circulated to members,[252] asked if a digital computer could answer questions of a familiar logical type:

> Given that:
>
> 1. All the communications I have received which were written on white paper were typed,

[249] See SJ Heims. *The Cybernetics Group*. MIT 1991

[250] 1949 GC/179 B.3, Wiener to Bates 13 December. "Unfortunately, as I have already written to Grey Walter, my trip to Europe has had to be cancelled this year. However, I do expect to be over in January 1951. The reason for the delay is that I am now in Mexico, and my school does not want me to be absent for two terms in succession. I would like very much to see you when I do get over in 1951."

"Wiener made a formal arrangement permitting him to spend six months in alternate years with Rosenblueth in Mexico, engaging in collaborative research." [Heims SJ. *The Cybernetics Group*. MIT, 1993 p50]

[251] GC/179/B.3. There are several copies of this note, one copy is underlined (by Bates?) as above.

[252] GC/179 B.3, Slater to Uttley. 25 October 1949

2. No typewritten matter has been sent to me in an unsealed envelope,

3. Some of my bills are on white paper,

4. I have not paid any bills which did not come in unsealed envelopes,

Have I paid (a) all (b) any of my bills?

Slater, providing the answers "(a) No, (b) Unknown, insufficient information," shows his main concern was the capacity of the machine to recognise the case with insufficient information as being unanswerable. The potential of a machine to handle a large number of related propositions reducing both the tedium and confusion of a pencil and paper solution is not seriously doubted, but "could one rely on it to distinguish among the naturally occurring questions which might be put to the machine, those which could and those which could not be answered?". It would not be satisfactory to give only the answer to (a), reliability required also a clear declaration from the machine of what was unanswerable. This was a general requirement "and might apply to numerical or mathematical data of quite different form to the data given above".

There is no direct evidence for Uttley's response; but Hick, writing to Bates a few weeks later, suggests he was inclined to an overly complex, mathematical solution.

> I saw Uttley on Friday. He had not sent in his solution then, but thought it might involve "group theory". In very complicated cases, it possibly would, but it seemed to me that problems as elementary as Slater's could be dealt with by a suitable sorting machine, using punched cards or whatnot.

Hick recognised the human factor was significant, both in posing and solving such problems. With this kind of logical problem much of the difficulty was that of extracting essential meaning from a "smoke-screen of loose, quasi-ambiguous, and unnecessary verbiage". The solver was tricked into jumping to a conclusion rather than analysing the situation systematically. Yet, it had to be conceded that in most everyday situations (i.e. those not involving contrived puzzles) the intuitive approach would be appropriate: "doing it systematically would take far longer". This deliberate obfuscation in setting logical puzzles led to a misapprehension:

> But the result seems to be a natural tendency to think that logic is of a different order from maths, which I believe is not so. So if one wanted a machine to solve logical problems, it would doubtless be possible in theory, but one would have to teach it (or give it a knowledge of) Modern English Usage - and keep it up to date![253]

Thus, in Hick's view, there is a distinction between a logic problem in its essential form which is algorithmically soluble, and practical instances in which the underlying logic is clouded and a knowledge of language, fluid and

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[253] GC/179 B.3, Hick to Bates. 8 November 1949

uncertain, is essential. Both the possibility and the limitation of an 'expert system' is anticipated:

> I don't see much future in a machine to sort out this sort of nonsense - it would be infinitely cheaper to hire an expert (the naturally-occurring article, instead of the synthetic). Where there is a large mass of material, as in legal evidence, it might be amusing to feed it into a machine, in the hope of finding out who was the biggest liar![254]

Ross Ashby's response to Slater's question is also preserved. He outlines in detail an algorithmic method based on Boolean logic. There are two stages, "a conversion to Boole's algebraic form followed by a prescribed and unvarying process." The set form of the process is important, otherwise the process of programming would, of itself, be a solution. Once the problem has been classified in Boolean terms a particular question can be resolved by inserting a possible answer into the data and looking for a contradiction. Slater's indeterminate second answer can be revealed by inserting both possible (Boolean) responses. If both cases yield a contradiction then the information is unreliable, if neither then it is insufficient. Thus Ross Ashby concludes it is possible for a machine to solve a logic problem. But the linguistic problem remains:

> But I would like to add that if the machine is told that no glap can be a bilter, and that every mizzle always is a bilter, then it will insist that no glap can possibly be a mizzle. One wonders what value can be given to such information.[255]

The idea of learning is central to Ashby. He had described the "homeostat", a simple four element self-stabilising feedback mechanism, in the journal *Electronic Engineering* in 1948. Provocatively entitled *Design for a Brain*, this was followed by a book length treatment under the same title in 1952, with a second edition in 1960. As we have already noted in the case of both Craik and Wiener, the inspiration for this device owes little to the digital computer. The transformation in the concept of machine is due to the thermionic valve, which effectively decouples control from mechanical action. Power, in the ancient sense of control is no longer subject to "backaction" from a mechanical linkage. Thus, with electronic control, a machine –powerful in the modern engineering sense of rate of work done– is controlled by a machine with cognitive power.

Crucial, to Ashby, is the cognitive machine's capacity to develop a behaviour unanticipated by its designer. It must not only outperform the strategy programmed by the designer, it must also improve with practice.

Ashby's vision of the machine appears somewhat wild-eyed and Wellsian. Able to outperform the human brain it might "explore regions of intellectual

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[254] *ibid.*

[255] GC/179 B.3, "Note on Dr. Slater's Question" initialled W.R.A.

subtlety and complexity at present beyond the human powers."[256] Such a machine, fed with statistics and scientific facts, and its incomprehensible output obeyed, leading to a resolution of political and economic difficulties "by its understanding and use of principles and natural laws which are to us yet obscure". The machine would have a "temperament" dependent upon the engineered details of its design yet inscrutable to its designer. The self maintaining machine will be selfish: "Whatever the problem, it will judge the appropriateness of an action by how the feedback affects itself: not by the way the action benefits us."

> How will it end? I suggest that the simplest way to find out is to make the thing and see.[257]

The piece brought considerable response "some sarcastic, some complimentary", a selection together with a response from Ashby was printed in the journal in February 1949[258]. Ashby admits the second part to have been deliberately provocative at the suggestion of the editor: "Nor do I apologise." It is a good thing in any exploration, he writes, to look to the distant horizon. "That the brain is mechanistic in part cannot now be doubted." As a hypothesis, it is one that is beginning to yield riches. While "Ultimate truths are best left to the ultimate future" a research programme for the next decade, bringing together brain physiology and electronic engineering, had been set out by Grey Walter.

> The nervous system includes devices for integrating, differentiating, frequency modulation and discrimination, wave synthesis, storage and scanning, and makes elaborate group transformations of signals from one co-ordinate system to another; signals arriving as spatial pattern are displayed on a time base, others containing data on frequency differences are projected on spatial co-ordinates, and so forth. When all these have been reproduced electronically, *as individually they easily can be*, and combined in a homeostatic individual, we shall certainly have something –or it will have us.[259]

Ashby later emphasised the abstract mathematical nature of cybernetics:

> Cybernetics stands to the real machine –electronic, mechanical, neural, or economic– much as geometry stands to a real object in our terrestrial space.[260]

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[256] WR Ashby. 'Design for a Brain' *Electronic Engineering* December 1948.

[257] *ibid.*

[258] The provocative nature of the piece may be connected with similar journalistic exploits of the time provoking Sir Geoffrey Jefferson's *Mind of Mechanical Man* in June 1949. [See also A Hodges. *Alan Turing: the enigma*. Vintage 1992. p403-4]

[259] Dr W Grey Walter. 'Design for a Brain–Discussion' *Electronic Engineering* February 1949. [my italics]

[260] W Ross Ashby. *Introduction to Cybernetics* Methuen 1956. quoted in PR Masani. *Norbert Wiener*. Basel: Birkhaüser 1990

This view of cybernetics, as an abstract principle: a formal mathematical theory of mechanism operating within a Bergsonian concept of time (non-reversible, indeterminate), came to dominate later thinking on the subject.[261] But, as will later become apparent, such a broad conception made it difficult to fit cybernetics within existing academic domains. Nor for a long time was either mathematical technique or computational capacity equal to these ambitions. Meanwhile the fast development of the electronic digital computer made the alternative 'physical symbol system' approach, most notably championed by Newell and Simon,[262] ever more viable.

A sense of the limitations of a mathematical and algorithmic approach is found in a presentation made by Bates to a Symposium on Information Theory at the Royal Society in September 1950. The new theories of information and servo-mechanisms did offer biologists new unifying concepts but they were "overlaid, and to some extent obscured, by detailed mathematical extensions of no direct applicability".[263] A presentiment of the dynamicist construction of mental process can be detected when he suggests that organic memory is not stored data but "more akin to the 'instructions' in a computing machine".

> We can only infer the memory exists by a particular pattern of muscular activity – the spoken word is the result of a particular pattern of muscular activity.[264]

There appears an underlying tension between the desire of the scientist - working within an underlying tradition of a positivist interpretation of nature - to have explanatory theories, and the clinicians necessary pragmatism, deriving observation from particular cases. Bates writes in his notebook in September 1951: "Autonomics better to have wrong theory than no theory." A little later he writes: "importance of only vulnerable theories also simple, economy of hypotheses."[265]

Bates founded the club in order to discuss 'cybernetics' in a relaxed atmosphere. But almost from the start there were diverse opinions as to the purpose and future direction of the club. "What is the future of the Ratio Club after everyone has said his piece?" Ross Ashby was in favour of a much more formal society.

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[261] See Masani *op cit.* chapter 18 'Cybernetics and its historical origins'

[262] See: Allen Newell, Herbert Simon. 'Computer Science as Empirical Inquiry: Symbols and Search' *Communications of the ACM*, March 1976, pp 113-126.

[263] GC/179 A.24, Bates JAV 'The Significance of Information Theory to Neurophysiology' 28 September 1950

[264] *ibid.*

[265] GC/179 A.25, Bates. ms notebook, on cover: "B6 September 1951". [The handwriting is indistinct and the reading 'Autonomics' is possibly doubtful. It would suggest that when Uttley subsequently adopted the term as the name of his Division at NPL ten years later, it was at least a familiar term among his own circle. I have found no evidence that Bates knew anything of the work of Popper.]

> ... I think it might help to give point to our meeting if we agreed on a formal statement of range of topics. We might take "those subjects common to biology, physics and mathematics". But is this not precisely Biophysics? Then why are we not the Biophysical Society?[266]

Such a society should invite outside speakers, who "should bring their technique with them rather than their philosophy." Less discursive, it should "move to the attack on real problems". He attached a list of subjects for discussion; from the technical "methods of analogue computing", through "ecological dynamic systems" and "randomly assembled systems" to the "cybernetic faults of the Civil Service."[267]

The eighth meeting of the club on the 21st April 1950 included discussion of the club's management, how speakers were to be chosen –"volunteers or conscription"– and choice of subjects. The topic recurred at the next meeting where Ashby's proposal to concentrate on one problem over several sessions until a "definite conclusion generally accepted" appears to have been debated. There is no record of what, if anything, was resolved at this meeting. The club proceeded, holding fairly regular meetings until the autumn of the following year. Guests during this period included the mathematician and cryptographer IJ Good of the Admiralty Research Laboratory, Philip Woodward, an associate of Uttley at TRE and JZ Young, introduced by Donald Sholl.

The nineteenth meeting, on 8th November 1951, at which Gold spoke on "What is happening to the Universe", was poorly attended. The members outside London were finding it too expensive to travel, Woodward having written to Bates in October that he could not justify his attendance as being "on official business". Grey Walter suggested they meet quarterly and not always at the National Hospital in Queen Square. Bates wrote to Woodward:

> I think there is no doubt that we are going to have to change our habits, because the attendance of people outside London in the past three or four meetings has been very meagre, and they have all given financial difficulties as the reason.[268]

Hick, commenting on this, suggested expense was not the only factor:

> I should have thought 50% attendance was pretty good, considering the first flood of enthusiasm for the new ideas has worn off, and that we all know each other now.[269]

At a meeting on the 21st December it was agreed to have half a dozen meetings a year with two outside London. Meetings at Cambridge in May and Bristol in October were agreed.

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266 GC/179 B.3, Ross Ashby to Bates. 26 February 1950

267 See appendix A1.

268 GC/179 B.11, Bates to PM Woodward (TRE), 21 November 1951.

269 GC/179 B.11, Hick to Bates. 10 December 1951

Ashby pressed again for a Cybernetics Society open to all but could not get support.[270] It was, to the others, difficult to visualise what objective or aim a society for Cybernetics could work toward. In a reply to Ashby, Bates noted that in the two years since the club's formation the situation had changed "Cybernetics is now entirely respectable." The cybernetic analogy could be introduced without fear of unconsidered rejection: "there is no one left to preach to." If it was no longer needful to consider the perils of scientific apostasy, there was yet reason to restrict membership to the circle of cognoscenti.

> It is also a subject on which even the most intelligent people are apt to waffle, and this might reach terrible proportions if the membership was open to all.[271]

A final attempt by Ashby to steer the club in the direction of a learned society occurred in May 1952. JZ Young, professor of anatomy at University College had attended as a guest of Donald Sholl on several occasions. Ashby proposed that Young become a member, and this may have had the support of Bates. It would have been contrary to the spirit of the club, which had decided at its instigation that seniority was to be a bar to membership. Horace Barlow opposed any change; it might raise the status of the club but it would also bring about tendency to deference; there were other ways to flatter "JZY" if that was intended. Donald Sholl also opposed any change by which the Club "would degenerate to a minor professional body." In a letter to Bates, commenting on Ashby's motion, he wrote:

> 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.[272]

There is no record of the formal outcome of the debate at the Club's twenty-fifth meeting on 19th June, nor change to its membership. The club met on seven occasions in 1952, including a meeting in Cambridge, though the planned October meeting in Bristol was cancelled as many members had teaching commitments. At the November meeting (in London) Grey Walter spoke on Ashby's recently published book *Design for a Brain*. There were only four meetings the following year, and three in 1954. In 1955 there was only a weekend excursion to the west country; after a demonstration and dinner at

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[270] Ashby did not attend but a letter from him was read at the meeting.

[271] GC/179 B.11, Bates to Ashby. 18 January 1952

[272] GC/179 B.14, Sholl to Bates. 28 May 1952

TRE on Friday 6th May, the party moved on to Bristol: a demonstration by Ashby at Barnwood House Hospital, followed by lunch at Grey Walter's Burden Neurological Institute.

A number of factors contributed to this falling off. Some have already been mentioned: once the personal introductions had been effected there was little reason to maintain even an informal constitution, acceptance of cybernetic ideas opened wider forums, to grow would introduce formality to the proceedings. And, as careers progressed, there was less opportunity for informal meetings. The time when, as professors, they would no longer be eligible for membership approached.

It is not clear how many meetings of the Club were attended by Alan Turing. As has been noted above, the difficulties of travel from Manchester, made it unlikely that he attended most meetings. But he gave two presentations to the club and a photograph records his presence at the Cambridge meeting in May 1952[273]. By the time the Ratio Club was formed, Turing had outlined the design of the NPL's Automatic Computing Engine and moved on to Manchester. His first presentation to the Club was on 7th December 1950 when he spoke on *Educating a Digital Computer*. There is no record of the exact content of this talk but it may well have been along the lines of *Computing Machinery and Intelligence* which had appeared in *Mind* in October 1950. Bates wrote to Grey Walter on 13th December: "Turing was very entertaining and JZ Young came along to add fat to the fire."[274] What is perhaps of most interest at that time is a contrast between the idea of a machine learning and thus, as for Ross Ashby, the implication of unpredictable performance, and the conception of intelligence. The concept of intelligence is yet one of, fundamentally, giving the *correct* answers to questions. Learning, in other words is cybernetic, but intelligence is still reserved as a description of conscious deliberative, *representational* reasoning.[275]

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[273] 'MacKay's Photo' of which there are two prints and a negative among Bates' papers in the Wellcome Archive (GC/179 B.25). The photo is also reproduced as a frontispiece to Uttley's *Information Transmission in the Nervous System* (1979). A letter from Uttley requesting a copy for publication is also at GC/179 B.25. Though there is no direct attribution of the photo to the Cambridge meeting and Uttley's caption dates it to 1951, the May 1952 meeting seems most probable. The cost of the meeting was overestimated by Pringle and in an undated note (GC/179 B.14) Bates writes "At the Cambridge meeting approx. 9/- per member was oversubscribed, and I am returning 7/- plus MacKay's Photo to those in it; others can have copies for 2/-." The outdoor location, with several members seated on the ground, favours a summer season. The style of building in the background (ancient stone rather than twentieth century redbrick) also suggests Cambridge rather than the National Hospital at Queen Square. Finally, Pringle wrote to Bates on 25 April 1952: "everyone will be coming except Merton and Woodward"—neither appear in the photo.

[274] GC/179 B.7, Bates to Grey Walter. 13 December 1950.

[275] See J Haugeland. *Mind Design II*. MIT 1996:

During a time when the symbolic logical form of computation (of which the eponymous Turing Machine may be considered the theoretical archetype) became dominant, Turing's own thoughts were moving in the 'cybernetic' direction. Turing's second presentation to the Club, on 8th February 1952, was entitled *Chemical Origins of Biological Form*. It may be presumed to cover much the same ground as *The Chemical Basis of Morphogenesis* published in August 1952.[276] Perhaps it is only with hindsight that this work can seen as falling firmly within a cybernetic rather than the logicist approach to computation. As Kelso (1997) notes "only quite recently have the patterns predicted by Turing been observed experimentally, and Turing's theory still figures quite prominently in developmental biology."[277]

This survey of the Ratio Club and its activities, is not a complete account of 'cybernetic' ideas over the years 1943–1958. There were other individuals pursuing similar enquiries, in particular an overlapping confluence of engineers, mathematicians and linguists with similar interests motivated by Shannon's 'information theory'. Though very influential in the 1950s 'information theory' no longer exerts quite such a hold on the collective consciousness of computing. The same however cannot be said for the eponymous Turing Machine. Turing's argument contributes to a larger project: the formalisation of mathematical procedure; in a sense the final working out of the positive, scientific mode of thinking that had been advancing increasingly triumphant since the seventeenth century. Yet this attempt at formalisation had revealed some limitations to mathematical reasoning. Ironically—for the result was to give new impetus to the status of the mathematico-logical worldview in our culture—Turing's paper lends support to this constraint. It may therefore be useful at this point reflect upon

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> By concentrating on conversational ability, which can be exhibited entirely in writing (say, via a computer terminal), the Turing test completely ignores any issues of real-world perception and action. Yet these turn out to be extraordinarily difficult to achieve artificially at any plausible level of sophistication. And, what may be worse, ignoring real-time environmental interaction distorts a system designer's assumptions about how intelligent systems are related to the world more generally. For instance, if a system has to deal or cope with things around it, but is not continually tracking them externally, then it will need somehow to "keep track of" or *represent* them internally. Thus neglect of perception and action can lead to an over emphasis on representation and internal modelling.

[276] AM Turing FRS. 'The Chemical Basis of Morphogenesis' *Philosophical Transactions of the Royal Society -Series B, Biological Sciences* Vol. 237 pp37-72. [received 9 November 1951 - revised 15 March 1952 - published 14 August 1952]

[277] JA Scott Kelso *Dynamic Patterns* MIT 1997 (p4) "...Turing patterns were first unambiguously established in an iodine reaction, first by Patrick de Kepper's group in Bordeaux, France (1990), and shortly afterward by Harry Swinney's group in Austin, Texas. (*ibid.* p12)

Turing's thesis. In particular to give attention not to his mathematical argument but the manner by which it is advanced.

Mathematical arguments have both a formal, rigorous mode and an informal mode "drawing diagrams, providing examples, underlying motivations, narratives, intuitions, applications, and intended interpretations."[278] The informal mode, using the means and techniques of natural language, though discounted in the logicians' view of what makes a mathematical proof is yet essential if the proof is to be convincing. Turing's presentation of the Turing Machine has this informal character. In fact at one point he explicitly states:

> All arguments which be given are bound to be, fundamentally, appeals to intuition, and for this reason rather unsatisfactory mathematically. The real question at issue is 'What are the possible processes which can be carried out in computing a number?'[279]

The context has been stated most clearly by Martin Davis:

> In 1928, a little textbook of logic by Hilbert and Wilhelm Ackermann, entitled *Grundzüge der theoretischen Logik*, had been published. The book emphasised first order logic, the logic of *and*, *or*, *not*, *if...then*, *for all*, and *there exists*, which the authors called *engere Funktionenkalkül*. The authors showed how the various parts of mathematics could be formalized within first order logic, and a simple set of rules of proof was given for making logical inferences. They noted that any inference that can be carried out according to their rules of proof is also *valid*, in the sense that in any mathematical structure in which all the premises are true, the conclusion is also true. Hilbert and Ackermann then raised the problem of *completeness*: if an inference is valid (in the sense just explained), would it always be possible, using their rules of proof, to obtain the conclusion from the premises? This question was answered affirmatively two years later by Gödel in his doctoral dissertation at the University of Vienna. Another problem raised in the *Grundzüge* by Hilbert and Ackermann was the Entscheidungsproblem, the problem of finding an algorithm to determine whether a given proposed inference is valid. By the completeness theorem from Gödel's dissertation, this problem is equivalent to seeking an algorithm for determining whether a particular conclusion may be derived from certain premises using the Hilbert-Ackermann rules of proof. The Entscheidungsproblem was called the 'principle problem of mathematical logic', because an algorithm for the Entscheidungsproblem could, in principle, be used to answer any mathematical question: it would suffice to employ a formalization in first-order logic of the branch of mathematics relevant to the question under consideration. Alan Turing's attention was drawn to the Entscheidungsproblem by Newman's lectures and he soon saw how to

[278] Brian Rotman. 'The Technology of Mathematical Persuasion' in Lenoir, *Inscribing Science; scientific texts and the materiality of communication*. Stanford 1998.

[279] AM Turing. 'On Computable Numbers, with an Application to the *Entscheidungsproblem*' *Proc. London Mathematical Society* 1937 [section 9]

> settle the problem negatively. That is, Turing showed that no algorithm exists for solving the Entscheidungsproblem. The tools that Turing developed for this purpose have turned out to be absolutely fundamental for computer science.[280]

If one problem can be shown to be insoluble by algorithmic means then there can be no universal algorithmic proof procedure. But before Turing could show this (i.e. that not everything in mathematics could be so decided) he had first to define the concept of an algorithm. An intuitive, informal conception of what constitutes an algorithm is insufficient for Turing's argument. Such a notion is sufficient when the algorithm produces a positive solution to its problem, the result validates the method. But to prove the negative case: that no algorithm is possible, requires a definition of algorithm that is both exhaustive and exclusive.

He must give to the informal notion of an algorithm some formal definition which can then be the basis of his mathematical argument. Yet equally he must command assent from his mathematical audience. He must convince them that this formalised algorithmic method is sufficiently broad in scope that it can encompass any of the proper forms of mathematical reasoning. (The whole argument would be of no significance if it were merely to show a limit to the efficacy of some particular technique—it must encompass any and every method by which a formal proof might be set out on paper.) So Turing's argument must effect a transition from informal to formal definition. But the argument itself must needs be transitional. He has to convince his audience that their hitherto informal notions of what constitutes an algorithm are fully covered by his formal definition; it has to be informal, it has to carry intuitive conviction.

The Turing machine is a definition of the hitherto informal and ill-defined notion of algorithm. Starting with a "computer", a person with pencil and paper, Turing progressively simplifies the actions this person may perform, and the 'states of mind' they need have until, in a final rationalisation, the person is replaced by a machine.

His model of an algorithm is both simple and comprehensive: reading a single symbol, marking a symbol on paper, erasing a symbol, the facility to scan forward or back over this chain of symbols, a current 'state of mind', a unique and finite set of rules regulating scanning and substitution of one symbol for another. There is no time limit, the supply of pencil and paper inexhaustible. But this generic set of actions is in need of some constraint. To make a bridge to the formal argument, it must be shown that this putative human calculator is following a strict set of rules.

Once the person is replaced by a mechanism, the algorithmic process is defined in a manner which makes proof possible.[281] Thus the informal imagery

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[280] Martin Davis. 'Mathematical logic and the Origin of Modern Computers' in *The Universal Turing Machine*. (ed. R Herken) OUP 1988

of the human computer, transformed to the idealised, but still visualisable, image of a tape machine, enables a formal notion of algorithm to be established. It is here that Turing's argument makes its greatest intuitive appeal. Like any such appeal to an innate sense of what is possible in the world it depends on what his audience already know, a tacit and empirical understanding of how and what things are. Unlike Maxwell in 1873, Turing in 1937 is not asking his audience to forget what they already know, on the contrary he will avail himself of their previous knowledge. It is an appeal to a world familiar with machines.

It is, I suggest, an argument that can be carried only because of the time and culture in which it was advanced. He is writing for the machine age. An audience so familiar with machines that they no longer need to marvel at how they work—they are integral to modern life. But note also how Turing's machine is a very simple machine, one whose mechanical processes can be readily assimilated. Unlike, say, a radio, it does not depend upon occult components and invisible fields. The image is typewriter-like, yet the mechanism far simpler—it has all the exposed and apparent simplicity of something like a bicycle. The causal chain set out as clearly as an illustration by Heath-Robinson. By the 1930s machines were part of everyday life, the curious could understand a mechanism that could be explained in everyday language. To the uncurious the function was no longer magic. Today we are less curious and our electronic machines have no visible nor moving parts. An appeal based on how things work can no longer be a direct appeal to what our common senses assure us about the world.

The Turing machine concept was refined, to that of a Universal Turing Machine, one that can mimic the behaviour of any Turing machine. Subsequent development of computing machines has led to the assumption not only of an equivalence between the electronic digital computer and the UTM but the assumption that all possible computing machines are necessarily Turing machines. Thus a proof of the limits of algorithmic methods has not only come to be seen as a limit to computation, but sometimes, in a curious inversion, is sometimes taken to define a universal scope for the digital computer—if it is not digital it is not really computing. Turing does not define positively the scope of machine, or any other kind of, computation. He gives formal definition to the hitherto informal notion of an algorithm, and thus, what an algorithm, so defined, can not do.[282]

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[281] See Martin Davis. *op cit.*

[282] The populist version of the UTM argument runs something like this: a UTM can be programmed for any computation, a digital computer is equivalent to a UTM, therefore a digital computer can accomplish any possible programming task (limits of speed and memory excepted). What is overlooked is the very restricted definition of program implicit in the formalisation of what is meant by an algorithm.

Cybernetics, as conceived by Wiener and Ashby, was an essentially mathematical theory of machine behaviour in Bergsonian time. It was at once both cognisant of the trend toward stochastic theories in physics and yet had ambitions to be a predictive science of behaviour in the determinist and materialist tradition. Cybernetics in this form was too broad a system to fit within any established discipline. Without notable empirical success its principles could not, like differential equations, be transformed into general techniques applicable across diverse fields. The term itself, suggesting to a classically educated administrative class connotations of government and control rather than scientific prediction, and popularised in terms of a world of "intelligent" machines, could never escape a slightly sinister aura.

On a broader basis the topic appealed to many whose biological interests in brain function had been stimulated by wartime work involving electronics and servo-mechanisms. These interests predate the creation of electronic digital computers on the Turing machine model and the analogies of brains and machinery employed are much broader. The computer is only one of many computing devices and processes of learning and homeostasis are treated as being as significant as symbolic reasoning.

This work presages present day interests in dynamicist approaches to understanding intelligence and autopoiesis as a developmental mechanism. That these ideas were not developed further at the time can be attributed to the difficulties of making empirical and theoretical advances at that time. Computers were not powerful enough, nor mathematical theory sufficiently advanced to build the mathematical models required. At the same time the direct application of realised Turing machines to relatively simple and direct problems of logic and symbol manipulation was advancing rapidly. The direct commercial and other uses of these machine, the desire for immediately useful results, drove development in the direction of "physical symbol system" model of computing. In biology also a symbolic model –molecular biology– has skewed the study of morphology toward understanding processes as being "coded" and following a preordained "program".

Paradoxically, it is the success of this dominant form of computing that has now led to computing machines sufficiently powerful to permit a reengagement with dormant "cybernetic" ideas. A revival prompted also by a sense that a logicist approach to artificial intelligence not delivered on its promises.

The Ratio Club perhaps lacked a philosophical sophistication and weight, that Craik, might have supplied. To them Wiener and McCulloch were too rhetorical, the "purple stuff" not to their taste. The social concern of Wiener writing primarily to a affluent American audience went unremarked. To the austerity of a post-war, post-imperial Britain this was irrelevant. They certainly shared the interest in feedback, they had had 'Wiener's ideas before Wiener'. They were, on the whole, wary of Wiener's mathematical arguments. They were mostly physiologists, amateur engineers, conscious of their mathematical limitations. Their concerns were empirical, centred on the clinic and the laboratory, abstract theories were desired as a key, but were not of intrinsic interest. Thus although they accepted statistics and 'communication theory' the interest of Wiener in non-linear and dynamic systems, the work on the ergodic theorem, upon which his reputation as a mathematician was founded tended to pass them by. It is possible that Craik, had he lived, may, in his pragmatic way, have remedied some of these lacunae. There is a philosophical substrate to Craik's ideas, as expressed in *The Nature of Explanation* that is not found in Wiener's *Cybernetics*. Wiener's meta-scientific concerns are more social than philosophical.

This chapter has considered 'cybernetics' in the light of a particular case; its influence on a small group of self consciously junior academics resuming their careers in post-war Britain. From this standpoint Wiener's *Cybernetics*, and the work of others in the USA, is less a revelation than a reflection and trumpeting of ideas already known. The theme of the "West Country Cyberneticists"—in contrast to the Americans, more centred on medical practice and human physiology—has been explored by Rhodri Hayward in a conference paper presented in 1998.[283] Arguing against an interpretation of cybernetics as a dehumanising wartime rhetoric he returned to the west country origins of cybernetics for a different view, a holism, promoted by a conservative elite of scientists and medical practitioners opposed to the mechanising tendency of modern life.

This group have latterly been the subject of a paper by Andrew Pickering which considers the renewed interest in the history of cybernetics as reflecting a contemporary dissatisfaction with "the reductive, linear, Newtonian, paradigm that still characterizes most academic work in the natural and social sciences".[284] In Pickering's view, the English cyberneticians in particular represent a transition from epistemology to ontology. They illustrate what Pickering has described as 'the mangle of practice': doing science is not so much representing and analysing the world as a continued reassessment of the relation and interplay between human and material agency.

A similar concern to reappraise what constitutes a science is to be discerned in the work of Philip Mirowski who has traced the cross-currents of influence that have entangled the models of physics and economics. In the twentieth century our culture's conception of physical reality changed profoundly, as a consequence "it became much more possible to see orthodox economic theory for what it really was: a bowlderized imitation of nineteenth-century

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[283] Rhodri Hayward 'West Country Cybernetics; Grey Walter, Ross Ashby and the medical contribution to the history of Artificial Intelligence'. New Directions in the History of British Computing. Manchester (16-17 June 1998)

[284] Andrew Pickering. Cybernetics and the Mangle: Ashby, Beer and Pask. *Social Studies of Science* 32/3 (June 2002) pp413–437

physics."[285] Drawing on the work of Emile Meyerson[286] he suggests "The reason mathematics 'works' so well in science is that it is the result of a long and arduous process of adjustment of the formalism to our contingent experience."[287] In both disciplines, hypothetical mathematical entities ('energy' and 'utility') became conflated with substance. As substances they had, by analogy, to obey conservation laws and so mathematical entities were adjusted to save the phenomena.

> Invariants are not to be seriously found 'out there'; in a real sense they are 'in here'. Our understanding of the world is structurally inseparable from our understanding of our somatic selves and our social selves. Our very livelihoods, in the broadest possible sense, are predicated upon invariants whose existence cannot be proven but whose instrumentality renders our actions coherent.[288]

Neo-cyberneticist interpretations, such as those of Pickering and Mirowski, represent a counter-current to contemporary scientism. A position analogous to the opposition to the spirit of modernism detected by Hayward. A tension between 'two cultures' seems always present: not only the apposition of disciplinary allegiance but also of philosophies. To Jean-Pierre Dupuy it is also a matter of geography:

> [On an American university campus]...on the one hand, students of literature are initiated into the mysteries of French-style 'deconstruction', taught to celebrate the death of the human subject and to repeat ad nauseam that man is not his own master and that such awareness as he may have of his own affairs is severely by a sort of tyranny of the unconscious; while at the same time their fellow students in the economic, political, and cognitive sciences learn to systematically reduce social institutions to voluntary agreements between fully conscious and free individuals.[289]

Dupuy's study of cybernetics, contrasting the "mechanisation of the human" represented by Wiener's Macy group with the humanist traditions of European philosophy, finds the failure of cybernetics in its rejection of the phenomenological; there was a 'missed rendezvous with the human sciences'.

The membership of the Ratio Club was built around informal personal contacts, many a consequence of interdisciplinary wartime work. Although there was a recognised categorisation into "biological and electrical folk,"

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[285] Philip Mirowski. *Against Mechanism: protecting economics from science.* Rowman & Littlefield 1988 p5

[286] Emile Meyerson. *Identity and Reality* [1908] Dover 1962

[287] Philip Mirowski. *More Heat than Light: economics as social physics: physics as nature's economics.* Cambridge 1989 p6

[288] *ibid.* p138

[289] Jean-Pierre Dupuy. *The Mechanization of the Mind: on the origins of cognitive science.* Princeton 2000 p.xiii

these were but different approaches to a mutual interest in understanding the working of the brain.[290]

Unlike the Macy group in America there was not a sociological or anthropological dimension to the discussion. Some members, it is true, did voice the wider implications of their work. Ashby had an interest in the implications for psychiatric medicine, MacKay sought to reconcile a mechanistic world view with the notions of personal responsibility inherent in his religious convictions. But these were, nonetheless, primarily matters of individual behaviour not that of humanity in aggregate.

The Ratio Club, perhaps more that the Macy Group, had a distinctly biological, and brain centred perspective. Their central concern is the functioning of the *material mechanism* of the brain. Sometimes, as the next chapter will show in the case of Uttley, this takes a rather abstract character. But Uttley was still seeking a mathematical *analogue* of the brains *mechanism*. A logicist approach, one that put pure thought—a neo-Platonist conception of rationality —at the focus, is less evident among the members of the Ratio Club.[291]

As we have seen, Turing's outlook had moved from the *naïveté* of the "Turing Test" to the far more complex, multi-layered and material modelling of his work on morphogenesis. Other members also, either were never notably influenced by the logical positivists or soon came to see this outlook as unsatisfactory. MacKay's interest in "meaning" was a search for something richer than Shannon's *Communication Theory*, an attempt to recover something of the common sense view of "information" as implying meaning.

In 1974 the Royal Society held a discussion meeting organised by RV Jones: the subject "*the effects of the two world wars on the organisation and development of science in the United Kingdom*." John Pringle, who had been a member of the Ratio Club, spoke of the influence upon Biology. In his opinion the second

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[290] "At the next meeting could the medical men give the mathematicians and machine designers the important facts and really important ideas about brain function and properties. [...] The more machine designers can know of such facts, the more likely they are to spot resemblances and deduce properties." [GC/179/B.11, Uttley to Bates, 8 November 1951.]

[291][Servo Theory] "has however only the mathematics to deal with a very restricted class of systems of practical importance to some branches of engineering, and it cannot treat in any new or useful way data derived from biological experiments - *pacé* the activities of some enthusiasts. To those who are seeking to give an account of behaviour of an animal in terms of known physical properties of its components with the minimum of *a priori* assumptions Information Theory appears, like Servo Theory, to bring forth new concepts, overlaid, and to some extent obscured, by detailed mathematical extensions of no direct applicability." [GC/179 A24, Bates' ms notes for a lecture 'Significance of Information Theory to Neurophysiology' 28 September 1950.]

World War, unlike the first, had, in general, encouraged the development of science.

The Central Scientific Register, drawn up in 1938/9 and supplemented by informal versions and personal contacts within universities,[292] had, as intended, prevented the waste of many exceptionally talented lives. Pringle's own attempts to join the Air Force (he had founded the gliding club at Cambridge) were thwarted and he was, instead, directed to the Air Ministry Research Establishment. It was, as Pringle remarks, not a biological war. Apart from some contributions to a more productive agriculture the specialist knowledge of the biologist was not required.[293]

Thus it was, that a great number of biologists were given a brief retraining in electronics and their (nominally at least) rationalist, numerically competent, scientific mind-set turned upon Radar, fire-control and similar problems. There were also contributions to physiology, psychology and medicine, and new fields such as ergonomics founded. Looking back, Pringle thought that the greatest contribution to biology came, not from any particular prompting of research, but through the political influence and organisational skills that scientists had acquired by working outside their narrow specialism. It was this that had contributed much to the enhanced status and influence of scientists in the post-war world.

But there were also more subtle influences that shifted the thought patterns of biologists and, perhaps less certainly, put some biological context into other fields. A hobbyist's interest in electronics (essentially radio) was widespread in the 1930s, so the retraining of biologists for radar work had a flying start. Relatively primitive electronic devices had been employed to explore physiology and since early in the century biology had begun the move, in the fashionable climate of logical empiricism, to model itself after physics: to become a quantitative rather than descriptive science. Neuro-physiological studies in particular had begun to take advantage of electronics to investigate the central nervous system (CNS) as a mechanism and circuit.

This influence of electronics on biology is not to be confused with the influence of physics and physicists on the development of what in mid-century was known as bio-physics and the slightly latter and correlate field of molecular-biology.[294] That influence was, in some measure, a reaction against

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[292] See for example various correspondence among the papers of Sir Gordon Sutherland. [Cambridge University Library: Add8353]

[293] Medical training was, of course, another matter. Although significant developments in medicine occurred, and are given due consideration within Pringle's presentation at the Royal Society's discussion, they fall outside the scope of this thesis.

[294] An example of bio-physics would be the investigation of the molecular structure of proteins by infra-red spectroscopy. Molecular biology drew on this work but the essential physics content is internalised. The physics content of molecular biology is

the intrusion of uncertainty and statistics into atomic physics. A desire for both mathematical formalism and a tangible visualisation of the entities and processes involved. Biologists, with a tradition of observation and classification rather than Galilean[295] experiment, were in contrast inescapably conscious of the unpredictability and recalcitrance of the objects they studied.

The multivariate, somewhat unpredictable behaviour of an electronic valve circuit, had something in common with animate matter in a collecting jar; the environmental influence (on, for instance, the propagation of radio signals) could not be wholly isolated. It is notable that the notebooks of the biologists such as Pringle and Bates are sprinkled with circuit diagrams. These are never, even in latter years, solid state circuits.[296] The invention of the transistor (*c.*1947) had come after the war, which had brought about considerable advance in the design and manufacture of valves. The inevitable surplus post-war had facilitated the development of ad-hoc laboratory equipment. Solid state devices came later, their development driven by the requirements of space-borne technologies and large scale, mass produced computing devices. They were not within the purview of the biologists workbench.[297] Thus we have the biologist, at home with a nature inherently multivariate—with analogue circuits and valve mechanisms. It is quite another world-view compared to that of the biophysicist and molecular biologist seeking a reductive formalism, the calculated rationality of the logic state machine.

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that of chemistry, it contributes not so much to the empirical investigation as to an internalised explanatory theory of molecular behaviour.

[295] Galilean - a critical experiment, to test a hypothesis. In this typology other types are the inductive (Bacon), interrogating nature; deductive (Kant), the consequence of axioms, 'experiments in pure reason'; and the demonstration (Aristotle), to illustrate or convince.

[296] It may be that, with increasing seniority, there was less involvement with the details of experiment, and increasing sophistication had put design outside the scope of the amateur designer.

[297] See George Dyson. *Darwin Among the Machines*. Penguin. 1998. p108

Chapter 4. Autonomics

> ...what is troublesome is religion and when counting gets to be religion it gets to be troublesome.
>
> Gertrude Stein *Everybody's Autobiography* 1938

We have already seen how in the post-war years the National Physical Laboratory had become "a rather dead place". Individual enthusiasms and exceptional talent could not counter the fact that the NPL in 1945 was badly placed to move into the most promising areas of industrial and scientific research: it had little home grown expertise in the new high speed electronic technologies, nor the mechanisms of automation and control that were attracting industrial interest.

Though the ACE computer, designed by Turing, had many novel features that justify its place in a history of scientific instruments, it contribution to a nascent computer *industry* was limited. NPL was to contribute much to the development of both algorithmics and software engineering in subsequent years, but by and large this was not a dividend of the ACE design. The ACE then, is peripheral to the story to be told here, by the mid 50s it was clearly both obsolete and incomplete, only an institutional inertia–a reluctance to be seen to write off a failing project–explains the continued development of the full scale ACE once the Pilot ACE had been completed. The Control Mechanisms and Electronics Division, originally proposed in the late forties was finally instituted in 1954. But it remained an uncertain creation, without strong leadership it was failing to deliver a programme of industrial research in automation, it became sidetracked into pet projects of peripheral importance. The task of building the ACE assumed an unwarranted significance: the one big task which gave the division a common focus.

In the years 1957-64 an attempt was made by an ambitious new Director, Sir Gordon Sutherland (1907-1980), to improve the status and prestige of NPL. At a time when both educational expansion and technological development found favour it became both more academic and a pioneer of new research. It was an attempt to overcome its reputation as "a rather dead place". One instance of this is the transformation of the CME Division into the Autonomics Division which, led by its Superintendent, Albert Uttley (1906-85), approached the then nascent field of Computing Science from a distinct and original perspective. There was a strongly cyberneticist flavour to Uttley's Autonomics, its roots were in physiology and psychology as much as engineering and mathematics. For a variety of reasons it did not prosper, its successor–a more conventional, Computer Science Division–was it may be argued more successful in its delivery of industrial research. But Autonomics does deserve study: as a model, however imperfect, of other ways of seeing a science based around the technology of automatic computing.

There are two strands in the development of computing to which NPL offers no exception; it was, at its inception, an alliance of mathematics and engineering interests. The NPL's Mathematics division had been created in 1945. It was, from the point of view of the administrative Civil Service, a computing service; it met the demand for both computation and advice on numerical methods from a variety of government departments. However the technical committee advising on the establishment of a mathematical laboratory, whose chairman was Sir Charles Darwin, Director of NPL, recommended an extension of this role to "research into new computing methods, including the design of new instruments".[298] When the proposed laboratory was created at NPL this led to the building of both a differential analyser and an electronic digital computer—ACE—in the decade that followed; expensive laboratory equipment for a discipline that traditionally had needed none. Both projects brought the new Division into close association with existing groups at NPL. The differential analyser was constructed with assistance from the Control Mechanisms section of the Metrology Division. The expertise to build an electronic digital computer was more difficult to come by. Alan Turing's design of an Automatic Computing Engine (ACE) was not that of an engineer and it was only after his departure from NPL (on sabbatical at Cambridge in 1947 and then at Manchester from 1948) that real progress was made.[299]

The original purpose of providing a computing *service* was overtaken by the rapid spread of the new machinery, so that by the time ACE was operational in the late 50s many government departments could do their own computing. Indeed, some, particularly military applications, would always go it alone, as this internal memo at TRE circa 1945 shows:

> ... I should imagine that our natural reply is that TRE Maths Group being already in existence should be able to cope with all demands for mathematical calculation from the station as well as theoretical assistance and that we are therefore extremely unlikely to require the services of the NPL Maths Division at any time.[300]

The division quickly ran down the calculating service in favour of offering advice on methods of computation, becoming a pioneer in the development of

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[298] Report to DSIR Advisory Council 10 May 1944, quoted in M Croarken. *Early Scientific Computing in Britain*. Oxford 1990 p82

[299] The Pilot ACE, a reduced version of the original Turing proposal was demonstrated in 1951. It proved an effective machine for many tasks and was thus more than a prototype. The simplicity of its design brought reliability but made programming difficult. By the mid 1950s the importance of programming as an essential cost of automatic computation was becoming apparent and the engineering task of manufacturing a unique design was a strong argument against further development. Thus by the time the full-scale ACE was completed in 1958 it was already obsolete.

[300] Correspondence with NPL 1939-49 PRO: AVIA 7/927, JW Head to Dr. RA Smith, undated [December 1945?]

numerical methods and program language standards. By the late 60s mechanisation of computation had become so dominant that it had become a Division of Computer Science and Numerical Analysis. This then, was an example of one dominant strand in the development of 'computer science'; a transition from (applied) mathematics to computing as a science of algorithms.

A proposal for an Electronics Section at NPL was first made in the summer of 1946, as part of a proposal to hive off the Radio Division. It would be concerned with the application of electronics to industrial controls rather than radio and would take over work currently at TRE. The Radio Research Station was established in 1952 but the NPL's director in 1947, Sir Charles Darwin, was uninterested in electronics, being "unsure if it was really important" and thought such work more relevant to TRE. Attempts by DSIR to promote industrial electronics through trade associations also met with limited response. The major manufacturers and their trade body the Radio Communication and Electronic Engineering Association (RCEEA) were more concerned with consumer market demand for radios than industrial electronics.[301]

It was intended that stabiliser and computing techniques developed for bombing and astro-navigation would be adapted for mass production processes in industry "where the measurement and control of simultaneous variables is necessary". Transferring costs from the Ministry of Supply to DSIR would, at least nominally, reduce defence expenditure. Much interdepartmental discussion ensued; to which the Treasury, naturally, became a party. The possibility of a transfer of personnel was ruled out; the grading of Scientific Civil Servants diverged significantly between the departments, the ratio of senior (i.e. graduate) to junior scientific staff being higher at TRE as was the flexibility to accommodate interdisciplinary co-operation.[302] In addition, at war's end staff were leaving to take up appointments in universities and industry. In July 1946 TRE had 107 scientific staff, by October the number was 86 and still falling. A proposal mooted in September to establish an Electronics *Division* of NPL but to be based initially at TRE came to nothing. At TRE the computer circuit and instrumentation work of a team led by Albert Uttley had attracted interest. However a report in October 1946 concluded "all were appropriate to D.S.I.R. with the exception of the work of Dr. AM Uttley's team which is concerned with astro-navigation. It

[301] See NPL Application of electronic techniques in industry PRO: DSIR 10/365

[302] At TRE the ratio of Scientific Officers (i.e. Graduates, at a time when only ~3% of the relevant age group went to University) to Experimental Officers was 1:1, the Treasury thought a ratio of 1:2 appropriate throughout the Scientific Civil Service, but even if this ratio were acceptable, EOs with long experience were not available in the numbers required.

was felt that this work should properly be coupled with military problems."[303] Ultimately no transfers of either staff or projects took place, although TRE did undertake some work as a contractor to DSIR.

When FM Colebrook was appointed to lead the NPL Electronics Section he sought help from Uttley at TRE:

> I am coming to Malvern with Dr Smith-Rose on Monday 26th of April. I have now a special interest in "electronics" applied to industrial processes and to non-radio research, and I hope it will be possible for me to see Mr Jefferson on this occasion, I would also like to see Dr. Uttley and the work on electronic computation.[304]

Another meeting took place on 21 July:

> You may recall I am now responsible for the work on ACE at the National Physical Laboratory and in this connection would like to come and get myself up to date on the TRE work on automatic digital computation. In particular I want to discuss possibilities of mutual assistance with Dr. Uttley - assuming that is still one of his interests. Mr Wilkinson of the N.P.L. Mathematics Division also wants to talk to Dr. Uttley and we propose to come together.[305]

A Panel on Automatic Control Techniques (Chairman, Sir Charles Darwin) was formed by the DSIR in 1948, to assist in the application of electronic and control devices in industry. It was to operate initially for two years and the employment of up to six temporary staff was authorised. However only two staff, already employed at NPL, were assigned. And it appears that the efforts of HA Thomas of NPL to publicise the work were not fully supported. In February 1950 it was concluded that "automatic control mechanisms is a matter for the future and cannot be expected to have a very rapid influence on industrial productivity".[306] Efforts were to be redirected to longer term studies of fundamental techniques.

The NPL's Administrator wrote to his opposite number at TRE in 1948:

> Our Control Mechanisms Section is particularly interested in special electronics techniques, especially those involving the use of DC amplifiers and the Section feels it has insufficient experience in this field.[307]

The response from TRE was not encouraging, nor had it been earlier in the year when the Assistant Superintendent of TRE's Physics Department and head of its Electronics Division, Albert Uttley, had visited NPL for three days "to see if there was any work that TRE could do to assist NPL by the

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[303] NPL Proposed Formation of an Electronics Section. PRO: DSIR 10/273

[304] AVIA 7/927, FM Colebrook (NPL) to Chief Superintendent TRE 22 April 1948

[305] AVIA 7/927, FM Colebrook Officer in Charge, Electronics Section to WJ Richards Chief Superintendent TRE, 7 July 1948

[306] PRO: DSIR 10/365 PACT Report Covering the Period November 1947-February 1950

[307] AVIA 7/927, ES Hiscocks (NPL) to WJ Richards (TRE) 11 November 1948.

introduction of electronic techniques"[308] He was unenthusiastic in his report, adding in a note:

> My Personal Opinion is that as a liaison visit between two establishments the tour was useful, personal meetings are always useful; but it went no further. Conclusion No 1 [there was no immediate work for TRE to do] reached after 3 days' study is not likely to be greatly altered by a further visit. Nor in my view would it be proper for any research group to devote much of its effort to the attempted solution of problems of other groups working in other fields. Surely a research group should devote its principal effort to the primary problem within its own sphere and for which its workers are specially qualified and trained.[309]

Geographically vulnerable in wartime, NPL had not participated in the development of microwave radio and radar. As a consequence the lack of pulse coded electronics expertise at NPL made progress on building the ACE by the Electronics Section slow. Nor did the Control Mechanisms Section prosper: an offshoot of the Metrology Division doing much routine standards work its workshop began building the differential analyser required by the Mathematics Division. Both sections seem to have had little direction and their resources and effort drifted to supplying the new Division's needs for computing equipment. In 1954 the two sections were brought together in a Control Mechanisms and Electronics Division. This appears to have been supported by a revival of official interest in industrial application of control mechanisms, but much of the effort (estimated at 70%) was directed at clerical mechanisation[310]. In December 1954 a minute circulated within DSIR noted the lack of effort being devoted to automatic control by the CME Division. There was indeed a problem recruiting suitable personnel, but too much of the Division's resources were being devoted to the DEUCE, a putative commercial version of ACE.[311]

> The work on DEUCE[312] is intended to perfect what is already a useful machine for research workers. Its immediate usefulness will not extend far beyond research, and it is arguable that the gain to the country from improvement of the ACE may not be great compared with the valuable resources of scarce brainpower devoted to the work.

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[308] AVIA 7/927, Report on a visit by Dr A Uttley to N.P.L. from 14th to 17th January 1948.

[309] AVIA 7/927, AM Uttley to FS Barton, Director of Communications Development (DCD) Ministry of Supply, 22 January 1948.

[310] DSIR 10/365, Control Engineering, meeting 30 April 1954 at [Board of Trade] It may be noted here that clerical employment tended to be regarded as 'unproductive', it was not 'making things' Hence to concern to reduce the numbers employed in clerical work.

[311] Contemporary documents do not always distinguish between the Pilot ACE which had been operational since 1951 and the full-scale ACE which was not completed until 1958.

[312] correction added by PD Greenall (DSIR) 2/12/54 "ACE as distinct from pilot ACE"

> The scope for the use of electronic office machinery, if some can be developed which will perform simple tasks cheaply, is immense – there were 1 1/2 million clerical workers in Great Britain in 1951, and a large proportion must have been employed on routine tasks. Putting more of their work on to machinery might significantly reduce the overhead costs to industry, thus raising the standard of living and making Britain more competitive in export markets.[313]

The benefits of developing an export trade in such machinery was stressed, it being noted that apart from computing most of the Division's effort had been devoted to a defence contract: the industrial strength of the country and *inter alia* its ability to wage war, would arguably be better served by developing industry. Another project which provoked disapproval was the development of the an analogue computer model of the economy[314]:

> ...But regardless of whether Phillip's work on the analogue computor is useful or a menace to society, if NPL had shown the same diligence in finding an industrial use for the computor that they have shown in finding one in the field of abstract economics they would almost certainly have produced results of greater benefit to the community.

The full scale ACE was already obsolete when completed in 1958.[315] Nor could DEUCE be considered a commercial success: of the thirty-three manufactured by English Electric, twelve were for use within that company, most of the rest went to government or to industries with a strong defence related subsidy such as aircraft manufacture. The only overseas sales, to Australia and Norway, may also have had a defence role. A series of letters between John E Woolston of Atomic Energy of Canada Ltd. and PD Greenall at DSIR suggest one reason:

> ... When I left UK I understood English Electric were about to deliver to NPL the first engineered model of the ACE and had several other models coming along.
>
> ... Have they found markets for them all or are they still looking for possible customers?[316]

Greenall phoned Newman at NPL who said "AEC Ltd should be warned that EE go in for no sales talk or pressure (unlike certain other firms) but merely

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[313] DSIR 10/341, Minute by E Rudd 2/12/54, circulated to and with ms comments by Mr Gass, Mr Greenall, Dr Blount,

[314] Phillips' hydraulic model of the British economy can bee seen in the computing gallery of the Science Museum in London. I suspect, but cannot confirm, that the NPL work was in some way related to this.

[315] The late Donald Davies in an interviewed 11 February 1999 recalled that he had advised Sutherland to cancel ACE but there was already too much inertia to the project.

[316] Advisory Committee on High Speed Calculating Machines (Brunt Committee); 1953-60 General Correspondence. DSIR 10/321

provide factual & unenthusiastic answers."[317] and this was passed on to Woolston

> I am informed, very unofficially, that it is expected that, in the not too distant future, the firm expects to start production of a dozen DEUCES, and it is unlikely that all of those are bespoken as yet. It was guessed that they might not cost more than £40,000 each. And it was suggested that I should warn you of my informants personal impression that English Electric have not yet started to go in for the persuasive and high-pressure sales talk for their future machines, which might be associated with some other firms; apparently they give brief, factual, and unenthusiastic answers to questions which are put to them, but their machines are unlikely to be any the worse for this.[318]

It is therefore no surprise that the Canadians chose to purchase from the USA. In England too, DEUCE was often the second best choice: in 1957 The Atomic Weapons Research Establishment at Aldermaston was permitted the scare dollars necessary to purchase an IBM 704, a luxury not permitted their counterparts at Harwell.[319]

When the CME Division was finally created in 1954 FM Colebrook, who as head of the Electronics section had been responsible for ACE development, was appointed Superintendent. But Colebrook was in poor health and died without ever taking up the post. The Executive Committee expressed disappointment when his successor RH Tizard announced his resignation after only eighteen months.[320]

> Mr RH Tizard, newly appointed Superintendent of the [CME] Division, has tended his resignation and will leave at the end of May. [...] the only possible candidate at the NPL is Mr EA Newman, who has recently been promoted to Senior Principal Scientific Officer in the Division[321]

Though the Administration's preferred candidate, Newman's recent promotion may have counted against his further immediate advancement.

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[317] *ibid*

[318] *ibid*

[319] See J Hendry. Innovating for Failure: government policy and the early British computer industry. MIT 1989 p120. See also PRO: DSIR 10/321 Advisory Committee on High Speed Calculating Machines (Brunt Committee); 1953-60,

[320] Tizard went to the LSE where from 1956-8 he was a Research Fellow in Analytical Economics studying the application of automatic controls to economic systems. It is not clear if this was connected to the Philips model that had met with disapproval by the DSIR. [see above] However a comment on Tizard's merits as a shortlisted candidate to succeed the Earl of Halsbury at NRDC in 1959 suggest that he probably wasn't best suited to promote industrial rather than purely scientific research:

"Age 42, looks older. The best brain but rather out of the world. Highly specialised knowledge. No leadership. Intellectual but impractical approach." National Research Development Corporation 1955-59 Appointment of Managing Director. PRO: BT 258/334, GH Andrew to RN Quirk February 4 1959

[321] NPL Executive Committee Papers 1956. DSIR 10/409

And his appreciation of the need for industrially relevant research may not have been entirely in tune with the incoming Director's ambitions for NPL.

> I have been asked both by staff of the Board of Trade and of the Treasury why this country does not put more effort into the application of data processing techniques to industry. We are well behind the USA in many aspects of this vital work, and I am sure that CME should have a much larger staff available to put on such work.[322]

In Newman's view the main problem was the recruitment of suitable staff, the planned increases were not achievable because the "grading system was too rigid for a fast moving field". But there was great uncertainty as to just what computing research should be, for whom, by what institution? The 1956 Committee of Enquiry into the DSIR (Jephcott) had noted that

> It is clear that one part of the work now being conducted at the laboratory, that on computers and control mechanisms is of rapidly growing business and industrial importance. It is at least possible that the Government, by far the largest employer of office workers in the country, as well as a large industrialist in its own right, will need to have expert advice on the development of electronic mechanisms. It is however an open question whether work of this nature appropriately falls within the responsibilities of the National Physical Laboratory.[323]

So, from its formation in 1954 the Control Mechanisms and Electronics Division (CME), appeared to lack clear direction and a well defined role. The business of using computers for numerical work was the responsibility of the Mathematics Division. The developing expertise in programming techniques, arising out of use rather than construction also had its foundation in that division. If there was a continued need for analogue techniques they would be for automatic control rather than numerical analysis. But all these developments, even more than pure digital computation, could be supported by industry. The NPL's annual report for 1957, in a section devoted to plans for the next five years, noted:

> The Control Mechanisms and Electronics Division is still below the size which we considered appropriate when it was planned in 1953; we believe it should double its present staff. We encourage this Division to move away from the design and construction of computers and to study means of replacing by machines human faculties other than the ability to count.[324]

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[322] Sutherland Papers, Cambridge University Library, Add8353 C14, E Newmann (acting Supt CME) to GBBMS 9 Oct 1956

[323] Committee of Enquiry into DSIR (Cmd9734) June 1956. Quoted from the copy in Add8353 C36 originally sent to Smith-Rose Acting Director of NPL

[324] National Physical Laboratory. *Report of the Executive Committee for the Year 1957*, HMSO, 1958

From its foundation much of the NPL's work had been routine testing which was far from academic or original in its outlook. But Sir Oliver Lodge, speaking to the British Association in 1891 had argued for a much larger establishment than a Board of Trade laboratory with a "dignity and permanence of a national institution comparable with Greenwich Observatory".[325] Appointed by a committee dominated by the Royal Society, the position of Director carried some prestige and the post was often used to promote wider and more personal research interests. The opinions of the Director tended to shape the overall research programme. Sir Charles Darwin, in 1947, was unenthusiastic about electronics being "unsure if it was really important" and thought it "best left to TRE". His successor Sir Edward Bullard "had a reputation for liking the post, the house and the prestige but being very irked at the difficulties associated with personal research for himself".[326] The opportunities to conduct personal research were also of concern to his successor Gordon Sutherland.:

> Probably the most important point is whether the position is a purely administrative one or whether time and facilities are available for some personal research.[327]

Sutherland was a physicist, whose research interest, infra-red spectroscopy, was a technique of great importance to the new field of bio-physics - "the study of biologically important molecules such as protein and nucleic acids"[328]. Educated in Scotland, initially by his schoolteacher parents, he had taken a double degree in Mathematics and Physics at St Andrews before going to Cambridge in 1929. As he himself observed:

> Most Scottish graduates who in those days wished to go on to Oxbridge...registered as affiliated students and were able to get a B.A. in two instead of the usual three years. Naturally [a Scot] usually did very well against his English competitors having had three or four years at a Scottish university before he entered Oxbridge. No wonder the Scots at Oxbridge got a reputation as brilliant scholars and took so many prizes! All this arose because Scottish education in the higher forms at school and at universities (especially St Andrews) was much broader than the English, which had established a tradition of 6th form specialisation.. I took the Higher Leaving Certificate in eight subjects in my last year at school and at St Andrews studied Latin, English, geology and chemistry each for one year, in addition to pure and applied mathematics and

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[325] quoted by Gordon Sutherland in a lecture to *Ingeniörs Ventenskaps Akademien*, Stockholm, 8.May.1958 [copy in Add8353 D25]

[326] DH Whiffen to N Sheppard, 17 November 1980. Add 8353 A53, notes gathered by N Sheppard for the biography of Sutherland in *Biographical Memoirs of Fellows of the Royal Society*

[327] Sutherland to Brunt, 15 November 1955 Add 8353

[328] Sutherland to Sir David Brunt, mss draft and typescript copy, 15 November 1955, Add 8353 C1-C10

> physics. Although going up to University at 18 instead of 19, the Scottish graduate usually needed about one more year in the honours field to put him on par with the Cambridge B.A.[329]

He obtained a fellowship to study at the University of Michigan from 1931-33 and after spending the intervening years at Cambridge returned to Ann Arbor as Professor of Physics in 1947. He was elected a fellow of the Royal Society in 1949. In 1956 he returned to England as Director of the NPL, taking up his appointment on 15 September.

> Although I had a little previous experience of the Civil Service, it was not an easy adjustment to come from the relative informality of academic life and the expansive and optimistic atmosphere of USA to the rather strictly circumscribed life of a government scientist in a Britain beset with economic problems.[330]

It had been assumed that Sutherland would not wish to return from America. It was only after Sutherland had expressed his interest to the retiring Director, Sir Edward Bullard, and following a failure to attract other candidates — "those selected have in the end turned down the post for reasons which to me don't appear to reflect on the character of the post itself"— that Sir David Brunt wrote to Sutherland: "I have reason to think that you yourself might consider favourably the prospect of taking on this post".[331] An opportunity to pursue personal research interests was important.

> However you deal almost entirely with the remuneration [...] and although this is an important factor, it is not the only one. If it were, I should certainly stay where I am.[332]
>
> [...]
>
> [I] have recently had constructed here what is probably the best infra-red spectrometer in existence. [...] I see no reason why such an instrument, or even a better one, should not be built at the N.P.L. But what about the biophysics side? Would it be quite against the policy of the Board to start a small group in this very exciting field [...] To do good biophysics needs good physicists.[333]

However much of the subsequent correspondence was concerned with remuneration. Bullard warned Sutherland that negotiations were to be conducted with care, "It is very important not to accept without a written statement [...] I was offered £3250 verbally and told when I got here that this was an error and was paid £2500 for over a year"[334]. He continued: "The present salary is not an inducement [...] what effect an increase to about

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[329] quoted by N Sheppard in Sir Gordon Sutherland *Biographical Memoirs of Fellows of the Royal Society* (28) 1982

[330] Sutherland to GRD Hogg, 2 October 1957. Add8353 C29

[331] Brunt to Sutherland [11] November 1955. Add8353 C1

[332] Sutherland to Brunt, 15 November 1955 Add8353 C1

[333] Sutherland to Brunt, 15 November 1955 Add8353 C1

[334] Bullard to Sutherland, 21 November 1955 Add8359 C10

£4,000 would have, I suspect not much [...] many people feel that the N.P.L covers too wide a range of subjects that they are not interested in or know little about".[335] These negotiations over pay did not stop once written agreement was obtained: civil service grading structures, erosion by inflation, diminution by taxation etc., prompted constant re-negotiation during the years that followed.

Given the less than favourable circumstances, Sutherland's motivation for a return to England and to a position in which the erosion of Royal Society influence was already well advanced is far from clear[336]. His strategy of moving the NPL to a more academic role was one that accorded with an era of scientific self confidence and educational expansion. But it would seem better fitted to the interest of the Royal Society than the Government's desire to exert an influence upon the priorities of industrial research[337].

> Another thing which occurs to me is that the policy of the National Research Council in Ottawa and more recently of the Bureau of Standards in Washington of having a number of post-doctoral fellows is a very sound one and should be adopted by the N.P.L. One of the criticisms which has frequently been levelled at the N.P.L. is that it is a rather "dead" place. It seems to me this is inevitable with a group of people who have tenure and fairly well assigned tasks. To keep a place alive one must have able young men passing through who are given a good deal of freedom to work on problems of their own choosing within the framework of the facilities available at the institution. What are the chances of instituting such a scheme at the N.P.L.? The numbers must not be large but the stipend should be very attractive and the maximum tenure two or three years.[338]

The NPL was reorganised. Though Sutherland was not to find the time to pursue his personal research a reorganisation that created a Basic Physics Division accommodated his interest in spectroscopy. To JA Pople who was brought in to head the new division (later joined by DH Whiffen, another of Sutherland's circle) it seemed there was "a mandate to bring some modern physics into the place" as "most of the work was very classical"[339]. On arriving at NPL in the autumn of 1956 Sutherland had sought the opinion of the Superintendents on the state of NPL. The head of the Physics Division noted that there were a "considerable group of [Principal Scientific Officers], over 50, some not very bright".[340]

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[335] *ibid*

[336] Whiffen to Sheppard 17 November 1980: "*I believe he was ordered to improve the image of NPL since the appointing committee was aware that needed doing*" Add8353 A53

[337] Sir Harry Melville Permanent Secretary at DSIR was a physical chemist and FRS

[338] Sutherland to Brunt, 15 November 1955 Add8353 C1

[339] JH Pople to N Sheppard 2 Dec 1980. Add8353 A51

[340] memorandum from Superintendent of Physics division October 1956. Add8353 C14

A strategy of appointing younger senior people from outside and leaving Administration to the deadwood was attempted but was difficult to push through the Civil Service structure.[341] In November 1959 Sutherland informed DSIR that it was "difficult under current Civil Service regulations" to recruit staff, in particular he noted the delays that occurred whilst permission to advertise was obtained and restrictions on offering above the basic minimum salary, as obstacles to recruitment in competition with both universities and industry. Goodwin, Superintendent of the Maths Division, found obtaining deferment of National Service for Research Fellows difficult especially for basic rather than defence research.[342] In evidence to the Zuckermann Committee on Management of Research in October 1960 Sutherland explained the difficulties of managing according to Civil Service rules. Priority had to be given to internal candidates, plans for Research Fellowships were hampered by restrictions on the appointment of aliens, the recruitment of Experimental Officers was difficult now that the best could aim for a place at university.[343] The low pay scale for mechanics relative to industry meant only older men with accumulated pension rights were retained, whilst there were too many unnecessary porters and gatekeepers.

> On the few occasions when I have been allowed to take part in conversations with the Treasury I have been appalled at their ignorance of elementary science, and they are aggressively proud about it.[344]

Sutherland's predecessor as director of NPL, Sir Edward Bullard, had taken little interest in administration and this had led to increasing influence by the NPL's Administrator ES Hiscocks. The organiser of a symposium "Direction of Scientific Research Laboratories" held at NPL in 1956 he was recognised as "a man of great ability and persuasiveness", but "Hiscocks' empire" had caused resentment. The Superintendent of Physics noting that Administration needed to be a servant of the Director "but was dangerous acting in its own right." "Did he not refer to himself, in the recent Conference, as a 'Manager'

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[341] to Sir Cyril Hinshelwood: *"I am keen that people should not stay too long at NPL. I am sure it is healthy for the Laboratory to have a "through-put" of able young men who spend 8-10 years here."* , [undated *circa* 1958?] Add8353 H43

[342] Goodwin to Sutherland 19 October 1956, Add8353 C14

[343] The grades of the Scientific Civil Service mirrored those of its Administrative counterpart, which in turn, when conceived in the mid-nineteenth century, had been modelled on a military caste system of officer, NCO and other ranks. The Scientific Officer, equivalent to the Administrative Officer was a graduate, Experimental Officers and Executive Officers often had A'levels but had been rejected by universities. The lowest grade, the Scientific or Clerical Officer, whose experience compensated for lack of formal qualification, were often regarded as more useful. But these were the most likely to find more attractive employment in industry.}

[344] Sir Gordon Sutherland, evidence presented to Sir Solly Zuckermann's Committe on the Management of Research, 10 October 1960. Sutherland Archive, Cambridge University, Add8353 E48

of the laboratory, or some such phrase?"[345] The tendency to give priority to administrative rather than scientific excellence obviously stood in opposition to Sutherland's plans. As early as August 1956, when prior to formally taking up his post, Sutherland was consulted about prospective candidates as Superintendent of the CME Division the tension was apparent; Hiscocks writing, "I had hoped that our meeting had cleared up certain differences of outlook but apparently this is not so."[346] The conflict became acrimonious. The collection of Sutherland's papers at Cambridge include some pencilled notes written after what would appear to have been a particularly heated exchange in the autumn of 1956. It appears to have been provoked by the appointment of Uttley to the CME post, and though incoherent and difficult to read the tension is obvious: "Uttley transfer premature", "Directors car for Uttley", "Would not stand any of Superintendents above him in rank". On 17 December Sutherland wrote to Sir Harry Melville at DSIR on the need to appoint a deputy director, someone both trusted and non-administrative:

> The Civil Service rules can be used to block almost anything, but an experienced Civil Servant who is loyal to his chief can find ways and means to circumvent them or advise how to get them altered
>
> [...] Having been a professional administrator of scientific research, he over emphasises the importance of administration and the prestige of administration versus scientists[...] Naturally I cannot argue this sort of thing with someone who regards himself as one of the leading authorities on Laboratory administration without raising very bitter feeling and skilful obstruction.
>
> [...] There is no doubt that the Director, NPL, does need a deputy. [...] His deputy should be a physicist, who can talk on equal terms with the superintendents, and not an ex-chemist."[347]

In May of the following year it was announced that Hiscocks was to be Director of the UK Scientific Mission in Washington. The new head of NPL administration was HJ Hadow who, in his previous post (also at UKSM), had

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[345]. memorandum from superintendent of Physics division [BW Robinson] October 1956 "even in the eight years I have been here Hiscocks' empire has increased" Add8353 C14

The 'recent conference' was presumably 'The Direction of Research Establishments' a symposium held at NPL 26–28 September 1956. In a book published the same year he wrote: "there is a tendency in some quarters to think that administrative ability is more important than scientific ability and eminence. From the strict point of view of doing the job this is probably quite true and the main case for the scientist as director is the psychological effect on his staff. [...] in my own experience administrators have made a better job of guiding scientists than many scientists have of guiding administrators." ES Hiscocks. Laboratory Administration. Macmillan. p31

[346] Hiscocks to Sutherland 8 August 1956. quotation from hand-written letter that accompanied another typewritten note: "*I am sending herewith a summary of applications for the CME post...*" Add8353 C11 [See also next section on Uttley]

[347] Sutherland to Sir Harry Melville, 17 December 1956. Add8353 C15

assisted Sutherland in the trans-Atlantic negotiations preceding his appointment.

A Deputy Director was also appointed. The first (1958-60) Edward Lee came from the Admiralty Research laboratory and, as an ex-student of Sutherland, was probably chosen by him. The second (1960-2), George Mcfarlane being "essentially associated with the Malvern electronics establishment was very acceptable."[348] But as recalled by DH Whiffen, when the third Deputy Director JV Dunworth was appointed in 1962 it was "announced to senior staff by Gordon in a manner which made it clear that it was imposed on him by the exigencies of the Civil Service and in no way Gordon's choice."[349]

As Sutherland saw it there was an optimum size for a research laboratory that could be managed by one Director. The NPL was far larger than other laboratories administered by the DSIR, the Superintendents of its Divisions being equivalent to Directors of other laboratories. The removal of the Engineering and Radio Divisions in 1950 and 1952 was a necessary corollary of the creation of the Mathematics (1945) and CME (1954) Divisions. Early in 1957 each Superintendent was asked to put forward detailed proposals for his own Division and was told that the Research Council had invited "far reaching and imaginative suggestions" for new work within the scope of a DSIR station.[350] Shortly after Electricity, Metrology and Physics Divisions were reorganised to form Standards, Applied Physics and Basic Physics Divisions. The last-mentioned being largely a new creation reflecting Sutherland's own research interest. It was clearly Sutherland's intention that the routine standards and testing work should be shed in favour of more fundamental research. At Stockholm in May 1958 Sutherland spoke on *The Position of the National Physical Laboratory in British Applied Science*. "[It] pioneers the application of physics to a wide variety of industrial and national problems, nursing some of the latter through their formative years and then pushing them out at appropriate junctures in order to be able to enter fresh fields."[351]

There was a difficult balance between the basic standards and testing work of NPL which was dull but could be justified as needed, and more speculative research which, in the right political climate, might be justified as assisting industry but could tend toward irrelevance. PMS Blackett opined with regard to Sutherland's Five Year Plan of 1957: "the proposed expansion should only be carried out if it is found possible to attract a sufficient number of first-class scientific staff". There was difficulty in attracting the best to NPL, which was "deficient in highly qualified staff". Defence cuts might make the recruitment situation easier, but this was countered by industrial expansion. Defence cuts

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[348] Whiffen to Sheppard 17 November 1980, Add 8353 A51

[349] *ibid*

[350] 2nd Five Year Plan of NPL 59-64. PRO: DSIR 10/314

[351] lecture to *Ingeniörs Ventenskaps Akademien*, Stockholm, 8.May.1958 Add8353 D25

had also freed staff in industrial laboratories to work on other projects. As a result "industry was more concerned with soliciting Government finance for research than with making payments toward research in Government departments."[352] This general reluctance on the part of industry to contribute to pooled research made it difficult to judge what was really necessary: "the readiness of industry or Government agencies to pay for what they want done is a valuable indication of the worthwhileness of the work". In such circumstances a desire to improve the quality and status of the NPL's work was always likely to tend toward academic interest rather than industrial purpose.

> You invited me to write about the conditions at NPL which I find unsatisfactory when compared with some of the American Laboratories I have visited recently.[353]

Sutherland's change of direction for NPL, away from mundane testing and standards toward a far more academic and exploratory view of its research function, met with some foot-dragging both within NPL and without. A policy statement was prepared by Sutherland:

> 1. The NPL's industrial effort is not made because it is an inherent function of government; it is made because it is the national policy to promote and help research in industry.
>
> 2. The aim must be to help industry by being in the forefront in certain fields of research.
>
> 3. The general policy should be that, if any research or group of researches is carried out by NPL for a long period of time, good reason should be shown why it should be continued rather than the other way round.[354]

The Research Council of the DSIR was asked to give its approval at its meeting on 4th March 1958:

> Many of the changes now being made in the NPL are based on these premises and, since they represent a real change of outlook for some of the senior staff it is important for the Director to be able to quote them as having official approval at the highest level.[355]

It gave only lukewarm support, expressing the view that the document did not cover all aspects of the NPL's work.

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[352] all quotations in this paragraph from: DSIR Research Council: Committee to Review Organisation and Programme of NPL. PRO: DSIR 10/313. The members of the committee were: PMS Blackett, Sir David Brunt, Dr Willis Jackson, Dr HW Melville, Dr CM Crawley, GBBM Sutherland.

[353] Sutherland to J Popple September 1962 Add8353 C22

[354] NPL Research Policy in Relation to Industry. DSIR 10/324, AD 5/03 Director NPL to Secretary DSIR

[355] *ibid*

> ...whilst the Council were not prepared to give the statement their unqualified approval, they saw no objection to his using it as a general guide in dealing with the staff in the present reorganisation.[356]

Sutherland's ambitious Five Year Plan for the laboratory was subject to some pruning by administration at DSIR and after scrutiny by the Treasury a proposal to increase staff by 40 resulted in only 25 new posts. In January 1960 Sutherland informed a Committee to Review the Organisation and Programme of Work of the NPL:

> On his present expectation for staff it would be possible to make viable units of the Basic Physics and Control Mechanisms and Electronics Divisions but it would be impossible to allocate any staff to the others.[357]

By May in a revision of the 2nd Five Year Plan a planned total of 950 non-industrial staff by 1964 (an increase of 15%) was trimmed by the Treasury although the DSIR Research Council later sanctioned a maximum of 1050 provided the increase was funded by industry. In these circumstances a planned build-up of theoretical physics and applied mathematics was no longer possible.

In May 1961 Sutherland was invited to give informal evidence to the Robbins Committee investigating higher education in Britain.[358] It was followed by a formal submission proposing "A Technological University Adjacent to the National Physical Laboratory"[359] in April of the following year. These proposals were an extension of ideas first expressed to the Zuckerman Committee on the Management of Research in October 1960:

> The universities could make use of several of the NPL scientists to teach specialist courses to postgraduate students working for the PhD degree (Britain compares very unfavourably with the USA in the provision of such lecture courses). This would be good for the universities and good for the establishments. The counterparts of the NPL in Canada (NRC), USA (NBS) and Germany (PTB) all do this, but my unofficial approaches to London University on this matter have met with little encouragement.[360]

These proposals were similar to those advocated by BV Bowden and Sir Nevill Mott.[361] Whilst Bowden advocated using government establishments for postgraduate work, Mott thought the educational shortfall occurred at a lower level, he saw an opportunity to move older, unproductive, researchers into teaching. Sutherland's submission concentrated on the NPL case: its

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[356] DSIR 10/324, Research Council minutes 13 March 1958

[357] DSIR 10/314 Policy and Organisation 28 January 1960

[358] "Informal Evidence, by invitation to Robbins 26 May 1961" Add8353 E52

[359] Committee on Higher Education (Robbins) Unpublished Evidence. PRO: ED 118, HE(62)147 April 1962

[360] Committee on Management of Research (Zuckerman) 10 Oct 1960. Add8353 E48

[361] *Academic Quarterly* 22 March 1960 NF Mott in reply to a previous article by Bowden

expensive facilities such as the ship tank and wind tunnels were underused, postgraduate research would enliven the laboratory, its staff would benefit from teaching, students would see examples of industrially relevant research.

> It would be especially valuable to the research establishment to be able to increase a man's teaching and administrative responsibilities as his initiative in research declined in middle age. Similarly the younger men would not need to be so burdened down with excessive teaching as they are at many universities.[362]

But these proposals contained nothing to meet demands for industrially relevant research nor accountability for public funding. To move the NPL to the status of a research institute as Sutherland hoped implied less influence for Government. It might serve the interest of research scientists but not those of either industry or Treasury.[363] It was a last attempt to retain the Royal Society's influence.

> It seems to me that the NPL, [...] should be made National Institutes under the control of the Royal Society in the same way as Russian and Chinese Academies control certain research institutes in their countries.[364]

Such an academy was never likely in Britain, even at its height in the late 1950s Science was still one rung below Classics in the academic and administrative pecking order. Both research establishments relevant to industry and universities whose teaching and accreditation functions could find broad political and social support had the better case. At a time of increasingly self-conscious egalitarianism it was not the time to put the case for an elite and exclusive form of research institution.

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[362] ED 118, Notes on a Technological University Adjacent to the National Physical Laboratory. 30 March 1962.

[363] A Ministry of Education paper *The Essential Liberties of University Standing* makes this clear:

> The Ministry believe that so far as the Government is concerned there is one broad technique which could serve as a general guide. It is that any overriding limitation should be expressed as far as possible in terms of money and that any other requirements should be expressed, as far as can be quantified, in terms of minimum requirements. This technique saves the Government from interfering unduly in fields where the ultimate decision should lie with the universities or the grants committee and it gives both the universities and the grants committee the maximum freedom of manoeuvre. This is to a considerable extent the technique developed by the Treasury and the University Grants Committee.

in Robbins Working Papers & Internal Papers. ED 240/2, HE(E)62 MC23/7/71

[364] Add8353 C41 Sutherland to Sir Howard Florey, President of Royal Society 30 November 1962

The Trend Report[365] was published in 1964. The committee had considered two options for the future of the NPL, as a laboratory funded by grants from a Science Research Council or as a research establishment with an income from contracts placed by a proposed Industrial Research and Development Authority. NPL's Executive Committee, dominated by the Royal Society's nominees, favoured the former option. For the Executive Committee the main concern appears not a distinction between "pure" and "applied" research, but an insistence on a divide between industrial *research* and applied *science*. When Sir Nevill Mott made a speech on the opening of the NPL's Basic Physics Laboratory (13 May 1964) he noted that with the Trend proposals "'useless' science is to be linked with education and development, 'useful' science with industry".[366] He went on to suggest that NPL should function as a link between the two, that it should in some way emulate the laboratories of Bell and GEC in the USA.

But by that time Sutherland had already announced his resignation and by November had departed to become Master of Emmanuel College, Cambridge.[367] His successor was the Deputy Director JV Dunworth. The DSIR and Administration had finally prevailed, Dunworth was not a FRS and he and future Directors of NPL were not honoured with a knighthood.

A letter to Sutherland from Dunworth in November, 1964 gives news of how the NPL was developing.

> You will be pleased to know that Davies has decided to stay at the Laboratory for the time being, and I have been able to make very amicable arrangements between Uttley, Goodwin and Davies regarding the more immediate future. In particular, the software unit which was to have been in the Mathematics Division will now be transferred to Davies, whilst the staff concerned in Mathematics Division will be seconded to Davies for the time being until a more permanent solution to the whole problem can be decided.[368]

——oo–O–oo——

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[365] Committee of Enquiry into the Organisation of Civil Science (Sir Burke Trend). Cmnd 2171

[check also Committee to Review Certain Aspects of the Work and Organisation of the Scientific Civil Service (Sir Mark Tennant) 1964]

[366] Trend Report The Future of the National Physical Laboratory 63-64. DSIR 17/401

[367] Sutherland had given notice in April 1964. "It has been suggested to me by Mansel Davies that one of the reasons why Gordon was interested in moving back to Cambridge was because under the Ministry of Technology and Blackett's influence the climate was changing strongly towards applied rather than pure research at the NPL." Add8353 A51 Sheppard to Whiffen 3 Dec 80

5th September, 1956

Dear Uttley,

I am delighted to know that you have decided to put in an application for the post of Superintendent of CME Division. I look forward to seeing you at the interview on Thursday, October 16th. However, you must regard this last piece of information as highly unofficial because the official invitation to be interviewed will come to you from the Civil Service Commission.

Yours sincerely,

G.B.B.M. Sutherland[369]

———(())———

Radar Research Establishment
St Andrews Road
Great Malvern
Worcs

31 October, 1956

Dear Dr. Sutherland,

Thank you very much for your kindness to me yesterday; I believe that through it things have started off on the right foot. My wife joins me in thanking you for the delightful grapes.

I have heard today from the Civil Service Commission that my Certificate of Qualification for employment as Superintendent has been sent to the Establishment Officer of D.S.I.R., I have also heard from Wooldridge, the Deputy Establishment Officer, D.S.I.R. who is also a fellow oarsman of thirty years ago, that the matter of removal expenses has been agreed, that D.S.I.R. have asked Treasury for the maximum D.C.S.O. salary but have not yet had a formal reply and thirdly that although I have accepted an organisational D.C.S.O. post the award of a special D.C.S.O. grade is a 'decoration for life'; I am quite happy about this.

I enclose a copy of a letter I have received today from Brigadier Hinds together with a copy of my reply and I would make the following points, in confidence.

I fully support the new item 2B on the CME Draft Research Programme 1957/8 "Study of human beings as part of data manipulation or control systems" though I may suggest some rephrasing.

I shall want to build this work up slowly after the fullest consultation with Psychology and Physiology Departments of Universities. I would not be prepared to inherit this collection of M.O.S. commitments which are of very variable value; I could not support item 2(i) the Extra-Mural Research Contracts, or item 2(ii) [Co-relation of results of manual tracking experiments at different installations] which has not yet started.

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[368] Cambridge University Library, Manuscripts Collection Add8353 C35, Dunworth to Sutherland 26 November 1964.

[369] Add8353 H29, copy of letter to Dr. Albert Maurel Uttley, 5 September 1956

> As soon as possible I shall give the fullest consideration to the co-ordination of research in this field which is being carried out in Universities, the Scientific Civil Service and in Industry.
>
> Yours very sincerely,
>
> Albert Uttley

It is not certain, but seems likely, that Sutherland knew Uttley by reputation rather than personally before the exchange of these letters. Uttley's name may have been suggested by some mutual acquaintance: possibly BV Bowden whose 1953 account of British computing developments *Faster than Thought* had included a chapter contributed by Uttley and who had known Sutherland personally since before the war[370]. Or possibly WAH Rushton who had presented papers at Biophysics symposia organised by Sutherland[371] and was, like Uttley, a member of the Ratio club. Sutherland himself had had dealings with "the Malvern electronics establishment" during the war.[372] It might also be entertained that the DSIR interest in reinvigorating work at CME and a MoS interest in offloading less essential defence commitments may have colluded. The connection, like much of Uttley's biography, remains shadowy.

Albert Maurel Uttley was born 14th August 1906 in Stoke Newington, North London. His father George a "Professor of Singing", his paternal grandfather James a "builders foreman". It would appear the Uttley's came originally from Todmorden in Lancashire.[373]

Uttley gained a first class degree in Mathematics at Kings College, London in 1926, a teaching certificate the following year. It is not clear if he was ever employed as a teacher. He returned to Kings College to study psychology, gaining a BSc in 1936 (the year he married) and completing his PhD under Prof. FAP Aveling in 1940. The title, *The Phi Phenomenon*, refers to the illusion of movement that occurs when a series of modified images are presented in succession –the basis of cinematography.

> The ground covered in the last 24 pages has been theoretical and even tedious, but this slow approach to the problem has been essential. The disagreement as to what is seen in the Phi presentation, and the problem of why stationary stimuli should rise[sic] to an impression of movement,

[370] See Add8353 A3/4. The earliest letter from Bowden in the archive dates from 1939 when he was physics master at Oundle school.

[371] Add8353 D25, letter from Rushton 5 Oct 1959 re Biophysics Conference organised by Sutherland at Cambridge. "*It was new when I said it two years previously at your NPL Symposium...*"

[372] Sutherland was Scientific Assistant to the Director of Scientific Research at the MoS 1939-42

[373] Birth and marriage records at General Register Office

> cut to the very roots of the nature of perception of movement in particular.[374]

With the onset of war Uttley had had to move his laboratory out of London to Tunbridge Wells where the change of electricity supply from 100v DC to 240v AC had made a redesign of the experimental apparatus necessary. On the phi phenomenon, his conclusion, after some not entirely practical advice on the perfection of cinematography, was:

> The answer is to be found in the realisation that the word "see" has a limited use. We see visual sensations with their differences, and we see the perceptual elements which they build. We must be chary of evolving subjective concepts and saying that we see subjective space. Finally, we can no more see movement as defined by the physicist in terms of his physical explanatory concepts, space and time, than we can perceive any other of his conceptual constructions.[375]

This work may have formed the background to his employment at TRE after 1940. It was Uttley who designed the visual projection system for a flight simulator used for operational training in aircraft interception.

> The visual projection system, designed by A.M. Uttley, was used in the larger AI training installations at RAF Operational Training Units. The image, displayed on a hemispherical cyclorama mounted in front of the pilot, consisted of a night sky and ground of controllable brightness with a tail silhouette of a bomber which moved correctly in bank, range, azimuth and elevation in response to relative movements of fighter and bomber.[376]

Uttley also worked on other projects, a paper presented jointly with FC Williams in 1946 describes the Velodyne.

> In 1941 an electronic simulator was designed and built at the TRE to serve as the "flying unit" for their AI radar trainers. This computer was based on the ideas of F.C. Williams, famous for his later work on digital computers, and used the velodyne, another TRE invention, for integration. The d.c. method of computing was used in the simulation of the simplified fighter aerodynamics. The first model of this computer (the Type 8 Part II) was constructed by Dynatron Radio Limited in 1941 and many were used throughout the war. Later, in 1945, a more advanced flying unit including feel forces was designed by A.M. Uttley for use in a new AI visual crew trainer. This, however, never saw service.[377]

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[374] AM Uttley *The Phi Phenomenon* PhD Thesis 1940(Science), Senate House Library, University of London p44

[375] AM Uttley *The Phi Phenomenon* PhD Thesis 1940(Science), Senate House Library, University of London p104

[376] Kevin Moore. A Brief History of Aircraft Flight Simulation. <http://www.bleep.demon.co.uk/SimHist5.html> [12 May 2000]

[377] Kevin Moore. A Brief History of Aircraft Flight Simulation. <http://homepage.ntlworld.com/bleep/SimHist6.html> [1 September 2002]

There was also work on automatic astro-navigation ("blue sapphire") and Uttley was recognised as the designer of the TREAC computer [378].

The official history notes that it was on the initiative of the TRE Director WB Lewis that a number of senior staff were persuaded to remain after the war to form a Physics Research Group:

> This group, which was at first sponsored and financed by the Department of Scientific and Industrial Research but was reabsorbed into the Ministry of Supply's budget in 1951, laid the foundations of a Physics Department which subsequently became the largest and, because of the unclassified and therefore publishable nature of much of its work, best known department in the Establishment. [379]

Uttley's membership of the Ratio Club during those post-war years has been noted in the previous chapter. It was also in this period that Uttley contributed two chapters on the "Conditional Probability Computer" to Shannon and McCarthy's *Automata Studies* of 1956,[380] but he appears to have published little of note before that date.

Obviously Uttley was the favoured candidate of Sutherland, but EA Newman, acting as Superintendent after RH Tizard's resignation, was a strong internal candidate to succeed him[381]. The appointment of Uttley from outside NPL, though from within the Government Scientific Service, caused some friction. David Yates, who joined NPL in 1962 to work on machine translation, writes

> ...Ted worked a good deal by intuition and could not always explain cogently why he advocated a certain view, whereas Uttley was precise and inclined to be impatient and dogmatic.[382]

Externally, the minutes of the NPL Executive Committee suggest that the appointment was viewed with satisfaction:

> The Chairman [FP Bowden] added that Dr. Uttley was an outstanding candidate for the post and it is extremely satisfactory to have a man of his calibre to fill this key appointment.[383]

In May 1957, TGN Haldane, in a report to the Executive Committee was equally enthusiastic:

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[378] Uttley's description of this can be found in BV Bowden. Faster Than Thought. Pitman 1953

[379] DERA web page (was <http://www.dstl.gov.uk/html/aboutdstl/copy/edf.htm> [2001])

[380] Shannon CE. McCarthy J. *Automata Studies.* Princeton 1956

[381] Add8353 C11, ES Hiscocks to Sutherland 8 August 1956. To which is attached a ms note: '*I am sending herewith a summary of the applications for the CME post and shall be interested to know your selection for the short list. Perhaps you will list 6 of these people with two reserves - the later in order of preference. The only absolute "must" is our own man Newman.*'

[382] Yates 1997, *op cit* p63

[383] NPL Executive Committee Papers 1956. DSIR 10/409, Oct 23 1956

> It is particularly fortunate that at this pioneering period in the field of control mechanisms and electronics the Division should be operating under the inspiring leadership of Dr. Uttley as Superintendent.[384]

The report went on to recommend a doubling of the Division's staff, and a move toward fundamental research, leaving computer development to industry.

For Uttley, perhaps the attraction of NPL was the opportunity to shape a department to his own interests.

> From 1940 to 1966 I worked in applied research at the Telecommunications Research Laboratory and the National Physical Laboratory, whose wise directors believed that everyone should be able to devote 10 per cent of his or her time to "fun". For me, this was the Brain. Administration grew to occupy the remaining 90 per cent! [...] At NPL, Ben Burns and Tim Bliss joined my Autonomics Division for a year; the Carnegie Corporation of New York supported visiting Fellows from the USA; I was building conditional probability computers.[385]

Yates makes this assessment:

> He was a scientist at heart, devoted to investigating the natural world and applying scientific theories in the design of new technology, and was not motivated by policies of Government or opportunities for industrial development, or even by new computer applications except when they were relevant to his chosen field. His vision was of a long-term combined effort by engineers, physicists, mathematicians and life-science professionals to push forward together the technology of computing and the science of the brain; and he aimed to foster a stimulating university-style atmosphere in which these people from different disciplines could work fruitfully together. Many found this idealism inspiring, but it could also provoke irritation in those whose approach to achieving progress was more incremental and who saw him as politically naive.[386]

A notable feature of the Division's new building at NPL was a quiet room for contemplation, and Yates recalls an inscription placed over a notice board in the entrance:

> Study to be quiet, and ... work with your own hands.[387]

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[384] NPL Executive Committee Papers 1957. DSIR 10/410, NPL E10/57 Report of the Control Mechanisms and Electronics Division Panel

[385] Uttley AM. *Information Transmission in the Nervous System*, Academic Press, 1979 [pvii]

[386] Yates 1997, *op cit* p57

[387] And that ye study to be quiet, and to do your own business, and to work with your own hands, as we commanded you; *1 Thessalonians 4:11* "On the quotation AMU put up, no I don't think he would have edited it, and seeing how you put it with dots in the middle I think that's how it was in his inscription. [...] This is faithful to the Authorised Version apart from the initial capital and final full stop. I can just see that he wouldn't care for the reference to 'business' in the words omitted, which might sound commercial to a modern reader. In so far as we understood it at the time, we didn't really see its relevance; quietness didn't seem to be a quality particularly

Uttley's proposal for the future work of the Division, forming part of an overall NPL five year plan for 1959-64 was finalised in May 1957.

> The primary aim of the Division is to devise automatic ways of carrying out massive and tedious mental tasks which arise in industry and commerce. An associated aim is to study similar activities in men; this may lead to ideas for automatic methods.[388]

There would be no further work on the building of "general purpose digital computers" but further research contributions to the advancement of computing machinery were envisaged. The studies of clerical mechanisation were to continue. The growth area was to be "Automata Studies". Pattern recognition and learning were central to this, and it would be necessary to take into account the work of physiologists and psychologist.

> It is most important to discover which parts of a task are best performed by machine and which are best done by man; then there will not only be economy of research but in the end result there will be machines which are aids to man and not crude competitors.[389]

The Division, doubled in size, would have a total staff of 130 of which half would be of Scientific or Experimental grade. In a presentation to the committee on the 3. June 1957, Uttley emphasised the wide application of pattern recognition techniques, not only to visual identification tasks but to problems of language translation and data processing: "All recognition is selection of relevant information from a large mass". He expected machine building and analogue techniques to be less important as "Many special purpose machines could be simulated by programmes on a digital computer".

By November all this had been accepted:

> It has been agreed by the Review Committee that the time has come for a major effort in this field by the NPL, and Dr Uttley's programme has been enthusiastically received by the executive committee. The proposed increase in staff is the absolute minimum...
>
> The work on which this division will be engaged is not yet being carried out in universities. A pioneering effort is required, similar to that done by the Division in the computer field, which is now recognised as a suitable subject for university participation (cf. the recent establishment of computing centres in various universities).[390] There is no appreciable

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lacking, and the last few words sounded like an instruction not to delegate, again not very appropriate. I don't think it lasted long when DWD [Donald Davies] became Superintendent." David Yates, personal communication 20 March 1999

[388] DSIR 10/410, NPL E11/57 The work of the Control Mechanisms and Electronics Division: note by the superintendent

[389] *ibid*

[390] In 1956 a survey by NRDC had revealed only eight colleges offering courses in computing of which the most important were thought to be the Mathematics Laboratory at Cambridge University and Northampton Polytechnic. Northampton Polytechnic, which was actually in Finsbury, north of the City of London, presented

> overlap with industry, which is confining its attention to more immediate problems in the exploitation of computers.[391]

Despite the enthusiasm for the programme reservations were expressed regarding the introduction of biological research into a physics laboratory as may be inferred from this memo regarding the 1960 research program

> "Study of comparable biological systems" were first introduced into the programme in 1958-9. The form of words was agreed by the Godfather of the time Dr. T.G.N. Haldane. These items have been agreed for 58-59 and 59-60
>
> The total proposed effort in 60-61 is 10%.[392]

The NPL had organised a symposium on automatic digital computation in 1953. This was part of a programme of two symposia a year covering all aspects of NPL's work. The tenth such event was *Mechanisation of Thought Processes* in November 1958.

> The scientific discussion at the Teddington conference was robust to the point of rudeness. No doubt the reason for this is that the people attracted to work with learning machines are innately individualistic and somewhat intolerant of other people's ideas.[393]

As a letter from Lord Halsbury at NRDC shows the theme of computer use had been proposed by June 1956.[394]

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a course of 11 lectures on *Digital Computation and Calculation for Commerce*. The NRDC noted that, of a total of 436 students attending, 127 were accountants and 30 scientists and engineers. [NAHC NRDC 86/1/1, paper 106, Tuesday 13 March 1956] A later report noted "the difficulty of preparing programmes for computers has been continually and seriously underestimated. One machine doing general work requires 30-50 programmers to keep it working 24 hours a day." In the same report it was estimated that there were "less than 200 people in America with programming experience". [NAHC NRDC 86/4/4 report C5/701/18.6, undated]

[391] DSIR 10/410, 2nd Five Year Plan of NPL 1959-64, Directors Proposal (E.28/57) November 8 1957

[392] DSIR 10/397 Memorandum Uttley to JR Illingworth, Administration, 26 August 1959

[393] *New Scientist* 4 December 1958, p1447

[394] This one of a number of efforts by Halsbury to promote computing. In December 1954 the NRDC sub-committee on Electronic Computers considered a proposal for an "organisation devoting itself to the study of the application of Electronic Computers for industrial and commercial purposes" [NAHC NRDC 86/1/1. Minutes and Reports 9 December 1954]. A *London Computer Group* with links to Northampton Polytechnic and comprised largely of accountants and managers with practical experience of business applications was already meeting informally. Halsbury hoped this would form the nucleus of a nationally based society modelled on the ACM in America. But there was opposition from the professional bodies of accountants and engineers who feared a rival organisation. In April 1956 in a draft letter to Sir George Nelson, Chairman of English Electric and President of the Institute of Electrical Engineers, Halsbury wrote: "You should think of the society in

> My preliminary moves to enquire whether manufacturers would like to hold a Computer Exhibition in 1958 appear to be fructifying and it looks as if the Exhibition will be steered through under the auspices of OABETA[395] and RCEEA[396]. The manufacturers would like at this stage to hear rather more of the plans of the scientists for a symposium on the use of computers to be synchronised in London with the exhibition, and I have promised to bring somebody along who can tell them what is planned and projected at our next meeting.
>
> I imagine that somebody from NPL would be the appropriate choice but, in view of the fact that Tizard is known to be leaving, I think it better to write to you in your capacity as Secretary of the Brunt Committee before doing any more.[397]

The affinity between the 1957 five year plan for CME and what was presented in 1958 is clear, and there is no doubt that, whatever was envisaged by Halsbury in 1956, Uttley made the 1958 symposium his own.

> The Director said that a provocative programme has been prepared by Dr. Uttley and that the symposium is exciting a good deal of interest.[398]

The symposium may therefore be regarded as launching Uttley's Autonomics. In its programme[399] we can see traces of the pendant to Halsbury's trade show, remnants of the desultory CME programme, but most strongly the promise that had brought such enthusiastic endorsement from the Executive Committee. Some of the presentations, in particular the papers presented by John McCarthy (Programs with Common Sense) and Oliver Selfridge (Pandemonium: a paradigm for learning) are still regularly cited. After more than forty years the programme remains fascinating in its breadth, an open field.

There is a remarkable diversity of interests represented, both in the papers presented and the various affiliations of those who attended. The event appears to have attracted wide interest. Space was limited at NPL and not all the invited audience could be accommodated. It was mentioned in national and trade papers and the proceedings first published in 1959 were reprinted in

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terms of the Chemical Society rather than a professional institute like the Royal Institute of Chemistry or the Institute of Electrical Engineers [...] a society of users with common interest in the technological field but concerned with use problems and a side interest in the problem of logic, design or technology." [NAHC NRDC 86/46/1, Halsbury to Nelson 11 April 1956]

[395] Office Appliance and Business Equipment Trades Association. [From archives of the Business Equipment Trade Association held at the London Metropolitan Archive (NRA 33595)]

[396] Radio Communication and Electronic Engineering Association

[397] Advisory Committee on High Speed Calculating Machines (Brunt Committee); 1953-60 General Correspondence. DSIR 10/321, June 4 1956 Lord Halsbury, NRDC to AI Williams (Secretary to Brunt Committee)

[398] Executive Committee minutes 1956-63. DSIR 10/408 NPL, October 8 1958 Report by the Director

[399] See appendix A4

1962. Most of the delegates (174 of 229) were from Britain and 22 from the United States, six delegates from the USSR, the rest from Europe.[400] Analysis of the various academic interests is less certain, sometimes only the name and organisational affiliation of the delegate is known, in other cases historical changes may either mask or emphasise a difference of discipline; are cyberneticists a distinct species or should they be lumped with the computer folk or the psychologists? What is certain is that this was, as intended, a gathering of scientists not business people. Around a quarter, or more had some engineering training, usually in electronics, a mathematical education was more common than the presence of professed mathematicians. Though many continued to have an interest in computation, computers and automation in the years that followed I estimate that less than half could be said by the end of their careers to have followed a path to Computing Science or the computer industry itself. Medicine, in particular neurology, physiology and psychiatry, and what would become the Cognitive Sciences are well represented; at least a quarter appear to have studied biological rather than physical systems.[401]

The format was for papers to be circulated in advance and the subsequent discussion was recorded in the published proceedings. 'General Principles' was the theme for the first day and included papers by Marvin Minsky, Donald Mackay, John McCarthy ('Programs with common sense' with an anticipation of the Lisp language), Ross Ashby and Uttley. It thus covered a variety of future approaches to artificial intelligence: logicist, connectionist, cybernetic and cognitive. The concept of computing and of intelligence was equally inclusive, Mackay dwelt on the contrasts between analogue and digital computation, Uttley was concerned with biological models, in rather different ways both McCarthy and Ross Ashby sought a reductive and abstract modelling of behaviour.

Language, as code and translation, was the subject of the second day. A morning devoted to 'Automatic Programming' with papers by Grace Hopper, RA Brooker, John Backus and A P Ershov, was followed by an afternoon session on 'Mechanical Language Translation'.

The following morning covered 'Speech Recognition'. Once again a communication theorist's view of language predominated: a signal to be transmitted, translated, decoded. The afternoon session 'Learning in Machines' contained an account of perceptrons by Frank Rosenblatt and Oliver Selfridge's 'Pandemonium: a paradigm for learning'.

On the fourth day the Symposium split into two parallel sessions, 'Implications for Industry' chaired by Lord Halsbury and 'Implications for

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[400] The preface to the published proceedings claims "Nearly 200 delegates attended and of these about one third came from overseas." The figures given here come from my analysis of the list of delegates and include NPL staff etc.

[401] See appendix A3 for a list of those attending.

Biology' chaired by Professor JZ Young. Here perhaps is an indication of the tensions between the programme proposed by Uttley and the expectations of industrial research.

The Symposium on Mechanisation of Thought Processes may be considered as the event that launched 'Autonomics' at NPL; though it had been planned prior to Uttley's appointment, which had occurred the previous year, and the Control Mechanisms and Electronics Division was not renamed Autonomics until 1960 the event nonetheless was a showcase for the range and ambitions the Division was to entertain under the leadership of Uttley. On the whole those ambitions were not realised. There were notable contributions to computing from NPL; there was the work on numerical methods and the standardisation of Algol, but this was the work of the Mathematics Division not CME/Autonomics. Uttley's successor as Superintendent, Donald Davies, made a notable contribution to the development of packet switching and network security, but in the early sixties Davies' work on cryotrons was no more successful than the many attempts since to perfect a viable low temperature device.

In 1958 then, Uttley's vision of an interdisciplinary science built around the thought processes revealed by and associated with computer development was accepted. A prevailing positivism assumed that the application of scientific principles could tackle any problem. But in the decade that followed confidence ebbed away. Effort became concentrated on projects with more immediate prospects of success. In the field of computing this tended to be the extension and incremental improvement of existing technology. The hardware became more reliable, the libraries and layers of program code were built up. Computing technology was disseminated as a scaled down version of what was established in the late 1950s; less cost, less size, less time. The applications remained the same; calculation for science and commerce in ever greater volume—a logistic rather than cybernetic concept of control.

Part III

The only thing that anybody can understand is mechanics and that is what makes everybody feel that they are something when they talk about it. About every other thing nobody is of the same opinion nobody means the same thing by what they say as the other one means and only the one who is talking thinks he means what he is saying even though he knows very well that is not what he is saying. That is the reason that everybody thinks machines are so wonderful they are only wonderful because they are the only thing that says the same thing to any and every one and therefore one can do without them, why not, after all you cannot exist without living and living is something nobody is able to understand while you can exist without machines it has been done but machines cannot exist without you that makes machines seem to do what they do. Well anyway...

Gertrude Stein *Everybody's Autobiography*

Chapter 5 *Interlingua*

> To try to deal with all matters by logico-scientific language is as self defeating as to try to capture water in a net, or breeze in a bag.
>
> Phillip Wheelwright *Metaphor and Reality* 1962

The mid-twentieth century was an era with unbounded confidence not only in the capacity of science, but also in its underlying ethos of rationalism—the 'project of the eighteenth-century'. Which is not to say that human behaviour of itself became more rational. The growth of mass communication and organisation in the previous century had depended on an extension of literacy and basic education. No such constraint applied to the new media such as broadcast radio; oratory acquired for a time a new and terrifying power. If by the late 1950s doubts were beginning to creep in—a nuclear accident at Windscale[402] in 1957, the publication of Rachel Carson's *Silent Spring* in 1962—the confidence of the administrative class, in "Industry" as much as the Civil Service, that any problem could be solved with planning and management on a large scale was not easily overturned.

In 1947, in the second edition of *The Socialist Case*, Douglas Jay wrote "...the experience of 1940-47 in Great Britain has shown again that over a wide field far better results—not merely for production but for general consumption—can be achieved by such planning than by *laissez-faire*."[403] Whichever party governed, wartime experience had reinforced a prevailing positivism and confidence in the efficacy of government. The last of wartime rationing controls were not lifted until 1954. Jay's confident assertion that "the gentleman in Whitehall really does know better what is good for people than the people know themselves"[404] would persist within government circles for decades more. When in 1974 the Royal Society held a discussion meeting on 'the effects of the two world wars on the organisation and development of science in the United Kingdom', the speakers were drawn from a generation whose careers had prospered in the post-war years. The prevailing mood reflected an undiminished confidence in the value of the 'big science' approach.[405]

Post-war the biologists returned to their trade, but with a reformulated concept of behaviour. They had encountered at first hand the design, construction and operation of elaborate automatic machines. For the

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[402] Now known as Sellafield, Cumbria.

[403] Jay D. The Socialist Case, 2nd edn 1947 p245 (The first edition had been published in 1939.)

[404] *ibid.* p258

[405] The meeting was organised by RV Jones and attended by such 'elder statesmen' of science as PMS Blackett, and Lord Zuckermann.

biologists the machine was a model of animal behaviour. If the best way to understand the working of something is to build it yourself then this was the essence of the insight acquired by these biologists. There was a new interest in process and information.

Engineers and mathematicians had also been involved in wartime projects to develop automatic machinery and had come into contact with biological ways of thinking. But with less marked effect; the physics tradition was strong and in the post-war world the rapid development of discrete state technologies ran counter to the biologists approach. Indeed, physicists moving into life sciences tended to bring their physic with them.

The Control Mechanisms and Electronics Division at NPL, renamed by Uttley the Autonomics Division, may be seen as an attempt to fuse these traditions of physics and biology. As the eclecticism evident at the 1958 Symposium shows, its formation predates the formation of a distinct and well-defined field of computing, yet it had by the late 60s become a typical computing division.[406] The research component, with a strong foundation in applied mathematics was separated from the business of supplying computing services to NPL as a whole. Can we see in this transition some trace of the influences that brought about this enclosure of the field of computing?

The NPL had a rolling programme of biennial reviews of the work of Divisions by panels of three or four visitors appointed by the Executive Committee. These reports, together with the minutes of the meeting at which they were discussed (with the divisional superintendent in attendance) provide a series of snapshots of the development of Autonomics. The first review of Uttley's programme occurred in October 1959.[407]

Over three years CME staff numbers in scientific grades had risen by forty per cent, the number with higher degrees from one to seven. Uttley reported to the Executive Committee[408] that work on ACE and Clerical Mechanisation had finished, and he was clearly pleased that such industrial research could be dispensed with. Staff from ACE were to be transferred to work on High Speed Computing and the clerical mechanisation group to Mechanical Translation. The remaining programme was of "long-term basic research," but he felt it necessary to add in justification; "they all have clear cut, short term, practical objectives".

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[406] Donald Davies proposed, in 1968, to rename it Information Science but this was rejected as "something librarians do" and so it became the Division of Computer Science. (interview 11 February 1999)

[407] The work of the division was reviewed on 5th October 1959. The visiting panel of the Earl of Halsbury, AJ Young and JF Coales approved the work but noted that the biological work whilst "legitimate", "should only be a small fraction of the total effort".

[408] NPL Executive Committee. DSIR 10/412, Superintendent's report on the CME division, 13 October 1959 (E 27/59)

The report contains a strong plea for inter-disciplinary research. The key ideas of "learning" and "discrimination" would draw on mathematics, physics, engineering, linguistics, physiology and psychology. He notes the programme of the RLE at MIT,[409] and a proposal for an American "Institute of Information Sciences" as examples of the required attitude.

The succeeding paragraphs suggest he was already encountering resistance within the institutional structure of the NPL "I have restricted programme items to three.[410] A little effort on Information Retrieval might be worthwhile." In the paragraphs that follow the tone is clearly defensive.

> I do wish for freedom to draw from any subject which may be relevant to the above three goals. The fact that I might want to recruit, say, one neurophysiologist would not imply that I was planning the formation of a neurophysiology group. Such an idea would belong to the outdated "subject" idea of scientific research. Obviously such policy would lead to groups too small to be viable. Nor do I wish to form, say a Psychology group. I do not wish to form a Biology group at all. I only wish to pursue the three goals in what seems to me the best way, by an interdisciplinary attack.[411]

The frustration of administration intruding on academic work is also evident.

> The recruitment of graduates from outside the physical sciences is a problem of Civil Service procedure which must be solved. There is more paper work at NPL than at RRE [...] My time has been almost completely taken up with administrative matters and my scientific thinking has been negligible.[412]

His request that the division be renamed *Autonomics*,[413] "the present name is lengthy and not descriptive," was accepted and effected in April 1960. At the next review in 1961, the general impression was of continued overall satisfaction with the progress of the division.[414]

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[409] The Research Laboratory of Electronics at MIT where John McCarthy and Marvin Minsky established their Artificial Intelligence Group in 1958

[410] Mechanical Translation, Learning applied to Automatic Control, Pattern Recognition

[411] *ibid.*

[412] *ibid.*

[413] In both Uttley's report and the minutes of the Executive Committee's meeting of 16 November the proposed name is "Automatics". This may be a typist's mistake, an instance of the confusion that would always limit general acceptance of the name. However the term 'autonomic nervous system' had been used by neurophysiologists since the beginning of the century. It appears to have been adopted for a recent research initiative by IBM "The human body's self-regulating nervous system presents an excellent model for creating the next generation of computing, autonomic computing." <http://www.research.ibm.com/autonomic/manifesto/> [02 Sept 2002]

[414] 19th October 1961 when the panel consisted of JF Coales, Lord Halsbury, Denis Gabor, Bernard Katz (Biophysics at UCL), and PE Trier (Director Mullard Research Laboratories). The appointment of Gabor had been suggested by Uttley.

From 1962-3[415] Uttley was on an unpaid sabbatical year as Fellow at the Center for Advanced Study of Behavioural Sciences, Stanford University. It was here, he wrote later,[416] "in its monastic calm I had the big idea".[417] But there was another three years of administration at NPL before he could retire to "a studious atmosphere" and devote all his time to "mutual information networks".

A notable feature of Uttley's published work is the lack of an informal mode of explanation.[418] There is no introductory scene setting and it is difficult to discern the personally motivating principles and ideas within. Contrast this with Turing's 1937 paper *On Computable Numbers*. Turing's paper is probably better known for its description of the "Turing Machine" than the formal mathematical argument to which that is a preliminary. Uttley seems to be addressing an audience that has already, perhaps in the informal context of a Ratio Club, become at home with the context of his ideas.

Uttley retired age 60 in 1966, and joined the Experimental Psychology Laboratory, University of Sussex where he was Research Professor until 1973. In 1979 he published *Information Transmission in the Nervous System*, a collocation of "rewritten published work and new work which will not be published elsewhere, all merged, hopefully, in a continuous argument."[419] It appears to have had little impact.

The Autonomics Division, judged by criteria of lasting influence or significant invention must be judged a failure. There can be many reasons or opinions on the causes of this failure and the weighting to be attributed them. There is the emphasis placed by the Government on 'assistance to industry', the more attractive posts afforded to researchers by the expansion of UK universities and, further afield, by 'the brain drain'. After 1966, with government influence over research policy fully exerted and Uttley retired the Division fell into line with what can be seen as a conventional development of a computing division, one that can be observed in any number of academic institutions.

"Uttley mentioned he would like Gabor to be a member of the panel to report on the Autonomics Division. This seems quite a good idea, so if you agree, I am asking him and Halsbury if they will assist in this matter (DSIR 10 423 Autonomics Panel, August 5 1961 from JF Coales to Sutherland)

[415] There was a review on 8th October 1963 when the panel was JF Coales, ES Sellers (Asst General Manager, Refinery Dept, British Petroleum), PE Trier (Director Mullard Research Laboratories), Denis Gabor, and Bernard Katz.

[416] AM Uttley. *Information Transmission in the Nervous System*. Academic Press 1979. Preface (entitled 'Grace before Meat')

[417] "Suppose that a synapse varies in its effectiveness as the mutual information function of the activities on both sides of it."

[418] See Brian Rotman. 'The Technology of Mathematical Persuasion' in Lenoir. *Inscribing Science, scientific texts and the materiality of communication*. Stanford 1998

[419] *ibid*. p xiii

Autonomics was merged with the Mathematics Division; computing research was separated from the provision of computing services to the organisation.[420] Thus by the 1970s the NPL's Division of Computer Science was of a conventional type: algorithmics and electronic/communications engineering to the fore.

But what of the Autonomics programme itself? To what extent might the apprehension of what could be encompassed and achieved through computing research that had formed in the mind of Uttley and his circle be inherently flawed? Was there, apart from external constraints and unfavourable circumstances, a flaw within? Perhaps, despite a biological background running counter to prevailing trends, lines of enquiry were still enclosed by a *prolepsis*. We must remark again the difficulty of being completely original. The 'autonomic' strand was not, and could not, be detached from prevailing reductionism that was the characteristic of science. It can be seen in Uttley's own work: the psychologist could never suppress the mathematician beneath. Being able to see in a new light would not have brought solutions but may have made clearer the limitations and difficulties of the task attempted. One area of research serves to emphasise this point: machine translation.

Proposals for the mechanical translation of language are concurrent with the modern era: Leibniz and Descartes entertained the possibility and by the 1660s several 'mechanical dictionaries' had been published.[421] They followed the principle proposed by Descartes: a list of equivalent words in all known languages, linked by a common code number. Two factors seem at work encouraging this interest in translation: the decline of Latin as the *lingua franca* of the learned, and an interest in rational and logical forms of knowledge. (The basis of Becher's dictionary[422] was a list of ten thousand *Latin* words.) Though Latin would function as a source of neologisms for the new sciences (and vestiges remain to this day in taxonomy) it seems that the new discoveries had first to be explored, explained and modelled in more immediate and vernacular terms. The break with Latin may also be seen as marking a departure from ecclesiastical tradition and, particularly in seventeenth century England, removing from this new and arcane knowledge the suspicion of subversive plotting. An exhortation to write in a simple and plain style that would facilitate both scientific understanding and translation

[420] A similar process can be observed within commercial enterprises; data processing was a service, usually subordinate to accounting, a company that had developed significant computing competence often choosing to develop it as a separate source of revenue.

[421] Cave Beck (1657), Johann Joachim Becher (1661), Athanasius Kircher (1663). The re-publication of Becher's dictionary in the 1960s perhaps attests to an enduring appeal of this approach. See WJ Hutchins. *Machine Translation: past present and future*. Ellis Horwood 1986 p21

[422] *Zur mechanischen Sprachübersetzung: ein Programmierungsversuch aus dem Jahr 1661.*

became a common theme. The desire for a rationalised language is evident in Leibniz's famous exhortation: "let us calculate". In 1668, John Wilkins, in *An Essay towards a Real Character and a Philosophical Language* proposed "a regular enumeration and description of all those things and notions, to which marks or names ought to be assigned according to their respective natures".[423] This was more than dictionary lookup, and recognised that difficulties lay beyond the more immediate problem of syntactic manipulation. Yet in its desire to enumerate and classify it seems to recall the project of Bacon's *Novum Organum*.

These proposals were 'mechanical' in a sense we might today understand as algorithmic—that is capable of codification and being performed according to routine. It was not until1933 that patents for mechanical translation machines were issued independently in France and Russia. The 'Mechanical Brain' of Georges Artsrouni consisted of a keyboard and punched tape driven by a electric motor; a word was typed in, the coded tape searched and the equivalent word retrieved. It attracted the serious interest of the French railways and a prototype was exhibited in 1937. The patent issued in Moscow to Petr Smirnov-Troyanski was for an automated dictionary which functioned in a manner similar to the French device. But this dictionary was part of a more extensive system in which the language was to be analysed and transformed into a logical form prior to dictionary lookup and the resulting output (in logical form) rewritten into the target language. It is unclear how far development proceeded; information about Troyanski's work was suppressed until the 1960s.[424]

From the beginning some principles of machine translation were well established: a foundation in dictionary lookup, the stem dictionary, a specialised and limited vocabulary, a target market whose specialist knowledge might compensate for the imprecision of the translation, the concept of an intermediate language (*interlingua*), and the need for several stages of analysis and transformation. Also evident from these earliest proposals is an ambivalence about the nature of language. On the one hand translation—and the use of language in general—was seen as an algorithmic process. Yet it was also recognised that the machine translation might never replace a skilled linguist, it was only an automated dictionary.

The systems patented in the 1930s with their punched tape processing seem, superficially at least, to resemble the machines of Turing, both the conceptual Turing Machine and the realised Colossus of Bletchley Park. It would also appear that a vague and indirect knowledge of cryptographic work influenced the first post 1945 proposals for mechanical translation. Warren Weaver, vice

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[423] quoted in Weaver (1949) *op cit*, see Locke and Booth 1955 *op cit*

[424] There are claims both that that a photo/electronic version was constructed in the 1940s and that Troyanski envisaged automation of the pre- and post-processing stages. See Hutchins *op cit*.

president of the Rockefeller Foundation, wrote to Norbert Wiener in 1947 suggesting both that a computer might be used for translation and that the methods of cryptography might be applicable. As Weaver himself admitted, he knew nothing officially but had 'guessed and inferred' that there were 'powerful new mechanized methods in cryptography'. Wiener's reply was not encouraging: "the boundaries of words in different languages are too vague and the emotional and international connotations are too extensive to make any quasi mechanical scheme very hopeful."[425] In the same year Weaver talked with Andrew Booth and JD Bernal of the University of London on the subject of machine translation. Bernal's interest was X-ray crystallography and Booth, who had built a relay calculator at the university in 1946, was developing computing facilities there with this application in mind. The Rockefeller Foundation funded a fellowship at Institute of Advanced Study where Booth had learned of the work of von Neumann. The Foundation, although unwilling to fund the development of a numerical machine, was prepared to support the development of a machine for non-numerical work. On his return to London, Booth, who had talked of such applications with Turing in the mid 1940s, started work on a machine using dictionary lookup. It was demonstrated to Weaver in May 1948.

Because facilities in London were at that time limited Booth had built the machine at the laboratories of the British Rubber Producers Association in Welwyn, Hertfordshire where he had previously worked. It was there that Booth met and collaborated with RH Richens who had considered the idea of using punched cards for translations of a journal, *Plant Breeding Abstracts*, that he edited. It was Richens who observed that "linguists conversant with the grammar of a foreign language and ignorant of the subject matter provided much worse translations than scientists conversant with the subject matter but hazy about the grammar." Like Becher four centuries before and Troyanski in Russia, Richens proposed to separate words into stem and declension both to reduce the size of the dictionary and analyse grammatical information.[426] These principles were tested by Booth and Richens using punched cards and hand simulation and the work first published in 1948. But more influential was the memorandum circulated privately by Weaver in 1949.[427]

Weaver's memorandum shows him thinking of translation as akin to cryptography. It is presumed that the vocal sounds of a language are limited and the schemes by which they may be represented by written symbols

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[425] quoted in Hutchins *op cit*

[426] It is notable that the illustrations used were of Latin translation, it is important to keep in mind that this early work was not conducted by linguists, still less by professional writers and translators with an intuitive appreciation of language. (Compulsory Greek was not abolished at Oxford and Cambridge until 1920 and Latin was a requirement until 1960.)

[427] Around 200 copies were circulated. It was subsequently reprinted in Locke and Booth (1955) *qv*

equally constrained, various scholars of Weaver's acquaintance are cited in support of this. The "mathematician and logician Reichenbach" is also credited with suggesting that "for (apparently) widely varying languages the basic logical structures have important common features."[428]

Focusing exclusively on the problem of technical translation it is assumed that the 'problem of multiple meanings' can be solved by statistical analysis. Weaver hints at the forthcoming publication of Claude Shannon's wartime work on cryptography and "the statistical characteristics of the communication process" in support of this.[429] The overall sense is not only of aiming for a probabilistic 'good enough' translation but one that is founded on an underlying rational and logical foundation.

Weaver's understanding of the workings of language appears limited. In a foreword to Locke and Booth's *Machine Translation of Languages* (1955) Weaver comments on a change of words between the King James and Revised Standard Version of the Bible—a work "that had probably existed in some written form since the fourth century before Christ": "Sometime during the centuries of transcribing and translating there occurred a change of meaning…".[430] Weaver appears blissfully unaware of any distinction between errors of transmission over centuries—a problem of communication engineering if you like—and the judgement of translators, in diverse times and places, as to what might be the most faithful, acceptable, effective or beautiful invocation of the thought or intent implicit in a text. To Weaver, language itself is rational, elegance and style are a polish applied to a determinate meaning. The problem is not "*how* to translate" but "*what* to translate", once the correct text has been chosen the translation is assumed to be unambiguous: "for 'one speech' and 'few words' are simply not the same thing".

This assumption is made explicit by Booth and Locke in their 'Historical Introduction.'[431] "In much of the work that follows it is tacitly assumed that a one-to-one correspondence exists between the language of the original text and that of the translation." Though it admitted that testing by reversing the process would not yield the original text, "purely philosophical views" are of

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[428] *op cit* p17 Weaver does not state when this occurred, but it obviously predates the work of Chomsky by several years.

[429] Weaver had published a simplified account of Shannon's work in *Scientific American* (July 1949) and a joint work with Shannon *The Mathematical Theory of Communication* was published later that year.

[430] "Now the whole earth was of one language, and of one speech" [AV], Now the whole earth had one language and few words" [RSV] *Genesis* 11.1

In addition to the two versions cited by Weaver consider also the 'literal' version "And the whole earth is of one pronunciation, and of the same words," and Jerome's Vulgate (405) *erat autem terra labii unius et sermonum eorundem*.

[431] William N Locke, A Donald Booth. *Machine Translation of Languages*. MIT 1955 [reprint 1975 Greenwood Press]

no concern, only "what is useful" and "what is practicable". Once again there is a barely questioned assumption that an "interlanguage would be completely logical with simple, regular word formation and grammar, facilitating translation into any output language."[432] It is supposed that it is feasible to produce an end product without pretence to literary quality that nonetheless is of "considerable use to the reader who wishes to glean the *ideas* contained in the original text."[433] Thus *ideas* are perceived as atomic and determinate, it is essentially a mathematicians conception of the function of a language, of tokens that can be manipulated by logic.

"Language is a series of symbols representing ideas"[434] is stated as a general principle by Richens and Booth. Though their discussion ranges over a wide range of example languages, Albanian, Finnish, Indonesian, Arabic and Japanese among others, the reference standard remains English, moreover a nineteenth-century grammarians view of English: the ideal is Latin. The translation process is seen as primarily one of dictionary lookup; the separation of stem and inflection reduces the size of the dictionary; syntax makes a minor contribution to meaning. "the mere sequence of words, without any knowledge of syntax at all, is sufficiently revealing."[435] The dictionary design itself shows the assumption of English when it notes the difficulties created by languages which modify stems by adding prefix or infix rather than suffix.

Richens and Booth did not assume a computer in this early work; the merits of both punched card sorting and computers were considered and the sample translations given were conjectures and simulations not produced by machine. It may be of some interest here to compare some examples given by Richens and Booth with the output obtained from a web based service today

> Il n'est pas étonnant de constater que les hormones de croissance agissent sur certaines espèces, alors qu'elles sont inopérantes sur d'autres, si l'on songe à la grande spécificité de ces substances.
>
> v not is not/step astonish v of establish v that/which? v hormone m of growth act m on certain m species m, then that/which? v not operate m if v one dream/consider z to v great v specificity of those substance.[436]

is rendered by *systran*[437] quite fluently

> It is not astonishing to note that the growth hormones act on certain species, whereas they are inoperative on others, if one thinks of the great specificity of these substances.

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[432] *ibid.* p12

[433] *ibid.* p11

[434] RH Richens, AD Booth. 'Some Methods of Mechanized Translation' in Lock and Booth *op cit* p24

[435] *ibid.* p27

[436] The letters in italics show grammatical category: *v* vacuous, *m* multiple, *z* unspecific, *d* dative, *p* past.

A more technical German text is more difficult:

> Wenn in einem grösseren Gebiet zwei Formen nebeneinander leben, ohne sich zu vermischen, so gehören sie verschiedenen Form enkreisen an.
>
> if in a/one d large (more) area two form m beside one another live z without self to/too mix z, so belong/hear p z z different m form m circle m at
>
> If in a larger area two forms live next to each other, without mixing itself, then they belong to different form enkreisen.

It will also help establish context to consider the sort of machine capability envisaged in these early trials. Richens and Booth give two example of punched card methods requiring seven or ten runs through various arrangements of sorter, collator, punch and print. The stem dictionary is assumed to be of 5,000 words with a further 5,000 cards required to store information on word endings. They suppose a message text of one thousand words of no more than 10 letters. A translation time of between two and eight hours is estimated. They comment that a skilled human translator might be expected to do a better job in less time.

The automatic digital computer is assumed to have one to four thousand storage 'words' of 30-40 bits. The need for other auxiliary store is imperative as, with the exception of the magnetic drum, no storage "is absolutely reliable over periods of more than about 30 minutes."[438] A five bit letter encoding is assumed so in modern terms the store might be between 4 to 20 kilobytes. A magnetic drum would have capacity for a dictionary of perhaps 50,000 words. A single instruction is estimated to require one thousandth of a second. On this basis the estimated translation time is declared to be no more than 10 seconds. But to this must be added the time taken for teletype output at less than 7 characters per second. Consequently the electronic computer may be no faster than the punch card system. Computer output to punched cards might reduce the computer time to fifteen minutes but the problem of printing a human readable output would remain.

By 1966 when the ACE at NPL was scrapped, the machine translation program on that machine had the use of 800x48bits mercury delay line store (equivalent to 4.7kB), 32K words on four magnetic drums (192kB), and six magnetic tape units.[439] The maximum processing speed was 31,000 instructions per second. Both punched cards and paper tape were used for input and output. The dictionary contained 17,000 Russian words. Today, on a personal computer, Systran requires 32MB RAM per language.

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[437] <http://www.systransoft.com/> [April 2001]

[438] *op cit* p44

[439] The magnetic tape units were added in 1962, without it machine translation would not have been possible. See Yates (1997) p69.

Between the late 1940s and the start of the machine translation work at NPL in 1959 there had been a growing interest in the subject. Yehoshua Bar-Hillel, a student of Carnap, became the first full-time paid worker in the field in 1951 when he took up a position at MIT. He favoured an approach based upon logical rather than statistical analysis, a generalised grammar and specialised vocabulary. He saw that the most practical applications would be to domains such as metrology where the terminology and expressions are restricted in scope. He organised a conference on the subject there the following year at which the consensus appears to have been that development would be a two stage process. After word frequency analysis and dictionary building it would be possible to move on to syntax analysis. A further conference was held in London in September 1952. The subject attracted public attention in 1954 when in New York machine translation was demonstrated on an IBM 704 computer. This however was limited to a Russian-English vocabulary of a mere 250 words. In the same year the first journal devoted to the subject, *Mechanical Translation*, was published, edited by William Locke and Victor Ynge at MIT. A research group at Cambridge, led by Margaret Masterman, was started in that year.

The considered opinion in the early fifties was that although a perfect translation was unobtainable a less than perfect translation would be useful[440]. In the period up to 1966 this view persisted, but at the same time there was a growing interest in more theoretical work. In some measure the disenchantment with progress in machine translation can be seen as a consequence of unrealistic expectations being aroused by public interest and reinforced by academic interest in the supposed structures of language rather than practical translation.

The origin of the machine translation effort at NPL is unclear, certainly by 1957 when Uttley took up his post, there was growing interest, and the concern over Russian technological capabilities following the Sputnik launch in 1957 was an incentive. It is difficult to imagine that such an effort would have been devoted to Russian except in the context of the Cold War.[441]

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[440] Yoshua Bar-Hillel. Language and Information. Addison-Wesley. 1964 especially chapters 10-12. Whereas in 1951 he was cautiously optimistic that a limited form of translation was viable by 1960 he held that fully automatic high quality translation was impossible. He also cautioned against the objective of a smooth but '95%' correct translation on the grounds that a '5%' mistranslation concealed by an apparently fluent text would be significantly misleading.

[441] In the context of the USA Paul Edwards (*The Closed World.* MIT 1996) has argued that computing and allied technologies of automation held out a promise to "safely enclose the unpredictability and uncertainty of the real world inside the programmed microworld". (p301) Thus the rush to develop machine translation can be seen as an attempt to substitute a cybernetic mechanism for phenomenological experience. But, as we have seen in chapter 3, the development of cybernetics in Europe (including Britain) was less divorced from humanist traditions of thought. The 'closed world' isolationism, the stress on military rather than commercial motivations, portrayed by

But quite why this research should have been thought particularly suited to NPL is unclear. NPL was not directly concerned with intelligence gathering, and there was never any particular emphasis on providing translations relevant to the work of NPL. David Yates in his history of computing at NPL implies that the project was instigated by Donald Davies who saw an opportunity to make practical rather than philosophical progress in one of the more contentious subjects discussed at the 1958 symposium.[442] However Davies himself thought he had been "gently pushed" by Uttley into taking an interest in the subject. Uttley saw it as having links to pattern recognition, which was certainly close to Uttley's own interest, but there is no sign of a prior interest by Uttley in language processing.

In the five year plan of March 1957, the three main areas were: digital computing techniques, systems research (clerical mechanisation, factory control) and automata studies. Within the last, under "recognition of pattern" language translation is, with library retrieval and "interpretation of regulations", mentioned as a task requiring "memory and logical thought" which can be performed by computers.[443]

The draft plan was incorporated into a report prepared by Uttley in May 1957 prior to his meeting with the Executive Committee[444]. There the future work of the CME Division is described under four headings. Three: 'digital computer techniques', 'clerical mechanisation', and 'assistance to other divisions' are accorded little more than a paragraph each, reflecting Uttley's lack of personal interest. Far more attention is directed to 'automata studies', of which a sub-section concerns pattern recognition. He argues that existing methods for recognising printed characters are inelegant compared to processes assumed to occur in the retina. The principles of pattern recognition could also be applied to speech recognition, information retrieval and language translation. New types of machine may be possible based on theories of cortical organisation. Appearing before the committee in June he states:

> The central idea for future work is that of automatic pattern recognition, a pattern being defined as a set of properties which can be possessed or

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Edwards fits poorly with the European experience. It is difficult not to conclude that, in the case of Autonomics at least, the research was motivated primarily by a spirit of pure enquiry.

[442] See Yates (1997) p94. In conversation with myself (11 February 1999) Donald Davies remarked that he had no previous interest in MT but had been gently pushed by Uttley. Uttley worked to "get everyone behind him" and sought to discover and encourage research interests. He was strongly supportive of Davies work on high speed computers, a field in which Uttley had no interest.

[443] CME Draft Research Programme for Five Year Plan. 18 March 1957. [Typescript with ms cover page "Mr DO Clayden, Not for circulation to junior staff 21/3/57" document in the collection of DM Yates.]

[444] PRO DSIR 10/410 NPL Executive Committee Papers. The Work of the Control Mechanisms and Electronics Division. 27 May 1957

> not possessed. Examples on[sic] visual shape recognition, recognition that a document is relevant to a problem, recognition that a word is appropriate in the translation of a language....[445]

Pattern recognition, to Uttley, appears to have a statistical foundation: "recognition is associated with a measure of probability based on past experience; this involves a learning process."[446]

A questionnaire circulated to his staff in October sought to discover the degree of interest in the various new projects. Pattern recognition was mentioned but there was no specific reference to machine translation. Yet by the end of 1957 automatic translation had become a distinct project.

A letter from JF Coales to the Director in February 1958, whilst supportive of long term work on pattern recognition, is more cautious about translation.

> I am doubtful about the wisdom of getting involved in automatic translation except in so far as it can be worked in with 3[447] above. This is a subject which will be vigorously pursued elsewhere and particularly by IBM who can obviously deploy much larger resources on it than can NPL.[448]

Sutherland's reply reported Uttley's response,

> He does not intend to get deeply involved in work on automatic translation, but felt that as nothing appeared to be being done about Russian, it would not be inappropriate to have a Research Fellow on this topic.[449]

This seems slightly puzzling; work was being done on Russian in the USA by IBM and others and more general work was being done in London and Cambridge. Though there was interest within NPL in published Russian research this alone hardly seems to justify CME undertaking such a project.[450] When the project began there was only one Russian speaker in the MT group (David Yates who was recruited to NPL for this project). Though lack of language skills within NPL might make MT desirable it hardly made NPL an ideal place to base a translation project.[451] Speaking to the Executive

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[445] PRO DSIR 10/410 NPL Executive Committee Minutes 3 June 1957

[446] *ibid*

[447] Item 3 is Davies work on superconductive devices, it may be that item 4 (pattern recognition) is intended or that Davies had already been persuaded to take on the work.

[448] CUL Add8353 Sutherland Papers H5. JF Coales (Cambridge) to Sutherland, 24 February 1958.

[449] CUL Add8353 Sutherland Papers H5. Sutherland to JF Coales, 6 March 1958

[450] Coales' letter continues with arrangements for a trip to Moscow by both Coales and Sutherland. Sutherland, reporting to the executive committee in October 1958, said that "in general we are ahead in standards" but that "The Russians are catching up quite rapidly and we must keep in closer touch in future with developments in that country". [PRO DSIR 10/411]

[451] The MT group was later [1962] joined by a second Russian speaker Ssanzer. A report published in 1962 refers to a language course at NPL, 'Russian for Scientist'

Committee in 1959 Uttley again mentioned that no other group in the UK was working on Russian and expressed the view that each individual language needed to be treated as a separate problem.

In the Division's organisation chart of October 1959 mechanical translation is separate from the adaptive control and pattern recognition work overseen by Percy Hammond, being, with ACE, DEUCE and high speed computing techniques, the responsibility of Donald Davies. This arrangement continued to 1963 when, with Davies acting as Superintendent during Uttley's sabbatical at Stanford, machine translation was grouped with information retrieval and general services though it still appeared to have been a direct responsibility of Davies. This arrangement, of language processing being organisationally separate from pattern recognition continued to the end of the project in 1966.[452]

At the 1958 Symposium an afternoon was devoted to Mechanical Translation: RH Richens[453] compared human and machine translation, AP Ershov gave an informal talk on work in the USSR, and L Brandwood (who had worked with AD Booth at Birkbeck College) a classicist at Manchester spoke on pronoun reference in German. Richens characterised translation as formal transformation of symbols. Machine translation promised "to provide a highly developed analogue to an intricate operation till recently an exclusive pursuit of human beings."[454] It was a "breeding ground for new problems and new viewpoints concerning the philosophy of language" but he did not intend to confront those issues. He started by attempting to define "what a symbol indicates". (A task, which appears from the several pages of formal explanation that follow, to require considerably more symbols than characters in the sentence so analysed.) This appears to lead him to propose an *interlingua*, an intermediate language representing the 'naked ideas' and, it seems, taking into account the implicit information available to the human reader of a text who 'hears' the spoken word. He concluded that though there was no immediate prospect of MT rivalling a human translator, in some cases a more efficient or even superior translation might be possible.

In the discussion that followed Professor Bar-Hillel said that "during the last years Mr. Richens went off into lines of thinking which I do not believe will be very fruitful."[455] Richens' use of the term 'indicate' was vague even by the

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which had 12 students receiving 180 hours tuition over 2 years, after which all 'were able to make sense of Russian papers in their subject' [Physics] and half were participating in a conversation seminar. (CW Hanson. *The Foreign Language Barrier in Science and Technology* ASLIB 1962)

452 organisation charts for years 1959 to 1967 in the collection of DM Yates.

453 Richens had by this time moved to the Commonwealth Bureau of Plant Breeding and Genetics at Cambridge and his linguistic interests were associated with the Cambridge Language Group.

454 MoTP58 vol 1 p282

455 MoTP58 vol 1 p303

standards of ordinary language, "[He] uses these terms such that a sound indicates a letter, a letter a word, a word a concept, a concept other concepts as well as other entities" 'Naked ideas' had played a role in the speculations of Descartes, Leibniz, Locke and Bishop Wilkins and "they did not come to much good." Above all the problem of context was not as trivial as Richens seemed to suppose. In further discussion he noted that the disappointment with machine translation was a consequence of setting aims too high. Brandwood's analysis of German had shown that even the most careful fine tuning of rules did not result in a solution to obvious problems. Nor were Chomsky's recently obtained deep insights into the structure of languages helpful to machine translation. Its major impact was to show that syntactical analysis was more difficult than linguists had believed.

In 1964 Yehoshua Bar-Hillel, reflecting on the symposium of 1958, wrote "These meetings, [...] greatly strengthened my feelings about the utter inadequacy of the theoretical basis of [Machine Translation], and in particular about the poverty of the logico-mathematical underpinnings."[456] It is notable that Bar-Hillel, student of Carnap, "the old absolutist", sees the problem in terms of an absence of logic. At the symposium his criticism of John McCarthy's paper 'Programs with Common Sense' was hotly disputed both with the author and Oliver Selfridge, leading the *New Scientist* to report, "the scientific discussion at the Teddington conference was robust almost to the point of rudeness."[457] By 1960, in a paper whose forthcoming publication he had mentioned at the 1958 NPL Symposium, he had concluded:

> ...with all the progress made in hardware, programming techniques and linguistic insight, the quality of fully autonomous mechanical translation, even when restricted to scientific or technological material, will never approach that of qualified human translators and that therefore MT will only under very exceptional circumstances be able to compete with human translation.[458]

Yet the reason given for this conclusion is not the failure of logic but that human translators use, mostly subconsciously, *background knowledge* in order to resolve syntactical and semantic ambiguities. In these circumstances only machine assisted translation, in which a post editor reworked a machine's rough draft, had any future; and that only if effort were devoted to the

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[456] Bar-Hillel. Language and Information, 1964 p14

[457] See MoTP58 vol 1 p86

B-H "Dr McCarthy's paper belongs in the Journal of Half-Baked Ideas..."

OS ... "the old absolutist Prof. Bar-Hillel has really put his finger on something; he is really worried about the deduction actually made. [...]the notion of deductive logic being something sitting there sacred which you can borrow for particularly sacred uses and producing inviolable results is a lot of nonsense."

[458] Bar-Hillel. Language and Information, 1964 p182 first published in *Advances in Computers* 1960

partnership rather than "the doubtless intellectually much more exciting endeavour of establishing fully automatic, high quality translation."[459]

In the spring of 1959 Uttley spent six weeks visiting a variety of research institutions in the USA and much of the report he compiled on his return concerns mechanical translation. At Georgetown University he had an opportunity to view the later stages of the project that had first drawn public attention to the field in 1954 with the demonstration by IBM in New York. At Harvard he met Anthony Oettinger and reported: "A Russian-English stem dictionary has been completed which handles inflexion and which does not constrain future work on syntax since the latter has hardly begun at Harvard."[460] The Harvard dictionary had been derived from existing dictionaries and glossaries rather than a corpus of text. The stems had been derived on a similar principle: "not by subtracting end letters one at a time but by a definite logical process which they have derived from an examination of forms of inflexion. This rational process of classifying grammar , wrote Uttley, was "according to a fault-free set of rules." Uttley made arrangements to purchase the dictionary on magnetic tape.

At MIT he met another luminary of the field, Victor Yngve whose linguistic research group had a few years earlier included both Yehoshua Bar-Hillel and Noam Chomsky.[461] Yngve took a long view; really good translation would take many years, the production of dictionaries was a necessary first step but a trivial one. The great problem of multiple meaning required an understanding of syntax. In pursuit of this aim he was developing a method of tree diagrams "similar to the 'logic' tree of Newell and Simon" that would enable syntactic transformation from one language to another. Uttley noted, "syntax *must* come before semantics."

At the Rand Corporation on the West Coast he found an emphasis on character recognition. Professor KE Harper warned him that a typist was paid nearly as much as a translator, text input would soon be a bottleneck. At Ramo Wooldrige he saw a less logic based approach, a Russian speaking post-editor corrects the translation and the computer then classifies the changes. A human 'hypothesis maker' devises new rules from this analysis which are added to the program. At first they had not used a programming language such as that used by Yngve's group at MIT. But then, "It has been found quite essential for the hypothesis maker to make modifications of a far more subtle nature than of varying the instructions of the machine program." At Ramo Wooldridge Dr. D Swanson did not think Yngve's tree diagrams were essential for Russian-English translations as the word order of the languages

[459] *ibid.* p183

[460] Quotations here are from AM Uttley. A Visit to the United States and Canada in 1959. NPL, DSIR 1959 My thanks to David Yates for providing a copy of this document.

[461] See Yehoshua Bar-Hillel. Aspects of Language. North-Holland 1970 p343

were similar. They did however "advise us to use COMIT which Yngve will be happy to give us for the cost of the IBM cards."[462]

At the University of Washington in Seattle he met Erwin Reiffler's group. Reiffler agreed with the Ramo-Wooldridge approach. Though he approved of the Harvard dictionary he had developed his own with 130,000 inflected forms from a 30,000 word corpus of Russian general science texts. Specialist words he thought were not especially difficult for machine translation. Using a 'photoscopic' reader developed by IBM he had no problem with the massive storage needed.[463] He thought the inflected rather than stem type dictionary better for resolving the problem of multiple meanings.

Uttley believed that by obtaining the Harvard dictionary he would not be starting at the beginning as the American work had "covered about one third of the task".[464] Yet having obtained the dictionary it was decided to adopt a different morphology for the transliteration of Russian characters—a step which it was later admitted had set back the project by about two years.

Planning the research programme for 1960-61 the project was described as "Development of a useful system for translation from Russian into English". The team was formed from members of the clerical mechanisation group whose work had made little progress in the years before Uttley's appointment and whose work was generally seen as not suited to NPL. He noted "The first stage of the work is now beginning, which is that of dictionary organisation and treatment of the morphology of Russian words. Trained linguists are essential members of the team."[465] His plans called for a team of ideally six scientists, including two linguists, and five assistants. However, it appears that no linguist had been appointed by October when he reported that "Linguists are quite essential and urgently required". The intention was "to provide a translation service" with an "acceptable amount of post editing" and "at a tenth of the cost of human translation"[466] and at one hundred times the speed[467]. The visiting panel of the Executive Committee expressed some concern at this, preferring that the service be provided elsewhere once development was completed in three or four years.

It here that Uttley states most forcefully his view that research must be inter-disciplinary. All the projects of the division are related; they involve 'learning' and 'discrimination'. "The idea of dividing the research items into separate, watertight 'subjects' is logically false and can only lead to frustration and lack

[462] The ACE however did not use the standard IBM format cards.

[463] He suggested purchasing the IBM Photoscopic Information Storage Unit.

[464] PRO DSIR 10/412 NPL Executive Committee Minutes 20 October 1959

[465] PRO DSIR 10/397 NPL Research Programme 1960-61 25 August 1959

[466] PRO DSIR 10/412 NPL Executive Committee. Superintendent's Report on the CME Division 13 October 1959

[467] PRO DSIR 10/412 Executive Committee, Minutes 20 October 1959

of progress."[468] The programme items at NPL, he said, had been restricted to three: mechanical translation, learning applied to automatic control and pattern recognition.

By the time of the next biennial review in October 1961 Mechanical Translation was perceived by Uttley as having grown in importance. Two American groups were now offering crude word-for-word translation. "In the last two years France has built up from nothing an effort which is almost five times as strong as ours".[469] The research team at NPL now had four researchers and nine assistants. In common with projects elsewhere the interest had drifted from the purely pragmatic to encompass increasing "our knowledge of languages by applying exact methods to them." It was claimed that only four percent of Russian scientific papers were being translated. Translating at one hundred times the speed and less than one seventh of the cost "will clearly be economic". They were "half-way" to matching human translation. The chief effort had been devoted to the dictionary and the development of "Novel methods for dealing with Russian inflexitional[*sic*] patterns", but the Harvard dictionary had still not been converted to the new form. The small team had also spent some of the previous six months organising an international conference at NPL[470]. Speaking to the Committee on the 6th of November Uttley said that a translation "good enough for anyone familiar with the subject" should be achievable in two years, to make faster progress would require a larger team.[471]

By 1963 the visiting panel was able to see a demonstration of translation that was judged "useful". The installation of magnetic tapes for the ACE had been completed and the dictionary numbered 15,000 Russian words. There was talk of a pilot scheme to use the soon to be installed KDF9 computer at NPL to provide a translation service. It being thought that the KDF9 was of a size to perform "all the Russian translation that is required in this country" it was suggested that the National Lending Library might operate such a service. A pilot scheme could be in operation in six months though it would require some twelve months to assess the viability of such a service. There was no mention of how the programs developed for the ACE, a notably eccentric computer architecture, were to be transferred to another machine.

The 1963 biannual review saw some dissension among the panel regarding the machine translation project, the chairman Dennis Gabor adding a separate report. Automation was required for jobs that were either too difficult or too boring to be done by people. Translation was neither; the

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[468] *ibid*

[469] PRO DSIR 10/414 NPL Executive Committee. Superintendents Report on the Autonomics Division 10 October 1961

[470] The NPL hosted the 1961 International Conference on Machine Translation of Languages and Applied Language Analysis. As is evident from the title a more theoretical and purely academic interest was creeping in.

expansion of higher education provided scope to train linguists. Not only was translation an intellectually satisfying job, "if we want to live in one world with Russia and China, it will be a good thing to have thousands of people conversant with their language."[472] Doubt was expressed at the economic case, coding of the text was a bottleneck, dictation by a trained translator would result in a better translation at no greater cost. Unless a real break-through was made the effort should be wound up once word for word translation was achieved on the new computer.

Uttley was at this time on sabbatical at Stanford and J McDaniel who led the MT group was asked to prepare a report for the Executive Committee on the prospects for industrial development.[473] He described the team as consisting of eight: five programmers, two linguists and one assistant. The unsuitability of the ACE for non-numerical work was noted, reprogramming for a commercial machine was estimated to require 6-8 man years of effort. Developing a dictionary for each technical field might require 1-2 man years or 30 man years for a comprehensive polytechnical dictionary. The need to produce a typed output, in particular a means to integrate figures, formulae, and equations from the original was another problem to be addressed.

In April of 1964 Denis Gabor was appointed chairman of the review panel. He had previously expressed doubts about the value of work on Machine Translation, but this was not considered a disadvantage.

> ...Secondly, may I assure you on behalf of the director that your previous criticism of the direction which the research has taken in Autonomics division are in no way held to be a bar to your becoming Chairman of the Review Panel. Divisional Review Panels are not intended to be mere rubber stamps of the work that is going on in the division and criticisms made by them are most welcome and are taken very serious note of in deciding the future programme of the division.[474]

Machine translation was everywhere sponsored by government, there was no sign of commercial interest in providing such a service. Plans for a translation service assumed a centralised and regular program. It was intended to provide a systematic and scheduled translation of journals, not of any text on demand. There is an estimate that the market for machine translation might be £100-150,000 per annum, but it is admitted that the National Lending Library had

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[471] PRO DSIR 10/414 Executive Committee Minutes 6 November 1961

[472] PRO DSIR 10/417 NPL Executive Papers 1963, Report on Autonomics Division by the Chairman of the Divisional Panel 4 November 1963

[473] PRO DSIR 10/417 NPL Executive Papers 1963 Industrial Development of Machine Translation. 4 November 1963 (the official report list the staff devoted to MT as being 11 including 6 research staff)

[474] DSIR 10/423, Autonomics Panel, letter to D Gabor from TC Crowhall (secretary to executive committee) appointing him to chair the Autonomics review panel, April 17 1964

no funds for translation beyond its present budget of £60,000. The rate recommended by the Institute of Linguists for Russian translators was £4 per thousand words, the machine rate is estimated at about one eighth of this, mostly the cost of preparing the text. This preparation rate in itself doubtful; Gabor had repeated a warning given to Uttley on his visit to the USA in 1959, typists cost nearly as much as translators. Indeed with the post-war growth of office work, typists, especially at the constrained civil service pay rates, might be harder to find. The estimate of machine time at £250 per hour also seems unrealistic. It must be remembered that at this time computers, especially in financially destitute Britain, were major items of capital expenditure. The telecommunications infrastructure, unlike the USA managed by the Civil Service, was not favourable for timesharing. The economics of a data processing bureau operation had to take into account the huge capital cost of computers. As the pace of computer innovation increased the pay-back time for that investment was shrinking.

The NPL estimate of £250 per hour is stated as being based on costs for the IBM 7090 "for which translation performance figures have been published." A footnote mentions IBM7090 installations at CEGB and UKAEA. It would therefore seem that the costing is based on prices charged between government laboratories. This leaves open the question of a commercial rate for a translation service.

The cost of the ACE was essentially unknown, whatever budget headings were used a great deal of the cost was probably lost in the general activities of the Laboratory. The November 1958 issue of *Process Control and Automation* carried a table compiled by Tom Vickers of NPL's Mathematics Department "comparing some digital computers available in Britain". The ACE—"one built (not for sale)" is unpriced. Of the sixteen machines listed basic prices range from £23,000 to £500,000 for the IBM 704. It is noted that the cost of ancillary equipment can easily double the outlay. The commercial version of ACE, DEUCE, cost £50,000.

It seems likely that the computer time estimate was based on prices charged by NPL for other laboratory services and did not attempt to recover the full costs of ACE or of the programming work.[475]

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[475] What should the price for computer time have been? I trust here my own experience. In 1973, I worked in the London Data Centre of NCR Ltd. At that time the price charge for processing time on the NCR315 computers was £20 *per minute* i.e. £1,200 per hour. (It was rare for any job to run perfectly so that the most jobs took longer than the estimated time and there was much revenue lost to unchangeable time.) Between 1966 and 1973 computers became more common and perhaps less prohibitively costly, inflation caused prices to rise particularly wages, the benefits of computer leasing see-sawed. All these factors make comparisons difficult but, on the whole, it does suggest that, commercial market price for the processing time required would have been greater than £250 per hour.

The methods used at NPL, following the strategy determined by Davies, were largely pragmatic. Davies own involvement in the details of the project appear to have been confined to the work on organisation of the dictionary on magnetic tape. As has been noted linguists and Russian speakers were only slowly added to the project. As the project progressed the need for some semantic analysis became apparent. But this was never a 'first principles' approach, the need for 'an adequate English output' drove the inclusion of "*ad-hoc*, empirical and approximate procedures, always provided the English output benefits thereby".[476]

A detailed account of the techniques used at NPL is given in the Final Report issued in July 1967.[477] The report includes the full text of a sample translation of a scientific paper (Investigation of boundary layers between domains in some ferrites with structure *shpine*) in a 1965 issue of *The Journal of Experimental and Theoretical Physics*[478]. The processes involved in analysing the structure and translating three sample sentences are set out in some detail in the report. These are shown below, in the original Russian, as translated by NPL and a translation achieved by programs readily available today.

> С ломощью методики, использующей полярный аффект Керра, проведено исследование ширины доменных границ, их полярности и распределения в них намагниченности.

The NPL translation of 1966:

> By means of method, utilizing polar effect of Kerra, is conducted/installed/passed[479] investigation of width of domain boundaries, their polarity and distribution into/in them of intensity of magnetization.

Some examples from 2001:

> With ломощью techniques using polar effect eppa, research of width of domain borders, their polarity and distribution in them magnetization is carried out(spent)[480]

> With ломощью techniques using polar effect eppa, research of width of domain borders, their polarity and distribution in them magnetization is conducted[481]

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[476] Final Report p1

[477] J McDaniel, AM Day, WL Price, AJM Szanser, S Whelan, DM Yates. Translation of Russian Scientific Texts into English by Computer - a final report. National Physical Laboratory (Auto 35) July 1967. [my thanks to David Yates and Sylvia Chanter of NPL for a copy of this document]

[478] Журнал зкспериментальной и теоретической физики. т.49 1965 Вып. 3(9)

[479] In the NPL original uncertain translations are shown by printing the alternative words below each other. This, combined with the monospaced typewriter font, tends to hinder readability.

[480] <http://www.elingo.com/text/> [23 April 2001]

[481] <http://www.translate.ru/eng/> [23 April 2001]

With ëîìîùüþ a technique using polar affect Êåð ð à, it is carried out(spent) èññëåäîâàíèe width of domain borders, their polarity and distribution in them magnetization.[482]

With ëîìîùüþ a technique using polar hot blood Êåð ð à, it is conducted èññëåäîâàíèe width of domain borders, their polarity and distribution in them magnetization.[483]

With lomoshch'yu of the procedure, which uses a polar passion of Kerr, is carried out the study of the width of domain boundaries, their polarity and distribution in them of magnetization.[484]

A second example:

Затем порошковая суспензия осторожно убиралась и на выбрапные места направлялся модулированный поляризованный пучок света.

NPL in 1966:

Then powder suspenzion was carefully removed/tidied and/also onto/for chosen places was directed modulated polarized cluster/pencil/beam of light.

On the web in 2002:

Then powder suspension was cautiously cleaned(removed) and on the chosen places the modulated polarized bunch(beam) of light went.

Then powder suspension was cautiously cleaned(removed) and on elected places the modulated polarized bunch(beam) of light was directed.

Then powder suspension was cautiously cleaned(removed) and on âûáð àïíûå places the modulated polarized bunch(beam) of light went.

Then powder suspension was cautiously cleaned(removed) and on âûáð àïíûå places the modulated polarized bunch(beam) of light was directed.[485]

Then powder suspension carefully was removed also to vybrapnye places was directed the modulated polarized beam of light/world.[486]

Белыми и черными цифрами и стрелками показаны места записи границ.

By white and black ciphers/digits and pointers are shown place(s) of record of boundaries.

White both black figures and arrows(pointers) show places of record of borders.[487]

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482 <http://www.translate.ru/eng/erre.asp> [19 Sept 2002] (‘common lexis’) The text is exactly as returned to the browser. The conversion of the character set when no translation is possible is an artefact of differing html page coding techniques.

483 <http://www.translate.ru/eng/erre.asp> [19 Sept 2002] (‘business correspondence’)

484 <http://babelfish.altavista.com/tr> [19 Sept 2002]

485 <http://www.translate.ru/eng/erre.asp> [19 Sept 2002] (‘business correspondence’)

486 <http://babelfish.altavista.com/tr> [19 Sept 2002]

> By white and black numbers and pointers showed the places for the record of the boundaries.[488]

I think it reasonable to conclude from this informal demonstration that the quality of translation that could be achieved in 1966 was comparable to that achieved today.[489] The process today is faster and the presentation of the output more pleasing and convenient. But much of this advantage is a consequence of the adoption of word-processing for origination. The problem of data capture has been solved by eliminating the need for either transcription or pattern recognition. The improved appearance of the finished result likewise owes much to general purpose text formatting software.

In assessing the achievement of the NPL's Machine Translation project the judgement needs to consider how much the sample result presented in the group's final report was tuned to that particular text. How good would the results have been on a randomly selected text? How narrow and particular was the technical specialisation of the language targeted?

Some results of what appears to have been a fairly informal evaluation are given in the final report presented by the group in July 1967. It appears to have been a somewhat hasty and belated exercise conducted only after the decision to wind up the project. Potential 'customers', scientists with an interest in translated Russian journals in physics, electronics or electrical engineering, were asked to submit articles for translation. A machine translation was returned requesting comment on how useful it was. A second opinion on the translation was also sought from other specialists in the relevant subject. From 44 papers supplied, 19 were subject to evaluation[490]. From a free form evaluation the comments were graded on a nine point scale from 'useless' to 'fully adequate' The average rating being a little below "6. Mostly very good. A few sentences obscure, so that something essential may be lost, but normally clear enough."[491] The criticisms of translation quality

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[487] <http://www.translate.ru/eng/erre.asp> [20 September 2002]

[488] <http://babelfish.altavista.com/tr> [20 September 2002]

[489] It may be argued that the web based software used is a 'cut-down' version and does not represent 'the state of the art' today. This seems unlikely: a company that wished to sell its software would not wish to demonstrate with a poor example. Also, the suppliers of these services appear to be linked to agencies who also offer human translation. It would there seem that the sales pitch reinforces the message the MT remains suitable only for a rough appraisal of an unknown text. Finally, it can clearly be seen that, taking out of account speed and presentation, the 1966 NPL translation is good enough to stand comparison.

[490] The other papers were not translated or evaluated for a variety of reasons including six that were not translated by the time the ACE was scrapped.

[491] "the mean score is 5.6" McDaniel *et al.* Translation of Russian Scientific Texts into English by Computer - a final report. (Auto 35) NPL July 1967 [My thanks to David Yates and Sylvia Chantler of NPL for a copy of this report.] As David Yates

vary according to expectation. As the above examples show, the quality is at least a good as that in today's readily obtainable 'raw' translation. The other common observation of the evaluators has shown improvement over 35 years; the teleprinter output, with alternative words stacked up on additional lines is not easy to read, particularly for a text intended for fairly rapid scanning.

The final report did not evaluate the commercial potential, yet expresses what must be viewed as unjustified confidence in its virtues.

> We have not included a study of cost and speed within this evaluation experiment, as we do not have the market data to prepare a translation service specification that we could then refer such a study to. However it is evident that our machine equivalent of the human translator, i.e. input punching, machine translation and output printing (with no human post-editor) will show a clear advantage on both these points. It would be essential, though, to fit this component into an overall system which was specified carefully to fit the translation market.[492]

With this report MT disappears from NPL almost as mysteriously as it had commenced. A change of government, a new Ministry of Technology, and the dissolution of Executive Committee meant that there was no biannual review of Autonomics in 1966. Besides which Uttley had retired in July of that year and Sir Gordon Sutherland had resigned in April. Thus there appears to be no record of the final decision to wind down machine translation. It was in the air of course, Dennis Gabor had become chairman of the review committee in 1964, and made clear his continuing doubts about the value of machine translation. He had spoken on this subject at an NPL seminar. Yet this was but criticism, members of the committee had a role of oversight not executive intervention. Outside NPL the tide was turning against machine translation. In the USA the report of ALPAC[493] published in 1966 had the effect of closing off funding for machine translation research for many years. Only in the 1980's with American alarm over the Japanese government's sponsorship of advanced 'fifth generation' computers —reminiscent of the earlier reaction to the Russian sputnik—was funding for research restored. But there is no evidence of any direct influence of this report on the work at NPL. At NPL machine translation was simply switched off with the ACE.

Suppose we discount the possibly narrow focus of the target field. After all, this could, presumably, be rectified by greater storage capacity for both lexicon and context rules; something readily available on machines that were to replace the obsolescent ACE. The most obvious inferiority of the NPL results compared to those readily obtainable today is not the quality of translation *per se*. The NPL's specialised lexicon is obviously superior and

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(whose PhD thesis{London 1966} was based on this work) observes in his history (*op cit*) this verdict was perhaps a 'little rosy'

[492] Final Report p63

[493] Automatic Language Processing Advisory Committee

syntactic analysis, the capacity to render the English equivalent words in acceptable order and case, not notably inferior. Machine translation has improved largely by cleaning up the clutter caused by the presentation of alternative words and senses. This, once again, is not an advantage achieved by some fundamental improvement in technique. The possibility of choosing the most likely word in context by an analysis of adjacent text was known, having been mentioned in Weaver's memorandum, and there was a notable interest in Markov processes at the time. Only a relatively large and fast random access memory capacity was lacking.

The failings of the NPL's translation work, as indeed of all other efforts at the time, were primarily due to reasons noted originally by Richens and Booth and restated most forcefully by Denis Gabor in his review of the Autonomics Division's activities: the cost and time required for data entry. No matter how fast a card punch, magnetic tape or printer, the process is constrained by the need to present the text in machine readable form. A human translator could work from the original text, the resulting translation (whatever the degree of fidelity required), could be read in manuscript or typed up by any copy typist. For machine translation typing at the input stage was unavoidable and, moreover, might require some facility in reading an unfamiliar character set. Any mistyping at the input stage would have a major effect on subsequent processing.[494] Also there is no evidence that, with a pool of native speakers to be found among various émigré communities, there was any shortage of translators.[495]

A potential solution to this problem lay in another project at Autonomics: work was undertaken on optical character recognition. A linkage at the conceptual level was recognised by Uttley who saw both as being fundamentally concerned with automatic pattern recognition. NPL did have some substantive success in this project, it had advised the clearing banks on the most effective character set to be used for the automatic processing of cheques. But, once again, performance at the level required for data capture from a printed journal was not within the capacity affordable at the time. Even today, OCR from a scientific text with many special symbols would require careful proof reading post-scanning.[496] The real gain for modern

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[494] The author's own experience in preparing the Russian text in the examples here is instructive. It took several re-runs before all the misreading of similar seeming Cyrillic characters were eliminated. Without the correct spelling a full translation was not possible.

[495] One suspects that a possible subconscious attraction of machine translation may be rooted in a distrust of translators: a sense that machine translation would gain fidelity by being inherently disinterested and having a precise and formal word for word correspondence.

[496] Though I can claim no particular experience with scientific text, I have found that OCR of literary works of historical interest do require careful revision for a faithful account of original spelling and punctuation.

systems has come as a side effect of the near universal adoption of word-processing. The time and cost of machine readable input, though not, strictly speaking, eliminated, has been made invisible and irrelevant by origination in that form.

By the pragmatic criteria set for the project, that is the production of a useful and economically viable rough translation of technical material, the project was clearly in trouble almost from the start. It would seem that only the panic occasioned by early Russian satellite launches and some muddled thinking re the relation of translation to cryptography could have prompted the undertaking. There is, of course, that other justification of fundamental research: a spirit of pure scientific curiosity; it certainly motivated the researchers at NPL, but not their civil service masters, nor—cold-war fears aside—did it have a political resonance.

But how valid was that initial premise, that a rough word-for-word transcription with some syntactic rearrangement would be useful? The supposition of Weaver, that a text in Russian was really but English coded in strange symbols was, probably consciously, naive. Yet in some form it seems to have subsisted in the mind of many in the field. In their ordinary life they appear as aware as anyone of the natural ambiguity and intentionality of language, yet as scientists they approach machine translation as if it were something other. The myth of science as dispassionate and objective takes hold, the original text is presumed to have such qualities, one language is algorithmically related to another. That Weaver chose the example of a language with an unfamiliar orthography is significant; it emphasises the otherness, the detachment.

Think rather, of something more familiar to the native English speaker, French say, or German or Italian. Particularly when immersed among its native speakers, I do not hear (or read) a code; an admixture of context, tone, some vocabulary, and the wit and will to hazard the meaning of words with near English equivalents, furnishes a great deal of understanding. Sufficient, I would contend, to sense the gist and mood of a conversation.[497] Mechanical translation must perforce proceed without much of this information.

Crucially, the schema envisioned in all this early work tended to assume that the key words, the ones that might be placed in an abstract, would also be most essential to a translation. But such words, mostly technical terms, will tend to be the same or similar in many languages. A expert in a field might reasonably be expected to recognise them in an otherwise unfamiliar language. In reading comprehension it is the 'little words' that are often the source of greatest difficulty and significance: propositions and conjunctions, inversions and contradictions. Yet these tended to be regarded as the most redundant. Set aside the unfamiliarity of Russian script and it becomes

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[497] As my German friend was fond of remarking: "Yes, you understand perfectly, you just do not know what it means in English."

clearer: an expert scanning a foreign journal could probably have extracted as much meaning at sight as was to be achieved by these early attempts at translation.[498] The near instant response and low cost that makes *machine assisted* translation useful today was not possible in the 1960s. By 1966 the project had reached a natural boundary, not just in terms of engineering, but more significantly a limitation of the positivist conception of language on which it was founded.

As Alex Gross shows in his 1992 essay 'The limitations of computers as translation tools'[499] there was a school of linguistics in the USA of the 1940s that was ignored by those working on machine translation. Leonard Bloomfield, Edward Sapir, and Benjamin Lee Whorf belonged to an anthropological tradition. Their 'science of linguistics' was comparative and relativistic. Like the anthropology of Boas and Malinkowski, it insisted on careful and thoughtful observations and a non-judgmental view of different cultures and their languages.

> [Bloomfield's views] stressed a relativistic view of language and culture and the notion that languages spoken by small indigenous groups of people had a significance comparable to that of languages spoken by much larger populations. They willingly embraced the notion that language, like reality itself, is a complex matrix of factors and tended to reject simplistic generalisations of any sort about either language or culture. Moreover, Bloomfield certainly saw his approach as being a crucial minimum stage for building any kind of true linguistic science.[500]

Ten years after his death these ideas were replaced by the school of Chomsky whose linguistics "shared a certain philosophical groundwork with computational linguistics" though he himself had no interest in machine translation. In this scheme languages were related by 'universal grammar', meaning derived from 'deep structure', A transformation from surface level to deep structure and the reverse was supposed easy and likened to the logical manipulation of computer languages.

> They are not relativistic or cautious but universalist and all-embracing; they do not emphasise the study of individual languages and cultures but leap ahead into stunning generalisations. [...] In many ways they reflect the USA of the late 1950s, a nation proud of its own new-found dominance and convinced that its values must be more substantial than those of "lesser" peoples. Such ideas also coincide nicely with a seemingly perennial need academia feels for theories offering a

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[498] Again I appeal to my own experience here, I find I can glean as much from scanning a text in German, Italian or French as is yielded by any of the web or PC programs available. A machine translation is only worthwhile if available on demand and instantly at no cost.

[499] Alex Gross. 'Limitations of Computers as Translation Tools' in J Newton (ed) *Computers in Translation*. Routledge 1992

[500] *ibid.* p107

> seemingly scientific approach, suggestive diagrams, learned jargon and a grandiose vision.[501]

It is an approach that tends to emphasise the written rather than spoken word. In particular is discards 'subtext': all the expressive power that is contained not in a record of words, but in the context and manner of speaking. Words that sounded rather than read, like smiles or ceremonies, display intentions and relations. In this context it is not possible to just look at a text and type it out in target language. "If human beings could do this, then there might be some hope of computers doing it." The translator is "a writer who just happens to specialise in foreign languages."

Bar-Hillel draws a parallel between structural linguistics and mathematical logic. "Just as mathematical logic, regarded for years as the most abstract and abstruse scientific discipline, became overnight an essential tool for the designer and programmer of electronic digital computers, so structural linguistics, regarded for years as the most abstract and speculative branch of linguistics, is now considered by many a must for the designer of automatic translation machines."[502] Chomsky's work , which is more concerned with the rules that *generate* a grammatically correct sentence than the extraction of information *from* a sentence, builds on this logicist tradition. It had no influence on the largely pragmatic early work on machine translation but only on later more theoretically oriented approaches.

Writing of Japanese MT work in 1986, Makato Nagao[503] insists that machine translation systems have made fundamental advances since 1965. Yet everything he discusses has echoes of approaches tried, or at least considered, before. He argues that, particularly in the case of Japanese analysis of syntax and semantics must proceed in parallel; a dictionary requires not just the fully inflected forms of words but semantic information. It needs to be massive, to contain for each word rules for its use, the permissible relations to other words. The meanings people normally deal with are not those of a theory of symbolic logic. The merits and demerits of a 'pivot language' are considered; the question of *interlingua* revisited. Only three things had changed since these matters were debated at NPL in 1958 and 1962: the language of interest, the cost and capacity of machines and, one problem solved, the origination of machine readable text.

Another decade again sees much the same situation: computers are useful only as tools for human translators. The Internet makes translation services available world-wide for even the most obscure language and translators can thereby keep close to the sources of colloquialism and neologism. The sources of material that *must* be translated tends to be governments with official

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[501] *ibid*. p108

[502] Yehoshua Bar-Hillel. *Language and Information*. Addison-Wesley 1964 p 186

[503] Makato Nagao. *Machine Translation: how far can it go?* Oxford 1989 [Tokyo 1986]

policies of bilingualism. The commercial market is more fickle; the demand for translation is greatest where the languages significant for trade are least alike. But if the market is important then machine translation is rarely good enough. Machine translation programs are today readily available at insignificant cost. But the market for software does not appear large, and companies offering it (e.g. Systran) would appear to be using MT as a way of promoting their human translator services. Machine translation works best in a restricted domain, for general use it is acceptable only if cost free as is the case of various web based services. So it remains the case that research in MT is largely motivated by a research interest in linguistics rather than a demand for new technology. It is accepted that a fully automatic 'perfect' translation is impossible. The computer is just a component; for the professional translator a workstation, for the researcher a severe test of any theory that claims language is rational.

There is something of a mystery surrounding machine translation at NPL. When Uttley moved to NPL he made a considered break with the past. Writing to the Ministry of Supply on his appointment he took care to avoid carrying over commitments from his TRE work. "I think it is necessary to build slowly in a new environment uninfluenced by past decisions".[504] He also discontinued projects at NPL that were not relevant, such as the work on clerical mechanisation and development of computers. Yet machine translation, though a new thing, both for Uttley and NPL, does not really fit. It does not appear that NPL had any exceptional need for translations for its own purposes, it certainly had no linguists. If there were a need surely a translator could have been employed. The ACE was built as a purely mathematical tool. Despite its origins in a design by Turing, who certainly had imagined non mathematical applications, it was quite unsuited to large volume text processing. It had to be modified by the addition of magnetic tape before machine translation was possible. Nor was it suitable for the new 'high level languages', the crucial factor preventing any continuance of the work once ACE was closed down. Uttley certainly saw translation as a form of pattern recognition, but the translation work was entirely unconnected with the pattern recognition work in Autonomics. Uttley had never done any work on machine translation yet he encouraged Donald Davies to oversee the project—it was not a particular interest of Davies. Apart from Dennis Gabor the Executive Committee were supporters to the end. It ended after all not with the demise of the Executive Committee but because the ACE was switched off.

A crowd psychology was at work in encouraging the vogue for machine translation, but this in itself seems an inadequate explanation. There had been

[504] Add8353 H29. Uttley to Brigadier GH Hinds, Ministry of Supply, 30 October 1956

ample warnings from a variety of philosophical standpoints that the task was, in its ideal form, not possible, and the pragmatic justifications, outside the worlds of Government and Academia, simply did not add up. Did it spring from a desire to preserve something of the spirit of the Ratio Club, using Turing's own machine to do one of the tasks suggested in his original ACE report? Or could it have been some form of the idealism of 'removing language barriers'; did Uttley view these, like the old fashioned subject areas that separated sciences, as enclosing boundaries, a barrier to the best thought processes?

Chapter 6. *logon techne* or, What is a Program?

> To be still searching what we know not by what we know, still closing up truth to truth as we find it (for all her body is homogeneal and proportional), this is the golden rule in theology as well as in arithmetic, and makes up the best harmony in a church.
>
> Milton *Areopagitica*

There is a main line to the development of computing science—the confluence of mathematical and engineering interests. Academically mathematics has tended to dominate, it has the higher status, a position that was well established in the mediaeval universities and reinforced by Renaissance neo-Platonism. By the nineteenth-century a firm foundation in mathematics was seen as the mark of a mature science, thus physics became the exemplar of a 'complete' science and throughout the twentieth century motivated a remaking of biology in the manner of physics. The establishment of a 'profession' of engineering has likewise demanded the promotion of mathematical over empirical methods.

The development of computing at NPL might be seen as an exemplar of this progress, from the establishment of the Mathematics Division in the 1940s, through its close association with the Control Mechanisms and Electronics Division in the 1950s to an eventual merging as a computer science division in the mid 60s. Yet the brief flourishing of the Autonomics Division under Albert Uttley suggests other lines of development, 'a path less travelled'. Again, institutional history, Uttley's move to the University of Sussex, a notable centre for 'cognitive science' research, seems a clear pointer to another stream in the development of 'computing'. A line of development that retains a strong affinity with biology and, in some respects, is resistant to the reductive trend of modern biology. Yet to see but two—an engineering of mathematics and a biology of machines—is insufficient to fully account for the character of what, for lack of a better term, may be called 'computing science'.

In this final chapter I want to point the way toward a wider *prolepsis*, that is, a broader selection of models and analogies from which we might build a deeper understanding of this *scienza nouva*. But that requires you, the reader, to see things 'in the light of a different character'; yet I cannot hope to explain without mapping onto what is already familiar. So let us retrace the story so far.

By the early 1940s there was a well developed interest in brain physiology and function among biologists. This interest was not motivated primarily by philosophical (i.e. the 'mind-body problem') or psychological (the nature of intelligence) interests. Such psychological interest as there was concerned behaviour rather than mental states or processes.[505] The interest of these scientists was not cognition, that is internal mental events, abstract and introspective. It was grounded in the traditions of biology, the observation, classification and understanding of biota as objects in the world.

As a result of wartime redeployment—there being little demand for biologists *per se*—a significant number of biologists received training in electronics and worked on problems of machine control and the analysis of systematic or automated processes. One aspect of this was the development of operational research. But this was mathematical and statistical in its foundation and its subsequent development showed that characteristic trend of post-war science: toward the discrete, the logical, and a causal basis for explanation. Post-war the biologists returned to their trade, but with a new interest in behaviour. They had encountered at first hand the design, construction and operation of elaborate automatic machines. For the biologists the machine was a model of animal behaviour. If the best way to understand the working of something is to build it yourself then this was the essence of the insight acquired by these biologists. There was a new interest in process and information.

Engineers and mathematicians had also been involved in wartime projects to develop automatic machinery and had come into contact with biological ways of thinking. But the effect seems less marked, the physics tradition was strong and in the post-war world the rapid development of discrete state technologies ran counter to the biologists' approach.

Thus in the early 1950s there were two kinds of interest in computers and automation. One, the biological strand, approached automation as it did biota: as objects embedded in environment, exhibiting behaviour. The biologists wished to know how it worked; as a simplified model of real life it was potentially tractable. The other, primarily mathematical and physical approach, saw computers and automata as agents of ratiocination, representing the world symbolically. The process of problem solving was detached from that of sensation and action. Transition across this Cartesian divide—problems of deixis and reference, and a distinction between understanding and knowing—were largely ignored or reduced to a subsidiary instance of problem solving.

By the late 1950s development in computing had shifted the balance in favour of this 'computational' (i.e. discrete, symbolic, logically rational) view of computing and shaped the emerging discipline of computer science. The development of solid state devices and ferrite core memories reinforced the conception of the computer as a discrete and logical device for the solution of digitised problems. The development of programming languages engendered a new form of Cartesian dualism: the division of hardware and software.

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[505] It may thus properly be described as behaviourist, though without many of the connotations associated with the term in the partisan context of latter behaviourist v. cognitivist debates.

Computing was thus circumscribed by the academic and scientific, social and technological, *habitus* of those who had invented it. In particular there is a sense of a hierarchy of sciences, established in the nineteenth century, whereby the 'maturity' of a science is appraised by its likeness to physics. And the 'completeness' of physics is attested by how much it can be reduced by formalism and described in the language of mathematics. Biology in particular, in the course of the twentieth century, has been reformed into this mould. Organism and process have been reduced to code and computation. Yet mid-century is was possible to see the analogy turned about: the autonomic nervous system was a valid model of the computer: analogue rather that digital, embodied rather than algorithmic. Perhaps it was too soon; the unfulfilled promises of artificial intelligence have brought into question (if not into focus) what quality it is we evaluate and prize as 'intelligence'. Only now do we begin to see that biology has something to contribute to our understanding of computing—the world is its own best model.

Defining computing science, seeing what it really is, presents difficulties that are at present insoluble, it is too early to tell. In part this is simply a case of muddied waters; not only the fast pace of change but also a confusion of aims. As Edsger Dijkstra observed: "In their capacity as tool, computers will be but a ripple on the surface of our culture. In their capacity as intellectual challenge, they are without precedent in the cultural history of mankind."[506] At present it remains difficult to separate the tool from the challenge. The enormous commercial significance of computers as tools lends strength to the habit of apprehending them as industrial equipment, as machinery—hardware and software—suited to methods of mass production. As an intellectual challenge both underlying tradition and the particular origin of computers favour the view of computing as a mathematical science.

In the context of an examination of the changes in the nature and status of biology in the nineteenth century Joseph Caron defines a set of criteria by which the emergence of a distinct science may be identified.

> I take as criteria for the existence of a science called 'biology', (a) a distinct cognitive content for that science; (b) evidence of a debate in the scientific community about the existence of the science or about the postulates of the science; and (c) traces of the shaping and informing of the means of production and reproduction of a science known as 'biology' (its intitutionalization).[507]

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[506] EW Dijkstra. 'The Humble Programmer' ACM Turing Lecture 1972 [http://www.cs.utexas.edu/users/EWD/ewd03xx/EWD340.PDF 30 Sept 2002]

[507] Joseph A Caron. 'Biology' in the Life Sciences: a historiographical contribution. *History of Science* xxvi (1988) pp 223–268 [p224]

These criteria may usefully be applied to the case of computing science. In doing so there is evidence to confirm the arrival of a science of computing from the late 1950s. At least there is according to criteria b and c, about (a) a distinct cognitive content, the identification may be less certain.

For evidence of institutionalisation: in Britain we can note the creation of university courses at post-graduate level, and the formation of the British Computer Society.

'A proposal for the formation of a society for those interested in the use, design and construction of electronic computers and electronic data processing machines' was discussed at a specially convened meeting at the Institution of Electrical Engineers (IEE) on 7th May 1956.[508] It was chaired by D Hennessey of the National Research and Development Corporation and attended by representatives of academic, research, manufacturer, and computer using interests. Hennessey outlined a proposal for a "society of individuals with a common interest in the subject matter arranging to meet so as to interchange ideas so as to provide a medium for publication."[509] It was not his intention to propose an institution or qualifying body. Writing to Sir Edward Bullard the day after he reported

> Not altogether to my surprise, the house was divided. Those who know most about the subject are most enthusiastic for the Society, the vested interests behave like vested interests.[510]

The main opposition came from the IEE who favoured a body to coordinate the existing professional interests. In a draft letter (not sent) to Sir George Nelson, president of the Institution, Hennessey stresses "you should think of a society in terms of the Chemical Society rather than a professional institute like the Royal Institute of Chemistry or the Institution of Electrical Engineers."[511] A meeting of the NRDC's sub-committee on Electronic Computers, held 17th April 1956, had expressed "the desirability of not permitting control to pass into the hands of the Institution of Electrical Engineers."[512]

That same sub-committee also discussed 'courses in computer technology'. A total of eight institutions were, according to information obtained from the Ministry of Education, offering courses in 1956: Northampton Polytechnic, Coventry Technical College, Northern Polytechnic, Acton Technical College, Leeds College of Technology, Manchester College of Technology, Hatfield Technical College, and Cambridge University. The NRDC had also, since

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[508] NAHC 86/46/1 (A36.1/1/1) Letter ,D Hennessey to Sir Edward Bullard 8 May 1956 , with notes of meeting at IEE,.

[509] *ibid.*

[510] *ibid.*

[511] NAHC 86/46/1 (A36.1/1/1) Draft letter to Sir George Nelson, 11 April 1956

[512] NAHC NRDC 86/1 Minutes of Sub-committee on Electronic Computers 17 April 1956

1953, offered scholarships for "the training of programmers and computer engineers at Manchester University and the Mathematical Laboratory at Cambridge."[513] At Cambridge four scholarships were offered to support a one year course leading to a Diploma in Numerical Analysis and Automatic Computing. Not all these scholarships were taken up; it appears suitable candidates were able to obtain funding from other sources. A similar scheme operated at Manchester. When the subject of renewal of the arrangements came to be discussed in 1956 Professor Williams at Manchester requested that funding be extended to PhD and post-doctoral positions.

Instances of a debate about the existence of a science of computing began in the 1960s and continue.[514]

> Computer science is at once abstract and pragmatic. The focus on actual computers introduces the pragmatic content: our central questions are economic ones like the relations among speed, accuracy, and cost of proposed computation, and the hardware and software organisation required. The (often) better understood questions of existence and theoretical computability - however fundamental - remain in the background. On the other hand, the medium of computer science - information - is an abstract one. The meaning of symbols and numbers may change from application to application, either in mathematics or in computer science. Like mathematics, one goal of computer science is to create a basic structure in terms of inherently designed concepts that is independent of any particular application.[515]

In 1968 a conference at Garmisch had introduced the idea of 'software engineering'.[516] Was this part of the debate about the 'postulates of the science' or does it point to some uncertainty over 'cognitive content'? The notion of software engineering shows the influence of the formalists and mathematicians, for instance, as late as 1984 Hoare could write,

> Professional programming practice should be based upon underlying mathematical theories and follow the traditions of better-established engineering disciplines.

he continues,

> [...]we have only recently come to a realisation of the mathematical and logical basis of computer programming. We can now begin, however, to construct program specifications with the same accuracy that an engineer surveys a site for a bridge or road, and on this basis, we can now

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[513] *ibid.*

[514] It permeates many of the papers presented at a recent conference on the history of computing. See: Hashagen *et al. The History of Computing: software issues.* Springer 2002

[515] George E Forsythe. 'What To Do Till The Computer Scientist Comes' *American Mathematical Monthly* (75) 1968

[516] See various references in: Hashagen *et al. The History of Computing: software issues.* Springer 2002

> construct programs proved to meet their specification with as much certainty as the engineer's assurance that his bridge will not fall down.[517]

This assumption that mathematical proof underlies the engineers assurance is wishful thinking, real engineering occurs in context, it is not theory but how things actually behave that matters.[518] In order to discover the cognitive content we need a better understanding of what it is to do 'computing'.

To present, for example, program development as a species of gardening, is not to say that a 'software engineering' view is misguided but simply insufficient to comprehend computing in full.

> Business people are comfortable with the metaphor of building construction: it is more scientific than gardening, it's repeatable, there's a rigid reporting hierarchy for management, and so on. But we're not building skyscrapers—we aren't as constrained by the boundaries of physics and the real world.
>
> The gardening metaphor is much closer to the realities of software development. Perhaps a certain routine has grown too large, or is trying to accomplish too much—it needs to be split into two. Things that don't work out as planned need to be weeded or pruned.[519]

The tension between pragmatism and theory is also evident when Dijkstra says:

> When, for instance, computers became available as industrial products, it was a commercial imperative for the budding computer industry to disassociate its products as far as possible from any form of mathematics, the latter being viewed as the pinnacle of "user-unfriendliness."
>
> ... the question of how to get and keep the physical equipment more or less in working condition became in the early days the all overriding concern. As a result, the topic became (primarily in the USA) prematurely known as "computer science"—which, actually, is like referring to surgery as "knife science"—and it was firmly implanted in peoples minds that computing science is about machines and their peripheral equipment.[520]

It is in this comment on the inappropriateness of 'knife science' that we see that it is the cognitive content rather that methodology that is ultimately in dispute. Widespread adoption of glorified typewriters promoted as 'computing' further confuses the difference between using a device and 'working with' a tool. Also it is worth bringing to mind in this context the

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[517] C.A.R. Hoare. 'Programming: Sorcery or Science?' *IEE Software* April 1984

[518] See for example, E Ferguson. *Engineering and the Mind's Eye*.Cambridge 1994

[519] Andrew Hunt, David Thomas. *The Pragmatic Programmer: from journeyman to master*. Addison-Wesley 2000 p184

[520] E W Dijkstra. 'On a Cultural Gap' *The Mathematical Intelligencer* (vol. 8 nr 1) 1986

notion of an instrument: 'software is like a soap bubble, a computer is like a violin'[521]

Dijkstra sets out a dichotomy between 'tool' and 'intellectual challenge'. It is, of course, a variation on a familiar theme, the distinction between manual and mental labour, between knowledge of technique and symbolic capital, 'useful' and 'useless' knowledge. But what is meant by computer as tool? If we consider the computer in a particular role, say word-processing, to name the most obvious incarnation of the virtual machine, then it may not be unreasonable to consider this 'but a ripple on the surface of our culture.' Even so it could be countered that the availability and employment of tools has and will continue to be historically and socially significant. And the nature of the intellectual challenge is similarly undefined, but it may be inferred that Dijkstra has in mind something that falls within the reductive and mathematical traditions that characterise the notion of a science. Avoiding the temptation to separate the tool from the theory, it may be admitted that it is the *use* of a computer that provides the intellectual challenge. The computer is unique among tools in its nature as a virtual machine that crosses the established boundary between mechanism and mind, and thus well-established social and intellectual classes of mental and physical labour.

Computing then, appears to be a science that lacks a distinct cognitive context. How, if it is the study of algorithms, is it to be set aside from mathematics? If engineering—and there is much to be said for this view—what material, what physical restraint, does it work with. Can it be neatly distinguished from, say, electronic engineering? If it is design in the abstract, can it still claim to be a science, or should it be thought an art or craft?

Art, craft, or science: these are perhaps mostly matters of status in an academic and social hierarchy, of no real significance to the content or method. As to what is studied, if we set aside machinery as the business of engineers and algorithms as the province of mathematics, what is left but the software: intangible, yet certainly of economic and cultural significance, it is difficult to imagine a computing science that could ignore software altogether.

Software cannot be completely separated from algorithms or machinery of course. On the one hand some software such as embedded processors and real-time control systems are engineering—the physical constraints are clearly there. And that other boundary— between algorithm and program is difficult to draw. Nonetheless there is an ecological niche: programs are not pure abstraction, they have to work, come to life and make things happen. The key is to consider the program not as code—a set of instructions controlling a machine—but as text, an expression of ideas about process, an evocation of what is to happen. But first, consider some well-established concepts of

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[521] Marvin Minsky, quoted in Harold Abelson, Gerald Jay Sussman. *Structure and Interpretation of Computer Programs*. MIT 1996

programming language. Examples of which, as it happens, first began to flower around the time of that 1958 symposium.

At NPL in November 1958, an entire day was devoted to automatic programming: Grace Hopper talked of the present status and future trends in automatic programming, John Backus spoke on the properties and performance of Fortran. Automatic programming at this stage meant the automation of coding: linking and re-using fragments of code, building upon subroutines, using mnemonic codes for machine instructions. Principles that were more or less self-evident once a stored program computer was available.[522] The anticipated next stage was—

> With Flowmatic in hand to cope with the problem of coding, the next step must be to attack the problem of automatic *programming*, when the computer can be relied upon to specify the runs of a problem in an optimum manner, design the data layouts and in general behave like the 'electronic brain' it has so often been called. Ability to code rapidly and without errors should accelerate research programs in data retrieval, language translation and the many other problems urgently awaiting solution.[523]

We see here how programming is contrasted with coding in terms of computer operation; the tasks Hopper sets out might today be thought of as tasks of an operating system: automatic coding is clearly distinct from the idea of a programming *language*. "It is not really a language, it is actually another coding system."[524]

But even at this time the potential of programming languages was recognised. Later that day, in a discussion of the Manchester Autocode, Marvin Minsky noted the advantages of having a standardised language that could be accessible to people who had no particular interest in the detailed operations of a one-of-a-kind machine.

> ...it might be wise to design your machine (and programming language) so that it will be compatible with programmes written for one of the more widely available machines. One might as well take advantage of the fact that there has been no system of copyright protection for programmes and programming systems—they may be regarded as much like mathematical formulations—and to do things so that your laboratory will be able to use and contribute to the pools of exchangeable programmes.[525]

Minsky went on to note that advances in automatic programming might lead to a convenient form of language in which to publish program descriptions.

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[522] Hopper cites : MV Wilkes, DJ Wheeler, S Gill. *The Preparation of Programs for an Electronic Digital Computer*. Addison-Wesley 1951

[523] Grace Murray Hopper. 'Automatic Programming, present status and future trends' *MoTP58* pp191-2

[524] *ibid.* p198

[525] *MoTP58* p227

John Backus presented Fortran as such a language. It was an example of a 'synthetic machine' that could provide operations such as floating point arithmetic that were unavailable on the real machine. "These languages use mathematical notation and a variety of statements make the writing of most procedures of numerical computation a natural and concise process."[526] But these languages were still 'machine-like': mathematical notation had to be traded against the engineers' desire for efficiency. Algol, the language that gave priority to the notation of algorithms, was not mentioned at Teddington despite being first published in that year.[527]

Another language that put the expression of program ideas before efficiency, was Lisp. John McCarthy did not talk of Lisp at Teddington but the form of the ideas it was invented to express is evident in his presentation of *Programs with Common Sense*. "The *advice taker* is a proposed program for solving problems by manipulating sentences in formal languages"[528] Here we have not so much a language as a program for manipulating language. The idea of a 'synthetic machine' has begun to merge with the concept of a language. What is more it is not—despite the attachment to formalism—an essentially static language, i.e. a medium in which to publish algorithms, but an expression of process: a means to solve problems. Lisp was intended as another theoreticians' language but, as Paul Graham suggests, almost by accident it came to be something else:

> It's because Lisp was not really designed to be a programming language, at least not in the sense we mean today. What we mean by a programming language is something we use to tell a computer what to do. McCarthy did eventually intend to develop a programming language in this sense, but the Lisp that we actually ended up with was based on something separate that he did as a theoretical exercise—an effort to define a more convenient alternative to the Turing Machine.
>
> As McCarthy said later,[529]
>
> > Another way to show that Lisp was neater than Turing machines was to write a universal Lisp function and show that it is briefer and more comprehensible than the description of a universal Turing machine. This was the Lisp function eval..., which computes the value of a Lisp

[526] J W Backus. 'Properties and Performance of FORTRAN Systems I and II' *MoTP58*. p234

[527] Perhaps because Mike Woodger, a member of the Mathematics (not CME) Division at NPL and closely involved with the specification of Algol, was not at the Symposium but abroad on business connected with that language specification. (The published proceedings that list him as attending the symposium are in error, notebooks now in the Science Museum Library show that he was on a trip to Germany and Switzerland at the time.)

[528] John McCarthy. 'Programs with Common Sense' *MoTP58*. p77

[529] Paul Graham. 'Revenge of the Nerds' (lecture at the International ICAD User's Group conference in May 2002) <http://paulgraham.com/icad.html> [08 Sept 2002]

> expression.... Writing eval required inventing a notation representing Lisp functions as Lisp data, and such a notation was devised for the purposes of the paper with no thought that it would be used to express Lisp programs in practice.[530]

These were first steps toward 'high-level languages' but the physical machine was never far from mind. In the years that have followed, the underlying machine has become ever more remote. Even languages such as C, often regarded as putting the programmer 'close to the machine,' only provide an illusion of a hardware interface. In modern computers the layers of abstraction—of hardware that is itself designed by computer program, of microcode and firmware, hardware abstraction layers and interfaces, drivers and devices, libraries of code and virtual devices—present, even at the lowest level, only a pretence of working with hardware, a virtual machine.

Yet this virtual machine tends not to feature in the typology of programming languages. Programming languages may be described as high or low level, procedural, functional, object-oriented, etc. They may be designed for a particular domain or market, for varying levels of skill, but—except in regard to the *implementation* of a language, Java being the most prominent example—the virtual machine goes unregarded. Programmers work with virtual machines, but tend, or pretend, not to notice that they do so.

By turning our attention to the virtual machine, identifying certain variants and thereby typifying certain programming languages by the form of virtual machine they address, we not only detect once again the tension between the mathematical and engineering aspects of computing but also see how we might identify that elusive 'distinct cognitive content'.

Consider what we might mean by 'virtual machine'. There is first, the virtual machine as a mathematical abstraction, specifying components, their relation and state transitions. Such a machine may be the foundation for the proof of an algorithm or specification. The Turing Machine is the obvious instance. Though, as we have shown in a previous chapter, such a machine may, for didactic purposes, be visualisable and appeal to a 'common sense' of what is possible, it is in essence a formal and Platonic construction. Implementation, making a real physical machine in its image, is irrelevant.

Though Turing's version is the most obvious, we can detect its influence elsewhere. The original intentions of the designers of Algol, for instance, to have a language in which to discuss algorithms, implies a virtual machine of this type. If, the account quoted above of the original intent of Lisp is correct then that too is a language of the pure abstract machine; the fact that Lisp might be a useful practical tool is but incidental.

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[530] Quoted by Graham from: John McCarthy. 'The implementation of LISP' <http://www-formal.stanford.edu/jmc/history/lisp/node3.html> [08 Sept 2002]

The characterisation of a program and its language as an invocation of an essentially abstract machine may apply even though it is closely modelled on an actual machine. The MIX machine, devised by Donald Knuth as a vehicle in which to express algorithmic analyses in his *Art of Computer Programming*, is a clear example. Introducing MMIX, its replacement, in the 1990s Knuth justified his use of an assembly level code addressed to an imaginary machine rather than simply presenting the work in a high level language. Apart from the necessity of a low level description of fundamentally low level machine processes, a comparison of performance was impossible without an accurate representation of the characteristics of a putative machine. Moreover, languages go in and out of fashion.

> In the 1960s I would probably have chosen Algol W; in the 1970s, I would then have had to rewrite my books using Pascal; in the 1980s, I would surely have changed everything to C; in the 1990s, I would have had to switch to C++ and then to Java. In the 2000s, yet another language will no doubt be *de rigueur* I cannot afford to rewrite my books as languages go in and out of fashion; languages aren't the point of my books, the point is rather what you can do in your favourite language. My books focus on timeless truths.[531]

Thus the assembly language of MMIX is a mathematical language, a means of expressing universal and timeless truth.

By contrast an engineering centric view is more pragmatic, never entirely separate from details of implementation. The decomposition of a system into standard components and interfaces, prefabrication and sub-contracting of work, all have their equivalent in software as in any other form of engineering. Layers of abstraction, libraries of code, the chunking of processes, simplify design by hiding the detail of hardware implementation.. They make the process of software construction manageable. The virtual machine in this case is a design specification.

> In the mean time a pattern emerged for the co-operation between me and my hardware colleagues [...]. After the functional specification of the next machine had been written down (usually by me), that document served as a kind of contract between us: it told them what machine to design and construct, while I knew what I could count upon while writing all the basic software for the machine. The target of this division of labour was that my programs would be ready by the time the construction of the machine had been completed.
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> [...] I found it perfectly normal to program for not yet existing machines. As a by-product it became firmly ingrained in my mind that I programmed for the abstract machine as specified in the original document, and not for the actual piece of hardware: the original

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[531] Donald E Knuth. *The Art of Computer Programming*, Preface to First Fascicle. May 1999

> document was a description but a prescription, and in the case of a discrepancy not the text but the actual hardware would be at fault.[532]

Whereas in the case of the algorithmic virtual machine the existence of a real machine need be no more than plausible as an implementation of its mathematical premises, in the engineering centric view there is, at least implicit, an assumption that a physical implementation is possible. But, as layers of abstraction increase (i.e. as virtual machines are built upon virtual machines), the programmer can no longer presume to know in detail how the program text is transformed into hardware action. The relation between virtual machine at the higher level and the substrate of hardware may be considered supervenient. That is, there is a consistent relation between the program code *as represented in the machine* and the observed action of the hardware[533] yet, except in the simplest of cases, no certain understanding of the process in between.

Similarly, it may be argued that between the programmer's conscious intention and the production of a program *as a text* the relation is also one of supervenience.[534] But while it is, at least in principle, possible to trace and verify all the steps between the code and the physical activity of a running program, the relation between human consciousness and action remains the 'hard problem' of cognitive science. As Brian Cantwell Smith has said:

> Computational experience raises a cautionary flag with respect to too great a materialist enthusiasm about cognition, or intentional phenomena more generally. [...] with respect to computer systems *we already know the answers to all the physiological questions* (we have the source code and wiring diagram), without that necessarily leading to any serious understanding, at the right explanatory level, of "what the program is doing".[535]

The programmer designs a virtual machine and is obliged to work with a virtual machine, to focus attention upon components and processes designed, referenced, and used by the programmer on the assumption that they are as solidly real as the hardware itself. The programmers vision is focused on the objects found within the program, the final effect in the world depends upon trust in the implementation by the machine.

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[532] Edsger W Dijkstra 'What led to "Notes on Structured Programming" EWD1308-0 [http://www.cs.utexas.edu/users/EWD/ 19 January 2003] Dijkstra goes on to observe that this, seemingly intuitive, practice was not universal: "I read an American article on why software was always late; I remember being very amazed when I read that limited availability of the hardware was the main cause..."

[533] This does assume that the operation of the machine is consistent and the program in some broad sense 'correct'.

[534] I do not believe an invocation of 'supervenience' explains much: a secular and technical terminological fix to paper over a 'god of the gaps' or a shortfall of eliminative materialism.

[535] Brian Cantwell Smith. *On the Origin of Objects*. MIT 1996 p148

There is a discontinuity between the program as written (something perceived by its maker) and the code *in situ* (something executed by the machine). An interface between symbols and semantics. This disjoint relation may also give some insight into the relation between the algorist's virtual machine—an idealisation, on which to base a proof—and the software engineer's virtual machine—a simplification, by which a design may be rendered with greater clarity. The virtual machines of the program code 'stack up' upon the underlying physical implementation, so even at the highest level these abstractions may be presumed to have a causal relation to the hardware. But the algorithm does not belong to this chain, but rather floats above, fully abstract but not supervenient. The relation is indirect, mediated by the program's designer or even user, who can make a mapping between algorithm and program as written. It is something like the computational models of cognitive science, an analogue that explains observed behaviour *as if* it were the consequence of a hypothesised mechanism.

Now consider the case of the machine user. In some cases the relation is best considered as a direct one between user and hardware. Such might be the case of an embedded device, say the engine management system of a car. A program that intervenes between the accelerator pedal and the engine is no more nor less a mystery to the driver than a purely mechanical linkage to a carburettor would be. But most deliberative and conscious use of a computer is of a different order; the user of a word processor, web browser or spreadsheet interacts with a virtual machine. Without software a computer is a dead thing, a program transforms it into a particular machine, a tool designed for a particular task. (Many programs may share the same machine and the user may combine the use of several such tools.) In this case the user, like the programmer, interacts with a virtual machine. And, as with the programmer, there is some disjunction of the causal chain: between the user's model of the behaviour of the tool, and the machine processes that respond to user input.

How should we characterise the relation between the programmer's and the user's virtual machine? One answer is to regard them as one and the same: both tangle, ultimately, with the same physical machine. The virtual machine may be envisaged as an artefact that will actually perform the program's function. Thus, for example, a 'word processor' is the character assumed by a computer that performs a word-processing program.

> My first remark is that, although the programmers activity ends when he has constructed a correct program, the process taking place under control of his program is the true subject matter of his activity, for it is the process that has to accomplish the desired effect; it is this process that in its dynamic behaviour has to satisfy the desired specification. Yet, once the program has been made, the "making" of the corresponding process is delegated to the machine.[536]

[536] Edsger W Dijkstra. 'Goto Statement Considered Harmful' *Communications of the ACM* 11(3) March 1968 pp 147-148

The program in this sense is like an engineering drawing, except that there is no distinct 'manufacturing' or 'construction' phase—unless, that is we think of compilation in this light. The actual machine (a virtual machine) and its design or plan merge into one artefact. To have *written* a program is also, at once, to *build* a virtual machine.

But there can, I believe, be an alternative view: that although the layers of abstraction, virtual machinery, of designer and user do ultimately merge upon the same running code, the process is a gradual one. There is no need to go right down to the metal (or silicon). An interaction between users and designers of such a virtual machine occurs at a virtual machine level. There is, effectively, a communication between programmer and user mediated by virtual machines. Moreover, this relation may be characterised as a literary one—between author and reader, between a text and its context.

Programs are *written*, they have a representation in symbols just as words may be printed on a page. But a text is more than paper and ink, it is also something immaterial, phenomenological, virtual. We may 'suspend disbelief' and respond to the characters and events of a novel as readily as to the history constantly enacted before us; there is no firm boundary between imagination and memory. Programs also build virtual worlds from text, create an imagined space in which writer and audience interact. Can they be considered a form of literature?

What do I mean by literature? An awareness of those qualities and techniques in the use of language that enable the imagined, the fictive, to stand in our consciousness equal in status with what is physical and real. Knowingly using words to conjure up other worlds, other lives. Literature is not a superficial aesthetic dressing on a factual text, nor some moral virtue granted to certain kinds of writing.[537] To discuss the literary qualities of a text is to consider how a particular kind of 'virtual machine' is made to do its work. It is in this context that the program may be appreciated as a literary artefact, the programmer as author and impresario, the computer an actor performing a part that is written. What matters is not the book in hand, not the staging, but what goes on between reader and writer, an interpretation of a shared text.

In discussing programming we tend to disregard this 'literary' level. Either the virtual machine is eliminated—we pretend it to be the real thing (such is the case when C programmers claim to be 'close to the machine'), or we go right down to the silicon—supervenience becomes eliminative materialism.

The history of programming since the 1960 can be viewed so as to show a gathering awareness of the literary character of the program. It is in this body

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[537] For an insight into the influence of classicism, and the tension between technical and moral values in the criticism of literature see: EMW Tillyard. *The Muse Unchained: an intimate account of the revolution in English studies at Cambridge*. Bowes & Bowes 1958

of 'program literature' that, I suggest, we find a distinct content which may set 'computing' aside from both mathematics and engineering. Not, perhaps something that would readily earn the epithet of 'a science' but certainly an 'art of computer programming'. That is something akin to the *logon techne* of fifth century Athens—the art of using words and language.

To give an account of this developing perception let us start with the author of *The Art of Computer Programming*, who offers the unique interest of being both representative of the strong mathematical tradition in computing science and yet one whose writing is marked by a strong sense of literary qualities.

Knuth's work may in time come to epitomise an early transitional phase in the development of the art of computing. As the titles chosen for some of his works (*The Art of Computer Programming*, *Literate Programming*) and the decision to defer work on his magnum opus until a program to typeset the text could be completed testify, Knuth brings a literary sensibility to his work. Yet that work lies firmly in the algorithmic tradition. His coding, particularly in the *Art of Computer Programming* which dates from the late 60s, is both mathematically abstract and very close to the machine. The commitment to program correctness and structure in the TEX and METAFONT programs, admirable as it is, places that work in the context of a1970s debate over programming style. In *Literate Programming*, which is largely an account of Knuth's development of those programs, literacy is essentially a matter of fully accounting for design decision through extensive documentation. It is an approach that reflects a moment when consciousness of programming style and the potential of computer typography collude but the possibilities subsequently opened by pervasive online access have yet to be realised. As he notes in the log he kept while debugging TEX: "**14 Mar 1978**. Came in evening after sleeping most of day, to get computer at better time. Some day we will have personal computers and will live more normally."[538]

In 'Computer Programming as an Art', a lecture he gave in 1974[539] Knuth talks of the seven arts (or sciences) that comprised the curriculum of the medieval university; "at least three of the original seven liberal arts are important components of computer science."[540] Knuth does not state which three, though we might guess that among the *trivium* logic would be certain

[538] *op cit* p295 [I am writing this thesis chapter at a personal computer, it is 6am on 7 May 2001, having been up all night. Is there some fundamental reason why programmers/writers work the graveyard shift? Perhaps it is the need to 'preserve state', total immersion in a creative process?]

[539] Donald E Knuth. 'Computer Programming as an Art' *Turing Award Lecture* presented at ACM Annual Conference, San Diego, 11 November 1974. reprinted in *Literate Programming*

[540] *Literate Programming* p3. The lower division was the *trivium*: grammar, rhetoric, logic; the higher *quadrivium* comprised: arithmetic, geometry, music, and astronomy.

of inclusion, and from the *quadrivium* arithmetic. Students of programming languages could make a strong case for grammar, Knuth, among others, has written of finding analogies between music and programming. Arithmetic and astronomy would find a place in the mathematical heritage of computing. But it follows from my argument here that a place should be found for what at first sight appears the least likely of those arts—rhetoric.

Knuth's essay 'Literate Programming' describes the results of ten years reflection upon the craft of programming. In 1974 'Structured Programming with **go to** Statements' concluded with some thoughts on the languages of the future: higher levels of abstraction, program manipulation systems for debugging and analysis, small modules that combine data definition and processing. A common thread was the importance of structure, programs needed to be compositions, orchestrated like a symphony. By 1984, this had led to the creation of WEB, a system for the writing of literate programs demonstrated by the publication in book form of the TEX and METAFONT programs.

TEX had been designed by Knuth originally for one purpose; to enable him to employ the full capabilities of computer typesetting in the production of his projected seven volume *Art of Computer Programming*. WEB applied that new technological capability to the writing of program code itself; programs were to be written with the human reader in mind as well as the coding of machine instructions.

It consisted of two related programs that transformed the programmers source text into both program code for the machine and a readable description of the program. The creation of source code and documentation proceeded as a unified process. The output of TANGLE.WEB was a program (in Pascal though it might be another language) formatted for the machine, without any of the indentation etc that had become customary as a means of promoting readability for the programmer. The programmers version, derived from the same source, was the product of the WEAVE.WEB program which produced a TEX file for printing. This 'literate' text was arranged in the order most convenient for exposition, the actual Pascal code appearing like the lemma of a mathematical text, to demonstrate or assert the correctness of the narrative account.

A pertinent criticism of the focus and style of Knuth's literate program approach was provided by Doug McIlroy, who was invited to be a critic of this new literature in *Communications of the ACM* in 1986. Knuth's example program was to solve the problem:

> Given a text file and an integer *k*, print the *k* most common words in the file (and the number of their occurrences) in decreasing frequency.[541]

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[541] *Literate Programming* p151

The solution, described by Knuth over six pages, was indeed a convincing demonstration of WEB and fascinating for its data structure, but for all its elegance it was not, said McIlroy, a good engineering solution. In place of Knuth's monolithic program, tuned to a specific problem, a UNIX shell script could accomplish the same task. Six short lines of code would suffice to process a ten thousand word file in thirty seconds.

> Knuth has shown us here how to program intelligibly, but not wisely. I buy the discipline. I do not buy the result. He has fashioned a sort of industrial-strength Fabergé egg—intricate, wonderfully worked, refined beyond all ordinary desires, a museum piece from the start.[542]

Some vestige of this approach can be seen today in the Javadoc system, but on the whole programmers have not embraced the full potential of 'pretty printing'. In part this may be a reluctance of programmers to write more than is functionally required. But two developments since 1984 have probably also had an influence. Firstly languages have achieved higher levels of abstraction. In 1974 Knuth was still concerned with program efficiency writing "the optimizing compiler would have to be so complicated (much more than anything we have now) that it will in fact be unreliable".[543] But today's processors are too complex for any sort of hand tuning of assembly level code. Since the 1980s processor instruction sets (in particular RISC architectures) have been designed to suit the compiler not the human coder. Knuth's suggested interactive program manipulation systems are used but at a much higher level.

A second development that gathered pace from 1980s has also profoundly changed the notion of what might constitute literate programming. Interactive programming has diminished the importance of a printed program text. Coupled with the development of higher level languages this has created a new kind of literacy. Just as the structured programming of the 1970s tended to make the flow chart as a technique of program design obsolete, so interactive editing and a growing familiarity with high level programming concepts has tended to create a generation of programmers with a greater fluency in writing and reading code.

Knuth developed his system having perceived the difficulties of comprehension created by programs that were primarily addressed to the computing machine rather than a human programmer. Both problem and solution stem from an era when the programming task could be taken to be one of getting the machine to do what it was told. (Or at least, as is often noted, 'do what I mean'.) Knuth's programs are grounded in an era of batch processing, the essentially abstract manipulation of a body of data that is created offline. That task has not gone away, but there is now another, more significant, dimension to programming. The program today is most likely an

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[542] *Literate Programming* p174

[543] Structured programming with go to Statements *op cit* p54

interactive program. The program is not addressed *to* the machine but *through* the machine to an interactive user. Thus the programmer creates an object, abstract and intangible, but not ideal in the manner of mathematics. Today's programs do not construct theorems or demonstrate proofs, they are tools and artefacts that are used by people.

This requires a change in the psychology of programming, something I believe is captured, however imperfectly, by thinking in the mode of rhetoric as much as grammar and logic. This view of interactive programming is not simply a matter of taking a user's eye view (even if that user is only the programmer), the programmer must constantly switch from that outside view to the interior view of the code as a working mechanism. The interactive program must necessarily act in unexpected ways, the manner of its use is outside the programmers control. Like any work of art it has a span outside that envisaged by its creator. The audience response cannot be predicted. A new style of programming (and computing education), needs to address this rhetorical art. That is, not the abstract manipulation of data, but the use of a new form of language, a new relation of text and actions.

Knuth characterises science, as a systematic arrangement of knowledge into generalised 'laws'. Science, "saves us from the need to think things through in each individual case; we can turn our thoughts to higher-level concepts."[544]

> Science is knowledge that we understand so well that we can teach it to a computer; and if we don't fully understand something, it is an art to deal with it. Since the notion of an algorithm or computer program provides us with an extremely useful test for the depth of our knowledge about any given subject, the process of going from art to science means that we learn how to automate something.[545]

Note here how algorithm and computer program are regarded as synonymous. Also, implicitly, there is a tension between 'depth of knowledge' and 'higher-level concepts'. The transition from art to science, it seems, might be a case of knowing 'more and more about less and less'. It may be instructive to quote here the observation of the economist JM Keynes:

> The economist, he told his Cambridge students in 1933, always knows more than he can say. "When you adopt perfectly precise language", he went on, "you are trying to express yourself for the benefit of those who are incapable of thought." Keynes strictures were in vain. Since his day, economics has become so formal, or mathematicised, that even economists can no longer understand what they are saying to each other, still less to the educated public. Keynes appeal as an economist has always been to those who prefer fertile thinking to elegant proofs.[546]

[544] *Literate Programming* p4

[545] *ibid*

[546] Robert Skidelsky. 'Skidelsky on Keynes' *The Economist*. 25 November 2000

An academic perspective is apparent not just in the assumption that most programs are short, which Knuth acknowledges, but also his remark that most programs are ephemeral.

> Most programs are probably only run once; and I suppose in such cases we needn't be too fussy about even the structure, much less the efficiency, as long as we are happy with the answers.[547]

We are reminded here of McIlroy's criticism of the Common Words program. In that case Knuth's purpose was academic, the exposition of literate programming, and an elaborate exposition of what might have been accomplished with a six line UNIX script justified by the context. This academic view tends to obscure the nature of programs as *artefacts used by people*. Instead they naturally become theorems to be documented like mathematical texts: an informal mode exposition reinforced with lemma.

> Let us change our traditional attitude to the construction of programs. Instead of imagining that our main task is to instruct a *computer* what to do, let us concentrate rather on explaining to *human beings* what we want the computer to do.[548]

This is halfway there, though directed at human beings it is still a concern with telling a computer what to do. The next step is not literate programs but a literature of programs. That itself requires two elements: the first, which has reasserted its significance in recent years with the free software movement, is the exchange and reading (by people not machines) of programs. Knuth's TEX and his method of exposition (WEB) have had a significant influence in this respect. But a literature is not primarily a body of text, a literary culture not necessarily a literate one. Literature creates an imaginative world, one that exists in the imagination or, in our case, is realised by a computer[549]. In this sense the computer becomes a *rhapsode*, it invokes that other world for a non-literate audience. A literature of programs thus requires a criticism and appreciation that goes beyond the analysis of algorithms and the concerns of engineering or business optimisation. It requires an aesthetic of programs as human creations for human use. It is in that sense that the art of programming needs to be a *rhetorike techne*.

Knuth's 'literate programming' is in its essence the method of biblical exegesis: commentary upon a text that is in a sense absolute and timeless in its meaning. He assumes his program to be a Platonic object whose meaning

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[547] Knuth. 'Structured Programming with go to Statements' in *Literate Programming* p76

[548] *Literate Programming* p99

[549] I should make it clear hear that I *do not* have in mind the uses of computers for entertainment, playing of games etc. *Any* program is the invocation of a world that does not materially exist.

would be retained whatever the language of its expression. Contrast this to the idea of language as essentially fluid and untranslatable.

Knuth is not alone in seeing literary qualities in code.

> Programming is, like any form of writing, more often than not experimental. One programs, just as one writes, not because one understands, but in order to come to understand. Programming is an act of design. To write a program is to legislate the laws for a world one has first to create in imagination. Only very rarely does any designer, be he an architect, a novelist, or whatever, have so coherent a picture of the world emergent in his imagination that he can compose its laws without criticism from that world itself.
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> [...]
>
> The difficulties that ensue are no more rooted in syntactic rigidities than is say, the difficulty of writing a good sonnet rooted in the rigid form demanded by that class of poem. To write a good sonnet or a good program, one must know what one wants to say.
>
> [...]
>
> It is in fact very hard to explain anything in terms of a primitive vocabulary that has nothing whatever to do with that which has to be explained. Yet that is precisely what most programs attempt to do. A computer's successful performance is often taken as evidence that it or its programmer understand a theory of its performance. Such an inference is unnecessary and, more often than not, quite mistaken. The relationship between understanding and writing thus remains as problematical for computer programming as it has always been for writing in any other form.[550]

And again,

> The programmer, like the poet, works only slightly removed from pure thought-stuff. He builds his castles in the air, from air, creating by exertion of the imagination. Few media of creation are so flexible, so easy to polish and rework, so readily capable of realizing grand conceptual structures [...]
>
> Yet the program construct, unlike the poet's words, is real in the sense that it moves and works, producing visible outputs separate from the construct itself. It prints results, draws pictures, produces sounds, moves arms. The magic of myth and legend has come true in our time. One types the correct incantation on a keyboard, and a display screen comes to life, showing things that never were nor could be.[551]

And also Abelson and Sussman,

> First, we want to establish the idea that a computer language is not just a way of getting a computer to perform operations but rather that it is a novel formal medium for expressing ideas about methodology. Thus

[550] Joseph Weizenbaum. *Computer Power & Human Reason*. Penguin 1976

[551] Frederic P. Brooks. *The Mythical Man-Month*. Addison-Wesley 1995

> programs must be written for people to read, and only incidentally for computers to execute.
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> the essential material [...] the techniques used to control the intellectual complexity of large software systems.[552]

Gerald Jay Sussman, speaking to an audience at HP Laboratories, Bristol on 26 July 2000, drew a comparison between programmers and "poets and composers and performers". He suggested that the experience of programming had taught us something new about problem solving; that ideas of recursion, search, analysis and debugging, the use of rule systems had advanced as a consequence of the experience of computing. Yet this business of "problem solving by debugging almost-right plans" was something shared by the non-programmers. He thus presented the paradox of something new-found which yet, it would seem, predated the computer. Citing certain authors—Edgar Allen Poe, Baudelaire—who had seen their craft as analogous to that of the mathematician and mechanic, he took what is, in essence, the mathematicians perspective: computing had yet to achieve formalism, it was awaiting its Euclid.

In a recent and wide ranging essay Richard Gabriel considers the future of software development:

> Founding assumptions can echo for decades. In software we have suffered especially from such echoes. From the earliest days of software—the mid-1950s—a particular view has prevailed of what software is and why it is created that now or soon will make our work intolerable.[553]

Computing evolved with the assumption that its uses were confined to scientific and engineering computation, its theory founded upon mathematical constructs in which equivalence and universality were considered "the touchstone of expressiveness". Even today, the assumptions of numerical computation are paramount. "All significant programming languages are expressively, conceptually, and aesthetically equivalent to Fortran and assembly language." The methods of software development are equally chained to early models, everything brought together and linked by a "mythical belief in master planning". "Such beliefs were rooted in an elementary-school-level fiction that great masterpieces were planned, or arose as a by-product of physicists shovelling menial and rote coding tasks to their inferiors in the computing department." As a result software became

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[552] Harold Abelson, Gerald Jay Sussman. *Structure and Interpretation of Computer Programs* MIT 1980 preface

[553] Richard P. Gabriel, Ron Goldman. 'Mob Software: The Erotic Life of Code' ACM Conference on Object-Oriented Programming, Systems, Languages, and Applications on October 19, 2000, Minneapolis, Minnesota, USA. <http://www.dreamsongs.com/MobSoftware.html> [20 August 2001]

See also Richard P Gabriel. 'Master of Fine Arts in Software' <http://www.dreamsongs.com/MFASoftware.html> [23 August 2001] merchandise; there was no body of software as literature—“code as code is looked down on”. In such circumstances, its craft and artistry hidden from view, the education not only of the programmer but of the computer user at large suffers.

> When software became merchandise, the opportunity vanished of teaching software development as craft and artistry. The literature becomes frozen. It’s extremely rare today to stumble across someone who is familiar with the same source code as you are. If all remnants of literature disappeared, you’d expect that eventually all respect for it—as an art form, as a craft, as an activity worthy of human attention—would disappear. And so we’ve seen with software: the focus is on architecture, specifications, design documents, and graphical design languages. Code as code is looked down on : the lowest rank in the software development chain is the ‘coder’—right alongside QA drone and doc writer.[554]

The autonomic machine is in equilibrium with its environment, the ‘program’ is not abstracted away. In contrast the digital computer encourages a Cartesian dualism of hardware and software. Building programs (virtual machines) is easier than making robots, as was implicitly recognised by the advance of digital over analogue computation. This abstraction of program from machine tends to mislead us as to how the machine works. We are tempted to see thinking in abstract, Platonist terms as analysing sense data rather than awareness of objects in the world. Computing has developed in this dualist and analytical tradition. So that computing as the science (art) of programming is not mechanisation of thought processes. But in as much as the program allows us to create a virtual machine, we can return to the business of mechanisation, of building machines that think. And we can understand this process of programming, making virtual machines, as one of describing, and thereby creating, objects of our imagination. *Writing* programs, for the reason all writers write: “to create a world as real as but different from the one that actually exists.”

But are skills with natural language transferable to the realm of programming—one form of literacy transmuted to another? I do not believe that is so in any simple and direct sense. What I suggest here is that programs evoke and describe virtual machines and their processes in a manner analogous to the way literary language evokes and describes ‘worlds as real as but different to the one that exists’ in our ordinary experience of life. The skill to write a program is not the same as that required to write a novel (though I think it may be of the same order); it would undoubtedly take a rare person to do both equally well. But writing programs *is creating* a virtual machine, it is ‘pure thought stuff’, it us not a science of pure abstraction in the manner of mathematics, nor does controlling machinery accurately capture what it is like to create a significant program. The program is a virtual machine, distinct

[554] Richard & Goldman. *op cit*

from the physical computer required to instantiate it, yet that need to instantiate is significant, it is what makes a program different from an algorithm, a purely abstract mathematical form. Like successful storytellers, whose creations may suspend disbelief but cannot disregard it entirely, programmers are not free to dream anything, their creations have to work on a real (and imperfect) machine.[555]

The first of computing's intellectual challenges then, is to recognise its own boundaries, to become, not perhaps self-conscious, but to have an independent identity. A self-image has to grow beyond emulation. The story of computing so far has been one of following prevailing social and intellectual models, most notably those of mathematics and engineering. But, as we have shown, there were, and are, other patterns and moulds. If computing has a significant part to play in cultural history, it must surely become more human centred, that is humanist; weaving into its rational heritance, the biological and the imaginative capacities of the creatures that created it.

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[555] Rodney Brooks (speaking at HP Labs Bristol 15 May 2002) noted that we are gradually becoming our machines. He remarked that when first learning to drive a car was something 'other', a machine outside that had to be controlled, with familiarity we become embedded within it, the tool is an extension of ourselves. This can also be observed with the users of artificial limbs.

Chapter 6.9.0 *coda*

Things have contexts, but only a person has perspectives.

Wheelwright, *Metaphor and Reality*. 1962

This work began with curiosity, to bring to light some neglected perspectives in the study of computing. The unenclosed territory, false starts and misplaced hopes, the road less travelled, prospects that remain clouded. It is to be hoped that some of these byways may prove of interest and be reopened, that some new knowledge may be found by looking again by a different light. Some things have only been touched upon here, much we have left undone.

I did not set out to give an account of Autonomics at NPL and was unaware that the trail would lead to the Albert Uttley or the Ratio Club. They remain peripheral to my main interest which is to understand better what the art of computing, as an intellectual and cultural phenomenon, is, or may be. Yet curiosity engenders yet more curiosity. The figure of Uttley remains in the shadows—biographical detail is elusive. So often my enquiries have revealed only that little is known.

The result is that I can provide a probable context but the landscape is unpopulated. I can tell you of debate about mathematics education in the early twentieth century but cannot be certain if, possessing of a mathematics degree and a teaching qualification, Uttley ever taught mathematics. Also evident is the difficult presentation of his work. I take the opinion of Jack Copeland as confirmation that the impenetrability of *Information Transmission in the Nervous System* is not solely due to my ignorance of the subject:

> The trouble with Uttley's work is that few people who read it can understand it! Had he taken the trouble to write more accessibly he would, I am sure, have had much more influence than he did.[556]

The account of Autonomics is also incomplete. In the institutional setting the relation with the Mathematics Division remains unexplored. Of the research programme of Autonomics, only machine translation has received attention here. A fuller account could be given of other major projects: of pattern recognition and automatic control. And, of course, there is insufficient account here of the biological work: an animal house was indeed a strange thing to find in a physics laboratory.

Yet the omission most regretted is that I have been unable to give a full expression here to the idea of the program, not only as the essential subject matter of computing, but to present it convincingly as a craft of literary creation. The problem is not only of space and time but primarily of finding an appropriate 'light of different character' by which the idea may be fully articulated. A model or analogy, as Craik noted, is chosen because it is in some manner more convenient than the thing modelled. One aspect of this

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[556] Jack Copeland. personal communication [email 25 March 1999]

convenience is that metaphor facilitates a transition from the greater to the less familiar. To explain the art of computing as a *logon techne* however demands of the reader a double journey into unfamiliar territories. Hence what follows is an alternative closure to this work, a recapitulation in another mode.

It is always difficult to perceive something new except upon the model of what we already know. The result is a partial and incomplete understanding that can only be rectified by experience. Our present perception of computing remains strongly influenced by its origins as a mathematicians' machine. A notion of mathematics as the arbiter of what constitutes a 'science' has long been ascendant. As a result even a view of computing from a more empirical standpoint has been conditioned by the aspirations of those in engineering disciplines to acquire the status accorded to a science. From the beginning there was an interest in computing machinery that took a different perspective: that of biology—autonomic mechanisms to be understood by observation rather than the proving of theorems. But the prevailing trend in biology in the twentieth century tended in the opposite direction: toward a discrete and computational interpretation at a molecular level. The fate of the Autonomics Division at NPL provides a particular instance of these tensions. The creation of Autonomics coincides with a critical point in the history of computing, the time when a distinct discipline of 'computing science' formed. The field thus enclosed represented the dominant mathematical and engineering interests, the distinctly biological view surviving only peripherally in sub-fields such as cognitive science. But it was at about that time that a truly distinctive and unique feature of computing began to emerge: software. Software is an amorphous concept, being both intangible yet an engineered 'product'. It has been argued here that insufficient attention has been paid to software as a form of literature. In this sense software is about programs and their programmers. The invisibility of the literary dimension may be attributed in some measure to the views on the nature of language prevailing among those most familiar with computing.

There is a process of self selection at work: those attracted to computing tend to be most at home with a mathematical or mechanical view of the world. Academic computer scientists rarely write substantial programs themselves, they are more concerned with theories than implementation. Many programmers are self taught and programming, even when it is central to their employment or livelihood, may still be secondary to doing physics, or engineering, or biology. And of course much 'computing' is done by outright amateurs blindly following the recipe in cookbooks. To many, perhaps most, of these the notion of program as literature is simply alien.

On the other hand there is perhaps an even greater danger in appealing to the intuitions of those at home in a literary or humanistic mode of thinking. Since the late eighteenth century the spirit of romanticism has infused the notion of literature, 'program as literature' too readily implies that the hard problems: of grappling with myriad levels of detail, of imagination meeting the intransigent mechanism of nature head on, can somehow be disregarded. Programming as literature *is* hard work; programs must not merely carry conviction but must work as mechanism. The aesthetics of programs are classical rather than romantic: that is they demand that a creative expression be derived from working within a set of rules, of making anew out of a restricted repertoire of established expressions and forms.

It is in pursuit of a better model of what programming is that I would commend a study of rhetoric, though the term 'rhetoric' is itself problematical.[557] Firstly, because of all the modern associations of rhetoric: with vain posturing and insincere speech. There is also the presentation of classical and renaissance rhetoric as it has come down to us filtered through the Victorian schoolroom. A nit-picking classification of 'figures of speech' and difficult Greek and Latin terms for what is, in ordinary fluent speech, intuitive, natural, and thereby unconscious. Secondly, we have the rhetoric of modern theory. Never entirely free from a pejorative tone, this tends analyse every thought and action as well as speech in search of an alternative reading. Rhetoric in this sense means every historical 'actor' is assumed to be saying one thing in order to bring about something contrary or suppressed. Nothing is to be taken as it seems, we must always suspect ulterior motives. In such a reading of history everyone begins to appear as either a helpless puppet or duplicitous schemer. None of these versions of rhetoric will do; for a model of programming as a *logon techne*[558] it is necessary to return to the historical root and also to cast off the negative impression promoted by Socrates and Plato.[559]

In this model *rhetorike* (a term probably invented by Plato and the fount of its subsequent negative image) was a response to a profound change in the relation of written and spoken word that occurred in the fifth century BC. A fundamentally oral culture became literate. The move toward literacy had begun some four centuries before but its cultural impact had been limited. The predominance of the spoken word had persisted and with it the authority of those who could speak well in public. It is probably the importance attached to speech that ensured that in adapting Phoenician script the Greeks added a unique feature: the vowel signs by which not just a sign of a word but

[557] Though brought to my attention too late to influence what I have to say here see the introduction to Ladina Bezzola Lambert. *Imagining the Unimaginable*. Rodopi 2002 for an interesting discussion of both metaphor and rhetoric.

[558] Though *rhetorike techne*, is more familiar, *logon techne* better captures a sense of the profound impact of a conscious literacy. See Thomas Cole. *The Origins of Rhetoric in Ancient Greece*. Johns Hopkins 199 p98

[559] What follows is necessarily a very imperfect outline, the reader, if interested, will find a fuller discursion upon the origin of *rhetorike* in appendix 5. The work of Cole(1991) and deRomilly(1992) cited in the Bibliography are particularly recommended.

the spoken word could be recorded. The result was a language of poetry rather than commerce. In the fifth century changes in political organisation placed ever greater importance on public speaking in the law courts and assemblies of Athens. But literacy, by enabling speech to be set down, had made it possible for public speaking to become a skill that could be taught. For the first time words could be abstracted from the stream of consciousness. Thinking became visible, analytic. The form of words that might best persuade an assembly could be studied, practised, and reproduced. A new technology, literacy, had made possible a new way of thinking and a new form of literature.[560] Interdependent, speech and its writing had changed each other.

How should this apply as a model of, or for, programming? We can see programming as an equally profound intellectual challenge: of fixing in script the flow of process and algorithm. What is fixed is a text that can be studied and imitated but there is also something intangible, a power of persuasion over machinery, a power to invoke "things that never were nor could not be." The economic and social value of 'computing' has created a demand for handbooks or *techne* of our new form of words. This demand can be exploited by charlatans, misunderstood by those who feel threatened by it. But most of all it is a means, I hope, to come to grasp the essentially intangible yet real nature of a program: that is of its having *effect* in the physical and *affect* in the mental world. Programs *work*, if they do not they are mere plans or at best unproven algorithms. But they are nonetheless, in essence, mental phenomena—not shrink wrapped products, nor 'virtual' machinery, nor anyone's 'intellectual property'. It creates a world, as real as, but different, from the one that is. It is this intangible 'work of fiction' character of the program that is most in need of elucidation, *writing* software is like any other kind of writing.

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[560] So profound is this change that our term for poetry and prose implies that writing is the primary form, we are reduced to referring to 'oral literature'.

A1. Ross Ashby's Topics

The Bates papers at the Wellcome Institute contain two versions of Ross Ashby's list of topics for discussion by the Ratio Club. The first typescript(GC/179 B.5) is headed "Subjects for Discussion - submitted by W.R. Ashby", the date of "18. Feb 50" added in manuscript. This would have been after the sixth meeting of the club on the 16th February. The list accompanied Ashby's letter to Bates of the 26th February, in which he suggested a concentration on "real problems" rather than talking of "unsolvable philosophical questions." The list consists of 34 numbered topics ending with "if all else fails, 34. The effect of alcohol on control and communication with practical work"

The second version (also GC/179 B.5) is dated 15th May 1950 and consists of 28 items. Many are the same as the first list though the description is a little longer; full sentences rather than just titles. It was discussed at the Club's ninth meeting on 18th May.

Subjects for Discussion - submitted by W.R. Ashby 18. Feb 50

1. "Noise" in machines and brains.
2. Stochastic processes.
3. Physical processes which are independent of exact localisation
4. Transformations of information and their invariants.
5. Behaviour of mechanisms defined only statistically.
6. The latest news on what the big computing machines are doing.
7. Methods of analogue computing.
8. Discontinuous servo-mechanisms.
9. Modern mechanical memories.
10. Pattern-recognition in machines and brains.
11. The uses of unstable equilibrium.
12. Rhythmic processes in machines and their relation to the alpha rhythm.
13. The statistical mechanics of systems freely supplied with energy.
14. Randomly assembled systems.
15. New possible applications of mathematics to biology.
16. Mechanisms showing conditioned reflexes.
17. The transmission of information through sense organs.
18. Telepathy, extra-sensory perception, and telekinesis.
19. Imitating life: what has been achieved.
20. Principles and designs for a chess-playing machine.
21. Free-will.
22. The study of systems only partially accessible to observation.

23. Dialectical materialism and cybernetics.
24. "Activity is the only road to knowledge" says the Revolutionist's Handbook. Do we agree?
25. Systems which necessarily include the observer.
26. The diagnosis and treatment of insanity in machines.
27. Cybernetic principles and the behaviours of primitive organisms.
28. Ecological dynamic systems
29. The cybernetics of Great Britain and the bee-hive.
30. Cybernetic faults of the Civil Service
32. The next General Election is going to be won by the Cybernetic Party. What should be our first legislative act?
33. A meeting at which members can ask assistance in problems which have proved difficult.

Suggested Topics for Discussion

[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 a 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– of: 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 incoordinated[*sic*]?

[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?

W.R Ashby 15th May 1950

A2. Meetings of the Ratio Club

The numbers are those used Bates on small (4x3 inch) paper slips sent to members. This numbering also appears on the list of meetings Bates compiled with Ian Boal[561] in January 1985. This list however is incomplete and inconsistent with other evidence in the archive. Also the additional material in the archive donated by Donald MacKay contains additional references to dates of meetings and speakers. The list that follows is a collocation of this evidence including the scattered and sometimes cryptic notebook entries detailing the numbers attending and the cost of 'beer and full bellies'.

1 Wednesday 14 September 1949

Warren McCulloch

Attended: Ashby, Barlow, Bates, Dawson, Gold, Hick, Little, MacKay, McClardy, Merton, Pringle, Shipton, Sholl, Slater, Uttley, Westcott

Absent: Grey Walter

Beer & Wine £7-0-0, Food £4-0-0, Collected £7-10-0 (3-10-0)

2 Tuesday 18 October 1949

Dawson, MacKay, Uttley, Sholl

Beer & Wine £4-0-0, Food £1-4-0, Collected £7-10-0 (1-4-0)

3 Thursday 17 November 1949

Gold, Bates, McClardy, Walter

discussion of Slater/Uttley "bills on white paper"

Beer & Wine £4-5-4, Food 1-12-0, Collected £8-0-0 (1-8-4)

4 15 December 1949

Pringle, Merton, Little, Hick

those present include Turing, but attendance possibly poor and some talks may have been postponed

Beer & Wine 3-14-0 +[*sic*] 4-0 = £3-10-0, Food £1-12-0, Collected £2-15-0, (0-18-8)

Thursday 12 January 1950

(date stated on account written on back of envelope) [GC/179 B24]

Beer & Wine £2-11-8, Food £1-12-0, Collected £5-0-0 (0-12-0)

5 Thursday 19 January 1950

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[561] This appears to have been in connection with research for Steve Heims' account of the Macy Group (*The Cybernetics Group* MIT 1991). Though correspondence in the Bates archive suggests a chapter on the Ratio Club was contemplated this does not appear in the published edition.

Slater Paradoxes are Hogwash

MacKay Why is the Visual World Stable?

absent: Hick

6 Thursday 16 February 1950

Shipton, Slater

Absent: Pringle

Beer & Wine £3-12-0, Food £1-12-0, Collected £4-15-0 |£2-4-8

7 Thursday 16 March 1950

Ashby, Barlow

Absent: Hick, Pringle

Guests: A. Bowdler (Sholl)

Beer & Wine £3-6-4, Food £1-12-0, Collected £6-0-0 |£2-11-8

8 Friday 21 April 1950

L. Allen (Burden Neurological Inst.) Half the Extra-Pyramidal System

title refers to McCulloch?

Absent: Shipton

Beer & Wine £3-6-4, Food £1-12-0, Collected £6-0-0

9 Thursday 18 May 1950

Grey Walter & Uttley Pattern Recognition

Patterns are not necessarily visual: “how would a mole generalise the problem”

Absent: Gold, Hick

MacKay, Barlow

Discussion followed Ashby's proposal to 'attack' problem until solved. There was also a suggestion of a “postal portfolio” of questions and answers to be circulated among members. It appears however that the portfolio was lost in the post or mislaid by one of the members in the London, Cambridge, Bristol triangle. The idea was mentioned again at the meeting on Future Policy eighteen months latter (see 21 December 1951) but no items from the portfolio appear extant.

Beer & Wine £3-5-4, Food 1-12-0, Collected £4-0-0

10 Thursday 22 June 1950

Absent: Walter

Woodward Elementary Basis of Information Theory

Beer & Wine £3-6-4, Food £1-12-0, Collected 7-10-0

11 Tuesday 18 July 1950

Gold, MacKay, Sholl Concept of Probability

Beer & Wine £1-18-0, Food £1-12-0, Collected £2-15-0

12 Thursday 21 September 1950

Pringle Noise in the CNS

[Bates to MacKay 8 September 1950 (GC/179 B.21]

cf Pringle Papers E3: Tea Party Talk 16 Feb 51, Random Events in the Nervous System.

? Monday 2 October 1950

Bates, Dawson, Slater, Merton, Sholl, MacKay, Westcott, Little, Gold, Rushton, Pringle, Barlow, Ashby, Shipton, Uttley, Woodward [GC179 A22]

Absent: Grey Walter, Hick, Turing

Guests (1 paid by Uttley)

[Not listed in 1985, may be associated with Symposium on Information Theory (26/9/50). Attendances noted by Bates' in notebook B3, GC/179 A.22]

13 Thursday 7 December 1950

Turing Educating a Digital Computer

"Turing was very entertaining and JZ Young came along to add fat to the fire"[562]

Guests: IJ Good, JZ Young

Absent: Walter

14 Thursday 22 February 1951

Grey Walter Adaptive Behaviour

A demonstration of the "tortoise"

15 Thursday 5 April 1951

Rushton Shape and Size of Nerve Fibres

I suppose it would be in the best Ratio biology tradition—a few facts with small bearing on information theory and a little information theory with small bearing upon the facts[563]

Guests: JZ Young

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[562] Bates to Grey Walter 13 December 1950 GC/179 B.7

[563] Rushton to Bates 15 February 1951, GC/179 B.7

16 ?

17 31 May 1951

Ashby Statistical Machines

Absent: McClardy[564], MacKay (in USA)

Guests: IJ Good

18 Thursday 26 July 1951

Bates Telepathy

I shall put forward some reasons for thinking that all the "evidence for telepathy" that is not actually fraudulent is more likely to be accounted for by a variety of unconscious interference with data, or by well meant muddle, than by hypotheses of some genuinely novel Sixth Sense.

Guests will include Parsons, a member of the Society for Psychical Research, and Salvin, one of the elite of the Magic Circle.

I can supply on loan reprints of the much publicised papers of GG Soul.[565]

Guests: IJ Good, Philip Woodward[566], Parsons (Soc. Psychical Res.) Salvan (Magic Circle)

19 Thursday 1 November 1951[567]

Gold What is Happening to the Universe

(on Popper)

Absent: Good, Shipton, Uttley, Woodward

"poorly attended"

[may have been on 8th November]

20 Friday 21 December 1951

Future Policy

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[564] Bates to Ashby 15 May 1951 "Turner McClardy no longer considers himself a member, McKay is in the States" GC/179 B.9 MacKay remained in USA until the end of the year Bates to MacKay 13 November 1951 "There are some American publications which seem to be scarce here and I wonder if you were bringing copies back" Cerebral Mechanisms in Behaviour Jeffreys (1951) [Hixon Symposium], 6th Macy Conference on Cybernetics 24 March (1949) [GC/179 B.22]

[565] GC/179 B.9

[566] Bates to Ashby 15 May 1951 includes Woodward, together with Turing and Rushton as new members. GC/179 B.9

[567] This meeting may have been on 8th November. The date is 1st November on Bates' circular but letter from Uttley (26 October 1951) and Shipton [GC/179 B.11] refer to the date of the meeting as 8th November. Another letter to Good also suggests some confusion over the date of the meeting.

I read your letter to them but I could not get support for the idea that the club should "take a specific objective or work for some agreed aim".[568]

There was agreement to hold 6-7 meetings per year, 2 outside London, to present 2 papers per meeting, and a revival of the idea of a circulating postal portfolio.

Absent: Ashby, Westcott, Woodward

21 Friday 8 February 1952

Turing Chemical Origins of Biological Form

Woodward Theory of Observation

"at the last meeting[569] we were rather flooded out."

Unfortunately there is no list either guests or members present on this occasion.

22 Thursday 20 March 1952

Uttley Problems of Pattern Recognition

MacKay Meaning in Information Theory

"...I do not think that on this occasion you missed very much. Uttley's very interesting work is being expanded & published soon, and MacKay's ideas on Meaning were not quite up to his usual power, so I think we thought, anyway."[570]

cf Computation of Pattern Differences TRE report 17 Nov 1952 [Pringle Papers A8]

Absent: Pringle, Rushton, Turing

23 Friday 2 May 1952 - Saturday 3 May 1952

Cambridge

Pringle Processes Involved in the Origin of Life

Present: Ashby, Barlow, Bates, Dawson, Gold, Hick, MacKay, McClardy, Pringle, Rushton, Shipton, Sholl, Turing, Uttley, Westcott, Slater, Walter

Absent: Merton, Woodward

Guests: Giles Brindley (London Hospital), Gurney Sutton

Peterhouse, Cambridge

"Everyone will be coming except Merton and Woodward"[571] It seems reasonable to assume "MacKay's Photograph" was taken on this occasion. Gold's proposed talk on the Origin of Solar System was not given. Hick may

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[568] Bates to Ashby 18 January 1952 GC/179 B.12

[569] Bates to Good 26 March 1952 GC/179 B.13

[570] Bates to Good 26 March 1952 GC/179 B.13

[571] Pringle to Bates 25 April 1952 [*ibid*]

have given a lab demonstration on the Saturday morning but had “no talk to give”

24 Thursday 19 June 1952

Bates The Problem of Memory

“I will collect points made by eminent Americans in 3 recent published discussions”

Westcott Logic of Discrimination

Attended: Bates, Dawson, Slater, Gold, Uttley, Little, MacKay, Rushton, Walter, Shipton, Ashby, Westcott.

discussion of Ashby’s proposal of JZ Young as member

Guests: “2” [GC/179 A.31]

25 Thursday 31 July 1952

Barlow The Size of Eyes

Sholl American Interests in Brain Structure

Attended: Bates, Dawson, Slater, Sholl, MacKay, Uttley, Barlow, Woodward, Pringle

Guests: 1 with Uttley, 1 with Barlow [GC/179 A.31]

26 Friday 24 October 1952

Bristol

Cancelled - “at least 6 members prevented from attending by teaching commitments”

27 Thursday 6 November 1952

Hick Design of Randomising Devices

Walter On Ashby’s *Design for a Brain*

28 Thursday 11 December 1952

MacKay Perils of Self Awareness in Machines

Merton Sorting Afferent from Efferent Messages in Nerves

29 Thursday 19 February 1953

Uttley & Sholl Pattern Discrimination in the Visual Cortex

originally scheduled for 15 January, "discussion of Uttley's pre-circulated paper - intended demonstration of an electrical model of a 'basic unit' - correlation between proposed networks and anatomical appearances"

see *Nature* vol. 171, p387 28 February 1953

The paper was the result of a discussion between Uttley and Sholl in Bates’ office: Uttley wrote to Bates on 27th November 1952 “if there is anything of value in this paper it is very largely due to the Ratio Club”. There is a ms draft of Bates reply to Uttley: "... In the 20 minutes I have had in it you have thoroughly exited me [with] the [possibilities] of [unitary?] measure which seem intuitively to supply exactly the sort of rationalisation of information theory *à la* Shannon that physiologists have been looking for."[572]

? Thursday 7 May 1953

V. Little Absorption of Radio Frequencies by Ionic Materials

GD Dawson The Signal Noise Problem

30 Thursday 2 July 1953

Warren McCulloch

"he is lecturing in London on New Techniques in Physiology, The problem of Inference for EEG; The problem of Coding in the CNS; and Into the Den of the Metaphysics, and the points he raises will be the basis for discussion."

? Thursday 22 October 1953

Shipton The Toposcope

(Demonstration of 12 Channel Phrase and Frequency Indicating EEG Display Unit.)

IJ Good Discussion on Probability

"points raised by GS Brown in *Nature* 25.7.53 p154 & 26.9.53 p594"

31 Thursday 11 February 1954

IC Whitfield, JT Allanson How Does the Nervous System Carry Information?

32 Thursday 17 June 1954

PA Merton Servo Control of Muscle Movement

PM Woodward Introduction to Group Theory

33 Thursday 25 November 1954

Slater, Woodward Negative Information in Relation to Data on Blood Grouping in the Selection of Identical Twins

Prof. CH Waddington FRS Development as a Cybernetic Process

"subject suggested by Pringle and Barlow"

34 Friday 6 May - Saturday 7 May 1955

Malvern

Demonstrations & dinner in TRE mess "no formal discussion"

Bristol

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[572] GC/179 B.15

demonstration at Barnwood, lunch at Burden

? Thursday 15 September [1955]

"The next meeting of the club will be Thursday 15 September at the National Hospital; Sherry and Buffet 7pm. I am inviting about 10 of those (from overseas) who will be giving papers to the Information Theory Symposium." [GC/179 B23]

"total 35" ... "US: Peter Elias, JCR Licklider, Wolf[?], Warren McCulloch, Oliver G Selfridge, Henry Quastler, Benoît Mandelbrot" Home: JT Allanson, IC Whitfield, [S Brown?], Colin Cherry, JD North, [Frituyn?] [S Linwood/Leonard?] [GC 179/A46]

35 Thursday 27 November 1958

at MoTP58

"It has been suggested it would be a good thing to make Uttley's N.P.L. Symposium the excuse for a Ratio Club meeting. It would seem that Thursday November 27th is the obvious day, as the proceedings at Teddington are devoted to biology and the session ends at 6.30. The chef here has agreed to provide food for 7.30 onwards. I suggest this should be a 'social reunion' without guests and without a set piece, and we might discuss after dinner whether a more ~~permanent~~[573] formal organisation is needed, and if so how we could encourage it.

If you have not paid your £3 for the whole N.P.L. Symposium, but would like to attend the biological papers on Thursday, Uttley tells me you are invited to do so."[574]

Present: Bates, Barlow, Dawson, Sholl, Slater, Uttley, MacKay, Woodward, Hick

Absent: with expressed regret: Grey Walter, Merton, Westcott. with expressed lack of interest: Ashby, McClardy. without expression: Rushton, Pringle, Little, Good. emigrated: Gold, Shipton

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[573] deleted word in ms

[574] GC/179 B19. There is also a ms draft at GC/179 A.59.

A3. The Symposium

National Physical Laboratory

Symposium No. 10

Mechanisation of Thought Processes

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Session 1.

General Principles

Chairman Dr A M Uttley, NPL

1 *Some methods of artificial intelligence and heuristic programming*

Dr M L Minsky, MIT, USA

Discussion on paper 1 Chairman DR. W S McCulloch, MIT, USA

2 *Operational aspects of intellect*

Dr D M Mackay, Kings College, London

3 *Programs with common sense*[575]

Dr J McCarthy, MIT, USA

4 *The mechanism of habituation*

Dr W Ross Ashby, Barnwood House Hospital, Gloucester

5 *Conditional probability computing in a nervous system*

Dr A M Uttley, CME Division, NPL

Automatic Programming

1 *Automatic programming; present status and future trends*

Dr Grace Murray Hopper, Remmington Rand Univac, USA

2 *Some technical features of the Manchester mercury autocode programme*

Mr R A Brooker, Manchester University

3 *Automatic programming; properties and performance of FORTRAN systems I and II*

Mr J Backus, IBM, USA

4 *The work of the Computing Center of the Academy of Sciences of the USSR in the field of automatic programming*

Dr A P Ershov, Academy of Sciences of the USSR

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[575] John McCarthy's paper is available from www-formal.stanford.edu/jmc

Session 2.

Mechanical Language Translation

Chairman: Dr L Mehl, Ecole Nationale d'Administation, Paris

5 *Tigris and Euphrates - A comparison between human and machine translation*

Mr R H Richens, Cambridge Language Research Group

6 *Pronoun reference in German*

Mr L Brandwood, Department of Classics, Manchester University

7 *Automatic translation in the USSR*

Dr A P Ershov, Academy of Sciences of the USSR

Session 3

Speech Recognition

Chairman: Prof. Colin Cherry, Imperial College, London

1 *Sensory mechanisms and sensation*

Dr I C Whitfield, Saint Elizabeth's Hospital, Washington, USA

2 *An analogue of the speech recognition process*

Prof. D B Fry and Mr. P Denes, University College, London

3 *The perception of speech*

Mr P Ladefoged, Edinburgh University

Learning in Machines

Chairman Mr. J F Coales, Cambridge University

4 *Two theorems of statistical separability in the Perceptron*

Dr. F Rosenblatt, Cornell Aeronautical Lab, USA

5 *Learning machines*

Dr A M Andrew, CME Division, NPL

6 *Pandemonium: A paradigm for learning*

Dr O G Selfridge, MIT, USA

Session 4a.

Implications for Biology

Chairman Prof. J Z Young, University College, London

1 *Sensory mechanisms, the reduction of redundancy and intelligence*

Dr H B Barlow, Physiology Laboratory, Cambridge University

2 *Stimulus analysing mechanisms*

Dr N S Sutherland, Institute of Experimental Psychology, Oxford

3 *Agatha Tyche: Of nervous nets - the lucky reckoners*

Dr W S McCulloch, MIT, USA

4 *Medical diagnosis and cybernetics*

Dr F Paycha, Paris

Discussion on paper 4

Chairman: Sir Frederick Bartlett, Applied Psychology Research Unit, Cambridge

5 *Models and the localisation of function in the central nervous system*

Mr R L Gregory, Psychological Laboratory, Cambridge

6 *Some questions concerning the explanation of learning in animals*

Mr A J Watson, Psychological Laboratory, Cambridge

7 *Information, redundancy and the decay of the memory trace*

Dr John Brown, Birkbeck College, London

Session 4b.

Implications for Industry

Chairman The Rt. Hon. The Earl of Halsbury, NRDC, London

1 *Automation in the legal world*

Dr L Mehl, Ecole Nationale d'Administation, Paris

2 *The mechanisation of literature searching*

Prof. Y Bar-Hillel, The Hebrew University, Jerusalem

3 *To what extent can administration be mechanised?*

Mr J H H Merriman and Mr D W G Wass, HM Treasury, London

4 *Possibilities for the practical utilisation of learning processes*

Dr S Gill, Ferranti Ltd., London

Discussion on paper 4

Chairman: Mr J Merriman, HM Treasury, London

5 *Automatic control by visual signals*

Dr W K Taylor, University College, London

6 *An analysis of non-mathematical data-processing*

Mr E A Newman, CME Division, NPL

7 *Physical analogues to the growth of a concept*

Mr G Pask, System Research Ltd., London

Lecture - Demonstrations

Machina Reproducatrix

Dr A J Angyan, Physiological Institute, Budapest

Conditional Probability Computer

Dr A M Andrew, NPL

A Simple Computer for Demonstrating Behaviour

Dr W Ross Ashby, Barnwood House Hospital, Gloucester

Automatic Pattern Recognition

Dr W K Taylor, University College, London

Library Retrieval

Mr S Whelan, Royal Radar Establishment, Malvern

A4. MoTP58, list of attendees

The following list is based upon the attendance list on pages 965–978 of the published proceedings. I have simplified the full postal addresses in the original and added an indication of disciplinary interest. Where no biographical information has been available to me this disciplinary interest has—apart from the evidence of institutional affiliation—been largely derived from bibliographical records that may reflect career development after 1958.

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A5. *rhetorike*

> From the moment it enters Western thought, *rhêtorikê* is invoked as the name for a specialised art of pragmatic discourse *that cannot exist* in and of itself, an art that cannot be centered or narrowly contained within the pragmatic realm, if it is to produce anything worth desiring. *Rhêtorikê* from the beginning is the misname of an art that keeps resolving itself back into, or that simply becomes, or that always was, something like an Isocratean *logôn technê*. [576]

Most recent studies of the origins of rhetoric acknowledge the influence of Eric Havelock who, in a series of publications starting with *Preface to Plato* in 1963 argued strongly for the profound influence on Greek culture of a transition from orality to literacy.[577] It was, argues Havelock, a gradual change in which the modes of an oral culture persisted and predominated despite widespread literacy. Havelock built upon earlier work: that of Milman Parry in the 1920s and later Albert Lord, which reappraised the work of Homer in the light of modern studies traditional oral storytelling. In this interpretation, the 'Homeric question' which had been debated since the eighteenth century, is resolved by the assumption that *Odyssey* and *Iliad* were originally oral poetry, the work of many generations. Homer was perhaps the name of a noted bard, or the compiler of a later written edition. Even when written down these texts were but to assist the memory of *rhapsodes* performing to a pre-literate audience of listeners.

> ... a situation best described as craft literacy, in which the public inscription is composed as a source of referral for officials and as a check upon arbitrary interpretations. As for the poet, he can write for his own benefit and thereby can acquire increased compositional skill, but he composes for a public who he knows will not read what he is composing but will listen to it.[578]

Poetry, above all that of Homer, formed an 'encyclopaedia' of customary behaviour, a repository of traditional wisdom, of *nomoi* and *ethea*.[579] For reasons that are not clear it appears that the Greece of the 'dark ages' (approx 1200–700BC) was an entirely oral society. Unlike the Mycenaean palace cultures that preceded it, there were no scribes. In these circumstances

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[576] Jeffrey Walker. *Rhetoric and Poetics in Antiquity*. OUP 2000 p40

[577] For criticism of Havelock's interpretation, in particular that alphabetic literacy was a significant factor in the evolution of logical, abstract modes of thinking see John Halverson. 'Havelock on Greek Orality and Literacy' *Journal of the History of Ideas* 1992 pp148-163

[578] Eric Havelock. *Preface to Plato*. Harvard UP. 1963 p39

[579] *nomoi* later meaning law but always carrying an earlier sense of custom. *ethea* originally a haunt or lair but by Aristotle's time 'ethics' *ethos* is thus more personal in its application than *nomos*.

effective speaking was a source of power.[580] When literacy did come to Greece, with the adaptation of the Phoenician alphabet, it came with an innovation; an enhanced capacity to record speech, signs for sound rather than symbols of things. It appears that from the beginning this new script was used for poetry.[581] The lyric poets had turned to writing by the seventh century, it was no longer, as it was in the Mycenaean world, merely a adjunct to the recording of inventories and decrees.[582]

> The alphabet proved so much more effective and powerful an instrument for the preservation of fluent communication than any syllabary had been. And by the fourth century its victory was nearly complete, meaning that the original functional purpose of the poetic style was becoming obsolete.[583]

Truth was initially speech—efficacious speech, and was a privilege reserved for poets and diviners, those who possessed the power of *Mnemosyne*. It was the change from 'truth' to 'opinion' that made rational thought possible; a transition supported by the particular conditions—social, political, intellectual—of fifth century Athens. *Logos* (speech, argument) became both a means to act upon others and a means of knowing reality. The old speech was immediate, was realised as action:

> Magicoreligious speech is pronounced in the absolute present, with no before or after, a present that, like memory, incorporates 'that which has been, that which is, and that which will be'.[584]

There was a change in the manner of making speech In archaic Greece there are songs (*aoidê*) and hymns (*hymnoi*) of a singer (*aoidos*), and there are eloquent words (*epea*) spoken by the wise ruler (*basileus*). By the fourth century BC the singer had become a maker of verse.

> Emancipating poetry from the religious universe to which it had belonged (and to which it would long remain attached), the choral poets developed the basis for a reflection on language that greatly benefited the budding development of rhetoric and linguistics. Through metaphors that define discourse not only as something 'woven' but also as a 'construction', they insisted so much on the materiality of discourse that—in about 450BC —they ended up being called *poiêtai* ('artisans',

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[580] See Havelock *op cit* pp126-7

[581] The earliest records, from the eighth century, are snatches of hexameter verse on pottery. Though the alphabet was derived from that of the Phoenician traders, examples of early mercantile use of the alphabet do not appear to have survived.

[582] See Marcel Detienne. *The Masters of Truth in Archaic Greece*. [*les Maîtres de vérité dans la grèce archaïque* 1967] Zone Books. 1990 p110

[583] Eric Havelock. *Preface to Plato*. Harvard UP. 1963 p 137

[584] Detienne. *op cit* p74

> 'producers', 'builders'); thus Homer himself could finally begin his career as a 'poet'.[585]

The new speech was conscious of ambiguity: of a relation to reality, but also, through its influence over people, a power to be confused with reality. It was political, the speech of dialogue and debate: secular, temporal, autonomous and distinct from action. In this context poetry, the old magicoreligious speech, might seem to be an art of trickery and illusion.

The tenth book of Plato's *Republic* is taken up with an attack upon poetry, Havelock asks why. "For Plato, reality is rational, scientific and logical, or it is nothing." What is to be objected to in poetry is its *mimesis*, in this he sees not just imitation, but the casting of a spell upon the audience, who thereby identify with the artists vision. Plato was witnessing the transition from orality to literacy, oral habits of mind remained potent, and whilst they did, Plato's philosophy would in a sense fall upon deaf ears.

> Once it is accepted that the oral situation had persisted through the fifth century, one faces the conclusion that there would persist what one may call an oral state of mind as well; a mode of consciousness so to speak, and, as we shall see, a vocabulary and syntax which were not that of a literate bookish culture. And once one admits this and admits that the oral state of mind would show a time lag so that it persisted into a new epoch when the technology of communication had changed, it becomes understandable that the oral state of mind is still for Plato the main enemy.[586]

A written language that recorded speech enabled the recording of dialogue. The language was freed of the necessity for the mnemonic devices of the old poetry.

> ... the use of a written alphabet—for breaking up words and putting them back together in new ways, for catching exact rhythms and idiosyncratic constructions, for combining different styles and meters in the same work and maintaining the integrity of each—gave the poet a device for recording speech with vastly more individuality than was formerly possible. Unencumbered by the formulaic, mnemonic demands or oral recitation, the dramatic writer could, with the help of this new technological 'ear' of speech, record human discourse with a freedom not to be matched until the invention of the wax cylinder.[587]

As seen by Plato the poets *mimesis* is not just a creative act but a power to bind to his audience, it is in essence dramatic; "this way of reliving experience in

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[585] John Scheid, Jesper Svenbro. *The Craft of Zeus: myths of weaving and fabric*. (tr. Carol Volk) [*Le métier de Zeus: Mythe du tissage et du tissu dans le monde gréco-romain* 1994]. Harvard UP 1996 p121-2

[586] Eric Havelock. *Preface to Plato*. Harvard 1963 p41

[587] Jennifer Wise. *Dionysus Writes: the invention of theatre in ancient Greece*. Cornell UP. 1998 p50

memory instead of analysing and understanding it, is for him 'the enemy'."[588] While poetry reigned it was an obstacle to effective prose.[589]

The sophists attacked by Plato knew the power of speech and had gained a new power to work its effect: one they were willing to exploit. Words could work upon the emotions as Gorgias states

> Speech is a great power, which achieves the most divine works by means of the smallest and least visible form; for it can even put a stop to fear, remove grief, create joy, and increase pity.[590]

In the analysis that follows Gorgias attributes this magical power of words to the uncertainty of knowledge, the limitations of memory: we resort to mere opinions, half formed thoughts which are changeable. Speech works upon opinion, moulding this insecure part-knowledge to the speaker's purpose.

According to Havelock the pre-Socratics were oral thinkers, their language not yet adequate to express ideas of the abstract. Abstraction required the reflection permitted by writing things down, but also a public receptive to a novel form of communication.

> Any attempt to reinterpret the history of the Greek mind as a search for concepts not yet realised and for a terminology not yet invented confronts a formidable obstacle in the traditional reports preserved in Hellenistic and Roman antiquity. These assume that the earliest philosophers of Greece were engaged from the first with metaphysical problems, and formulated solutions which presuppose a mastery of the abstract: that in fact they were philosophers in the modern sense of the word.[591]

A warning of the dangers of interpretation of sparse evidence in the light of present concerns that is echoed by Jacqueline de Romilly:

> ... each modern philosophical school tends to read these sparse fragments that have survived from the intellectual movement of the Sophists as a whole in such a way as to detect in them their own particular problems and prejudices. Some detect rationalism of the purest kind, others existentialism. Nowadays, the tendency—unsurprisingly—is to detect the elements of a Philosophy of language.[592]

de Romilly sees in this a cautionary tale

> ...to make it easier to understand how something can go wrong in the dialogue between any school of dense theoretical thought and its contemporary public, which is only more or less well informed and capable of understanding it.

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[588] Eric Havelock. *Preface to Plato*. Harvard 1963 p45

[589] Havelock's argument is obviously influenced by some version of the Whorf thesis: that thought is dependant upon vocabulary.

[590] Gorgias. *Helen* quoted in de Romilly p66

[591] Eric Havelock. *Preface to Plato*. Harvard UP. 1963 p.viii

[592] Jacqueline de Romilly. *The Great Sophists in Periclean Athens*. OUP 1992 p xi

Plato's philosophy succeeded in throwing off the spell of *mimesis*, but not without cost.

> But the narrowing down of the problem of experience to one of physical perception had the effect also of narrowing the object of experience from the total event-series down to physical things in the series. Philosophy gradually forgot its original objective which had been to throw off the mnemonic spell of the narrative. It substituted the attempt to throw off the spell of material things.[593]

The 'philosopher' who Plato introduces in the *Republic* as the ideal of political authority is "the man who is prepared to challenge the hold of the concrete over our consciousness, and to substitute the abstract."[594] "He was one therefore who had by conscious and we might say eccentric effort defied the ethos of his own culture.[595]

> By the late sixth century, at least, they had been appropriated for that skill *par excellence* to which the Greeks gave prestige, namely the skill of the bard. His was pre-eminently a skill in the command of effective communication, both of word and of content. *Sophia* therefore might denote his power as a musician or versifier, but equally his authority as a teacher, the voice of the traditional experience which lay behind his verse. With the slow transition from verse to prose and from concrete towards abstract the man of intelligence came to represent the master of a new form of communication equally consecrated to educational purposes, but now anti-poetic. In short, *sophia* always remained 'skill of speech' and 'skill of mind' but the kind of speech and the kind of mind changed.[596]

The sophists (*sophoi*, sages) were not Athenians for the most part, Protagoras arrived in Athens from Northern Greece soon after 450, Gorgias from Sicily around 427, the movement covers the second half of the fifth century—of Athens greatness following the Persian wars and later fall. The sages of old were "not a profession but a state of being" but the sophists were professionals. "They knew certain methods and could teach them. [...]They were selling intellectual skills, selling them dearly what is more."[597] It was an innovation, this idea that intellectual knowledge was directly useful and worth paying for. What scandalised Plato is now the commonplace of 'intellectual property'.

It was a time when philosophy shifted attention from the universe to man and the Sophists offered an understanding of human nature. "Of all things the

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[593] Eric Havelock. *Preface to Plato*. Harvard UP. 1963 p250

[594] Eric Havelock. *Preface to Plato*. Harvard UP. 1963 p281

[595] Eric Havelock. *Preface to Plato*. Harvard UP. 1963 p282

[596] Eric Havelock. *Preface to Plato*. Harvard UP. 1963 p287

[597] Jacqueline de Romilly. *The Great Sophists in Periclean Athens*. OUP 1992 p5

measure is Man, of the things that are, that they are, and of the things that are not, that they are not."[598]

> The Sophists seemed as essential to fifth-century Athens as brilliantly effective physicists would be in the event of an atomic war in the twentieth century. [599]

The Sophists offered a new kind of education, the traditional athletics, music and poetry were united in their stress upon discipline and harmony, on the cohesion of a social group.[600] The singing and dancing was choral, the poets learnt by heart. The sophists offered a practical skill, one that brought advantages to the individual, but a skill of citizenship rather than a profession. Yet the Sophists overstated their case, claiming to be able to teach everything, overlooking both natural talent and the effect of training and habituation. When Isocrates set up his school (half a century after the first appearance of the sophists) he set this out in *Against the Sophists*.

> they do not attribute any of this power either to the practical experience or to the native ability of the student, but undertake to transmit the science of discourse as simply as they would teach the letters of the alphabet. [601]

Rhetoric in the narrowest sense is self conscious manipulation of the written or spoken medium to ensure the most favourable reception of the intended message. It can be distinguished from eloquence, which may be unpremeditated, and from a display of artistry which may be an end in itself. In this narrow sense it is indifferent to the inherent value and character of the message: it is not, as in some modern reformulations, "either an overall science of discourse or an art of practical reasoning and deliberation."[602] It is a deliberate working upon the imagination and, as Cole remarks, has its beginning in an effort to make the written word do the work of the spoken word.

> It is the characteristic mode assumed by verbal skills in the wake of the intellectual revolution which, during the course of the sixth and fifth centuries BC, laid the foundations for Western thought—and, as such, the natural successor to poetry and eloquence. [603]

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[598] Protagoras *On Truth* (B1) [quoted in deRomilly p84]

[599] Jacqueline de Romilly. *The Great Sophists in Periclean Athens*. OUP 1992 p25

[600] de Romilly notes the evidence in for instance Aristophanes' *Clouds* of tension between the new intellectuals and the traditional athletic education. p38-9

[601] quoted in de Romilly p53

[602] Thomas Cole. *The Origins of Rhetoric in Ancient Greece*. Johns Hopkins UP. 1991 p ix

[603] Thomas Cole. *The Origins of Rhetoric in Ancient Greece*. Johns Hopkins UP. 1991 p1

But term rhetoric has become divorced from its origin as a philosophical[604] enquiry. The term *rhetorike techne* (the 'speakers' art) is first recorded in Plato's *Gorgias*, it may well be a Platonic invention. It is Plato, who in an attempt to differentiate his own philosophical programme, first discredits and sets aside this proto-philosophy of language. And Aristotle builds upon Plato's foundation, and so on, down to the present. The focus continues to be on what Plato attacked; and the history we have is largely that written by his successors. Yet rhetoric was an essential part of communication even for Plato. It does not meet the "philosopher's standards of accuracy, coherence and consistency, but is still necessary if the communication is to be fully successful."[605]

> For a crucial series of decades in the course of the fifth and early fourth centuries a host of Sophists, scientists, physicians, polymaths, logographers, orators, statesmen, dramatists, and exegetes disputed among themselves the position left vacant by the collapse of poetry's undisputed claims to be the moral and intellectual mentor of Greece. Victory went ultimately to the group of combatants best able to develop, first, a satisfactory (philosophical, as it turned out) alternative to the poetic world view and, second, a new form of discourse (artistic—that is, rhetorical—prose) capable of rivalling poetic performances in its power to satisfy the curiosity, engage the sympathies, and fire the imagination of an audience, whether hearers or readers. Victory also went, as matters turned out, to a group that insisted on a rigid initial separation of the two tasks, and on the strict subordination of the second to the first.[606]

Rhetoric is from the beginning an equivocal term, our first evidence is in Plato where it is appropriated to differentiate his 'philosophy' from other claimants to *sophia*, especially those who stressed the power (for good or ill) of epideictic speech over the dialectic of Socrates and Plato.

> ...metered discourse was no longer the whole of epideictic by the fifth century BC: philosophers, sophists, and historians had begun to produce an epideictic prose. The category of epidiectic discourse had been ramified still further. We might attribute this development to the continued differentiation of poetic kinds: once 'song' is expanded into 'poetry' and poetry divided into the sung and the 'spoken', the spoken in turn divides into the metered and the unmetered. 'Prose' thus emerges as unmetered poetry, in other words a kind of free verse. That seems plausible, but certainly the overwhelming causal factor in this development is the advent of literacy in archaic Greece. [607]

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[604] It should be noted that 'philosophy' is also, in this context, a neologism or even anachronism.

[605] Thomas Cole. *The Origins of Rhetoric in Ancient Greece*. Johns Hopkins UP. 1991 p13

[606] Thomas Cole. *The Origins of Rhetoric in Ancient Greece*. Johns Hopkins UP. 1991 p29

[607] Jeffrey Walker. *Rhetoric and Poetics in Antiquity*. OUP 2000 p21

Poetry in the oral tradition is not just ornamental speech or entertainment it is a means of remembering in the absence of written records. (Cole suggests Mnemosyne, the muse of poetry, is not the *memory* of the individual but *information retrieval* for all.) This information is moral as well as practical, it binds a society together. Rhyme, assonance, metre, repetition and formulaic phrases; such are the devices of poetry to preserve memory. But once this poetry is written down the gain in fixity also brings to attention the distortion that may accompany transmission by word of mouth. The figures and devices are exposed to examination. But the written word also introduces ambiguities: it is no longer embodied, gesture and intonation, immediate response and rephrase are suppressed.

In the transition from orality to literacy there is an intermediate stage in which texts carry the poetic devices of poetry but are not a record of oral poetry. Neither are these early texts composed for a reader. The texts of Gorgias, Antiphon etc. are composed for a speaker, they are it seems for practice and demonstration.[608] They are not speeches in themselves but compressed models, an educational toy.[609] A later development sees these works becoming concerned with the classification and naming. It is only after this stage that a process of analysis occurs leading to the work of Aristotle, the model for all the later works on rhetoric. But before that there was also a development of 'performance' texts, complete pre-written speeches for particular occasions. Thus Cole argues:

> ... analytical meta-language can only develop on the basis of a close study and comparison of particular pieces of persuasive eloquence, and such close examination is only possible when these pieces of eloquence are available in written form.[610]

By the early fourth century a true reading text had emerged: one that mimed oral communication rather that merely serving as a prompt copy. Thus come about the Socratic dialogues of Plato that make the reader a virtual auditor of the dialectic debate.

> These were efforts at a full-scale revelation and recreation of Socratic *ethos* as well as Socratic method, and at a mode of presentation that

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[608] See Cole *op cit* pp71-81

[609] These practical demonstration texts were known as *techne*, the same term later used to refer to a systematic treatise such as that of Aristotle. Isocrates criticises these texts when he writes of "a fixed *techne* as a model for a process that is creative." The specificity of these texts to Athens, with its metropolitan and predominantly oral culture is contrasted with Ionia, where the isolation of the islands favoured the development of reading texts. See Cole *op cit* p81

[610] Cole *op cit* p112

> would give the whole flavour of philosophical debate as experienced by the listening spectator.[611]

Once written communication can achieve the full effect of its oral predecessor the advantages of the medium become apparent: the facility to cut and paste, to bring together and rework the ideas and styles of others, to continually scrutinise and improve, all the time subject to deliberate thought outside the flow of ideas and occasion.[612]

It is, suggests Cole, in Plato's generation and to a significant extent by Plato himself, that two achievements come together, and it is this combination of medium and message that account for their continued influence.

> ... the 'saving' of the individual phenomena as reflections, imitations, or approximations of an unchanging order of reality, and the saving of the power, colour, and effectiveness of individual pieces of oral discourse as embodiments, captured in writing, of permanently valid messages perfectly adjusted to the needs of unique situations. [613]

A similar conclusion is reached by de Romilly

> The miracle is that one and the same city, in the course of the same period, gave birth to two kinds of thought, the one totally opposed to the other, the one humanistic and practical, the other transcendental and idealistic. [614]

The development of rhetoric depended on the particulars of time and place: the 'metropolitan' character of Athens which enabled a dense accumulation of thinkers to exchange ideas in person; a cultural tradition which placed great value on the spoken word, reinforced by political and legal development; and a unique development of literacy as an adjunct to the spoken word rather than the media of official (scribal) record. The pressure to be an effective public speaker, caused the insight and innovation of the Sophists to be welcomed but also misunderstood. The market demand was for a simplified, cookbook, version of their art—the *techne*. This led to distrust and even rejection of the new art which appeared to debase previously rare skills of public authority. But between the time of Socrates and Aristotle (three generations?) it became assimilated as a set of complementary skills; as the ability to make the written word do the work of the spoken word, with all the advantages that accrue from capturing and reworking ideas free of the constraints of immediate experience. To think out of the flow of time was the foundation for all analytical thinking—the foundation of philosophy. But this analytical thinking also fed back upon the spoken word, the concept of a good public speaker was influenced by the aesthetic of the rhetorical handbook.

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[611] Cole *op cit* p116

[612] See Cole *op cit* p121

[613] Cole *op cit* p158

[614] Jacqueline de Romilly. *The Great Sophists in Periclean Athens*. OUP 1992 p236

> When the tide ebbed, and things returned to normal, the wild ambitions of those early days bequeathed to the Sophists' successors not only the mature and useful science known as rhetoric but, alongside it, all the branches of research and the new disciplines that rhetoric had engendered. Grammar was one, with its enquiry into forms of speech and vocabulary, together with all the different studies connected with what we of the twentieth century have reverted to calling 'discourse', in the widest sense of the tern. Another of rhetoric's legacies was logic, for it is quite clear that, in this field as in others, Aristotle's chief contribution—as he himself recognised—was to draw theoretical lessons from the techniques which the Sophists had been the first to put to practical, empirical use.[615]

It was the Sophists who first made the distinction between custom/law and nature (*nomos* and *physis*). But these abstract ideas taken up by a wider public were modified: nature became not something universal and impersonal but human nature. No longer a debate about 'what is and what is not', it became a challenge to established order. And as Athens lost its former power the Sophists bore the blame. The new ideas began to be seen as justifying amoral and self seeking behaviour.

de Romilly argues the sophists had a '"lucid humanism" They did not accept absolute truths, nor justice, by divine agency but rather sought utility. "What is good for human beings comes along, with abundant proof to support it, to take the place of 'the good' pure and simple."[616] It was a civic virtue—be good to others because you are bound to them—that could be imparted by education. But the Sophists had no answer to the problem of the 'free rider', of those who behave badly and escape social sanction. Against this "the corner stone of Socrates' thought is the idea that an unjust action harms the one who commits it, damaging his soul, and nothing could be worse than this."[617] It is this concept, taken up and amplified by Plato as an abstract and absolute good, that prevailed.

> ... the case of the Sophists demonstrates how the conviction of their own originality can lead to over-confidence in the field of practical application, and also how soon those who follow the newly blazed trail strive to outdo their precursors. On the side of the public, we see how adulation may go hand in hand with hostility and irony, and how success itself brings in its wake distortion, exaggeration, and misunderstandings. Influence sometimes take unpredictable and undesired forms.
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> [...] Every new technique, every new doctrine, every new word introduced by thinkers gradually, sometime imperceptibly, changes the sensibility of all. [618]

[615] Jacqueline de Romilly. *The Great Sophists in Periclean Athens*. OUP 1992 p90

[616] Jacqueline de Romilly. *The Great Sophists in Periclean Athens*. OUP 1992 p187

[617] Jacqueline de Romilly. *The Great Sophists in Periclean Athens*. OUP 1992 p187

[618] Jacqueline de Romilly. *The Great Sophists in Periclean Athens*. OUP 1992 p239

Writing is not just an inert medium representing speech. Seen as a virtual tool, writing is prosthetic, it extends a human capability and thereby changes the relation of human to world.

Without writing (or some other means of recording) there can be no exact repetition, the *Mnemosyne* of the oral bard is not our concept of memory. Yet it does preserve and maintain a continuity of shared experience. Current practice is thereby sanctified as tradition, whilst an adaptation to the present world is maintained.

> Romantic theories of art replaced *mimesis* with *poiesis*, and the history of literacy, with its 'Gutenberg' practices and habits of mind, eliminated performance from many spheres of artistic life. Before writing, of course, all poetry was enjoyed exclusively through the bodily realities of performance; but for literates, verbal art came to mean 'literature', things that are read.]
>
> The opposition, again, is between a general discourse education that aims to cultivate an ethically responsible, well-reasoning, and articulate intelligence, and a newly popular but superficial training that aims only to cultivate a certain rhetor-esque fluency in a handful of civic speech genres. Isocrates, in sum holds out for the broader tradition of sophistic *logôn technê*, a tradition that includes pragmatic *rhêtoreia*—or has recently come to include it—but that cannot be limited to it, if an art worthy of being taught is to remain. [619]

Socrates in the *Gorgias* speaks of true and false arts, rhetoric is to justice as cookery is to medicine. Socrates demands of Gorgias what knowledge is specific to rhetoric [in effect a discipline must have a unique subject or competence], to Aristotle however it is not a subject in itself but a tool, like dialectic, that acts upon some other form of knowledge. (On Rhetoric I. 1. 1 and I. 2. 7)

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[619] Jeffrey Walker. *Rhetoric and Poetics in Antiquity*. OUP 2000 p33

Bibliography

> Milton produced *Paradise Lost* as a silkworm produces silk, as the activation of his own nature. He later sold his product for £5 and became a merchant.
>
> Karl Marx. Capital Vol.I (Penguin 1976 p1044)

Archives

Full references are given in the footnotes:

Sutherland Cambridge University Library, Department of Manuscripts and University Archives: *Add 8353.* Sutherland, Sir Gordon Brims Black McIvor (1907-1980) Physicist, [NRA 25481 Sutherland]

Pringle Oxford University: Bodleian Library, Special Collections and Western Manuscripts: —. Pringle, John William Sutton (1912-1982) Zoologist, correspondence and papers, [NRA 29728 Pringle]

Bates Wellcome Library for the History and Understanding of Medicine: Bates, John A. V. (1918-1993) and the 'Ratio Club',*GC/179.* [NRA 40212 Bates]

BECTEA London Metropolitan Archive

NAHC University of Manchester. National Archive for the History of Computing: National Research and Development Corporation *NRDC 86/46/2*

Woodger Science Museum Library: A Catalogue of the Papers of M. Woodger, left in room 113, Bldg 93, NPL Teddington on his retirement 1983-03-31

Yates Personal Collection of David Yates

PRO Public Record Office

Published Works

Where the original date of publication or title is historically significant and different from that of the edition consulted this has been noted in square brackets.

Harold Abelson, Gerald Jay Sussman. *Structure and Interpretation of Computer Programs* (2nd edn) MIT 1996

Jon Agar. Office Machinery of Government: Treasury O&M and the mechanisation of government work. [unpublished? paper Manchester U. 1997]

Christopher Alexander. *Notes on the Synthesis of Form*. Harvard UP. 1964

TE Allibone. 'Denis Gabor' in *Biographical Memoirs. Fellows Roy. Soc.* (26) 1980 pp. 107–147

Peter Alter. *The Reluctant Patron: science and the state in Britain 1850-1920.* (tr. Angela Davies) [*Wissenschaft, Staat, Mäzene. Anfänge moderner Wissenschaftspolitik in Grossbritannien 1850–1920*. Stuugart 1982] Oxford: Berg 1987

RD Anderson. *Universities and Elites in Britain since 1800.* CUP 1992

AM Andrew. *Brains and Computers*. Harrap 1963

F Ankersmit, H Kellner. *A New Philosophy of History*. Chicago,1995

Julia E Annas. *Hellenistic Philosophy of Mind*. U. California 1992

Bruce W Arden. On Introducing Digital Computing. *Communications ACM* (7, 4) April 1964 pp. 212–214

Aristotle. *On Poetry and Style* [the Poetics] (tr. GMA Grube) Hackett 1989

Aristotle. *On Rhetoric: a theory of civic discourse* (tr. George A Kennedy) OUP 1991

JA Armstrong. *The European Administrative Elite*. Princeton 1973

WR Ashby. Adaptiveness and Equilibrium. *Jnl. Mental Sci.* 1940 pp. 478–483

WR Ashby. Design for a Brain. *Electronic Engineering* December 1948 pp. 379–383 [see also correspondence in subsequent issue February 1949 pp. 62–63]

W Ross Ashby. *Design for a Brain*. (1st edn 1952) 2nd edn Chapman & Hall 1960

WR Ashby. Effect of Controls on Stability. *Nature* (155 3930) 24 February 1945

WR Ashby. The Facts and Methods of Cybernetics [review of N. Wiener. *Cybernetics*] *Jnl. Mental Sci.* July 1949 pp. 716–724

W Ross Ashby. *An Introduction to Cybernetics*. Methuen 1956

WR Ashby. Principles for the Quantitative Study of Stability in a Dynamic Whole System; with some applications to the nervous system. *Jnl. Mental Sci.* April 1946 p. 319–323

W Ross Ashby. What is Mind? objective and subjective aspects in cybernetics. in *Theories of the Mind* (ed. Jordan M Scher) Collier-Macmillan 1962

W Aspray. *Computing Before Computers*. Iowa 1990

William Aspray. The Scientific Conceptualization of Information: a survey. *Annals of the History of Computing* (7, 2) April 1985

William F Atchison, John W Hamblen. Status of Computer Sciences Curricula in Colleges and Universities. *Communications ACM* (7, 4) April 1964 pp. 225–227

Ayer AJ (ed). *Logical Positivism in Perspective: essays on* Language Truth and Logic. Croom Helm. 1987

AJ Ayer *et al.* The Revolution in Philosophy. Macmillan 1956

Charles Babbage. ——— *and his Calculating Engines: selected writings.* (eds. Philip Morrison, Emily Morrison) Dover1961

Charles Babbage. *Passages from the Life of a Philosopher*. Longman1864

Charles Babbage. *Reflections on the Decline of Science in England*. [1830] (ed. Martin Campbell-Kelly) Pickering & Chatto 1989

Francis Bacon. *Novum Organum* (tr. Thomas Fowler) Oxford 1884

Francis Bacon. *The New Organon* (tr. Michael Silverthorne) [*Novum Organum* 1620] CUP 2000

Lawrence Badash. The Completeness of Nineteenth-Century Science. *Isis* 63 (1972) pp. 48–58

Robert M Baer. *The Digital Villain: notes on the numerology, parapsychology, and metaphysics of the computer*. Addison-Wesley 1972

Cyril Bailey. *The Greek Atomists and Epicurus*. Oxford 1928

Cyril Bailey. *Prolegmena to Lucretius* de rerum natura, OUP 1947

Philip Ball. *The Self-Made Tapestry: pattern formation in nature*. OUP 1999

Stephen Bann. *The Inventions of History: essays on the representation of the past*. Manchester UP 1990

DW Barron. *An Introduction to the Study of Programming Languages*. CUP 1997

George Basalla. *The Evolution of Technology*. CUP 1988

Peter Baumgartner, Sabine Payr. *Speaking Minds: interviews with twenty eminent cognitive scientists*. Princeton 1995

John Bender, David E Wellbery eds. *The Ends of Rhetoric: history, theory, practice*. Stanford 1990

Henri Bergson. *Matter and Memory* (tr. NM Paul, WS Palmer) [*Matière et Mémoire* 1908] Zone Books 1991

Edmund Callis Berkley. *Giant Brains: or machines that think*. Wiley 1949

Isaiah Berlin. *Vico and Herder: two studies in the history of ideas*. Hogarth Press 1976

Meurig Beynon. Liberating the Computer Arts [paper presented by invitation at DALI 2001 (First International Conference on Digital and Academic Liberty of Information), University of Aizu, Japan, March 2001]

Wiebe E Bijker. *Of Bicycles Bakelites and Bulbs, toward a theory of sociotechnical change*. MIT 1995

PMS Blackett. *Studies of War: nuclear and conventional*. Oliver & Boyd 1962

B P Bloomfield (ed). *The Question of Artificial Intelligence*. Croom Helm 1987

Margaret A Boden. *The Philosophy of Artificial Intelligence*. OUP 1990

Margaret A Boden. *The Philosophy of Artificial Life*. OUP 1996

George Boole. *An Investigation of the Laws of Thought, on which are founded the mathematical theories of logic and probabilities*. London: Walton and Maberly 1854

Andrew D Booth. *Machine Translation*. North Holland 1967

Pierre Bourdieu. *Pascalian Meditations* (tr. Richard Nice) [*Méditations pascaliennes* 1997] Polity Press 2000

Pierre Bourdieu. *Practical Reason* [*Raisons Practiques* 1994] Polity Press 1998

BV Bowden. *Faster Than Thought: a symposium on digital computing machines*. Pitman 1953

Robert Boyle. *A Free Enquiry into the Vulgarly Received Notion of Nature*. [1686] CUP 1996

Franz Brentano. *Psychology from an Empirical Standpoint* [*Psychologie vom empirischen Standpunkte* 1874 & 1924] (ed. Oskar Kraus, tr. Antos C Rancurello, DB Terrell, Linda L McAlister) Routledge 1995

Frederick P Brooks. *The Mythical Man-Month: essays on software engineering*. Addison-Wesley 1975 & 1995

Rodney A Brooks. *Cambrian Intelligence: the early history of the new AI*. MIT 1999

Stuart Brown, John Fauvel, R Finnegan. *Conceptions of Inquiry*. Open University/Methuen 1981

Robert Buderi. *The Invention that Changed the World: the story of radar from war to peace*. Abacus 1998

JC Burnham. How the Idea of Profession Changed the Writing of Medical History. *Medical History* (Supplement 18). Wellcome Institute for the History of Medicine1998

Ben Delisle Burns. *The Uncertain Nervous System*. Arnold 1968

HW Buxton. *Memoir of the Life and Labours of the Late Charles Babbage Esq. FRS*. [1880] MIT/Tomash 1987

Joseph A Caron. 'Biology' in the Life Sciences: a historiographical contribution, *History of Science* xxvi (1988)

R Canary, H Kozicki H. *The Writing of History: literary form and historical understanding*. Wisconsin 1978

DSL Cardwell. *From Watt to Clausius: the rise of thermodynamics in the early industrial age*. Heineman 1971

DSL Cardwell. *The Organisation of Science in England*. Heineman 1972

Rudolf Carnap. *The Philosophical Foundations of Physics: an Introduction to the philosophy of science*. Basic Books 1966

Rudolf Carnap. *The Unity of Science*. (tr. M Black) Kegan Paul Trench Trubner 1934

BE Carpenter, RW Doran (eds). *A. M. Turing's ACE Report of 1946 and other papers*. MIT/Tomash 1986

ME Carvallo (ed). *Nature, Cognition and System* (3rd international conference on Systems Research, Informatics and Cybernetics, Baden-Baden Aug 1986). Kluwer 1988

Alfred Chapuis, Edmond Droz. *Automata: an historical and technical study*. (tr. Alec Reid) [*Les automates : figures artificielles d'hommes et d'animaux: histoireet technique*, 1949] Neuchâtel: Éditions du Griffon 1958

Paul Ceruzzi. Crossing the Divide: architectural issues and the emergence of the stored program computer, 1935–1955 *IEEE Annals of the History of Computing* (19, 1) 1997

Paul Ceruzzi. Electronics Technology and Computer Science, 1940–1975: a coevolution. *Annals of the History of Computing* (10, 4) 1989

B Chandrasekaran, J Glasgow, NH Narayanan (eds). *Diagramatic Reasoning: cognitive and computational perspectives*. AAAI/MIT 1995

Colin Cherry. *On Human Communication: a review, a survey, and a criticism*. MIT 1957

W Child. *Causality, Interpretation and the Mind*. OUP 1994

Noam Chomsky. *Aspects of the Theory of Syntax* (RLE special technical report 11) MIT 1965

Noam Chomsky. *Syntactic Structures*. Den Hague: Mouton 1957

Paul M Churchland. *Matter and Consciousness*. (rev. ed.) MIT 1984

RH Coase. *The Firm, the Market, and the Law*. U. Chicago P. 1988

J Cockcroft (ed). *The Organization of Research Establishments*. CUP 1965

Emmeline W Cohen. *The Growth of the British Civil Service 1780-1939*. Allen & Unwin1941 [reprinted, Cass 1965]

H Floris Cohen. *The Scientific Revolution: a historiographical inquiry*. Chicago 1994

Thomas Cole. *The Origins of Rhetoric in Ancient Greece*. Johns Hopkins 1991

Stefan Collini. *English Pasts: essays in history and culture*. OUP 1999

Harry Collins, Trevor Pinch. *The Golem at Large: what you should know about technology*. CUP 1998

Etienne Bonnot de Condillac. *Essay on the Origin of Human Knowledge* (tr. Hans Aarsleff) [*Essai sur l'origine des connaissances humaines* 1746] CUP 2001

B Jack Copeland, Diane Proudfoot. On Alan Turing's Anticipation of Connectionism. *Synthese* 108 1996 pp. 361–367

John Cornwell. *Nature's Imagination: the frontiers of scientific vision*. OUP 1995

Rodney Cotterill. *Enchanted Looms: conscious networks in brains and computers*. CUP 1998

Kenneth JW Craik. *The Nature of Explanation*. [1943, reprinted 1952] (reprint with postscript) CUP 1967

Kenneth JW Craik. Theory of the Human Operator in Control Systems: I. the operator as an engineering system. *Brit. Jnl. Psychology* XXXVIII(2) December 1947 pp. 56–61

Kenneth JW Craik. Theory of the Human Operator in Control Systems: II. man as an element in a control system. *Brit. Jnl. Psychology* XXXVIII(3) March 1948 pp. 142–148

Florian Cramer. Free Software as Collaborative Text. [conference paper *Interface 5* Warburg-Haus Hamburg 15. Sept 2000] http://userpage. fu-berlin. de/~cantsin/homepage/writings/copyleft/free_software/free_software_as_text/en//free_software_as_text. html 14. June. 2002

Mary Croarken. *Early Scientific Computing in Britain*. OUP 1990

JG Crowther. *Statesmen of Science*. Cressett 1965

O-J Dahl, EW Dijkstra, CAR Hoare. *Structured Programming*. Academic Press 1972

T Dalyell. *A Science Policy for Britain*. Longman 1983

Oliver Darrigol. Baconian Bees in the Electromagnetic Fields: experimenter-theorists in nineteenth-century electrodynamics. *Stud. Hist. Mod. Phys.* 30. 3 (1999) pp. 307–345

Joseph Ben-David, Randall Collins. Social Factors in the Origins of a New Science: the case of psychology. *American Sociological Review* 31. 4 (1966) pp. 451–465

DW Davies, DLA Barber, WL Price, CM Solomonides. *Computer Networks and Their Protocols*. Wiley 1979

Martin Davis. Mathematical Logic and the Origin of Modern Computers. in *The Universal Turing Machine* (ed. R Herken) OUP 1988

PB Denes, EN Pinson EN. *The Speech Chain: the physics and biology of spoken language*. (2nd edn) WH Freeman 1993

Daniel C Dennett. *Conciousness Explained*. Penguin 1993

René Descartes. *Meditations on First Philosophy: with selections from the objections and replies*. (tr. John Cottingham) [*Meditationes de prima philosophia* 1641] CUP 1996

Marcel Detienne. *The Masters of Truth in Archaic Greece*. (tr. Janet Lloyd) [*les Maîtres de vérité dans la grèce archaïque*, 1967] Zone Books. 1990

Keith Devlin. *Goodbye, Descartes: the end of logic and the search for a new cosmology of mind*. Wiley 1997

EJ Dijksterhuis. *The Mechanization of the World Picture*. (tr. C Dikshoorn) [*De Mechanisering van het Wereldbeeld*, Amsterdam 1950] OUP 1961

Edsger W Dijkstra. *A Discipline of Programming*. Prentice Hall 1976

Edsger W Dijkstra. The End of Computing Science? *Communications of the ACM* 44(3) March 2001

Edsger W Dijkstra. Goto Statement Considered Harmful. *Communications of the ACM* 11(3) March 1968 pp. 147-148

EW Dijkstra. The Humble Programmer ACM Turing Lecture 1972 [http://www.cs.utexas.edu/users/EWD/ewd03xx/EWD340.PDF 30 Sept 2002]

EW Dijkstra. On a Cultural Gap. *The Mathematical Intelligencer* (8. 1) 1986

Edsger W Dijkstra. *Selected Writings on Computing: a personal perspective*. Springer-Verlag 1982

Edsger W Dijkstra What led to "Notes on Structured Programming" EWD1308-0 [http://www.cs.utexas.edu/users/EWD/ 19 January 2003]

Michael Duggett. The Evolution of the UK Civil Service 1848–1997. British Council, July 1997 http://www.britishcouncil.org/governance/manag/civil/ (07 July 2002)

Jean-Pierre Dupuy. *The Mechanization of the Mind: on the origins of cognitive science*. (tr. MB DeBevoise) [*Aux origines des sciences cognitives* 1994] Princeton UP 2000

George Dyson. *Darwin among the Machines*. Penguin 1999

WJ Eckert. *Punched Card Methods in Scientific Computation*. [1940] Babbage Institute/MIT 1984
Umberto Eco. *Kant and the Platypus: essays on language and cognition*. Secker & Warburg, 1999
David Edgerton. *Science, Technology and the British Industrial 'Decline', 1870–1970.* CUP 1996
Paul N Edwards. *The Closed World: computers and the politics of discourse in cold war America*. MIT 1996
Norbert Elias, Herminio Martins, Richard Whitley (eds). *Scientific establishments and hierarchies* Dordrecht: Reidel , 1982
Chris Eliasmith. The Third Contender: a critical examination of the dynamicist theory of cognition. in *Mind Reading: introductory selections on cognitive science* (ed. Paul Thagard) MIT 1998
WS Elliott. *The Computer—Master or Servant*. (Inaugural lecture as Professor of Computing) Imperial College. 27 February 1968
Epicurus. *The Epicurus Reader: selected writings and testimonia*. (tr. Brad Inwood, LP Gerson) Hackett 1994
Dylan Evans. The Search Hypothesis of Emotion. *British Journal for the Philosophy of Science.* 53 (2002) pp497–509
Stephen Everson. Epicurus on Mind and Language. in *Language* (ed. S. Everson) CUP 1994
Anthony Farrington. *The Records of the East India College Haileybury & other institutions.* (India Office Records, Guides to Archive Groups) HMSO 1976
Edward A Feigenbaum, Julian Feldman (eds). *Computers & Thought* [1963] AAAI/MIT 1995
ES Ferguson. *Engineering and the Mind's Eye*. MIT 1992
Paul Feyerabend. *Against Method: outline of an anarchistic theory of knowledge*. NLB 1975
Richard P Feynman. *Lectures on Computation*. Penguin 1999
MI Finley. *The Ancient Greeks*. Penguin 1966
Gemma Corradi Fiumara. *The Metaphoric Process: connections between language and life*. Routledge 1995
Ludwick Fleck. *Genesis and Development of a Scientific Fact*. (tr. Fred Bradley, Thaddeus J Trenn) [*Entstehung und Entwicklung einer wissenschaftlichen Tatsache: Einführung in die Lehre vom Denkstil und Denkkollektiv* Basle: 1935] Chicago 1979
Jerry A Fodor. *In Critical Condition: polemical essays on cognitive science and philosophy of mind*. MIT 1998
Jerry A Fodor. *The Language of Thought*. Harvester 1976
George E Forsythe. An Undergraduate Curriculum in Numerical Analysis. *Communications ACM* (7, 4) April 1964 pp. 214–215
GE Forsythe. What To Do Till the Computer Scientist Comes. *American Mathematical Monthly* (75) 1968 pp. 454–462
Michel Foucault. *The Order of Things: an archaeology of the human sciences.* [*Les mots et les choses* 1966] Routledge 2002
Don Fowler. Second Thoughts on Closure. in *Classical Closure* (eds DH Roberts, FM Dunn, D Fowler) Princeton 1997
George Frazer, Neville D Watts (eds). *Process Control and Automation* (vol. 5, no. 11) [issue devoted to computing] November 1958

Geoffrey K Fry. Civil Service Systems in Comparative Perspective. [paper presented at Indiana University April 1997] http://www.indiana.edu/~csrc/fry1.html (07 July 2002)

Steve Fuller. *Science*. Open University 1997

Steve Fuller. *The Governance of Science*. Open University 2000

Dennis Gabor. *Innovations: scientific, technological and social*. OUP 1970

Dennis Gabor. Inventing the Future. *Encounter* (XVI, 5) May 1960

Richard P Gabriel Master of Fine Arts in Software. <http://www.dreamsongs.com/MFASoftware.html> [14. June. 2002]

Richard P Gabriel, Ron Goldman Mob Software: the erotic life of code. <http://www.dreamsongs.com/MobSoftware.html> [14. June. 2002]

Hans-Georg Gadamer. *Reason in the Age of Science* (tr. Frederick G. Lawrence) [*Vernunft im Zeitalter der Wissenschaft*, 1976] MIT 1981

Hans-Georg Gadamer. *The Relevance of the Beautiful and other essays*. (tr. Nicholas Walker) CUP 1986

Galileo Galilei. *Dialogues Concerning Two New Sciences*. [*Discorsi e Dimostrazioni Matematiche intorno à due nuoue scienze*, 1638] (tr. Henry Crew, Alfonso deSalvio [1914]) Dover 1954

Peter Galison, David J Stump. *The Disunity of Science: boundaries, contexts, and power*. Stanford 1995

Howard Gardner. *The Mind's New Science: a history of the cognitive revolution*. Basic Books 1987

Martin Gardner. *Logic Machines and Diagrams*. McGraw-Hill 1958

Sebastian Gardner. *Kant and the* Critique of Pure Reason. Routledge 1999

Alan Gauld, John Shotter. *Human Action and its Psychological Investigation*. Routledge & Kegan Paul 1977

Frank H George. *The Brain as a Computer*. Pergamon 1961

Timothy van Gelder. Dynamics and Cognition in *Mind Design II* (ed. John Haugeland) MIT 1996

Timothy van Gelder, Robert F Port. It's About Time: an overview of the dynamical approach to cognition. in *Mind as Motion* (eds RF Port, T van Gelder) MIT 1995

Stanley Gill. The Philosophy of Programming. *Annual Review in Automatic Programming I* (Papers read at the Working Conference on Automatic Programming of Digital Computers, Brighton, 1–3 April 1959) Pergamon Press 1959

Eduard Glas. Model-Based Reasoning and Mathematical Discovery: the case of Felix Klein. *Studies in the History and Philosophy of Science*. March 2000, 31A(i) pp71-86

Eduard Glas. Socially Conditioned Mathematical Change: the case of the French Revolution. *Studies in the History and Philosophy of Science*. 33 (2002) pp709–728

Janice Glasgow, N Nari Narayanan, B Chandrasekaran (eds). *Diagramatic Reasoning: cognitive and computational perspective*. AAAI Press 1995

David K Glidden. Epicurean *Prolêpsis*. *Oxford Studies in Ancient Philosophy* III (1985) pp175–215

Herman H Goldstine. *The Computer: from Pascal to von Neumann*. Princeton 1972

EH Gombrich. *Art and Illusion: a study in the psychology of pictorial representation*. (The A. W. Mellon Lectures in the Fine Arts 1956) [1st edn 1960, 2nd edn 1962] (3. edn) Phaidon 1968

EH Gombrich. *The Story of Art*. Phaidon 1950

David Gooding. *Experiment and the Making of Meaning*. Kluwer 1990

Brian C Goodwin. *How the Leopard Changed its Spots: the evolution of complexity*. Weidenfield & Nicolson 1994

Brian C Goodwin. *Temporal Organisation in Cells, a dynamic theory of cellular control processes*. Academic Press 1963

Saul Gorn. Mechanical Languages: a course specification. *Communications ACM* (7, 4) April 1964 pp. 219–222

Johann Götschl (ed.) *Erwin Schrödinger's World View: the dynamics of knowledge and reality*. Kluwer 1992

GW Grace. *The Linguistic Construction of Reality*. Croom-Helm 1987

Paul Graham. *ANSI Common Lisp*. Prentice Hall 1996

Paul Graham. Beating the Averages (presented at: *Franz Developer Symposium* Cambridge MA. 25 March 2001) <http://www.paulgraham.com/paulgraham/avg.html> [14. June. 2002]

Paul Graham *On Lisp: advanced techniques for Common Lisp*. Prentice Hall 1994

Edward Grant. *The Foundations of Modern Science in the Middle Ages: their religious, institutional, and intellectual contexts*. CUP 1996

M Greenberger (ed). *Computers and the World of the Future*. MIT 1962

Richard L Gregory. *Eye and Brain: the psychology of seeing*. [1966] (5th edn) OUP 1998

Richard L Gregory. *Mind in Science: a history of explanations in psychology and physics*. Weidenfield & Nicolson 1981

Andrew Grosso. Legally Speaking. *Communications of the ACM* 45(2) February 2002

Ian Hacking. *The Social Construction of What?* Harvard UP 1999

H Haken. *Principles of Brain Functioning: a synergetic approach to brain activity, behaviour and cognition*. Springer 1996

Hermann Haken, Anders Karlqvist, Uno Svedin (eds). *The Machine as Metaphor and Tool.* Springer-Verlag 1993

Daniel Stephen Halacy. *Computers: the machines we think with*. Harper & Row 1962

Ward C Halstead (ed). *Brain and Behaviour: a symposium*. California 1950

John Halverson. Havelock on Greek Orality and Literacy. *Journal of the History of Ideas* 1992 pp. 148-163

CW Hanson, M Phillips. *The Foreign Language Barrier: in science and technology*. ASLIB 1962

David Harel. *Algorithmics: the spirit of computing*. Addison-Wesley 1987

PM Harman. *The Natural Philosophy of James Clerk Maxwell.* CUP 1998

PM Harman (ed). *Wranglers and Physicists: studies on Cambridge physics in the nineteenth century*. Manchester UP 1985

Rom Harrè (ed). *The Physical Sciences Since Antiquity*. Croom Helm 1986

A Harrington. *Reenchanted Science: holism in German culture from Wilhelm II to Hitler*. Princeton 1996

Brian Harrison. The Rise, Fall and Rise of Political Consensus in Britain since 1940. *History* (84. 274) 1999 pp. 301–324

MG Hartley. *An Introduction to Analogue Computers.* Methuen 1962

DR Hartree *Calculating Machines: recent and prospective developments and their impact on mathematical physics.* [CUP 1947] MIT/Tomash 1984

DR Hartree *Calculating Instruments and Machines* [U. Illinois P 1949] MIT/Tomash 1984

Ulf Hashagen, Reinhard Keil-Slawik, Arthur Norberg (eds). *History of Computing: software issues*. Springer 2002

John Haugeland (ed). *Mind Design II: philosophy psychology artificial intelligence*. MIT 1996

Eric Havelock. *The Muse Learns to Write: reflections on orality and literacy from antiquity to the present*. Yale 1986

Eric Havelock. *Preface to Plato*. Harvard UP. 1963

Michael Heim. *Electric Language: a philosophical study of word processing*. Yale UP 1987

Michael Heim. *The Metaphysics of Virtual Reality*. OUP 1993

Steve J Heims. *Constructing a Social Science for Postwar America: the cybernetics group 1946–1953*. MIT 1993

Steve J Heims. *John von Neumann and Norbert Wiener: from mathematics to the technologies of life and death*. MIT 1980

J Hendry. *Innovating for Failure: government policy and the early British computer industry*. MIT 1989

R Herken (ed.). *The Universal Turing Machine: a half century survey*. OUP 1988

DJ Hess. *Science Studies: an advanced introduction*. New York UP 1997

Yehoshua Bar-Hillel. *Aspects of Language: essays and lectures on philosophy of language, linguistic philosophy and methodology of linguistics*. Amsterdam: North Holland 1970

Yehoshua Bar-Hillel. *Language and Information: selected essays on their theory and application*. Addison-Wesley1964

Yehoshua Bar-Hillel. The Present Status of Automatic Translation of Languages. *Advances in Computers* (1) Academic Press 1960

ES Hiscocks. *Laboratory Administration*. Macmillan 1956

CAR Hoare. Programming: Sorcery or Science. *IEE Software Review* April 1984

Thomas Hobbes. *Leviathan*. [1651] (ed. Richard Tuck) CUP 1996

J-M Hoc, TRG Green, R Samurçay, DJ Gilmore (eds). *Psychology of Programming*. Academic Press 1990

Andrew Hodges. *Alan Turing: the enigma*. Vintage 1992

Douglas R Hofstadter. *Gödel, Escher, Bach: an eternal golden braid*. Penguin 1979

John H Holland. *Adaptation in Natural and Artificial Systems: an introductory analysis with applications to biology, control and artificial intelligence*. [1st edn 1975] MIT 1992

John H Holland. *Emergence: from chaos to order*. OUP 1998

John H Holland. *Hidden Order: how adaptation builds complexity*. Perseus 1995

John Hollander. *The Work of Poetry*. Columbia 1997

Max Horkheimer. *The Eclipse of Reason*. OUP 1947

Edward Hughes. Civil Service Reform 1853–5. *Public Administration* XXXII (1954)

David Hume. *A Treatise of Human Nature*. [1740] OUP 2000

David Hume. *An Enquiry Concerning Human Understanding*. [1748] OUP 1999

Andrew Hull. War of Words: the public science of the British scientific community and the origins of the Department of Scientific and Industrial Research, 1914-16. *British Jnl. Hist. Sci.* (32. 4) December 1999

Andrew Hunt, David Thomas. *The Pragmatic Programmer: from journeyman to master*. Addison-Wesley 2000

Bruce J Hunt. *The Maxwellians*. Cornell UP 1991

Bruce J Hunt. The Ohm is Where the Art Is: British telegraph engineers and the development of electrical standards. *Osiris* 9 (1994) pp. 48–63

GMK Hunt. Is Philosophy a 'Theory of Everything'? in *The Impulse to Philosophise*. (ed. A Phillips Griffiths) Cambridge University Press, 1992 pp. 219–231

WJ Hutchins. *Machine Translation: past, present and future*. Ellis Horwood 1986

IRE. 1956 Symposium on Information Theory (MIT Sept 10–12, 1956) *IRE Trans. Inf. Theory*. (IT-2, 3) September 1956

John Archer Jackson (ed). *Professions and Professionalization*. CUP 1970

Nicolas Jardine. Uses and Abuses of Anachronism in the History of the Sciences, *History of Science* xxxviii(2000) p251-270

Nicolas Jardine. *The Fortunes of Inquiry*. OUP 1986

Nicolas Jardine. *The Scenes of Inquiry: on the reality of questions in the sciences*. OUP 2000

Douglas Jay. *The Socialist Case*. [1938] (2nd edn) Faber 1947

Lloyd A Jeffress (ed). *Cerebral Mechanisms in Behaviour: the Hixon Symposium*. [1951] Hafner Publishing Co. 1967

H Jones. *The Epicurean Tradition*. Routledge 1989

Karen Sparck Jones, Julia R Galliers. *Evaluating Natural Language Processing Systems: an analysis and review*. Springer 1996

RV Jones (ed). A Discussion on the Effects of the Two World Wars on the Organization and Development of Science in the United Kingdom. *Proc. R. Soc. Lond.* A. 342 (1975) pp. 439–586

RV Jones. *Most Secret War: British scientific intelligence 1939–45*. Hodder and Stoughton 1979

Horace Freeland Judson. *The Eighth Day of Creation: makers of the revolution in biology*. Penguin 1995

Robert Kargon. Model and Analogy in Victorian Science: Maxwell's critique of the French physicists. *Jnl. Hist. Ideas* XXX. 3 (1969) pp. 423–436

Edward Kasner, James Newman. *Mathematics and the Imagination*. [1940] Penguin 1968

Stuart Kauffman. *At Home in the Universe: a search for laws of complexity*. OUP 1995

Thomas A Keenan. Computers and Education. *Communications ACM* (7, 4) April 1964 pp. 205–209

Evelyn Fox Keller. *Secrets of Life, Secrets of Death: essays on science and culture*. Routledge 1992

Donald R Kelly. *Faces of History: historical inquiry from Herodotus to Herder*. Yale UP 1998

Martin Campbell-Kelly. Data Communications at the National Physical Laboratory (1965–1975) *Annals of the History of Computing* (9, 3/4) 1988

Martin Campbell-Kelly. The Development of Computer Programming in Britain (1945 to 1955). *Annals of the History of Computing* (4, 2) April 1982

Martin Campbell-Kelly, William Aspray. *Computer: a history of the information machine*. Basic Books 1996

Martin Campbell-Kelly, Michael R Williams (eds). *The Moore School Lectures: theory and techniques for design of electronic digital computers*. [1948] MIT/Tomash 1985

JAS Kelso. *Dynamic Patterns: the self organisation of brain and behaviour*. MIT 1995

George A Kennedy. *The Art of Persuasion in Greece*. Princeton 1963

George A Kennedy. *Classical Rhetoric: and its Christian and secular tradition from ancient to modern times*. Univ. North Carolina 1980

Paul Kennedy. *The Rise and Fall of the Great Powers: economic change and military conflict from 1500 to 2000*. Unwin Hyman 1988

Brian W Kernighan, Rob Pike. *The Practice of Programming*. Addison-Wesley 1999

Brian W Kernighan, Dennis M Ritchie. *The C Programming Language*. (2nd edn) Prentice Hall 1988

Martin J Klein. Mechanical Explanation at the End of the Nineteenth Century. *Centaurus* 17 (1973) pp. 58–82

Morris Kline. *Mathematics in Western Culture*. [1953] Penguin 1972

Donald E Knuth. *The Art of Computer Programming I: fundamental algorithms*. [1st edn 1968] (3rd edn) Addison-Wesley 1998

Donald E Knuth. *The Art of Computer Programming II: seminumerical algorithms*. [1st edn 1969] (3rd edn) Addison-Wesley 1998

Donald E Knuth. *The Art of Computer Programming III: sorting and searching*. [1st edn 1973] (2nd edn) Addison-Wesley 1998

Donald E Knuth. The Art of Computer Programming. Preface to first fascicle May 1999 http://www-cs-faculty.stanford.edu/~knuth/fasc1.ps.gz 14. June. 2002

Donald E Knuth. Computer Programming as an Art *ACM Turing Award Lecture* 1974 [in *Literate Programming* 1992]

Donald E Knuth. *Digital Typography*. Stanford: CSLI 1999

Donald E Knuth. *Literate Programming*. Stanford: CSLI 1992

Donald E Knuth. *Selected Papers on Computer Science*. CUP 1996

Donald E Knuth, Luis Trabb Pardo. The Early Development of Programming Languages. in *A History of Computing in the Twentieth Century* (eds. N Metropolis, J Howlett, Gian-Carlo Rota) Academic Press 1980

Robert R Korfhage. Logic for the Computer Sciences. *Communications ACM* (7, 4) April 1964 pp. 216–218

H Kozicki. *Developments in Modern Historiography*. Macmillan/OU 1993

EA Krause. *Death of the Guilds: professions, states, and the advance of capitalism 1930 to the present*. Yale 1996

Peter Kugel. Computing Machines Can't Be Intelligent (...and Turing Said So). *Minds and Machines.* 12 (2002) pp563–579

Thomas S Kuhn. *The Copernican Revolution: planetary astronomy in the development of western thought*. Harvard 1957

Thomas S Kuhn. *The Essential Tension: selected studies in scientific tradition and change*. Chicago 1977

Thomas S Kuhn. *The Structure of Scientific Revolutions*. [1962] (2nd edn) U Chicago P 1970

Thomas E Kurtz. BASIC in *ACM, History of Programming Languages*. Academic Press 1981

Imre Lakatos. Methodology of Scientific Research Programmes. in *Criticism and the Growth of Knowledge* (I Lakatos, A Musgrave eds) CUP 1970

Imre Lakatos. Proofs and Refutations (I) *Brit. Jnl. Phil. Sci.* XIV(53) May 1963

Imre Lakatos, Alan Musgrave (eds). *Criticism and the Growth of Knowledge* [proceedings of the International Colloquium in the Philosophy of Science, London, 1965,vol 4]. CUP 1970.

G Lakoff, M Johnson. *Metaphors We Live By*. Chicago 1980

Ladina Bezzola Lambert. *Imagining the Unimaginable: the poetics of early modern astronomy*. Amsterdam: Rodophi 2002

T Lange. *Rethinking Higher Education*. IEA 1998

P Laplante. *Great Papers in Computer Science*. West Publishing 1996

KS Lashley. *Brain Mechanisms and Intelligence: a quantitative study of injuries to the brain*. [1929] Dover 1963

Bruno Latour. *Pandora's Hope: essays on the reality of science studies.* Harvard 1999

Simon Lavington. *Early British Computers: the story of vintage computers and the people who built them.* Manchester UP 1980

Richard Layard, John King, Claus Moser. *The Impact of Robbins*. Penguin 1969

DE Leary. *Metaphors in the History of Psychology*. CUP 1990

FR Leavis. *Nor Shall My Sword.* Chatto & Windus 1972

GW Leibniz. *Philosophical Essays* (tr. Roger Ariew, Daniel Garber) Hacket 1987

MC Lemon. *The Discipline of History and the History of Thought.* Routledge 1995

Timothy Lenoir (ed). *Inscribing Science: scientific texts and the materiality of communication*. Stanford 1998

Timothy Lenoir. *Instituting Science: the cultural production of scientific disciplines.* Stanford UP 1997

Hector Levesque (ed). Taking Issue/Forum: a critique of Pure Reason. *Computational Intelligence* (3, 3) August 1987

Y Lettvin, HR Maturana, WS McCulloch, WH Pitts. What the Frog's Eye Tells the Frog's Brain. *Proc. IRE.* 1959

JCR Licklider. Man-Computer Symbiosis. *IRE Transactions on Human Factors in Electronics* March 1960 pp. 4–11

OJM Lindsay. 'John Antony Hardinge Gifford, 3rd Earl of Halsbury' in *Biographical Memoirs. Fellows Roy. Soc.* (47) 2001 pp. 241–253

C Lloyd. *The Structures of History*. Blackwell 1993

Genevieve Lloyd. *Being in Time: selves and narrators in philosophy and literature*. Routledge 1993

WN Locke, AD Booth (eds). *Machine Translation of Languages: fourteen essays.* MIT/Wiley 1955

AA Long (ed). *Early Greek Philosophy*. Cambridge UP 1999

John Losee. *A Historical Introduction to the Philosophy of Science*. (3rd edn) OUP 1993

Lucretius. *De rerum natura* (with tr. WHD Rouse, Martin Ferguson Smith) Loeb Classical Library 1992

James McAllister. *Beauty and Revolution in Science*, Cornel 1996

John McCarthy. Programs with Common Sense in *NPL. Symposium on the Mechanisation of Thought Processes* 1959

John McCarthy. Recursive Functions of Symbolic Expressions and their Computation by Machine, Part 1 [*Communications ACM* April 1960] http://www-formal.stanford.edu/jmc/recursive.html 22 June 2002

Pamela McCorduck. *Machines Who Think: a personal inquiry into the history and prospects of artificial intelligence*. WH Freeman 1979

Warren S McCulloch, Walter Pitts. A Logical Calculus of the Ideas Immanent in Nervous Activity. *Bulletin of Mathematical Biophysics* (5) 1943 pp. 115–133

Warren S McCulloch. *The Embodiment of Mind*. MIT 1965

J McDaniel, WL Price, AJM Szanser, DM Yates. An Evaluation of the Usefulness of Machine Translations Produced at the National Physical Laboratory, Teddington, with a Summary of the Translation Methods. *2ème Conference Internationale sur le Traitment Automatique des Langues.* Grenoble 23–25 August 1967. [photocopy supplied by DM Yates. April 1999]

J McDaniel, AM Day, WL Price, AJM Szanser, S Whelan, DM Yates. Translation of Russian Scientific Texts into English by Computer—a final report. (Auto 35) Autonomics Division, National Physical Laboratory July 1967

Donald M MacKay. *Analogue Computing at Ultra-High Speed: an experimental and theoretical study*. Chapman & Hall. 1962

Donald M MacKay. *Freedom of Action in a Mechanistic Universe* (21st Arthur Stanley Eddington Memorial Lecture, Cambridge University 17 November 1967). CUP 1967

Donald M MacKay. *Information, Mechanism and Meaning*. MIT 1969

Donald A MacKenzie. *Knowing Machines: essays on technical change*. MIT 1996

Richard D McKirahan. *Philosophy Before Socrates: an introduction with texts and commentary*. Hackett 1994

Marshall McLuhan. *Understanding Media: the extensions of man*. [1964] Routledge 2001

Harold Macmillan. *The Middle Way*. [1938] Macmillan 1966

RH Macmillan. *Automation, friend or foe?* CUP 1956

RH MacMillan. *Non-Linear Control Systems Analysis.* Pergamon Press 1962

J MacMurray. *The Self as Agent: the Gifford Lectures 1953*. Faber 1957

A Malachowski. *Reading Rorty: critical responses to* Philosophy and the Mirror of Nature *and beyond*. Blackwell 1990

Robert Mandrou. *From Humanism to Science 1480–1700*. Penguin 1978

K Mannheim. *Ideology and Utopia: an introduction to the sociology of knowledge*. (tr. Louis Wirth, Edward Shils.) [*Ideologie und Utopie*] Kegan Paul & Co. 1936

PR Masani. *Norbert Wiener 1894–1964.* Basel: Birkhäuser 1990.

James Clerk Maxwell. *A Treatise on Electricity and Magnetism* Vol I [1891] OUP 1998

James Clerk Maxwell. Essay for the Apostles on 'Analogies in Nature' [1856] in *The Scientific Letters and Papers of James Clerk Maxwell Vol I 1846–1862* (ed. PM Harman) CUP 1990

James Clerk Maxwell. Lecture on Faraday's Lines of Force [1873] in *The Scientific Letters and Papers of James Clerk Maxwell Vol II 1862–1873* (ed. PM Harman) CUP 1995

K Menninger. *Number Words and Number Symbols: a cultural history of numbers* [1958] MIT 1969

Nicholas Metropolis, N Howlett, Gian-Carlo Rota. *A History of Computing in the Twentieth Century*. Academic Press 1980

George A Miller, Eugene Galanter, Karl H Pribram. *Plans and the Structure of Behaviour*. Holt, Rinehart & Winston 1960

Louis O Mink. The Autonomy of Historical Understanding. *History & Theory* V,1 1966 pp. 24–47

Marvin Minsky. *Computation: finite and infinite machines*. Prentice Hall 1967

Marvin Minsky. Form and Content in Computer Science. *Journal of the ACM* (17. 2) April 1970 pp. 197–215

Marvin Minsky. *Semantic Information Processing*. MIT 1968

Marvin Minsky, *Seymour Papert. Perceptrons: an introduction to computational geometry*. MIT 1969

Philip Mirowski. *Against Mechanism: protecting economics from science*. Rowman & Littlefield 1988

Philip Mirowski. Cyborg Agonistes. *Social Studies of Science* 29/5 (October 1999) pp685–718

Philip Mirowski. *More Heat Than Light: economics as social physics: physics as nature's economics*. CUP 1989

Jacques Monod. *Chance and Necessity: an essay on the natural philosophy of modern biology*. (tr. Austryn Wainhouse) [1970] Penguin 1997

Glyn Moody. *Rebel Code: Linux and the open source revolution*. Allen Lane 2001

RJ Moore. The Abolition of Patronage in the Indian Civil Service and the Closure of Haileybury College. *Historical Journal* VII , 2 (1964) pp246–57

Kevin Moore. A Brief History of Aircraft Flight Simulation. <http://homepage.ntlworld.com/bleep/SimHist1.html> [24 June 2002]

R Moseley. Science Government & Industrial Research: the origins and development of the National Physical Laboratory 1900–1975. (unpublished D. Phil) University of Sussex 1976

David E Muller. The Place of Logical Design and Switching Theory in the Computer Curriculum. *Communications ACM* (7, 4) April 1964 pp.222–225

Gilbert Murray. *The Rise of the Greek Epic* (4. edn) OUP 1934

Oswyn Murray. *Early Greece*. Stanford UP 1983

Makoto Nagao. *Machine Translation, how far can it go?* (tr. Norman Cook) [*kikai hon'yaku wa doko made kano ka*. Tokyo 1986] OUP 1989

Thomas Nagel. *The View from Nowhere*. OUP 1989

National Physical Laboratory. *The Direction of Research Establishments*: proceedings of a symposium held at the National Physical Laboratory on 26th, 27th, and 28th September 1956. HMSO 1957

National Physical Laboratory. *Mechanisation of Thought Processes*: proceedings of a symposium held at the National Physical Laboratory on 24th, 25th, and 27th November 1958. [2 vols] HMSO 1959

National Physical Laboratory. 1961 *International Conference on Machine Translation of Languages and Applied Language Analysis*. (Proceedings, Symposium No. 13, 5–8 September 1961) HMSO 1962

National Physical Laboratory. Report for the Year: 1901—1969. HMSO [Bound copy in Imperial College/Science Museum Library {card index only}]

Reviel Netz. Counter Culture: towards a history of Greek numeracy. *History of Science* 40(3) 129 September 2002 pp.321–352

John von Neumann. *The Computer and the Brain*. Yale 1958

John von Neumann. First Draft of a Report on the EDVAC, Moore School of Electrical Engineering, U. Pennsylvania, 30 June 1945 (ed Michael D Godfrey) *IEEE Annals of the History of Computing* (15, 4) 1993 pp. 27–75

John von Neumann. *Theory of Self-Reproducing Automata* (edited and completed by Arthur W Burk) Illinois 1966

Marie Neurath. *Machines Which Seem to Think*. London: Max Parrish 1954

A Newell, HA Simon. *Human Problem Solving*. Prentice-Hall 1972

Allen Newell, Herbert Simon. Computer Science as Empirical Inquiry: symbols and search. *Communications of the ACM*, March 1976, pp 113-126.

Isaac Newton. *The* Principia*: mathematical principles of natural philosophy*. (tr. I Bernard Cohen, Anne Whitman) [1st edn 1687, 3rd edn 1726] California 1999

John Newton (ed). *Computers in Translation: a practical appraisal*. Routledge 1992

AL Norberg, JE O'Neill. *Transforming Computer Technology: information processing for the Pentagon 1962–1986*. Johns Hopkins 1996

Sir Stafford Northcote. [The Northcote-Trevelyan Report] (paper 1713, February 1854). *Parliamentary Papers* 1854 XXVII. reprinted in *Public Administration* XXXII (1954)

JM Nyce, P Kahn (eds). *From Memex to Hypertext: Vannevar Bush and the minds machine*. Academic Press 1991

Mary Jo Nye. *Before Big Science: the pursuit of modern chemistry and physics 1800–1940*. Prentice Hall 1996

OECD. *University Research in Transition*. OECD 1998

Minako O'Hagan. *The Coming Industry of Teletranslation: overcoming communication barriers through telecommunication*. Clevedon: Multilingual Matters 1996

Walter J Ong. *Orality and Literacy*. Methuen 1982

AI Oparin. *The Origin of Life on the Earth*. (tr. Ann Synge) [*Vozniknovenie zhizni nu zemole*. Moscow: 1936, 2nd edn 1941. New York: 1938 & 1953] 3rd edn Oliver & Boyd 1957

Derek Partridge, Yorick Wilks (eds). *The Foundations of Artificial Intelligence: a sourcebook*. CUP 1990

Gordon Pask. *An Approach to Cybernetics*. Hutchinson 1961

Harold Perkin. *The Rise of the Professional Society: England since 1880*. Routledge 1989

Harold Perkin. *The Third Revolution: professional elites in the modern world*. Routledge 1996

AJ Perlis. Programming of Digital Computers. *Communications ACM* (7, 4) April 1964 pp. 210–211

Andrew Pickering. Cybernetics and the Mangle: Ashby, Beer and Pask. *Social Studies of Science* 32/3 (June 2002) 413–437

Andrew Pickering. *The Mangle of Practice: time, agency and science*. Chicago Press. 1995

John Pickering. Agents and Artefacts. *Social Analysis* (41,1) March 1997 pp. 45–62

Steven Pinker. *The Language Instinct*. Penguin 1995

Plato. *Georgias*. (tr. T Irwin) OUP,1979

Plato. *Republic*. (tr. Robin Waterfield) OUP 1993

Michael Polanyi. *The Study of Man: the Lindsay memorial lectures 1958*. Chicago 1959

Michael Polanyi. *Personal Knowledge: towards a post-critical philosophy*. [1st edn1958] Chicago 1974

Seymour V Pollack. The Development of Computer Science. in *Studies in Computer Science (Studies in Mathematics 22)* Mathematical Association of America 1982

Maurice Merleau-Ponty. *Phenomenology of Perception*. (tr. Colin Smith) [*Phénomènologie de la perception* 1945, 1st English edn 1962] Routledge 2002

Karl R Popper. *The Logic of Scientific Discovery*. [*Logik der Forschung* Vienna:1934, 1st English edn 1959] Routledge 1997

Karl R Popper. *Objective Knowledge*. Oxford 1972

Karl R Popper. *The Poverty of Historicism*. Routledge Kegan Paul 1957

RF Port, T van Gelder. *Mind as Motion: explorations in the dynamics of cognition*. MIT 1995

JWS Pringle. *Insect Flight*. OUP 1975
Vernon Pratt. *Thinking Machines: the evolution of artificial intelligence*. Blackwell 1987
MH Price. *The Development of the Secondary Curriculum*. Croom Helm 1986
S Pumfrey, PL Rossi, M Slawinski (eds). *Science Culture and Popular Belief in Renaissance Europe*. Manchester UP 1991
Hilary Putnam. *The Threefold Cord: mind, body and world*. Columbia 1999
Edward Pyatt. *The National Physical Laboratory: a history*. Bristol: Adam Hilger 1983
ZW Pylyshyn. *Perspectives on the Computer Revolution*. Prentice Hall 1970
ZW Pylyshyn, LJ Bannon. *Perspectives on the Computer Revolution* (2nd edn) Ablex 1989
Willard van Orman Quine. *Word and Object*. MIT/Wiley 1960
Z Radman. *Metaphors: figures of the mind*. Kluwer 1997
Anatol Rapoport. Mathematical Biophysics, Cybernetics and Significs. *Synthese* (8) 1949–51 pp. 182–193
Nicholas Rashevsky. *Mathematical Biophysics: physicomathematical foundations of biology*. Chicago. 1938
Nicolas Rasmussen. The Mid-Century Biophysics Bubble: Hiroshima and the biological revolution in America revisited. *Hist. Sci.* xxxv (1997) pp. 245–293
B Reading. *The University in Ruins*. Harvard 1996
N Rhodes, J Sawday (eds). *The Renaissance Computer, knowledge technology in the first age of print*. Routledge 2000
IA Richards. *The Philosophy of Rhetoric*. OUP 1936
IA Richards. *Principles of Literary Criticism*. [1926] Routledge 2001
H Rickert. *The Limits of Concept Formation in Natural Science: a logical introduction to the historical sciences*. CUP 1986
Lionel Robbins (chmn) Higher Education: report of the committee on higher education 1961–63. (cmnd 2154) HMSO,1963
Deborah H. Roberts, Francis M Dunn, Don Fowler. *Classical Closure: reading the end in Greek and Latin literature.* Princeton 1997
GI Rochlin. *Trapped in the Net: the unanticipated consequences of computerization*. Princeton 1997
William Rodgers. *Think: a biography of the Watsons & IBM*. New York: Stein & Day 1969
Jacqueline de Romilly. *The Great Sophists in Periclean Athens*. (tr. Janet Lloyd) [*Les Grand Sophists dans l'Athèns de Périclès*. 1988] OUP 1992
Richard Rorty. *Objectivity Relativism and Truth: philosophical papers, volume 1*. CUP 1991
Richard Rorty. *Philosophy and Social Hope*. Penguin 1999
Richard Rorty. *Philosophy and the Mirror of Nature*. Blackwell 1980
Arturo Rosenblueth, Norbert Wiener, Julian Bigelow. Behavior, Purpose and Teleology. *Philosophy of Science* (10) 1943 pp. 18–24
Stuart J Russell, Peter Norvig. *Artificial Intelligence: a modern approach*. Prentice Hall 1995
Gilbert Ryle. *The Concept of Mind*. [1949] Penguin 1990
Michael Sanderson. *Education and Economic Decline in Britain 1870 to the 1990s*. CUP 1999
Michael Sanderson. *Education, Economic Change and Society in England 1780–1870*. (2nd edn) Macmillan 1991

Michael Sanderson. *The Missing Stratum: technical school education in England 1900–1990* Athlone Press 1994

John Scheid, Jesper Svenbro. *The Craft of Zeus: myths of weaving and fabric*. (tr. Carol Volk) [*Le métier de Zeus: Mythe du tissage et du tissu dans le monde gréco-romain* 1994]. Harvard 1996

JM Scher. *Theories of the Mind*. Free Press 1962

Erwin Schrödinger. *What is Life?: the physical aspect of the living cell.* [1944] with *Mind and Matter* [1958]. and *Autobiographical Sketches*. CUP 1992

D. Sedley. *Lucretius and the transformation of Greek Wisdom*, CUP 1998

CE Shannon, J McCarthy (eds). *Automata Studies*. Princeton UP 1956

Claude E Shannon, Warren Weaver. *The Mathematical Theory of Communication*. Illinois. 1949

Steven Shapin. *A Social History of Truth: civility and science in seventeenth century England.* Chicago 1994

Steven Shapin. *The Scientific Revolution*. Chicago 1996

Steven Shapin, Simon Schaffer. *Leviathan and the Airpump: Hobbes, Boyle and the experimental life*. Princeton 1985

N Sheppard. 'Gordon Brims Black McIvor Sutherland' in *Biographical Memoirs. Fellows Roy. Soc.* (28) 1982 pp. 589–626

Sir Charles Sherrington. *Man on his nature* (The Gifford Lectures ; 1937–8). 2nd ed Cambridge 1963

DA Sholl, AM Uttley. Pattern Discrimination and the Visual Cortex. *Nature* (171). 4348, 28 February 1953 pp. 387–388

Arthur M Silverstein. "The End is Near!": the phenomenon of the declaration of closure in a discipline. *Hist. Sci.* xxxvii (1999) pp. 407–425

Herbert A Simon. *Administrative Behavior*. New York: Macmillan 1949

Herbert A Simon. *Models of Man: social and rational*. Wiley 1957

Herbert A Simon. The Patterned Matter That Is Mind. in *Mind Matters*. Lawrence Erlbaum 1996

Richard R Skemp. *The Psychology of Learning Mathematics*. Penguin 1971

Quentin Skinner. Meaning and Understanding in the History of Ideas, *History and Theory* vol. VIII 1969

G Slater. *The English Peasantry and the Enclosure of Common Fields*. Constable 1907

Alfred Smee. *The Process of Thought: adapted to words and language, together with a description of the relational and differential machines*. London: 1851

Brian Cantwell Smith. *On the Origin of Objects*. MIT 1996

Crosbie Smith. *The Science of Energy: a cultural history of energy physics in Victorian Britain*. Athlone Press. 1998

J-C Smith (ed). *Historical Foundations of Cognitive Science*. Kluwer 1990

CP Snow. *A Postscript to Science and Government*. OUP 1962

CP Snow. *Science and Government*. OUP 1961

CP Snow. *The Two Cultures* [1959] CUP 1993

D Sperber. *Explaining Culture: a naturalistic approach*. Blackwell 1996

D Sperber. *Rethinking Symbolism*. CUP 1975

R Steele (ed). *The Earliest Arithmetics in English*. Early English Text Society 1922

Guy L Steele, Richard P Gabriel. The Evolution of Lisp. *ACM SIGPLAN Notices* 28, 3 (March 1993) pp. 231–270

DM Steier, TM Mitchell (eds). *Mind Matters: a tribute to Allen Newell*. Laurence Erlbaum 1996

Gertrude Stein. *Everybody's Autobiography*. London: Heinemann 1938

Ian Stewart. *Life's Other Secret: the new mathematics of the living world*. Penguin 1998

Herbert Stoyan. Early LISP History (1956–1959) http://www8.informatik.unierlangen.de/html/lisp/histlit1.html 22 June 2002

Herbert Stoyan. The Influence of the Designer on the Design —J. McCarthy and LISP. http://www8.informatik.uni-erlangen.de/html/lisp/mcc91.html 22 June 2002

Christopher Stray. *Classics Transformed: schools, universities, and society in England, 1830–1960*. OUP 1998

Bjarne Stroustrup. *The C++ Programming Language*. (3rd edn) Addison-Wesley 2000

FJ Swetz. *Capitalism & Arithmetic: the new math of the 15th century*. Open Court 1987

Symposium on Information Theory (held in the lecture theatre of the Royal Society 26–29 September 1950). Ministry of Supply: [HMSO] 1950

A Tate (ed). *The Language of Poetry*. [Wheelwright, Brooks, Richards, Stevens] New York: Russell & Russell 1960

C Taylor. *Human Agency and Language*. CUP 1985

Paul Thagard (ed). *Mind Readings: introductory selections on cognitive science*. MIT 1998

D'Arcy Wentworth Thompson. *On Growth and Form* [1st edn 1917, 2nd edn 1942, abr. edn 1961] CUP 1992

EMW Tillyard. *The Muse Unchained: an intimate account of the revolution in English studies at Cambridge*. Bowes & Bowes 1958

N Tiratsoo, J Tomlinson. *The Conservatives and Industrial Efficiency 1951–64: thirteen wasted years?* Routledge 1998

KD Tocher. *The Art of Simulation*. English UP 1963

Howard E Tompkins. Computer Education. *Advances in Computers* (4) Academic Press 1963

Rodger Trigg. *Rationality & Science: can science explain everything*. Blackwell 1993

AM Turing. The Chemical Basis of Morphogenesis. *Roy. Soc.* 237 B. 641 August 1952

AM Turing. *———'s ACE Report of 1946 and other papers* (eds. BE Carpenter, RW Doran) Babbage Institute, Tomash/MIT 1986

A M Turing. On Computable Numbers, with an application to the Entscheidungsproblem. *Proceedings of the London Mathematical Society (2)* 42 1937 pp230–265

AM Turing. Computing Machinery and Intelligence *Mind* (59) 1950 pp.433–460

A Tustin. Do Modern Mechanisms Help Us to Understand the Mind? *Brit. Jnl. Psychology* (44) 1953

AM Uttley. *Information Transmission in the Nervous System*. Academic Press 1979

AM Uttley. A Visit to the United States and Canada in 1959. National Physical Laboratory, Department of Scientific and Industrial Research. August 1959

S Vajda. *Problems in Linear and Nonlinear Programming*. Griffin & Co. 1975

Ian Varcoe. Comment: practical proposals by scientist for reforming the machinery of scientific advice, 1914–17 *British Jnl Hist Sci* (33. 1) 109-114. March 2000

M Vasconcellos. Technology as Translation Strategy. *American Translators Association scholarly monograph series* volume II. State University of New York 1988

Z Vendler. *The Matter of Minds*. OUP 1984

Brian Vickers. Bacon and Rhetoric. in *The Cambridge Companion to Bacon* (ed. Markku Peltonen) CUP 1996

Brian Vickers. *Classical Rhetoric in English Poetry*. Macmillan 1970

Giambattista Vico. *New Science* (tr. David Marsh) [*Principii di una Scienza Nuova d'intono all natura della nazioni*, Napoli: 1725–44] Penguin 1999

WG Vincenti. *What Engineers Know and How They Know It: analytical studies from aeronautical history*. Johns Hopkins 1990

Jeffrey Walker. *Rhetoric and Poetics in Antiquity*. OUP 2000

Larry Wall, Tom Christiansen, Randal L Schwartz. *Programming Perl*. (2nd edn) O'Reilly 1996

W Grey Walter. *The Living Brain*. [1953] New York: WW Norton 1963

Françoise Waquet. *Latin or The Empire of a Sign: from the sixteenth to the twentieth centuries*. (tr. John Howe)[*Le latin ou l'empire d'un signe*, 1998] Verso 2001

Robert Wardy. *The Birth of Rhetoric: Gorgias, Plato and their successors*. Routledge. 1996

PC Wason, PN Johnson-Laird. *Thinking and Reasoning*. Penguin 1968

Douglas Wass. What Sort of Industrial Policy? (5th Shell International Lecture) University of St Andrews. 23 October 1985

SH Watson. *Extensions: essays on interpretation, rationality and the closure of modernism*. State University of New York 1992

Thomas J Watson. *A Business and its Beliefs: the ideas that helped build IBM*. McGraw-Hill 1963

JC Webb. *Mechanism Mentalism and Metamathematics: an essay on finitism*. Reidel 1980

Peter Wegner. Three Computer Cultures: computer technology, computer mathematics, and computer science. *Advances in Computers* (10) Academic Press 1970

Joseph Weizenbaum. *Computer Power and Human Reason: from judgement to calculation*. [1976] Penguin 1984

JH Westcott. An Introduction to Adaptive Control. in *An Exposition of Adaptive Control* (Proceedings of a Symposium held at the Imperial College of Science & Technology, London 1961) Pergamon Press 1962

Philip Wheelwright. *Metaphor and reality*. Indiana 1962

William Whewell. *———'s Theory of Scientific Method* (selections, ed. RE Butts). U. Pittsburgh P. 1968

Hayden V White. The Burden of History. *History & Theory* (5) 1966 pp. 126–132

Benjamin Lee Whorf. *Language, Thought, and Reality: selected writings of* ——— (ed. John B Carroll) MIT 1956

William H Whyte. *The Organization Man*. [1957] Penguin 1960

Martin J Wiener. *English Culture and the Decline of the Industrial Spirit, 1850–1980*. Penguin 1985

Norbert Wiener. *Cybernetics: or control and communication in the animal and the machine* [Chicago, 1948] (2nd edn) MIT 1961

Norbert Wiener. *The Human Use of Human Beings: cybernetics and society*. Eyre & Spottiswoode 1954

Norbert Wiener, JP Schadè. *Cybernetics of the Nervous System*: progress in brain research vol. 17. Elsevier,1965

Maurice V Wilkes. *Memoirs of a Computer Pioneer*. MIT 1985

MV Wilkes, DJ Wheeler, S Gill. *The Preparation of Programs for an Electronic Digital Computer: with special reference to the EDSAC and the use of a library of subroutines.* [Addison-Wesley 1951] Babbage Institute MIT/Tomash 1982

FC Williams, AM Uttley. The Velodyne (abstract of a lecture delivered at the Radiolocation Convention 29th March 1946) *Journal of the Institute of Electrical Engineers* (Part IIIa Radiolocation No. 1) pp. 317–318

Michael R Williams, Martin Campbell-Kelly. *The Early British Computer Conferences.* Babbage Institute/MIT 1989

Raymond Williams. *Culture and Society 1780–1950*. [1958] Penguin 1961

Niklaus Wirth. Program Development by Stepwise Refinement. *Communications of the ACM*, 14 (4) April 1971 pp. 221-227

Jennifer Wise. *Dionysus Writes: the invention of theatre in ancient Greece*. Cornell 1998

Ludwig Wittgenstein. *Philosophical Investigations* (tr. GEM Anscombe) [1st edn 1953, 2nd edn 1958] Blackwell 2001

Ludwig Wittgenstein. *Remarks on the Foundations of Mathematics.* 3rd edn Blackwell 1978

Ludwig Wittgenstein. *Tractus Logico-Philosophicus.* [1922] (tr. DF Pears, BF McGuinness [1961]) Routledge 2001

Lewis Wolpert. *The Unnatural Nature of Science*. Faber 1992

PM Woodward, SG Bond. *Algol 68-R Users Guide*. HMSO 1974

Ian Worthington. *Persuasion: Greek rhetoric in action*. Routledge 1994

David M Yates. *Turing's Legacy: a history of computing at the National Physical Laboratory 1945–1995*. London: The Science Museum 1997

Ido Yavetz. Oliver Heaviside and the British Electrical Debate. *Annals of Science* 50 (1993) pp. 135–173

FA Yeldham. *The Story of Reckoning in the Middle Ages*. Harrap 1926

JA Yelling. *Common Field and Enclosure in England 1450–1850*. Macmillan 1977

JZ Young. *Doubt and Certainty in Science: a biologists reflections on the brain*. (*BBC Reith Lectures 1950*) OUP 1951

JZ Young. *Philosophy and the Brain*. OUP 1987

M Young. *The Rise of the Meritocracy 1870–2033: an essay on education and equality*. Thames & Hudson 1958

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