MOLECULAR EXPLANATION FOR

INTELLIGENCE

INCLUDING ITS GROWTH, MAINTENANCE, AND FAILINGS

by
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THE CYBERNETICS OF CONCEPTS:
AN INTEGRATED SYSTEM OF POSTULATES TO EXPLAIN THEIR NATURE, ORIGINS,
USE, MALFUNCTION AND MAINTENANCE
WITHIN A NATURAL NEURAL-MOLECULAR MEDIUM
IN THE BRAIN

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* Original printed version of "chapter A3": — Traill (1978a): *Kybernetes*, **7**, 61.71.

† Using the original page-numbers, as shown in the margin of this online text.

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11
Declaration concerning publication
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Part A of this work was published in the journal *Kybernetes*: Chapters A1 and A2 as Traill (1976a), and Chapter A3 as Traill (1978a). This journal was, in the 1970s, published by:- Thales Publications (W.O.) Ltd., 6 Old Bond Street, London W1X-3TA.

Part B has been publicly available as a Departmental Monograph, (Traill, 1977), since December 1977.

Status of Part B — in 2006

"Part B" became separately available online in February 2006 on two websites: — *i.e.* as www.ondwelle.com/MolecMemIR.pdf *and also* www.wbabin.net/physics/traill8.pdf (same). Accordingly it has been omitted from this present already-oversized document. In any case the here-remaining text is reasonably self-contained without it, given the resumés within Part C.

For Section-headings of the omitted parts, see the Table of Contents, above — or see the *chapter-summaries* immediately following Part A.

(Summary & introduction to 1978 Thesis, Brunel University)

ORIGINAL SUMMARY of WHOLE WORK 12

Behaviourists and Logical Positivists commendably set out to purge loose prejudiced arguments from science; but where it is obvious that there remains some sort of "ghost" in their rational "machine", it is self-defeating simply to ignore its existence. Freud, Piaget, and the ethologists have made some progress in grasping this nettle — moving towards a material explanation of the "other-worldly" properties of the individual — but their models of the individual remain nebulously structured in their basic elements. Consequently such theories remain disturbingly controversial, and circumscribed in their applicability.

The present work accordingly sets out to bridge this gap by postulating plausible functions for the existing micro-structure which could account both for observed behavioural phenomena, and for many of the existing vaguer theoretical constructs. Part A develops such an explanation for Piagetian constructs, while Part B fills in some of the technical details concerning quantitative problems of signal generation, transmission, and selective reception.

Part C applies these notions to other non-Piagetian descriptions and interpretations of psychological phenomena, thereby offering an integration and reconciliation of various schools of theory. (Major areas considered include Ashby's "homeostat" approach, biological self-organization, sleep-modes and dreaming, Freudian theories of neuroses, and various theories concerning psychosis). The basic theory itself is meanwhile developed in much greater detail.

A recurring theme throughout the work is the notion that knowledge-acquisition by any independent system depends not only on "external" interaction with the "real" world, but *also* on an active seeking for internal consistency within the resulting "internal" model. This concept is crucial to the study in two ways:- (i) The operation of the brain-systems being considered, and (ii) As a guide to the methodology of the 13
present study itself — in an area where experimental data is uncomfortably sparse, and likely to remain so.

GENERAL INTRODUCTION 15

This work constitutes a many-angled attack on some of the age-old questions on what important transactions could be taking place inside the structure of the human brain. This is a tall order of course, and time will tell whether the theories proposed are more, or less, correct. But either way, I believe that this work has much to offer in the number of unquestioned (or seldom-questioned) assumptions which it brings to light. If my own solutions to the resulting problems should happen to be somewhat wide of the mark, this will be comparatively unimportant as long as the raising of the questions leads to further investigations and better answers.

Problems of intuitive thought — as a tool, and as a subject of study

The following simplistic resumé might perhaps orient the reader to the sort of approach being attempted here in relation to the understanding of the mind/brain, and the relation this work has to other previous ideas on the subject.

Let us start by differentiating between two types of thought .process: *Intuitive-subjective* versus *logical*, [or Freudian primary versus secondary-process thinking, or Piaget's Concrete Operations versus Formal Operations; or "M^1L" versus "M^2L" in the present work]. We may next consider that these two categories of thought, however we choose to define the difference, will manifest themselves in two relevant ways: (i) as an *object of study* — typically in other people including patients, but also in "ourselves-at-other-times", and in non-human animals; (ii) as the very *process* by which we engage in all such consideration.

Accordingly, this means that we can *think about Logical thought* (a) mystically, in an intuitive way, or (b) in a logical way; and similarly we may think about Intuitive thought in (c) an intuitive way, or else (d) in a logical way. Thus:- 16

		Process by which the study is made		
		intuitive thought and feelings	logical or systematic thought	
object of study	intuitive thought and feelings	(c) Novelists, Artists.	(d) (?Freud)	both important
object of study	logical or systematic thought	(a) Mystics, Animists, Teleologists.	(b) Bertrand Russell; Logical Positivists.	both important

— though to what extent these various analyses have so far produced helpful results, remains as a matter for debate.

Probably all such studies must *start* with an intuitive unsystematic approach like those on the left, and many problems might indeed forever defy any really systematic approach. Nevertheless, systematization is the aim for scientific treatments like the present work, so the right-hand items (b and d) call for further comment here:-

(b) One great achievement of Western thought has been the comparative rigour of its logical formulations. This has led to the hope that *absolute* rigour might be achieved, and this hope seems to have been the driving force behind logical positivism and other work related to it: Whitehead and Russell (1910-1913), Carnap (1928), and arguably also Wittgenstein's "Tractatus" (1921). In the event, such absolute rigour has turned out to be unattainable, as shown in effect by Gödel (1931), and as we may also see fairly easily if we ask ourselves "What is the *logical*
justification for our rules of logic?"! The question then becomes one of finding out how we are to 17
achieve good approximations to this ideal — a task tackled in their different ways by Popper, Piaget, and the later work of Wittgenstein.

(d) The equivalent study of intuitive or "non-rational" thought has been very much a neglected area, comparatively speaking. But there are two important reasons why we should try to remedy this deficit. Firstly, however much we may take an elitist view deploring the non-rational thought in others (Plato: *The Republic*), it nevertheless abounds all around us and even within our own "rational" selves — and there is no realistic prospect of making any significant change to this situation, as will be apparent from the works of the psychoanalytical schools, and also from the works of Piaget.

Secondly it follows from the inherent limitation of logical thought, just mentioned above, that such "irrational" thought is actually a necessary alternative to fill in the many *inevitable* "holes" in our logic. (In other cases such intuitive judgement may often be *more efficient* for the task in hand even if a logical solution does exist — as many a practical business-man or politician will know from experience). Accordingly, it seems high time that we knew more about such intuitive processes so that we could plan a better (logical) *utilization* for them, as well as giving us more *understanding* to deal with the "irrational" behaviour of others which was mentioned in the previous paragraph.

As things stand, there is some real justification in the claim that "good novelists are streets ahead of psychologists" in their portrayal of important issues in social life, (item "(c)" in the above table). Freud has, to be sure, done much to put this study of non-rational thought into a 18 rational framework[1]. Unfortunately though, in his day there was no adequate knowledge of neurophysiology on which he could have helpfully based such an attempt (though he did try), so he was forced to "build his house upon the sand" of constructs which were themselves largely intuitive in origin; so his system might best be seen as something of a provisional *semi*-structured model. Now that we know much more about the physical and physiological mechanisms of neural systems, we are in a better position to attempt to re-build "the house" (preferably the same house if possible) — but "on rock" this time.

Thus it is that a major purpose of the present project has been to produce some hard-if-controversial explanations for non-rational processes and their vicissitudes — but in more logical and explicit terms than has hitherto been possible. In short, this work aims to make good the felt-weakness of "(d)" in the above table.

In passing, we might notice that the diagonally-opposite entry, item "(a)", involves an intuitive approach to logical matters! One conspicuous manifestation of this is the occasional anti-intellectual swing against science and logic which becomes fashionable whenever sufficiently 19 vocal sections of the community feel justified in claiming that "the experts have got it all wrong". Sometimes indeed, they may be correct in their basic criticism — but even if they are, it does not necessarily follow that a straight regression to intuitive "solutions" will help in the slightest. On the contrary, if actually implemented for complex problems, such a doctrine is likely to lead to disaster — to the concentration camp and the gas-chamber, as Bertrand Russell remarked in his influential broadcast comments on D.H.Lawrence. More recently, Professor Max Hamilton's controversial address to the British Psychological Society (1973) also sounded a similar warning.

To abandon ourselves, blindly and totally, to the processes of intuition — without any constraints of level-headed logical thought — would be to abandon ourselves to sub-human barbarity. But to go to the other extreme and deny the necessity and existence of intuitive thought, would be to throw out the baby with the bathwater; and moreover it would probably provoke an eventual reflection from the very same intuitive forces which we were denying. Intuition then is certainly no panacea, but neither can we safely ignore it; and to understand it adequately, it would seem that we should at least try to describe its general operation — in the formal and rigorous language of "logical" thought. And this, of course, brings us back to "(d)" and the objectives of the present work.

It might be fair to say that the important contribution of Logical Positivism was to cause a general progression out of "(a)" and into "(b)". In retrospect it seems that this programme could not actually succeed in fulfilling its own perfectionist ideals, and it is now recognized as being logically false in this strict sense (Ayer, 1978, last column). However Ayer adds, with some

[1] Other writers also deserve a mention here:- Wittgenstein's later work (1953) poses many apposite questions illustrating the inadequacy of the conventional "logical" approach (and these questions are very interesting to re-read in the light of the current theory), but he himself does not have much to offer in answer to his own questions. Piaget comes nearer to giving an adequate account of sub-rational thought in his various discussions and studies of the Sensori-Motor stage — though it has been suggested that his account lacks the attention to *emotion* which any adequate account of social behaviour should contain (M. Jahoda, 1972, in answer to a question). The present work attempts to produce explanations which are compatible with both Piagetian and Freudian approaches; though to what extent this attempt has been successful is, of course, open to debate.

justification, "that it was true in spirit, that the attitude was right". It would now seem that the 20
missing factor was the recognition of the need for the internal-closure criterion of intuitive thought, and the acceptance that this is the only means by which we can "lift ourselves by our own bootstraps" out of the state of complete ignorance that our ultimate protoplasmal ancestors were faced with. Once again, this brings us back to "(d)" — and even to the artist's insights of "(c)", whenever more systematic approaches fail.

Subtopics and how they are allocated here

In broad philosophical terms, it should now be reasonably apparent how this present project relates to previous work. But it is a rather more difficult task to locate its *detailed* arguments alongside those pre-existing in the many interdisciplinary fields involved — simply because of their number and diversity. No proper discussion of such relationship to previous ideas will be attempted at this introductory stage, but these questions have been explored as they arose. Of these, the most important theoretical backgrounds-to-innovation will be found best summarized under the obvious chapter-headings; for example Scientific Method in Chapter C1, and Ashby's "Homeostat" in Chapter C4. Other less obvious backgrounds to topics within the overall project, are given at least a passing reference as follows:- Classical and operant conditioning, and Ethology (Section A1.4); Neurophysiological saltatory conduction (the latter half of Section B3.3); Embryological mechanisms (Section C5.3); Infra-red and micro-waves in biology (Chapter Bl); and the evolution of intra-organism communication (Chapter B5).

[By mid-1978, Parts A and B had already been separately published as 'hard copy':
Part A in the two *Kybernetes* papers (1976 and 1978, vols **5** and **7**); and
Part B as a 1977/1980 Brunel University monograph (Cyb.#24), which has now recently appeared online as www.ondwelle.com/MolecMemIR.pdf — (hence its actual text has been omitted here, since the hyperlinks to it seem to suffice). In contrast:
Part C has hitherto been publicly available only in university libraries. — RRT, 2006.]

This leads us to a brief consideration of how the overall project report has been divided up.
In Part A the discussion has, as much as possible, been kept deliberately abstract — considering 21
what formal functional structures would seem to be needed if the system were to be capable of behaving according to Piaget's depiction of dynamic human existence and (psychological) development. Having taken this abstract modelling as far as seemed profitable, the study turned to Part B in which it was sought to postulate, in some considerable detail, just what plausible real-physical-mechanisms could underlie the formal abstractions previously postulated. By now this amounted to a promising basic model of the brain and its more straightforward processes. It remained then to elaborate these ideas as much as was feasible, such as to try to develop detailed explanations for more-specialized types of behaviour and experience (notably sleep, neurosis, and psychosis), as a means toward testing the new postulates, developing others relating to fine detail; and meanwhile hoping for practical progress from such innovations. This latter task is dealt with in Part C.

The balance between Parts A, B, and C — and their interdependence

The size of the field encompassed by the present project is a compromise between two conflicting demands:- (i) The obvious need to stop somewhere and secure any loose ends as best I could, before the whole ensemble of ideas got out of hand; — "to keep it concise" in other words. (ii) On the other hand, given the paucity of direct evidence on the issues raised, it has been necessary to extend the ramifications of the theory as widely as is reasonably possible, so as to develop whatever information may be gleaned from interdisciplinary cross-comparison (or "internal closure", to put it into the current theory's own terminology).

Part A on its own is not particularly convincing. Part B is perhaps a little more compelling as it clearly contains at least three mutually-corroborating arguments and thus has its own self- 22
sustaining share of "internal closure in more than two dimensions". Nevertheless the new proposed model would be bound to look somewhat hollow if it could not offer an enhanced understanding of the function and malfunction of the mental system as a whole. It has thus been the task of Part C to attempt this application of the basic postulates to the more general theory, and this process needed to be taken far enough either to show up serious weaknesses in the basic postulates, or to constitute a persuasive argument that the theory is at least on the right track.

Ideally other disciplines should have been incorporated as well, in all detail which seemed relevant; (e.g. immunology, biochemistry, pharmacology, and experimental embryology). It is with some regret that such aspects have had to be dealt with superficially, or left out altogether. But the line had to be drawn somewhere; and there may even be some advantage in leaving such scope for other, less partizan investigators to cross-check the implications within other, so-far neglected, fields of study.

Some 18 interrelated biological insights which seem mutually corroborative

The most significant direct theoretical insights offered by the project would appear to be the following. (i) Firstly there is the epistemological challenge to the methodological doctrine that experimental observations are the only legitimate criterion for advancing theoretical models. This clears the way for making progress by other means, depending on the strategy of preferring models according to the degree of uncompartmentalized *internal* consistency which they show; though effective use of this strategy depends on the model being based on elementary entities which are adequately discrete, valid, and predictable-in-principle. (ii) Secondly there is the 23
complementary insight that it seems to be essentially the same strategy which enables biological brains to achieve their extraordinary feats of learning from unsystematic experience (in contrast to the comparatively trivial achievements of so-called "electronic brains").

This comparison gives rise to (iii) — a general observation that similar complex problems are likely to call forth functionally equivalent mechanisms which might or might not be physically equivalent; and of which, one such mechanism might or might not incorporate systems belonging to the other. (Such shared systems could then serve different functional roles with respect to the two different points of view). This, in turn, leads to another consideration: (iv) that a hierarchical "recursive" organization might be successfully evolved within some types of brain system; — an insight which may be regarded as an extension and generalization of Ashby's concept of the "homeostat"-type of adaptive control, and which thereby offers solutions for his own unsolved list of "antinomies".

We will turn now to insights as to basic mechanisms:-
(v) There is the general rule-of-thumb that the explanation for any "mysterious" behaviour in a system is often to be found in hitherto unexplored discrete *micro-structure* within a massed *population* of relatively stable units — which are normally observed as a collective whole, giving a misleading appearance of homogeneity and continuous-variation in its properties.

(This notion should not be considered novel, except in its present field of application. There is, after all, ample precedent for it in: the emergence of Chemistry following from the modern concept of atoms and molecules; the emergence of sub-atomic physics; the development of Planck's postulate to explain anomalies of radiation; Mendel's abstract concept of the gene — and the subsequent elucidation of this notion; the development of the concept of bacteria from 24
Semmelweiss's unstructured statistical correlations into the structured and discrete entities of Koch and Pasteur; the advance in neurophysiology when it became possible to isolate the response inside single nerve cells and fibres; and so on into other microbiological developments.

It also seems clear that it is our apparent inability to formulate a sufficiently structured view of the individual units within *social* systems, which has left the "social sciences" with a rather dubious reputation for any scientific qualities — so that they are often regarded more as "arts" — and justifiably so. The question of whether there is any feasible remedy for this, must be regarded as a separate issue — to which we will return shortly).

Anyhow, in the present context this has led to a critical re-evaluation of various doctrines about neurophysiological mechanisms;— doctrines which appear to rest on the dubiously-valid criterion of present-day limits to resolution in physiological experiments. (This evidently follows from the self-imposed constraint against seriously considering the existence of mechanisms which cannot be fairly-directly observed). This re-evaluation leads us to further potentially valuable insights:-

(vi) The next is that our phenomenal ability to handle conceptual *sequences*, in a reliably reproducible manner, would be most credibly explained by postulating that the basic encodings of memory will be organized in topologically linear "strings" or "tapes" of physical information-storage; — a formulation which also offers an explanation for the mathematical "sets" and "groups" evident in our thought processes. From there it is but a short step to the plausible supposition (vii) that such linear encodings could be string-like macro-molecules (probably DNA, RNA, or protein) similar to those used for genetic encoding. If this is correct, then the
above principle (v: population of micro-elements) may be interpreted as suggesting that practical 25
memory, as displayed behaviourally, will result from cooperative summations from the effects of such micro-elements.

(viii) The next insight arose unexpectedly during the investigation of an otherwise unhelpful theoretical idea. The surprise discovery was that, despite physiological doctrines to the contrary, myelinated nerve fibres are constructed in such a way that they could well be suitable media for conducting local infra-red signals through an inhospitable aqueous medium. Then (ix) the very ease with which this infra-red conduction might be aided or blocked by changes at lipid boundaries could act as a powerful means for transducing signals. Furthermore (x) it was seen that such co-axial paths might often favour *optical dispersion* of signals, and that this could actually *assist* the efficient utilization and sorting of signals (contrary to what one might expect from engineering-design practice).

(xi) The next conceptual innovation was to associate this likely *availability* of infrared-handling capabilities with the *need* for such abilities so that there could be proper control of emission and absorption spectra associated with the molecular changes presumably entailed in the chemical storage of memory — and presumably involving the postulated linear molecules.

Then, as a by-product of these considerations, there was (xii) the notion that infra-red or micro-wave interference patterns would offer a more credible means for controlling *embryological* growth than the more usual suggestion of chemotaxis. (This point may turn out to be rather more relevant than would seem at first sight, because it would seem to be rather more testable experimentally than most of the other ideas, and because it might be used to account for
the *production* of specific nerve-fibre geometries — as opposed to the functional significance of 26
these shapes).

The idea that the fundamental units of memory, for at least some purposes, were of molecular size rather than on the much larger scale of the synaptic junction, opened up a new range of possibilities leading to another insight: (xiii) The vastly greater likely number of such elements (and their much greater speed potential) made it plausible to suggest that all-or-most "recording" of memories actually depended on a vast system of continuing trial-and-error, and that these arbitrary micro-trials stood a reasonable chance of offering a correct interpretation of events — in amongst the many incorrect interpretations which would normally be promptly rejected as misfits. Such an interpretation seems to go a long way toward explaining the peculiar strengths and weaknesses of biological perception and mentation, as compared with its "counterparts" in modern technology.

(xiv) The next insight is simply a generalization of this concept, to the effect that physically-based Darwinian trial-and-error seems to be the driving-force behind *all* substantial biological progress; and that even our apparently transcendental thought-capabilities operate on fundamentally the same principle as the natural selection of beetles or bacteria; that they might even use the *same* physical mechanisms (though differently controlled in different organs of the body); and that thought processes are simply "evolution writ small", and involving the same sort of prodigious wastage amongst all but the actual winners.

On the more psychologically-oriented side of the question, these concepts gave a reasonable basis for explaining (xv) how the brain might handle its representation of "sets" (in the mathematical sense) — in a potentially hierarchical way, and with possible recursion in the organization, and even a limited amount of inversion to the hierarchy of control, giving a feedback-loop which could upset the brain's stability if it were abused by too prominent a utilization. As part of 27
this process, the brain was seen as being involved in continually re-grouping its mechanisms such as to keep alternative methods at its disposal — enabling it to adapt quickly when outside circumstances demand, or when one of the mechanisms fails internally. The development of hierarchical integration was seen as part of this process, and the striving toward the construction of "extensively defined sets" (as physical structures) out of the less-accessible information of "intensively defined sets" was seen as another aspect.

The next theoretical innovation was (xvi) to identify the observable *sleep-modes* as outward manifestations of these maintenance and mental-development processes.

Another insight was (xvii) to explain psychoses as a breakdown of these processes in various ways.

To complete the list, (xviii) neurosis was accounted for as trapped-states which were explicable in terms of the physical operation of the linear-encodings of memory; and this suggested ways in which these states might sometimes progress dynamically into a structurally-definable psychotic state.

A concluding remark should be made about the postulated molecular mechanisms. There is no reason to suppose that they in themselves constitute a complete basis for explaining brain function, without also implicating the more macro-phenomena at cell and synapse level. We should rather expect that any reasonably-complete explanation will require a consideration of both, in much the same way that any adequate understanding of the properties of Radium must take account of both its orthodox chemistry, and the sub-atomic characteristics of its nuclei.

Possible applications in the social sciences and elsewhere 28

There are several important *applications* which might arise from significant progress in this field of what we might call "micro-structural psychology". There is a clear potential role for such knowledge in clinical psychiatry and as a new ingredient to many aspects of philosophy and academic psychology; and there are less obvious potential by-products from this work in the theoretical domains relating to embryology and other questions of cell-dynamics. Another non-obvious potential application is to the broad field of social psychology — the field which we will now look at first, before discussing the others on the above list:-

(1) *The psychology of economics and politics.* Rather surprisingly perhaps, it was unsolved problems within the social sciences which originally prompted this project, and which seem to have attracted the most interest more recently. The problem which first seems to have come to me explicitly, was an unease about the way psychological factors were represented within the

allegedly quantifiable theories of economics — especially Keynesian economics and the "utility" concept of welfare economics. (These formulations should, for instance, be compared with those of Maslow (1954) — a book which gave me some early insight into the complexities of real human choice criteria). Such economic theories have had their critics within economics, especially since the oil crisis of 1973 (see, for instance: *Monthly Review*, Sep. 1970, Apr 1974; and *Politics & Money*, Apr-Jun, 1974), but to me it was the psychological naiveté of such formulations which seemed most striking.

The reason for this inadequacy was reasonably obvious:-
Although one might well be able to criticize the mathematical models by using well-informed verbal arguments, there seemed to be no way in which these critical insights could themselves be put into any sort of mathematical form, comparable to the existing naive formulae and such that any hard-headed practical planner could use them for deciding optimally about complex econom- 29
ic systems. Consequently, any real-life decisions of this nature had to be made either (a) using the naive models at face-value, or more likely (b) tempering the implications of such models with sober opinion from men-of-experience, who were able to judge the realities of the situation more-or-less accurately — but in a way that they could not fully explain or generalize, and using what we are pleased to call "intuition" or "primary process thinking". (This non-communicability also clearly leaves scope for antisocial "fudging" of opinions — whether deliberate or not; and this often results in serious distortions of the truth:- in Accounting (Gambling, 1978), and in Politics).

This difficulty seemed to suggest that the science of psychology might be able to help, either by explaining the psychological aspects of market behaviour (of consumer, producer, investor, etc.) in adequate detail, or at least by giving us a better understanding of the intuitive processes used to correct the naive models.

Unfortunately psychology itself has hitherto been just about as unsatisfactory in the precision, reliability, and generality of its mathematical models, so there seemed to be little it could offer in practical terms. To be sure, there has often been a symbolic gesture towards using quantitative psychology in public decision-making, but the evidence rather suggests that this use has been no more than a political ploy to support this-or-that decision which has *already been taken in advance* — with the decision-makers *then* casting around for existing arguments to support their case; (Gardner, 1975).

Statistical evidence on social matters often appears contradictory, equivocal, or simply inconclusive or ungeneralizable. And even if it suffers from none of these defects, it still lacks
the persuasive power that a structural theory, such as the atomic theory of chemistry, is able to 30
exert. Accordingly it seemed appropriate to seek for a possible solution involving theories which offered a structural approach to explaining thought and brain-activity — rather than mere correlation and factor analysis. This led me to work such as that of Jean Piaget and the ethologists; and the signs are that this was a significant step in the right direction. However, as they stand, the concepts put forward by these writers do not yet measure up to the structural specificity which seemed necessary; so it appeared worthwhile to postulate the type of formal "micro-mechanical" structure which could underlie the phenomena of psychology in general, and those studied by these writers in particular. Hence the formal theories set out in Part A, elaborated physiologically in Part B, and cybernetico-behaviourally in Part C.

The original problem then, presented itself as a politico-economic one, but it also seems pertinent to other social problems:- What are the real issues behind censorship of sex or violence for different age-groups? (Correlation studies are costly and inconclusive). What are the real issues relating to racial equality, intelligence, and ego development? (Issues hotly debated on the basis of orthodox statistical methods). What is the relation between antisocial behaviour of certain social groups, and society's ambivalent treatment of these groups? — and what is the relation of both of these "variables" to the evolution of ego/superego structures within the members of these groups? Will chronic unemployment lead to violent racialism and/or war as it did in the 1930s? — and is there anything we can do to stop it?

Some of these problems will indeed reflect back onto economics in the form of questions such as: •"Exactly why is Mr Q prepared to spend large amounts of money or time on — sending
his children to school Y rather than school Z; expensive cars; a political cause; the gambling 31
tables; a mistress; plastic surgery; or whatever?" •"Are some of these non-subsistence buying-tendencies due to an irrational and/or insatiable desire for something else? •What is the something, and is it actually attainable? Would he be better off *not* obtaining it? Is he taking the right actions to get this something anyhow? Is there anything society could or should do to influence him in any relevant way, and why? Is society already creating a problem — for instance by allowing misleading advertising, or other vested-interest activity with undesirable psychological side-effects?"

Questions which are more particularly relevant to *management* are:- •"When a worker has 'job satisfaction', what is it that is actually happening in his mind, and can we understand this process structurally?" Hence •"Just why is it psychologically important (where possible) to consult workers before acting; and how should this be done?" And hence perhaps •"Just why does Maoist Chinese society continue to spend large amounts of time and energy on consultative meetings of this sort, with apparent enthusiasm, despite the loss of production which this seems to entail, at least in the short term?" (From a cybernetic point of view, any system or custom which *actually* survives the test of time — despite apparent "ineconomies" — is likely to have more significance to it than would appear from casual inspection; and it is therefore likely to be a profitable field for close analysis. Similarly, systems which are surprisingly *unstable* will also be of practical and theoretical interest).

Other problems, more in the province of the accountant, are:- •"What do we mean by 'the social cost of unemployment'? And what positive steps are needed for social stability if we are
to pursue an overall course of capital-intensive production — making the national labour-force 32
largely redundant?" Or the introspective type of question: •"What is the basis for my intuitive preference for this statistical figure or that? And can I formalize this accounting procedure in such a way as to free myself from political pressure?" (Sterling, 1975; Gambling, 1978).

Unfortunately it is probably too early to properly assess what value my current work might be in helping to solve social questions like these, in terms of structured cybernetics. As they stand, these theories about the individual do not, by themselves, have anything much to say on *social* matters without further elaboration — but they may turn out to offer the right sort of framework on which a better, and causally-structured, understanding of social processes might be based. Anyhow that was the original aim of the project and some slight informal investigations along these lines have been initiated elsewhere (Traill, Ref '5/77' [draft only]), even if that part of the task has barely begun.

(2) *Psychiatry*. The most obvious field of likely application for the current theories, especially as developed in the two final chapters, is in clinical psychiatry and the general area of Mental Health. However this does not mean that the new ideas, even if heuristically or epistemologically valid, will necessarily produce any immediate payoff. In the longer term though, we should at least expect to gain some benefit from improved diagnostic criteria and categories, as well as a better understanding or the dynamics of non-static conditions. In addition, after suitable "research and development", we might reasonably expect to achieve some helpful progress in the pharmacology of mentally-mediated complaints — thus incidentally narrowing the distinction between "mental" and "physical" conditions.

Preventative measures, education and personality development, and socially-mediated ther- 33
apy, might also benefit from such understanding — but of course such matters are intimately tied up with the socio-politico-economic systems discussed above as item "(1)", and worthwhile progress in these fields is unlikely if it is tackled in a piecemeal way, or in the spirit of overconfident infallibility sometimes found amongst social reformers.

(3) *Philosophical enigmas.* This work also turns out to have a considerable bearing on many of the unresolved issues traditionally dealt with by philosophers. It seems to me that Sloman (1976, page 16, col. 2) is correct when he writes "If [experimentally] unrefutable theories are to be dubbed 'metaphysical', then what I am saying is that even important scientific theories have a metaphysical component, ...". But then, since the epistemological view promoted here (on a Piagetian basis) is that even the most obvious perceptions and experimental observations must ultimately rest on assumptions which can never be "properly" tested experimentally, it follows that *all* "physical" knowledge is strictly speaking metaphysical! The point need not be laboured here, but the implications are worth pondering, and they certainly support Sloman's further comment (*ibid.*): "The development of ... 'metaphysical' theories is so intimately bound up with the development of science that to insist on a demarcation is to make a trivial semantic point, of no theoretical interest. Moreover, it has bad effects on the training of scientists."

Other philosophical issues obviously related to the present work are those relating to the concept of "mind":- Body and mind; Other minds; Personal identity; and Free-will. The main contribution offered here is: the explaining away of many of the obstacles to identifying mind with brain-organization, including questions involving perception, and the "teleological" capabilities of mind.

Freewill perhaps remains more of an enigma. The present work at least offers some clarific- 34
ation of the likely connection between overt behaviour and the *indeterminacy* quantum effects at the level of sub-molecular physics. Many would regard this as vindicating the concept of freewill because of the commonly accepted view that experimental indeterminacy means that the real system itself is inherently fuzzy and undetermined. Although such a view might be misguided, such that in some sense the subatomic structure of the universe *might* actually be predetermined, we can still take some comfort that in practice we will always remain unable to divine the full detail of such predestination. So for all practical purposes we may continue to believe in free will.

In the realm of *moral philosophy*, it seems likely that any advance in our understanding of the nature of human feelings and objectives — and the cybernetics of the dynamic systems presumably involved — will give us a better insight into what is entailed in choosing between alternative non-ideal solutions. (This of course is closely related to *jurisprudence* and to the questions of politics and economics mentioned above). Not that we should expect too much in the way of perfect solutions to such imponderable questions, however much progress we might make in understanding them. After all, it may well be that there is no attainable means for reconciling the fundamental requirements of all systems competing within the same environment, and — try as we might — there may be no alternative to having apparently-avoidable suffering occur somewhere or other. It might even be the case that there is no practical way of avoiding some holocaust in which *all* will suffer "needlessly" due to such constraints as informational overload or mental limitation in the face of rapidly moving events; though the better the understanding, the better the chance we will have of foreseeing such disasters and of avoiding
them while there is still time — perhaps entailing some painful decisions about whether the ends 35
justify the means, and whether we really know what we are doing with sufficient precision to warrant any drastic action we may see as necessary. How, in other words, can we be reasonably sure that we will not ultimately be surprised by the indirect consequences of our actions?

(4) *Academic psychology*. The contribution that the current ideas may make to *academic psychology* can be summarized briefly:- They offer a new point of departure for experimental studies such as the work on mother-child interaction. They also offer a new point-of-view regarding methodology, in which cybernetic structural-theories are given a status equal to that of experimentation. Last but not least, they give a tangible basis for psychological theories so that students might be saved the unsatisfactory ordeal of trying to cope with essentially unstructured concepts, with the attendant danger of falling into gratuitous mysticism; (see Section A1.1).

(5) *Physiology*. Similar points may be made concerning *physiology*. Here it might well be worthwhile extending experimental investigations such as to take more account of possible phase-related infra-red phenomena and their likely interaction with molecular activity (despite formidable experimental obstacles due to absorption in water). Or more simply we might at least pay more attention to the possible significance of shape and size of nerve-fibres. Here too, there is scope for a greater use of a structured cybernetic approach instead of an overzealous pursuit of experimental purity. Finally there is a trend to mysticism here too, which should not pass unchallenged — even if it should happen to be correct! Professor Eccles[2] might conceivably be correct in suggesting that one's mystical soul resides in some unfathomable way at synaptic
junctions, but we would be ill-advised to meekly accept this view while there are still any 36
prospects for more tangible explanations in detailed cybernetic terms. Moreover even at a more down-to-earth level, physiological explanations are not always as rigorous in their basic structural concepts as we might wish, thus leaving the student to accept some detail as an article of faith.

(6) *Embryology and cell-navigation*. One unforeseen by-product of the current theory is the potential explanation that it offers for the apparently-purposeful locomotion and growth of cells, and especially the embryological development of tissue-structures into their characteristic shapes. In particular, the likely existence of certain distributions of coherent infra-red frequencies and their presumed interference-patterns could do much to explain why different nerve-fibres tend to develop into one or another of several fairly-well-defined types. Thus it seems possible that a careful analysis of such shapes, sizes, and distributions, might ultimately serve as an unsuspected source of information about embryological mechanisms — even if they should fail to support the (other) hypotheses concerning infra-red signal transmission. Actually though, we might well find that both these topics of interest (embryological development, and functional characteristics) are inextricably interrelated, and that it is through rudimentary *usage* that proper development can take place.

(7) *Demystification within physics*. Finally, another surprising by-product of the current work might turn up within the realm of physics. The perceptive reader might notice certain features of the quantum-explanations which are less specific than we might wish. Without going into detail, we may attribute at least some of this vagueness to the current doctrine in physics that mystery is allowable (or even laudable) within the subatomic domain, provided that this structural
ignorance can be by-passed using mathematical abstractions — thus allowing for short-term tech- 37
nological advance, even if at the cost of fundamental understanding. (This ploy has, after all, worked particularly well in *applied* physics — though it is arguable that the imitators of this approach in other applied disciplines have not fared so well).

The present project, as well as possibly uncovering the need for a re-examination of such principles-of-physics if they are to be applied properly to biology, has also offered an epistemological justification for breaking away from the straightjacket of measurability as *the prime* criterion for respectability — and also offered an accompanying methodological approach whereby this might be done, within any field of study.

[2] Unfortunately I have only encountered this view at second hand, but I presume it is properly expounded in Eccles (1970). [For my later comments see: Traill (1999, Appendix C): *Mind and Micro-mechanism*, Ondwelle: Melbourne: www.ondwelle.com/BK0_MU6.PDF . — *RRT, 2006*]

PREFACE TO THE 2006 ONLINE EDITION *"37a"*

What has changed in the intervening 28 years since 1978? — Much in some respects, yet little-or-nothing in others. On the one hand, clinical advances in pharmacology have sometimes been quite impressive in dealing with somewhat specialized problems at a practical level; and there have been the notable genome-project findings, with implications raised in "(e)" below.

On the other hand, regarding comprehensive basic theory, I am a little surprised that no-one seems to have yet offered any systematic *rival* suggestions to those developed here, (nor taken the opportunity to re-invented them independently, given their restricted circulation). Indeed I am not aware of *anyone even asking* some of the same questions explicitly, let alone trying to answer them. (E.g. "How is it that we are so good at rapidly handling *sequential* lists and texts, when the traditional neurosynaptic accounts offer no clue to this ability?" — See item (vi), above, on "original page 24" as indicated in the margin.)

Of course, one major problem is the complex interdisciplinary nature of the brain-as-a-system — and that must surely mean that any coherent overall explanation of it must also be both (i) very interdisciplinary, and (ii) at least moderately complex. The present account probably answers that description, so is hardly surprising if even willing individual readers have trouble assimilating it — and *collective* understanding presents a further challenge which is often overlooked, (see below).

I am nevertheless encouraged by a series of promising explanatory by-products ("spinoffs"!) which have developed out of this study, and these suggest that maybe I am doing something right. (I contrast this with theories which are either too vague for them to be extended meaningfully, or which simply petered out through insufficient coherence: like the work of C.L.Hull (1930, etc.).) Anyhow these spinoffs include **(a)** offering a perhaps-plausible solution to the century-old mystery (Donaldson & Hoke, 1905) of what controls the geometry of nerve-fibre cross-sections (Traill, 2000, 2005a); **(b)** Belatedly "umpiring" a flawed 1977 debate on long-distance insect communication, arguing that the available evidence *does* actually point to a solution: modulated-infrared *fluorescence* emitted from pheromones (Traill, 2005c). — Hence a supplementary conclusion from the given "conflicting" evidence (yet to be further investigated): **(c)** that insects may be able to receive modulated infrared *directly* (as such) into their nervous systems, whereas visible-light would have to be processed in the ordinary textbook fashion via action-potentials; (*ibid.*). **(d)** That single-celled animals plausibly use short-range (≈20µm) infrared signals in lieu of a nervous system, and that this range might be a causal factor in determining cell-size.

Issues less concerned with infrared include: **(e)** arguing (Traill, 2005b) that the new discovery (Mattick 2001, 2003, 2004) that about 97% of the human genome did *not* code for protein, left ample scope for some of the ncRNA ("non coding" RNA) to fulfil the role of Piagetian "schemes" — a possibility which Piaget himself had occasionally hinted at hypothetically, and which seems to coincide very neatly with the "tape" analogy introduced in Part A, here below. Also in connection with that, **(f)** it was possible to further reformulate Piaget's theories in terms of perhaps-plausible *material* mechanisms (instead of just relying on *abstract* "scheme" concepts as is still the norm in Piagetian literature). — (Traill, 2005b).

Finally, in social psychology, there is the concept **(g)** that *society-and-its-science* is perhaps best seen as a separate learning-being — much more detached from the individuals within it than one might suppose — and that it makes sense to interpret much of social dynamics in terms of its own separate Sensori-motor and Concrete-operations stages. (One might ask whether it has ever yet achieved a Formal-operations stage!); (Traill, 1999, Ch.4). This approach seems promising for future investigation of various social dysfunctions — usually involving this *society-system*'s tricky interface with individuals in their various roles, but depending far more on Darwinian trial-and-error than we might have expected.

Changes in this text itself have been kept to a minimum but in some places (notably sections C6.7 and C8.1) prolific insertions of new dark-blue *subheadings* have been used to assist readability. Likewise some distracting digressions have been consigned to footnotes to get them out of the way. New text and other *amendments* (including any visible punctuation, etc.) are nearly always *identified* by being wholly dark-blue. One exception is for the in-text references in Part A where the changes are inevitable-and-obvious since the original text used *reference-numbers* instead. The other exception is in a small part of section C6.7 (o.pp. 300-303) which has a higher-than-usual density of minor amendments. It thus seemed tidier in some of these cases to flag the alteration by no more than a token colour-change in a few key letters only. (These patches arose largely because of an ambiguity in the meaning of *signal shape*, and it seemed best to clarify the situation whilst maintaining the original text as much as possible).

One new illustration (Fig. C6.7/2a) has been added. Another illustration (Fig. C8.2/1) has been appreciably re-configured — mostly for copyright reasons — but the original may be found in the 1940 paper cited there. For various practical reasons I have retained the original page-numbers as margin-inserts (in dark yellow) — referring to them as "o.p. 300" etc.

Finally, my apologies for usages such as *"he…his"* instead or *"she/he…her/him"* etc.; but of course that is the way one wrote back in the 1970s, and I resist any temptation to pretend otherwise.

R.R.Traill,

Melbourne, May 2006.

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Part A

ACQUISITION OF KNOWLEDGE WITHOUT TRANSCENDENTAL ASSISTANCE: AN EXTENDED PIAGETIAN APPROACH[1]

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[Parts I and II later serving as thesis-chapters A1 and A2 — as follows]

Chapter A1

A DISSECTION OF PIAGET'S KEY CONCEPTS — (Part I of the Kybernetes paper, vol.5)

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SUMMARY OF CHAPTER

Piaget's basic objective is seen as an attempt to explain how the individual and his evolutionary forbears can collectively gain practical mental-models of the "real world" — starting ultimately from nothing, and without independent assistance.

This paper firstly sets out to clarify Piaget's rather abstract views on this matter by postulating a more detailed mechanistic basis for them, and then interpreting various observations in terms of the hypothetical mechanisms. (This analytical approach is thus primarily intended as a heuristic aid; though it is also shown to be prima facie compatible with some other, non-Piagetian paradigms.) It is concluded that a useful way of summarizing the process is in terms of the *combined* operations of preference for "internal closure" within the brain (Internal Coherence or consistency), and preference for "external closure" during interaction with the environment (Pragmatism or experimentation).

A1.1 Introduction: Should we Postulate Specific Mental Mechanisms in the Absence of Clearcut Evidence? 73c1

The work of Jean Piaget and his colleagues at Geneva, is best known amongst psychologists and educationalists, rather than philosophers — at least in the English-speaking academic world. Yet his position is fundamentally philosophical — examining, in considerable depth, various questions relating to knowledge and its acquisition. Moreover, even amongst the educationalists and psychologists who rate his work as important, their interest seems concentrated on the descriptive results of his observations (such as successive stages of intellectual development in children) or his experimental methodology rather than his ideas of the underlying causes — that is to say they are concerned with the *behaviour* of the unknown "black box" rather than the speculation about what is inside it (in line with the behaviourist-operationalist tradition of contemporary science, which we will look at in *Part II*).

The "opaque style" of Piaget's writing is often blamed for the misunderstandings and ignorance of his work, and there is some substance to the charge. But perhaps a more compelling explanation can be made in terms of his own theory. To put it simply — in order to *understand* concepts one must, at some stage, tie its elements down to something concrete which can be actually seen and/or handled in various ways; and the relationships between these elements need 73c2 to be such that we can actively manipulate them — at least in our "mind's eye", or better still by physical manipulation.

But in the writings of the Geneva school, the concepts propounded are often presented very abstractly, without the faintest suggestion as to what the underlying physiological mechanisms and entities might be. Actually such scientific caution in published accounts may well have been wise wherever the concepts were necessarily in a state of flux — public acceptance of a primitive formulation might well have ossified the ideas into a comparatively sterile dogma, a not-unknown occurrence in science.

On the other hand, the theory has been so extensively developed by now, that failure to anchor it into some sort of "visualizable" model seems likely to lead the theory into the realm of mysticism rather than science. For one thing, the rather open-ended (and therefore ill-defined) concepts are likely to drift in their meaning from time to time — both in the mind of the reader, and also in the mind of the writer, as evidenced by the ambiguous meanings such as those identified by Furth (1969). Furthermore it has by now become a Herculean task to plough through the extensive literature on the subject, especially as one has to remain alert to shades or abstract meaning; and it seems difficult to avoid this effort if one is to gain any sort of deep understanding of the subject matter. Yet if it were possible to anchor these concepts in something more concrete, it would almost certainly become possible to condense the theory into a more manageable form, as well as rendering it more precise and definable. 74c1

Such a sharpening of precision would inevitably mean an increase in *information content,* and therefore *refutability.* In the absence of any further evidence, this information could (according to Popper) be supplied by any hypothesis we liked to make, provided we are prepared to scrap our hypotheses whenever they are found to produce inferences which do not accord with subsequent evidence — or are found to upset the internal coherence in an unacceptable way. Moreover, in practice such a theory[3] may still be found useful after falsification, when there is no other theory of comparable precision or lucidity to replace it — though it would hopefully be reduced in status to that of, say, "a mere heuristic device". (E.g. the epicycle planetary model did once have quantitative value, even though its detail was structurally false.)

Thus it is that the main purpose of this part of the paper is to suggest some plausible hypotheses about the basic *mechanisms* of mental activity, and use this hypothetical material basis as a medium for re-formulating the principal ideas which the Geneva school have put forward on the genetic epistemology of the individual. But although the discussion will concern itself with material events, it will not be necessary nor desirable at this stage to link these directly with anatomical structure or physiological events — even where these have guided the nature of the hypotheses. Instead, we may make metaphorical use of orthodox paraphernalia of information handling: computer-tape, reports on desks, and such-like.

[3] "Theory" is used here as more-or-less synonymous with the notion of a "concrete model". The justification for this, in the present context, lies in the idea that a *formal theory* is ultimately anchored in the concrete reality of the real world, and our actions on it (see below).

A1.2 Popper's Concept of Mental Versus Physical

Let us start by borrowing some basic terms from Popper (1972, Chs 3 & 4) and pruning his descriptions of them to the bare minimum: 74c2

WORLD 1	WORLD 2	WORLD 3
Physical objects and physical states	States of consciousness	Contents of thought *and* its representation in writing, tools, etc.

This conceptualization fits in quite conveniently with Piaget's as we shall see, and it is in some ways *more* convenient in view of the linear-ordinal implications of "1, 2, 3" — it is emphasized by both Popper and Piaget that worlds 1 and 3 (as such) can only "intercommunicate" via conscious beings (world 2).

Some other comments are appropriate here. We will generally assume that there is no overlap between these domains, but note that strictly speaking, worlds 2 and 3 are necessarily part of the real world (world l) — that is unless we are considering a transcendental being (the existence of which I would deny). For the special case in which we are trying to observe, in detail, our own states of consciousness, we can expect to come up against some quite anomalous results (Landsberg & Evans, 1970); but these need not concern us here. Of much more relevance to real life however, is the fact that at least some of world 3 is quite obviously situated in the real world outside our bodies-in the form of libraries, physical models and tools, etc. It would therefore be a matter of interpretation whether someone is using a book (say) as a world 1 "*natural*" object, or as a world 3 "*symbolic*" object.

There is no reason why we should not subdivide these domains (as Popper himself concedes) and it would seem useful to divide world 3 according to whether it is within the brain "*world 3i*", or *outside* the brain "*world 3e*". The "*e, i*" notation here tentatively implies that the two sub-domains are, in some sense, operating in parallel.

We may also divide up world 1, and I suggest (using an arbitrary decimal allocation):

world 1.0 for actual phenomena and objects as they really are,

world 1.1 for signals which emanate from them,

world 1.2 for whatever is picked up by the sense organs . . . and so on through the more-or-less automatic pre-processing which is known to occur (Hubel & Wiesel, 1963; Inhelder & Piaget, 1959), until . . .

world 1.9 the results are dumped, as "progress reports" (in the form of dynamic display summaries rather than written reports) onto the "in-desk(s)" but they rapidly dissolve as further information comes in — except in so far as they attract the attention of "consciousness". 75c1

The decimal notation here implies a serial processing which obviously has a good claim to validity. However, there may well be some feedback between these stages (not to mention possibly large amounts within them), and to this extent our conceptualization will have to be considered as approximate only. This almost certainly applies much more strongly within the consciousness-domain (world 2), so I will not attempt any subdivision of this simple sort for that part of the system.

A1.3 Similar Concepts in Piaget 's Work

We may now start to clothe this skeleton with Piagetian terminology and concepts, plus any other artefacts which may help to render the ideas more intelligible. The main sources here are Furth (1969) and Piaget and Inhelder (Piaget & Inhelder, 1966).

Concerning the *world 1,* and the associated question of information presentation; I do not intend to dwell on the comparatively automatic processes involved in the operation of the senses and the re-organization of information which arises from it. For our immediate purposes it will suffice to say: that distortion will certainly occur (though in a more-or-less predictable manner); that such distortion will often be positively helpful (e.g. in selecting the features most likely to be useful — as determined by evolution of the species); and that the type of distortion can often be varied to some extent according to the whim of the subject (thus selecting what *that individual* has found most likely to be useful, e.g. minimizing the distortion and tending towards objectivity, by a judicious re-integration of distorted impressions).

Let us move on then to the all-important world 2. This is referred to *as thinking* or *knowing,* and the key concept is the *scheme,*[4] which we may speculatively think of as being made up of a population of identical strips of "computer-tape", where the replication may be seen as a means towards statistical and graded effects-based on an essentially on-or-off mechanism. Each replica may be thought of as encoding two zones: the first comprising a "label" by which it can be identified if encountered, and thus activated; while the second holds an encoding of a sequence of actions to be followed. We may formally use the term "*tape*" to refer to this hypothetical entity, 75c2
and use "*label*" and "*program*"[5] for the two zones (partly following computer jargon). The mere *linear* nature of this analogy is quite deliberate, though this does not at all rule out more elaborate functional-complexes as we shall see.

At birth, schemes are prominent in governing reflex behaviours of a fairly stereotyped variety. (There may well be other active schemes, and the uncoordinated movements of the infant could plausibly be attributed to them, but they produce no *organized* manifestations.) Later, however, *modifications* of the existing schemes develop, as also do new superordinate schemes which *call* sequences of the lower-order schemes, like sub-programming in computer practice.

It should be noted though, that the system as here described comprises many "programs" operating in *parallel,* unlike an orthodox digital computer. This implies the need for some sort of stabilizing coordination between those "tapes" actively involved; but such problems have already been encountered for some time in connection with biological growth, so this need come as no surprise. [Indeed the "tape" concept has some obvious micro-biological analogues (such as RNA or protein molecules), but there is no need for us to claim any identity with them at this stage.]

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[4] Where *scheme* is not to be confused with *schema* (*plur. schemata*). But note that English texts prior to about 1966-69 have tended to use "schema" for both meanings. (The *schema* will be discussed in §1.5.) In addition it will be convenient to introduce the term *schemoid* to deliberately include both senses, non-committally.

[Note added 2006: Although I still stand by these distinctions, I now doubt the wisdom of trying to use the *"schem..." words alone* to represent these shades of meaning (except in localized linguistic contexts — such as here perhaps). As just noted above, the main problem is that different writers and translators have had different understandings on the word-meanings, and it is now probably too late to reconcile such linguistic divergence. My 2005 suggestion was to depend instead on suitable adjectives to make the necessary distinctions — see Traill (2005b): *www.ondwelle.com/OSM02.pdf* (or .htm) or *www.wbabin.net/physics/traill2.pdf* —— RRT]

[5] Pascual-Leone (1970) following von Uexküll, uses "s" and "r" — stimulus and response — for the scheme *as a whole*; whereas here the scheme has been split into many replicated "tapes" .

A1.4 Application and Extension

Having looked at the bare fundamentals of the theory, I now propose to dive straight into using it to explain some generally accepted observations of behaviour, introducing further aspects and interpretations of the theory as we go. This seems easier on both reader and writer than the alternative elaboration using more-or-less pure abstractions, with perhaps unfamiliar terminology.

(i) Reflexes.

Let us start then with the reflex actions of the new-born, and in particular the "rooting reflex" in which the baby moves its mouth (turns its head) in the direction of a touch on the cheek. We may suppose that the mechanism of this *scheme* works like this: The stimulus is processed and eventually produces a coded internal signal pattern which calls for a particular *label* or range of labels. This call is equivalent to the paging-system of a large organization such as a hospital; and the call-signal itself need not be completely unambiguous as the specificity could be increased by 76c1
limiting its distribution to the relevant wing of the hospital.

The call would, in effect, ask all "tapes" with that label to "report to" a particular control centre[6]-or-mode in order to carry out their particular skill (or "program"). In the simplest case we shall assume that this results in the calling of identical tapes (all having the same program, as well as the same label) until the "control-centre" is full so that any further late-comers are turned away. Meanwhile, those in the "control-centre" collectively put their program into effect-by turning the baby's head, in this case.

If the stimulus is withdrawn, we may suppose that the "tapes" are ejected from the "control-centre-or-mode" — perhaps to be replaced by a scheme-system for an emotional frustration reaction. This emotional scheme may also be describable in terms of "tapes", but it need not be assumed that it will follow the same laws in the same detail.

If the head-movement brings the nipple (or finger, or whatever) *into* the mouth, we may expect a similar ejection of the tapes from the control-centre. But this time they will be replaced by a similar set of tapes comprising the next scheme in the adaptive sequence — that for cyclical sucking-actions. (Here we may think of each tape as being re-cycled, at an appropriate phase of the cycle, until it is eventually ejected due to interruption, frustration, or satiation.)

For such goal-seeking procedures in two (or more) dimensions it would be inefficient and cumbersome to have a completely different scheme for every goal-centred radius on which the stimulus might fall; yet clearly a stimulus to the left of the goal would call for a different response from one on the right, or above, or below, etc. It seems likely, therefore, that there will be (say) four different schemes corresponding to such cardinal directions, with labels to match; and any stimulus on an intermediate radius (say at "4 o'clock direction") will produce a *mixed* call resulting in both Right-Hand and *Below* types of label being called to the control-centre, in the ratio of (say) *2:1*.

Subsequent execution of this mixed scheme could take one of two forms: a hybrid action, with the musculature of both programs being called into play simultaneously and more-or-less proportionately. Alternatively we might get an alternation, with the more populous scheme being 76c2
activated at the expense of the other — but with re-adjustment of the population within the control-centre until the tapes of the first scheme are outvoted by the second, resulting in a switch of policy until the original direction-of-imbalance is restored again, and so on. (It may be

[6] Cf. Freud's (1917) concept of the preconscious and conscious as *rooms* (p 249). But in view of K. S. Lashley's work on brain-ablations,in the 1920's, and subsequent work, it would be unwise to take this "control centre" as being too literally localized; we should think rather in terms of *communicational* proximity .

significant that the eye-movements of babies start off with a tendency to move horizontally and vertically, rather than obliquely.) In fact, the second alternative is implicit in the first if the "more-or-less proportionate" weighting of the two stimuli turns out to be all-or-none (i.e. Boolean); so clearly one might include near-Boolean decisions as well. This is the sort of thing we would expect if the control-centre's "inner sanctum" would admit one-or-very-few tapes at a time. And perhaps the set-up also entails some risk that the encumbent tape will be ejected randomly.

(ii) Classical Conditioning.

Next it will be instructive to turn away from Piagetian paradigms to consider the time-honoured case of Pavlovian classical conditioning (Mednick, 1964). If nothing else, this may give us some sort of check on the plausibility of our model.

As a result of evolutionary processes, some types of stimuli are "inherently" rewarding.[7] Certain tastes in the mouth, for instance. Now we very quickly and unknowingly learn whatever other stimuli usually accompany these rewarding stimuli, and thus tend to respond to the new stimuli as if they constituted the original inherent stimulus. The *sight* of food is a case in point, or any other arbitrary neutral stimulus which happens to coincide with the reward — provided the individual can discriminate the stimulus. In fact the newborn infant cannot discriminate the sight of *objects in general* (as we shall see), but a particular tactile stimulus, or a very simple light or sound stimulus would do instead.

A plausible explanation of this, in terms of our "tape" model would be: The two stimuli, occurring more-or-less simultaneously will *both* call representative tapes into the control-centre *together* (though not necessarily into the "inner sanctum"). This physical or communicational proximity may then make possible some sort of cross-reference between those particular tapes actually "within" the control-centre. Let us suppose that some of them will swap labels with each other, like a genetic chromosome cross-over. Subsequently then, any presentation of the *new* stimulus will, in some cases, call forth a tape whose "program" part will cause activity appropriate to the *original inherent* stimulus. (Whether or not it will also produce similar 77c1
satiation and emotional effects is something which might be considered as a separate issue).

Anyhow repeated co-presentation of this sort could be expected to increase the effect, especially if there were some sort of weeding out of pairings which were *not* repeated on subsequent occasions ("extinction"). If the repeated learning process were interrupted by a distraction, we might say that the tapes would be ejected (in favour of those of the interrupting stimulus) and furthermore, that they would become dispersed somewhat from easy access to the control-centre, so any resumption of trials would show a drop in effective pairing ("external inhibition").

If the extinction process (due to a cessation of paired presentation) were considered to take place at a control-centre[8] then the spontaneous recovery during a long rest in the proceedings ("reminiscence") could be explained as a re-distribution of tape-strips: those from the general population replacing those "near" the control-centre which had been "wiped-out". Similarly an interruption stimulus during the extinction process ("disinhibition") would presumably clear the encumbent tapes away from the immediate control-centre, so that when they return, they would have partly replenished their proportion of "conditioned" tapes from the pool of tapes just outside the periphery around the control-centre.

[7] Unless temporarily vetoed due to satiation, or suppressed by an "abnormal" sequence of events (such as genetic mutation or mental trauma) producing conditions which we would describe as "pathological".

[8] Presumably the same one, though not necessarily.

(iii) Operant Conditioning[9]

In this case, instead of *two stimuli* being presented together and becoming "cross-referenced" to each other, it is arranged that a certain element of *behaviour* ("spontaneously emitted" by the subject) is paired with a significant stimulus. Now from a Piagetian viewpoint, the scheme is the basic world 2 representative of any elementary action or any elementary perceptual coding which gets that far (Furth, 1969, p 138). Thus the operant conditioning paradigm would appear to be little more than a variation on the classical conditioning theme — with a motor-scheme substituted for a sensory-scheme.

(iv) Innate Releasing Mechanisms (IRMs) and Fixed Action Patterns (FAPs).

These notions of the ethologists look very like congenital schemoids which, in the species concerned, are not amenable to modification by learning — though they may perhaps be incorporated as parts of more general schemoids. Thus a particular pattern of visual cues — such as the sight of an open beak within the nest-may constitute an IRM (so presumably causing a particular "label" to be called) with the consequence that a particular stereotyped sequence of behaviour such as a feeding reaction (a "FAP") will follow. This FAP is presumably the manifestation of a schemoid,[10] and is therefore (according to the current model) coded as a "program" on an ensemble of identical or interrelated "tapes". 77c2

Congenital walking-reflexes (etc.) in mammals may well be of a similar nature, *but* they are generally amenable to subsequent modification. This may be due to the availability of mechanisms capable of effecting changes, rather than the properties inherent in the schemoid, though both are possible-perhaps in combination.

A1.5 Application to Piaget's Theory Itself

We may return now to Piaget's own paradigms and consider how it is that an individual comes to the *hypothesis* that there are such things as reasonably permanent three dimensional "objects" in his environment. Or indeed how it is that the impressions which reach him visually can be *interpreted* as vaguely permanent two-dimensional pseudo-objects (also hypothetical from his point of view).

First let us look carefully at the mathematical concept of "mapping" from one domain to another (the "co-domain"), and the closely related concept of model-building. The nature of this mapping problem depends rather critically on whether we assume a transcendent role or not. [For the traditional Pure-Mathematician there is comparatively little problem. His position seems to be unashamedly transcendental (with respect to his limited universe of discourse — his *detached* "world 1"), so he can juggle meta-mathematical concepts until he has established whatever morphisms his artificial symbolic systems may offer. The position for the *scientist is* rather more debatable as he has to masquerade, for part of the time, as a transcendental being (possessing *episteme)* without strictly being entitled to do so; a point which we will take up again in Part II. As for the contemporary work on *artifical intelligence,* it is not entirely clear where it should be placed. This rather depends on the type of interaction (if any) the computer has with the "real outside world". But even purely internal computer exercises, with a static *(imitation) world 1,* can be immensely complex, even though such problems are arguably similar to those of Pure Mathematics.]

[9] E.g. see Mednick (1964)

[10] Despite thc stability displayed, this is probably a scheme (rather than a schema) for reasons to be explained (in terms of genetic replacement) in the second paper of this series (Traill, 1978a: Chapter A3, below).

But we are trying to do without transcendent observers.[11] The "conscious" observer will, in fact, be confined to world 2 (the co-domain) with no input or knowledge but the distorted, incomplete and *uninterpreted*[12] "reports" on his "in desk"-display. How then is he to set about building his model of the outside world (*world 1.0*)? 78c1

Let us be clear about this. Following the Geneva view it would seem that *something* does remain of the Kantian *a priori* knowledge, but this is explicable in evolutionary terms and plausibly consists of: (*a*) the particular procedures for automatically transforming (i.e. distorting) input information, in a way which tends to be adaptive within the natural limits imposed on it. (*b*) Basic input stereotypes corresponding to the end products of the perceptual process (e.g. carrying the information that a light/dark boundary is moving across the visual field in a certain direction at a certain speed, (Hubel & Wiesel, 1963) or ethological IRMs). These stereotypes, alone or in combination, may be considered capable of "calling schemoid labels". (*c*) A complete basic set of stereotyped action-elements for controlling the musculature and other effectors; these being "callable" by "program elements" within the schemoids. (*d*) A limited repertoire of preset (but often variable) schemoids serving as the *controlling* basis for hereditary reflexes. Possibly also (*e*) various schemoids for purely internal ancillary purposes.

Finally, (*f*) there is the very important propensity of the organism to reach *conceptual equilibrium* (a particular case of physiological equilibrium-seeking or "homeostasis"). This can usefully be thought of here as an effect which selectively supports the retention of those mental *structures* which collectively form a *coherent* whole — a *group* in the mathematical sense of maintaining *closure* when operated on in a particular manner. This means that, provided we stick to a particular set of "operators" (e.g. rotation by multiples of 90º in the plane), the elements (e.g. unit measures directed to North, South, East or West) will *not* produce new elements outside the set when operated upon (i.e. *not NE,* and *not up,* etc.). We may thus think of the structure as being, in some way, self-conservative or permanent — provided that increasing degrees of "groupness" 78c2
are rewarded by increasing stability.

Thus even though the baby initially knows nothing at all about objects *as such,* nor even that such "permanent things" exist, he nevertheless "tries to make sense" of his encounters with reality by saving up any apparent replications of apparent closure amongst the schemes existing in his world 2. So if the outside world has any closure/coherence at all, it is likely that some of this will be reflected, however imperfectly, in the observer's world 2; and it is thus open to identification with internally produced group-like structures. Moreover these internal structures will, with any luck, have a group-like structure which is at least something like that of the real world (world 1). With further luck, this structure may be improved on subsequently.

But note that there is no absolute guarantee of success. Of course the more elements we take into account in our mental model simultaneously (capacity and techniques permitting) the more likely that only the *right* solution will really give closure in the model, but we can never be *sure* in an absolute sense. For instance it seems likely that the child's first (learned) group-structure model of reality is one-dimensional, but models of reality assumed to have this property would not show a very good record of consistency. Similarly two-dimensional models show certain

[11] And this implies that we should also forgo the use of meta-linguistic devices (Gödel, 1931). Of course, in practice, it is *convenient* to use meta-linguistic concepts, but this implies an artificially limited universe of discourse or (equivalently?) an assumption of axioms (i.e. *episteme* in our present context).

[12] Strictly there is some minimal interpretation, e.g. see Hubel, (1963). But whatever interpretation the observer may have access to, it can have no more absolute truth-justification than his own hypotheses. Such inherited interpretations have arisen through a similar process phylogenetically, by evolution — a *pragmatic* criterion.

disconcerting irregularities when a toy disappears behind a cushion (becoming irreversibly "absorbed into it" from the infant's viewpoint, as Piaget has suggested). But can we be certain that a three-dimensional model is sufficient? The more mathematically inclined physicists would claim that there are not only four dimensions, but that they all have the same mathematical status (hence the mystical four-dimension concept of interchangeable space-time due to Minkowsky (1908), which has kept science-fiction writers busy ever since). Rather less spectacular is the notion that time does constitute a fourth dimension of comparable importance, but that it is qualitatively different; anyhow it would seem that the mind divides the dimensions up in this way — space first, and then time a good deal later, and by a roundabout method (Piaget & Inhelder, 1966).

Efforts toward closure/consistency/coherence take place in two directions. On the one hand there is "Formal reflecting abstraction" which takes place within world 2 (though possibly drawing on world 3i). This presumably takes the form of a free exercise of interrelated-scheme systems (especially during "REM" sleep, [13] and generally with motor-correlates largely suppressed), and it is plausible to suppose that this exercise aids the disintegration or modification of those systems which are seriously lacking in closure (at least as compared with other similar systems). In the same way, our number-system has been revised several times to cope with lack of closure under such reasonable operations as division and taking square-roots. 79c1

The other effort toward closure involves interaction with world 1 (anywhere between *world 1.0* and *world 1.9*). It is fundamental to Piaget's position that this process of perception is no mere passive taking-in of whatever fate may serve up on the "in-desk" (world 1.9). Such passively accepted "information" has *no meaning* beyond its possible role as an automatic IRM (like a punch-card fed into an ordinary digital computer); and such an activity betokens no intelligence in the sense of the organism being able to develop its own internal "mapping" ability.

Instead, the process will inevitably involve an active exercise of the subject's own schemoids, either in the form of a participatory intervention in the outside world (such as sucking, or turning), or tracing an outline by visual fixations, or selecting amongst the "reports" which then appear on the "in-desk" (consciously or otherwise). Or indeed any dynamic combination of such interventions.

Let us suppose that at some stage (before birth) the individual's *world 2* and *world 3i,* taken collectively, contain no structures other than hereditary schemes (and the "tapes" of which they are composed). For some arbitrary reason, a sufficient number of tapes for one of the schemes comes to "wander into the control-centre" and become active. The result — a kick or suchlike. This is then likely to become paired with whatever sensory schemes are active at about the same time, and these will include sense-schemes which have been activated as a *consequence* of the spontaneous action. Here we have the makings of an elementary representation of the *world 1.0,* which from the foetal point of view would be something like this (if it could introspect): "Something, somewhere, echoes back *in response to my thought* — cogito ergo *id est* (mihi)"! [I think, therefore *it exists* (for me at least)].

In other words, the thought/scheme and its associated action *precedes* any subjective awareness of any outside object as such. This formulation starts to explain how it is feasible to set up a "mapping" between the outside world and the thought-domain. It is no use the subject waiting for the outside world to just walk in and present its "map-position" credentials, so to speak. Instead the subject must start off himself with what amounts to a preconceived idea (e.g. that *no* outside world exists), and then see what happens when the idea is applied in practice. 79c2

[13] "Rapid Eye-Movement" sleep; e.g. see Piaget & Inhelder (1966, page xviii).

So we now have an alternative process of testing for closure — between our own mental models and their interaction with the outside world, via senses and actions. This sounds very like Popper's view (for knowledge at the scientific community level) that one should start with pre-conceived ideas and then test them with a view to scrapping them if they do not give "closure" in the sense just discussed. We shall come back to this point in *Part II,* but I shall just mention in passing that I think he has overemphasized this closure-with-reality at the expense of the first-menioned type of closure: closure within thought. I would claim that they are about equally important — *except* in the above-mentioned case involving the *first* item of feedback, when the thought domain has (as yet) no artefactual structures to work on, so no basis for closure-seeking within itself.

Before looking at further details relating to the way in which a mental "model" is *constructed* to represent an object, let us first see what is known or conjectured about the *nature* of such a model, once constructed. The Piagetian term for the special case of a two-dimensional model is "image". This image is not to be confused with any perceptual configuration, so it is quite different from the "image" of optics; but it is the means whereby a child can bring back (into thought) a "re-presentation" of something seen in the past, thus enabling a drawing to be made from memory.

Actually an image appears to be a particular case of a more general type of entity which Piaget and Inhelder choose to call a schem*a (schèma).* In general then, a schema will be used as a repository of any static canonical configurations[14] which "look like" being of more-or-less permanent value to the individual concerned. (This is suggestive of the concept of Long Term Memory (LTM) which is usually regarded as a separate psychological specialization, e.g. see Wickelgren (1970)).

Initially such schemata are associated with overt action, as in the case (mentioned by Piaget) of a child *opening its mouth imitatively* as a symbolic preliminary in attempting to open a match-box. Gradually however, the overt component (*world 3e*) often dies away leaving an "internalized" symbol (*world 3i*). Just what sort of physical basis such a schema might have is open to question. For the two-dimensional *image* it would probably suffice to postulate a sort of fossilized scheme — a scheme which had been made immune to modification, but could still be "called" in something-like the usual way, including subsidiary calls to other schemoids as required (also some calls might have affective or "simili-sensory"[15] affiliations, a possibility which may perhaps be denied to ordinary schemes). 80c1

Three-dimensional configurations are, however, less amenable to linear-type based mechanism. On the other hand though, it scarcely seems feasible that any general workable Biochemical mechanism could actually be connected into a three-dimensional *physical* structure — yet it is just possible to conceive of a *cross-referenced* call-system of free linear codings such as was postulated above for the schemes themselves. Furthermore, the lattice-structure implicit in such cross-referencing would, in principle, also serve for formal logic systems and presumably other abstract entities as well (e.g. see Inhelder & Piaget's (1959) "Early Growth of Logic", p. 273 ff.).

As for the physical location of such *world 3i* symbols, they *might* be located in a separate place away from the schemes of world 2; but there seems no pressing reason why they should, because their respective "domains" could be specified by code or properties rather than by

[14] Two or three-dimensional, or abstract groups of operators, or whatever.

[15] I would prefer the term "provinance" (borrowed from Archaeology), or perhaps "affect-contextual", but "simili-sensory" is the Piagetian term. Anyhow, whatever it may be called, we appear to be talking about the same phenomenon as that discussed by Penfield (1958, p 31 ff.) when he refers to "feelings of fear or familiarity" (etc.).

position (and it probably would be under such circumstances). Moreover, a separate location might be expected to produce transport and communication problems.

Thus it looks as though it might be fairly simple to explain the formation process for schemata after all. Given that a system of scheme "tapes" has collectively developed a sufficient degree of closure by an "equilibration" process, it seems likely that this success in the closure-test will result in a transmutation of the relevant "tapes", freezing them into their existing configurations. Indeed it would seem in keeping with chemical and micro-biological systems if this stability of structure were to follow automatically from closure — by some sort of continual dynamic exercise for instance (a concept faintly reminiscent of Hebb's (1949) "reverberating circuits", but on a much smaller scale of magnitude than that *implied* by him, though he was careful *not to insist* on any particular physical basis for his theory).

It seems likely that once formed, such schemata are seldom re-dissolved. However with disuse, *references* to such schemata within the schemes of world 2, would tend to be eliminated 80c2
more-or-less exponentially, so that ultimately the unused schemata would become very difficult to retrieve. The evidence for this is rather varied and circumstantial, including the psychoanalytical concept of barely-retrievable childhood memories, the work on LTM (mentioned above), and Penfield's (1958, p 31 ff.) neurosurgical probes which might sometimes be interpreted as generating calls to otherwise inaccessible schemata.

A1.6 An Orthodox Summary of Piaget 's Stages

We have now covered those parts of the theory which are most relevant to the arguments in Part II. However, to give some semblance of completion we may look, very sketchily, at the overall stages of development. (There are numerous texts on this *macro-descriptive* aspect of the topic.)

Approximately (0–2) years, "sensori-motor": Lacking schemata (internal symbols) and so lacking a sense of the permanence of objects.

Approximately (1½ –8) years, "pre-operational": Symbols for objects and then also for transformations (creating, transforming or destroying objects), but no coordination between them.

Approximately (7–11) years, "concrete operations". New closure systems involving both objects *and* their transformations. New schemes for dealing with basic object- and transformation-schemata are referred to as "operational". But use of these operational schemes requires the objects to be present (i.e. "concrete") and perceivable, perhaps because some symbol-handling capacity is fully stretched dealing with *operational* schemata so that there is none spare for handling *object* schemata.

Approximately (11+) years, "Formal Operations". The need for perceptual support for object representation is overcome, opening the way for the disposable-variables of algebraic systems and hypothetical object-relationship systems.

What underlying physical changes are implied by this progression? Several factors come to mind, though there may well be others. Firstly there is some degree of recursion as witnessed by the progression: simple scheme — compound scheme — operational scheme. Then secondly there is the fact that at least some of the underlying physiological capacities develop more-or-less autonomously ("maturation"). Progressive myelination of nerves (from the head down) is perhaps the best documented, but Pascual-Leone (1970) discusses the implications of a likely increase in attention-span with age (equivalent to a growth in the size of the "control-centre" in the tape-population model discussed above). 81 top

Furth (personal communication) raises another consideration: the distinction between learning and non-maturational development. It seems possible that such development (affecting overall outlook in a major way, and occurring in more-or-less discrete steps) is the result of qualitative re-organization of internal closure systems — similar to a phase change in a chemical system — when the stabilizing influences of the old system are overtaken by the influences conducive to stability in the new system.

Chapter A2

TRUTH AND OPERATIONALISM — (Part II of the Kybernetes paper, vol.5)

SUMMARY OF CHAPTER (73)

In the light of the above, Part II briefly discusses the views of Tarski, Popper and the Operationalists/Behaviourists concerning the nature of truth and the legitimacy of reductionism into unobservable domains.

"…toute logistique s'appuie sur des présuppositions intuitives: à lire les principaux logisticiens, comme Russell, v. Wittgenstein, Carnap, etc., on s'aperçoit vite qu'ils se réfèrent tous à certaines intuitions tenues par eux comme allant de soi dans la mesure précisément où elles échappent à la vérification logistique." 81c1

(Piaget, 1949, Introduction).

(Popper, 1963, Ch. 1), following Plato, goes to some pains to distinguish *episteme* (absolute unknowable true knowledge) from *doxa* (fallible but attainable human knowledge). Subsequently (Popper, 1972, early chapters) he develops his "three worlds" concept which we have just been using, in which *world 1* presumably embodies *episteme* and *world 3* presumably embodies *doxa.* In this he is encouraged apparently by Tarski's criterion of truth (episteme) as corresponding to reality (Popper, 1972: Chs. 8 and 9).

In so far as Tarski's criterion represents a move away from the operationalist view (that what cannot be observed does not exist, so episteme does not exist), then to that extent I share Popper's enthusiasm for the Tarski formulation. Yet it would seem that Tarski has erred somewhat in the opposite direction. Thus he writes rather disparagingly (Tarski, 1972): "other conceptions and theories of truth are also discussed, such as the pragmatic conception and the coherence theory." By contrast I would associate these two with the two types of closure attempted by the developing child as discussed in *Part I;* but note that these had to be used *in cooperation.* They would make very little sense taken in isolation, and that is presumably Tarski's (and Popper's) quarrel with them.

Tarski continues: "These conceptions seem to be of an exclusively normative character and have little connection with the actual usage of the term 'true'; none of them has been formulated so far with any degree of clarity and precision." It is to be hoped that the above condensed version of Piaget's theory may serve in some way to answer this latter challenge from Tarski. Truth-as-corresponding-to-reality (episteme) forms an essential part of the Piagetian system as described here, but there is no absolute guarantee that any mental model which purports to 81c2
represent reality does so faithfully. This applies to objects,[16] and also (it would appear) to logical and mathematical systems as well.

Operationalists in Physics and their Behaviourist counterparts in Psychology, seem to make the fundamental *theoretical* mistake of assuming that there is *something* which can be absolutely relied on. Thus *certain types of observation* are taken as infallible because they involve concepts like "object" which are so thoroughly ingrained at a very early age that we take them as valid *a priori*[17]. Similarly *certain types of coherence* in a symbolic model are often assumed to be *a priori* valid because they accord with a "logical schema" which was also so thoroughly ingrained at an early age, that we take *it* to be an *infallible framework* on which to hang relationships

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[16] In so far as there *are* such things as real *objects!* See Popper's discussion (1963) of Parmenides; not to mention the wave-particle duality of Modern Physics.

[17] Moreover any IRM-"concepts" would be ingrained even earlier: phylogenetically.

both old and new. (The same point has, of course, also been made about "self-evident" moral laws.)

I suggest therefore that the operationalist *theoretical* standpoint is inconsistent — or at best rests on an arbitrary division of doxa into "*near-enough-to-episteme*" and "*mere guesswork*". If the Piagetian view of knowledge-acquisition is accepted rigorously, then the consistent operationalist should, it seems, automatically become an agnostic and disclaim any knowledge about anything at all.

But of course, in practice, we must all adopt some sort of criterion of what we are to regard as indubitable. We must impose constraints on ourselves and our thinking or we will get nowhere (as Ashby points out) — and imposing constraint is arguably the purpose of closure-formation. The mistake is not in drawing such arbitrary distinctions, but in believing inflexibly that we have hit on *the* correct place to draw such a distinction, for such a dogmatic belief amounts to the
delusion that we have incontestably acquired some episteme. The cure would seem to be to keep 82c1
testing closure of both types (empirical and internal coherence) according to some suitable strategy. Just what that strategy should be is open to debate — episteme is denied us here too.

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[then serving as thesis-chapter A3 — as follows]

Chapter A3

ANALYTICAL THEORY OF SENSORI-MOTOR SPATIAL DEVELOPMENT[18] 61

SUMMARY OF CHAPTER

This paper develops more specific details on how natural mental-function might evolve within a wholly-material brain system, depending entirely on "self-organization" operating within a reasonably consistent environment.

It is assumed that mental development can, in principle, be explained in detailed mechanistic terms. The attempt is then made to give the outlines of such an explanation, drawing on existing physiological knowledge, and considerations of the practical "design" difficulties which such a system would necessarily have to face. RNA-*like* codeable strings are seen as the basic memory elements (rather than adaptable synapses). "Concepts" are explained as Piagetian mental models, built up in explained stages due to interaction with the real object, and encoded on the linear elements.

Coordination between these elements is seen as biochemical but with the added available intermediary of electrically mediated signals, allowing coordination at a distance. The likelihood that subsequent developmental periods may recapitulate the same overall strategy is considered.

A3.1 **Introduction** 61c1

The general outline of a mechanism to explain Piaget's theories has been described (Monod & Jacob, 1961; Traill, 1975) using basic elements differing substantially from those implied by Hebb (1949). It is now proposed to explore further the details of this system.

The previous papers postulated processes whereby Piaget's schemes and schemata could be encoded by replicated intercommunicating linear micro-objects. These are referred to metaphorically simply as "tapes" or "strings" to avoid prejudging the physical details of their structure; though it may help exposition to think of them tentatively as short RNA strips or suchlike.

Such *scheme-tapes* were envisaged as being generated initially with arbitrary "label" segments, and with blank, empty, or arbitrary "program" segments which could subsequently be altered to something more meaningful. In addition it was supposed that positive or negative segments or tags would be attached to these tapes according to reinforcement contingencies; and on the basis of these tags, the tapes would be preferentially replicated, or dissolved.

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[18] This is the second in a series of papers relating to the nature and acquisition of *concepts*, seen as having a material cybernetic embodiment within the brain. The other main papers so-far completed [in 1978] are (Traill, 1976: the above chapters A1-A2 — and 1977: "Part B" whose hyperlink follows below) respectively.

The program part of each tape was thought of as being made up of a sequence of code-units, and each of these was supposed capable of generating a specific code-*signal* into a control-network, or a limited part of a network. These signals were supposed capable of activating specific "sealed units" of stereotyped effector activity, or else capable of activating other schemes or schemata — thus making feasible "sub-programming," "cross-referencing" and other departures from strict linear reading of the "list" embodied by a single tape. Effector activity would of course include motor actions, but it also includes internal *modification* of input, and perhaps positive feedback to "hold" the status quo as a mechanism for attention. 61c2

The *senses* were seen as involving lengthy and complex pre-processing, with comparatively unalterable "hardware," (Hubel & Wiesel, 1962; Iversen, 1974) but with parameters controllable by "modifiers" governed by some of the effector code-signals just described. The end result of this preprocessing would be that a new set of *externally generated* code-signals would then find their way into the control-network, or part of it. These signals were then presumed to activate schemes, schemata or effectors in the same way as internally produced signals. Clearly feedback effects such a tracking could be explained on this basis.

Schemata were seen as scheme-like structures which had somehow acquired an inherent stability and were therefore virtually impervious to modification, unlike schemes. It was supposed that they owed this stability to some manifestation of corroboration (i.e. selfconsistency, or "internal closure," — as if implying that the "idea" inherent in the structure was likely to have permanent value, and was therefore worthy of storage in Long Term Memory). 62c1 However internal-closure was not seen as sufficient; there was also the need for "external closure" in the form of predictions of some sort, and some reality-testing of these expectations.

It is sometimes useful to refer to "scheme-like things" without necessarily distinguishing between schemes and schemata. In such cases the term "schemoid" will be employed. (Note that schemoids are taken to be *populations* of tape-like entities, and are not discrete unique entities as such; though they may be able to coordinate their activities such as to produce discrete unique action patterns.)

A3.2 The Nature of Hereditary Schemoids

Table I suggests a number of stimulus patterns which might reasonably "call a label," and a number of likely "sealed unit" effector stereotypes; these are depicted as being already paired-up into workable schemoid tapes. It seems fair to say that each such reflex schemoid starts off as being independent of any *voluntary* control; and furthermore that it has no special affiliation with any other schemoid capable of *reversing* its action, nor a special affiliation with one or more schemoids capable of achieving the same basic result in a *different way*. 62c2 If this is so, then we will need to be able to explain the *development* of such affiliations in order to account for the later acquisition of mental systems with the properties of mathematical "groups."

Another noteworthy point is that not all the reflexes are of obvious immediate use to the neonate. Clearly the rooting reflex *does* make immediate sense, and arguably so does the palmar reflex. It is rather less obvious what immediate use the neonate might find for the stepping reflex, though its *later* use can scarcely be denied. This comparison raises some delicate problems of stability: If all these congenital reflexes *are* stable, how do we come to master or eliminate some of them? If they are *not* stable, but amenable to classical and operant conditioning including extinction, then how is it that the stepping reflex comes to survive long enough for the infant to learn to walk?

TABLE I

Examples of congenital schemoids and the stimulus patterns likely to cause their activation. The body entries of this table give the probable outcome if the particular action occurs in the particular context; — each such outcome may then be evaluated for its adaptiveness. Thus evolution would favour the adaptive “main diagonal” and tend to leave other entries as “null.” However this could be modified by learning, as in Bruner and Bruner’s experiments (1968) in which *sucking* influenced the focus of the image.

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		CONGENITAL SCHEMOIDS			
		“palmar reflex” (close hand)	stepping reflex	“rooting reflex” to the right	shorten focal-length of eyes
STIMULUS PATTERNS (potentially important evolutionarily)	touch palm	likely to grasp object			
	foot touches floor		rudimentary walk-action (approval?)		
	touch right cheek			achieve counsumatory goal (oral)	
	retinal image out of focus				focus onto the object

As one plausible explanation for this we may attribute the permanence-despite-suppression to the supposed inherited nature of these particular schemoids. Whereas schemata *acquire* their stability and are then difficult to change; these reflex schemoids are seen as infinitely *replaceable* in their original form, from their original genetic source, but readily modifiable or suppressable 63c1 individually once they have been produced. In fact, they would then be just like any ordinary scheme tapes in this respect.

As a working hypothesis then, let us suppose that the heriditary reflexes are pre-coded on the actual genetic chromosome as DNA coding, and that RNA strips obtained from these sites would either *themselves* constitute ready-made expendable tapes capable of operating collectively as schemes, or else they would be capable of *generating* such tapes in some other physical form.

As well as ready-made hereditary schemes, other scheme-tapes would also presumably be produced in the same way, but as “blanks” which could not be used as programs until they had been modified.

A3.3 Calling the Labels of Schemoid “Tapes” which are Physically Remote: Technical Considerations

Ultimately any thoroughgoing theory of neural activity has to come to terms with the problem of how electri- cal signals and chemical storage of memory interact.[19] In the present paper this problem does arise, but it is posed in a somewhat different way: Given that we are considering hypotheticai chemical changes, these changes are postulated to be intimately concerned with

[19] The evidence for these two phenomena, considered separately, is scarcely to be doubted. For example, see Eccles (1964) or Katz (1966), and Ansell and Bradley (1973), respectively.

"calling the labels" of other remote chemical systems. In other words we are really talking about chemical interaction across a remote[20] distance — "by telephone" so to speak.

If such a system is to work at all, it is difficult to see how it could operate other than by electrical signals ¤[21] . The question then becomes one of explaining how such electrical effects, normally having a chemical effect at close range, could be *transmitted* selectively to relevant sites, and how they could be *used* there to call appropriate chemical systems into play, in the face of considerable amounts of random infra-red "noise" within the body. For this purpose, a mere
"blip" *at* the receiving end would probably not do; but if it could be analysed into a temporal or 63c2
spatial spectrum beforehand, then this could act very specifically — like a key received into a lock. Such analysis could be achieved if the intervening medium were to act "dispersively" (somewhat like a prism).

Calculations (Traill, 1977: "Part B" below) suggest that these requirements could reasonably be met simultaneously by using existing *myelinated* axon segments as wave-guides, with chemical re-activation at the intervening nodes. Thus the role of the myelin may be more than just one of speeding up the conduction of signals, it could also have a more subtle role — assisting in the decoding or sorting of jumbled signals.

If correct, this account might help to explain the comparatively stereotyped behavioural repertoire of invertebrates, and other animals including neonates whose axons are lacking in myelin. In such cases it is plausible that only local signals and simple traditional "spikes" will be usefully received.

In short then, the postulate that one schemoid can call others, in a highly adaptable way, need not necessarily be left as a metaphysical abstraction. On the contrary, it seems feasible that the process may be amenable to detailed explanation — the outlines of which have been briefly suggested.

A3.4 Schemes, and Piaget's first two Sensori-Motor Stages

The activity of the first stage is generally deuribed simply as an *exercise of reflexes.* Now I would at least like to raise the question of just what is meant by "exercise" in this context. In normal usage, "exercise" implies that something is being developed or corrected — though it is often not clear exactly what the basic changes are which make up this improvement. To say that the infant is "getting to appreciate the reflex schemes" scarcely does more than re-state the problem; so let us try to put more precision into our ideas here.

With this aim in view, we may look ahead to stage 2 to provide clues as to what has been going on clandestinely since birth. What then are we to make of the overt primary circular reactions of stage 2? Here we have "movements grouped in coherent systems superposed on the reflex systems" (Piaget, 1954, p. 211 ff), and "coordination between hand and mouth in thumb-sucking" just after one month *(ibid., p.* 106 ff); so what basic mechanisms might underlie these phenomena? And how do they arise?

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[20] distance >> (wavelengths for the resonance frequencies involved in the chemical changes concerned). Such chemically significant wavelengths are likely to be of the order of 1 to 100 microns, i.e. in the infra-red range.

[21]¤ Moreover, for such frequencies and distances, the signal must evidently use a *radiation field* rather than the simple reverslble *induction field* which is normally taken for granted in physiological discussions dealing with electrical signals. For further discussion see Skilling (1962), Chapter Xl, especially pp. 167-168.

Suppose that there is a particular situation "*A*" (or "*A*rm-in-striking-position"[22]) which the infant can *recognize* by some means, and that there is therefore some sort of schemoid element "*a*" which corresponds to this situation. Suppose also that, *provided* the situation is "*A*rm-in-position" initially, then a certain overt action "*B*ang" will in fact normally produce situation "*C*onsummation" which happens to be inherently rewarding to the infant. (And just as *a* is the internal coding for the recognized situation "*A*rm-in-position", so is *c* the coding for some positive consummatory effect, and *b is* the coding for the intervening action "*B*ang.") 64c1

Thus if the thought domain were populated by a sufficient number of "tapes" in the form "*a*-*b*-(*c*)," then whenever *A* happened to occur, it would "call" the label *a*, so that the program *b* would be put into effect. "(*c*)" would plausibly have a different status: as well as any subjective-effector role it might fulfil (presumably acting ultimately on the hypothalamus) it would also act as a positive-affect *tag,* serving to slow down the decay of the scheme-tapes "*a*-*b*-(*c*)." After consummation sensation *c*, the situation may be thought of as happening to relapse to *A*rm-in-position (due to gravity perhaps), so that if enough *a*-*b*-(*c*) tapes are still present, the cycle is likely to keep repeating — justifying the term "primary circular." Insofar as *B*ang was originally some sort of hereditary *reflex* response in its own right, then Piaget's description of "superposed on the reflex systems" would appear to be appropriate if this means *a* being superposed on *b* or onto a larger tape containing it. This description would also seem appropriate even if *b* had been modified from its hereditary form in the interim, by other earlier accommodations "superposed" on the original hereditary *b*', producing an artefactual *b*". But what about Piaget's "coordination between hand and mouth…," or Bruner & Bruner's (1968) "Eye, Hand and Mind"? We now have *parallel* activities, and moreover they are *coordinated* Can such phenomena be explained in terms of linear structures? Well some clues are offered in Monod and Jacob's (1961) conference summary; notably their *Model IV* in which two separate *linear* chromosome-controlled processes are mutually interdependent, due to the products of the first causing *inactivation* of the *repressor* for the second (thus facilitating it) — and similarly the products of the second facilitating the first. 64c2

Thus, in our terminology, we may have two types of tape operation in conjunction:

a(call label) → f_1(facilitate other) → p_1(proceed *if* facilitated) → *h*(hand program) → (*c*)(consummation).

a(same call) → f_2(facilitate other) → p_2(proceed *if* facilitated) → *m*(mouth program) → (*c*)(same consummation).

A number of variations on this theme are possible: notably involving more cross-feed facilitations, degrees of dependence on such support, and larger numbers of parallel tapes; but this example will suffice for our present discussion.

So then, if the infant does have mental structures like these by stage 2, how did they come to be there? Firstly, we should recognize that some of them may have been there all along. If they did not manifest themselves earlier, this may have been for maturational reasons, explicable in terms of our (a-f_1-p_1-h-(c) and a-f_2-p_2-m-(c)) model: There may not have been numerically enough of the relevant tapes available for them to adequately facilitate each other, or to reach some effector-threshold. Or perhaps the facilitation pathways were not yet available — maybe because of delay in myelination.

[22] As a means toward making the following algebraic approach somewhat easier to follow, we may use appropriate whole words or phrases as interchangeable algebraic symbols for their own initial letters. Thus *A*rm-in-position º *A*; *B*ang-(spoon) º B; … etc.

But assuming we are agreed that some particular behaviour pattern *has been learned* by stage 2, then how do we account for it? Take the "*a*-*b*-(*c*)" case first. It was suggested (Traill 1975 and 1976 (A1 above)) that the process starts with populations of two different hereditary scheme-tapes: (a_{label}-$a_{program}$) and (b_{label}-$b_{program}$-(c)) and that approximate simultaneity of use brings some of them into physical proximity — allowing for some "crossovers" to occur, so that we then obtain some tapes with the configuration: (a_{label}-$b_{program}$-(c)), as required. Presumably we would also get some (b_{label}-$a_{program}$); though without the "(c)" to protect it or induce its reproduction, this type of tape might not survive for very long.

Some practical variations on this can be achieved without changing the above "algebraic" statements. Thus, for instance, "b_{label}" might simply be absent, or be represented by a dummy mode, according to whatever provisions heredity might have made. Alternatively, perhaps the tape elements are joined in other less neat ways, possibly with some repressor action to quash the superseded elements. This seems rather haphazard and inefficient for a potentially recursive biological system, but perhaps we should be careful about dismissing it in view of its possible similarity to the original genetic systems.

Another more attractive idea is that isolated labels are tailor-made by incoming signals, and that they can then augment *or* replace existing labels on what was a hereditary scheme. They would thus be comparable to *episomes*[23] becoming attached to chromosomes. Anyhow these various suggestions could all be described as "tape transplants" of one sort or another; and perhaps we should simply leave it at that for the time being. 65c1

But what about the more complex systems like our (a-f_1-p_1-h-(c) and a-f_2-p_2-m-(c))? There would seem to be two possible sources for such non-hereditary new schemes: They *could* somehow be built up from scratch. But more plausibly, and more in accord with Piaget, they could result from "mutations" of other tapes. This would be essentially the same then as for the above simpler case; but possibly starting from more complex hereditary tapes. These pre-set tapes might in some cases, be retained genetically as *blank* schemes, specifically "with this purpose in mind" — a sort of construction-kit with some scope for coding according to local conditions. If this is the case, then *imprinting* would seem to supply a particularly striking illustration of such last-minute detail-filling.

When learning is less predetermined, we are left with the problem of how sensible-and-adaptive schemes can emerge from apparently arbitrary processes. But this looks very like the problem of evolution in miniature (and that should not surprise us if we think of mental activity as substltute-evolution: survival of the fittest amongst expendable models rather than amongst the macro beings of the "more real" world — analogously to the industrial use of expendable scale models and the like). A mutant tape might fail to survive because it produces no satisfaction, and therefore fails to be labelled with a supportive tag, according to our present postulate. Or the mutant tape and its potential collaborator-tapes may fail to provide the mutually necessary facilitation for their joint operation so that they fail for "technical" reasons.

Under such a general non-specific procedure, there is likely to be considerable wastage of potentially useful codings; but if these are on a molecular-population scale then this will be no great hardship — especially if their material is re-usable with minimal change. Also the process is likely to go on for some time before there is anything much to show for it. This then is presumably what is happening during the infant's first month (stage 1), before the overt consequences appear at stage 2.

Other species have different ecological requirements and limitations so we may expect different mixtures of: predetermined *fixed* schemoids; largely predetermined but *imprintable* 65c2

[23] which include such things as non-virulent viruses (Jacob & Wollman, 1971).

schemes; and open-ended *flexible* schemes. Thus a spider's repertoire of behaviour is highly stereotyped, and the human's repertoire is very much left empty "in anticipation" of appropriate filling-in from experience, while a new-born lamb shows conspicuous elements of all three types of schemoid: a well developed hereditary locomotive ability[24] on the one hand, plus a reasonably flexible intelligence potential, and also a tendency to imprint to its supposed mother.

A3.5 Taking Liberties with the Rigidity of Mathematical Groups

The mathematical group is much discussed in connection with Piagetian theories, though usually for the periods *following* sensori-motor. It implies the following properties, not all of them generally acknowledged — cf. Leech and Newman (1969) for example. In the discussion we will use "♦" to represent a generalized operator, including such operators as "+, –, ×, ÷" and many others which are not arithmetical.

Mathematicians accept that **A**–**B** ≠ **B**–**A**, but in practical group theory it is often not made explicit that **A** and **B** are expected to both belong to the same set; for instance both taken from the *unbounded* set of *real* numbers, in which case **A**·√**B** would not satisfy the requirements.

Here however, this sort of possibility is considered explicitly in "property (0)."

(–1) A "substrate" consisting of a recognizable *set of states;* and also a *set of fixed operators* which may be considered alternatively as a smaller set of *n* variable-operators whose properties may be *fixed* by a given parameter taken from a *set of parameters.*

(0) It is assumed for mathematical purposes that the parameter set is co-extensive with the set of states; thus we have (equivalently), *n binary*-operators applied as follows: **A** ♦ **P** = … [or writing it more rigorously: **A** ♦ (**P**) =…], where **A** and **P** are both members of the state set (though **P** also acts as a parameter) and "♦(. .)" is one of the *n* variable-operators; [or to be more explicit: the *n* variable-operators are $♦_k$(. .). where $k = 1, 2, ..., n$.]

(1) "*Closure*" which means that for "**A** ♦ (**P**) = **C**," **C** is always a member of the state-set as well as **A** and **P**. That is, the recognized operators cannot cause a break away from the recognized set of states; and this means that the whole universe of discourse has been encapsulated within the system, thus giving complete predictability (mathematically). 66c1

(2) An *Identity* element "**I**" (producing no change) is associated with each variable-operator, thus: $\mathbf{A}\ ♦_k\ (\mathbf{A}_k) = \mathbf{A}$ for all member states **A**, and for each *k* up to *n*. For example, in ordinary algebra: $a + (0) = a$ and $a \times (1) = a$. [It is also taken to be true that $\mathbf{I}_k\ ♦_k\ (\mathbf{A}) = \mathbf{A}$, where we now have **A** acting as the parameter.]

(3) *Inverse.* For any given variable-operator-cum-parameter, ♦(**P**), there will also be an inverse parameter which will undo whatever change was made originally. Thus if **A**♦(**P**) = **C**, then $\mathbf{C}♦(\mathbf{P}^{-1}) = \mathbf{A}$; where $\mathbf{P}^{-1}$ is the inverse parameter.

(4) *Associativity.* When a variable-operator, ♦, is to be applied twice, to three elements: A♦B♦C; then associativity will hold if and only if [**A**♦**B**]♦C = **A**♦[**B**♦**C**]. This can only make sense if condition (0) holds true — at least partially, because **B** *has to* act as both a state and a parameter *if* the equation is to be meaningful.

[(5) *Commutativity* is *not* required, even for mathematical groups; but as a negative example it may help to round off the picture. Thus in general **A**♦(**B**) = **F** while **B**♦(**A**) = **G**; where **F** and **G** need not represent the same state] A close examination of these properties rather makes

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[24] I am indebted to Dr. A. Sloman for raising the issue implicit in this example, and for useful discussion arising from it.

one wonder whether the supposed properties of a "mental-concept group" are really the same set as those specified for mathematical groups. The Pure Mathematician ignores the worldly implications of (-1); and he requires (0) whether he says so explicitly or not; while the remaining conditions are insisted on uncompromisingly.

From a practical biological point of view however, such rigid properties can only exist as an ideal; to be aspired to perhaps, and approximated on a "pass percentage" basis (see Gasking's (1960) "cluster" concept), or even attained by one *part* of the mind's activity, but never actually achieved rigorously. After all as Ashby (1956) has pointed out, such a closed and stable system is inert or dead as far as the rest of the world is concerned. Nevertheless, as we saw earlier, strivings after closure in our mental models are likely to form an indispensable part of our autonomous mental development. Thus it would seem sensible to consider the question "How can the mental system progress from comparative disorder towards its supposed goal of comparatively group-like structure?"

Initially it would seem that the three *sets of* entities [exemplified by the three letters in **A**♦(**P**) = **C**] are "regarded" as unconnected. But during developmental progression, the three sets gradually *tend* to become co-extensive as required for the mathematical group: properties 0 and 1, above; and arguably this is the most important change to emerge from the developmental process at this stage. Let us therefore talk of "*pre-groups*" in this context, to refer to those systems which have some of the properties of a group and some means whereby sets could be delineated in practice (by recognizing tags or properties of the members, "*intension*;" and/or by confinement within a boundary, "*extension*"). 66c2

A3.6 The Practical Mathematical Achievements of Evolution and Heredity

Clearly the underlying *substrate* for thinking arises through genetic means, and this presumably provides the material basis for the three sets of "states." The internal state-counterparts themselves may be considered as genetically-determined stereotypes, operating through highly specific pathways such as those in the visual cortex (Hubel & Wiesel 1959, 1962, 1963a, 1965). Moreover, the underlying mechanism might now be reasonably guessed at and "designed" (Hubel & Wiesel, 1961; Julesz, 1975).

[The details of such pathways, like embryological developments in general, cannot be divorced from environmental influences. (Hubel & Wiesel, 1963b, 1963c)]

Nevertheless it may be argued that the environmental influences here are normally sufficiently predictable for hereditary codings to "take them for granted" — just as we may normally make many cultural assumptions in our social encounters. Thus if the experimenter or clinician finds major lapses from the *developmental* norm, this may perhaps be explained as resulting from excessive ("unnatural") departures from the *physiological* norm (Ashby 1952, 1956). We may reasonably use the term "*ortho*-maturational" for the non-genetic component of a phenotype which does depend on the environment being in the "normal" range.

And another related side-issue: Hebb (1949) and many workers since, have made much of synaptic plasticity as the fundamental mechanism for mental functioning. By contrast, it would now seem that such changes might be in the nature of comparatively crude physiological readjustmcnts of overall weightings; and, important though this may be, it would not be directly responsible for the main transactions of ongoing thinking, perception, and action. Even its supposed role in Long Term Memory may be open to question, if only on the grounds of stability and economy.]

67c1

Anyway, a certain amount of perceptual pre-processing is provided as a hereditary legacy. Whether this pre-processing ends at the striate cortex with the degree of organization appropriate to that region, or at the inferotemporal lobe with *its* degree of organization, (Iversen, 1974; Pribram, 1972) or at any other centre, it still makes no difference in principle for our immediate purpose. All we need ask is that it should end *somewhere* in an array of inherited disparate state-indicators. (Of course this is meant to apply to the input channels for *all* sense modalities, not just the "more popular" visual system.) One important task then for any mentally adaptable animal is to set up *pragmatically meaningful* linkages between these perceptual elements: both within and across sense modalities. Meanwhile, insofar as such linkages are absent, there will be a corresponding degree of compartmentalization with respect to sensory input; indeed *qualitatively* there may be more than a superficial resemblance between this sort of conceptual separation, and such phenomena as (i) those experienced by split-brain subjects, or (ii) the compartmentalization of beliefs according to the role being played.

Similarly we may think of genetic or maturational "sealed units" of effector post-processing, by which the thought domain can put its "conclusions" into effect: either as *motor* action, or by internal *modifier* action — controlling attention, assimilation, and such like. Again it is not vitally important just where such effector channels start to be ready-made, and where they are still a direct manifestation of variable scheme action; — we may assume that there will be some of each, so both must be explicable within the terms of the model. Indeed, we might even have to contend with a *gradual* or *joint operation* transition from one to the other.

Finally, as we have already seen, some of the variable schemes themselves are likely to be of genetic or ortho-maturational origin.

Now in terms of the mathematical terminology of the previous section: if we can decide on where to draw the !ines between (i) sensory input, (ii) internal variable scheme-thought, and (iii) effector output, then we can notionally draw up a matrix similar to Table 1, for any *integrated* thought-centre. Emphasis is put on "integrated" because it seems appropriate to consider each informationally-isolated unit as a *separate* thought unit until such time as it achieves some sort of meaningful dialogue with other units, and thereby loses its autonomy. Thus it can be seen that Table I itself fails to qualify as it stands, because each successive triplet of (row)-(column)-(diagonal element) forms an isolated unit on its own — so we actually have *four* separate integrated units, rather than *one* as implied. 67c2

Moreover, at birth we may assume that for an exhaustive table of sense-patterns and effector-patterns, many will lack even this degree of integration. In general we may think of them as "Conditional Stimuli" and "Conditional Responses" which are still "neutral," so they correspond to just an identifiable column, or just an identifiable row, but lacking any entry in the body of the table. [Things *might* be different at the micro level; for example if each relevantly labelled tape has an *arbitrary* program, so that it is integrated on its own, but not if it is considered as a statistical representative of a population. However not even this complication would arise if the program part of each tape were "blank" or simply absent. Of course for such micro-distinctions, any exhaustive table would be of astronomical dimensions!]

Anyhow, the ortho-maturational and hereditary achievement may be said to be the "drawing up" of such an exhaustive table, but *leaving most of the entries empty* — though allowing some "diagonals." Indeed insofar as row and columns represent *patterns* of input and output, it is an open question whether these rows and columns are even all endowed with identifying codings at this stage: these could be added arbitrarily later. As for size: if we assume that the basic *constituents* of these input and output patterns are limited by anatomical constraints, and the number of fixed hereditary "sealed unit" combinations of them is also limited, then it is possible that an exhaustive table would be no more than huge, rather than "immense" in Elsasser's sense (1961) of $10^{a \text{ huge number}}$, as we might otherwise expect.

A3.7 The Practical Mathematical Achievements of Stage 1 (Manifested at Stage 2)

The achievements of stage 1 may then be described as a filling-in of some more diagonal elements in the table — or perhaps discovering that the particular entry prescribed by heredity-or-maturation does *not* work and should be replaced by a different entry. It is, of course, simply a matter of convenience which entries fall in the diagonal position. The point is that at this stage there should tend to be a *single* preferred response pattern to a given stimulus pattern; and whatever this turns out to be, if we can arrange for the relevant entry to be kept on the diagonal, then our stimulus patterns and response patterns will turn out to be listed in corresponding order. We 68c1
should also bear in mind that any real imposed stimulus-pattern is likely to be assimilated as a *series* of *recognizable* sub-patterns which *include internally generated components*, so from moment to moment the part of the table "in use" is likely to change dramatically; this partly explains how the *number* of rows and columns might be kept to a manageable size — many of the response patterns concluding by "passing the buck," or simply running in parallel.

Another description of these attainments is to say that the infant has mastered a considerable number of (disconnected and probably temporary) one-dimensional abstract spaces at "groupment" level, (not "group" because there is no appreciation of a reversal operation as such — even though a sort of "zero-resetting" may, in fact, occur by default). In other words, given a recognizable initial situation, the infant may habitually come to produce the right effector action pattern to bring about a consummation; but without involving any invidious choices between comparable paths, if only because other potential choices will have been forgotten.

A3.8 Achievements During Stage 2

Assuming that one-dimensional link-ups do occur in stage 1 as described, then to what extent are these made permanent? — and when, and how? We may suppose that initially such schemes are quite ephemeral — though their statistical decay-rate may depend on the extent to which their population is tagged as rewarding and therefore worth preserving for the time being — until something more pressing drives them out of the infant's attention. Where such schemoid formulations do become more permanent we may reasonably identify the resulting stable populations as schemata, *even if they may have a rather more primitive connotation than is usual in Piagetian discussions.* As for when such schemata develop: they may well be theoretically capable of formation right from birth or earlier, but probably do not play a *leading* role until stage 2.

Just how this stability is reflected structurally remains to be explained. Let us take up the earlier suggestion that such permanence derives from corroboration of some sort; and in this case it is likely to take the form of mutually supportive cross-feed, as in Monod and Jacob's *Model IV,* discussed above. But suppose this is true, we have already been attributing this sort of mutual support to normal ephemeral schemes — at least in some degree; so where might the difference be?

Well it could be essentially *just* a matter of degree — perhaps involving some sort of 68c2
"critical-mass effect," in which any exceeding of a critical population density gives a local locked-on system. Another possibility is that the hypothesized electrical intermediaries in the cross-feed paths come to be circumvented, thus giving a much more immediate "alliance" capable of withstanding disruptive influences. These two possibilities would seem to be mutually compatible; and indeed it would seem necessary to postulate something like the prior *local* growth of direct mutual support in order to explain how the supposed electrical links could be *superseded.*

Anyhow, whatever the mechanism, any new promotion of a significant body of ephemeral schemes into long-term schemata is likely to have profound effects on mental functioning, and it rather looks as though stage 2 produces just such a major milestone comparable to the boundary which occurs later between the "Pre-operational" and "Concrete Operations" sub-periods. Moreover, the reason is probably similar: Adequate numbers of schemes for manipulating elements basic to the particular period[25] have just acquired reasonable permanence and salience, thus opening the way for more time-consuming explorations of group-like structures. This means that reverse operations, consideration of alternatives, and more advanced waiting-for-response-before-proceeding become postible within the respective contexts.

A3.9 Stages 3 and 4 (Approximately)

Let us consider Table I again. By now the infant has managed to manufacture a substantial number of apparently unique "diagonal" elements, and the infant has also managed to give these diagonal action-codings sufficient permanence so that they might *now* be worthy of attention in their own right — as if they were perceptual "sealed units." This then opens the way for higher order schemes which refer to *other schemoids* by "name," without necessarily calling them into effect. Amongst other things, we may suppose that such inward-looking higher schemes would be able to "list together" those lower order schemoids which were capable of performing a particular consummatory task; (perhaps by first arbitrarily calling such lower schemoids, and then "forgetting" them if they fail to produce the arbitrarily pre-set expectation).

This would be an important step forward. It would give a means for forming practical *sets* of one-dimensional linkages; and these presumably offer *alternative* routes, b_1 and b_2, to the elementary consummation (c) — given a specific starting situation, a. We therefore now have a basis for producing a mental model of an *enclosure* around a *two-dimensional* space[26]. (Piaget & Inhelder, 1948). This in itself is only a beginning: as it stands, such a link-up does not constitute a model of an object — nor even a two-dimensional equivalent of an object — because it is restricted in both its starting-point and in the direction in which the boundary may be traced. 69c1

However, other types of higher order scheme should also be attainable. Suppose for instance that there are now two recognized starting points (a_1 and a_2) from which c may be reached deliberately; and that the infant comes to discover, by chance, that a_2 lies on one of the alternative routes which start from a_1 — and vice versa. This then gives a means whereby the reversal pair a_1a_2 and a_2a_1 can be classified as a special sort of set, whose usefulness in group construction then becomes open to discovery. In order to explain the attainment of a set which included a reversal *away from* the consummation state c, it would seem necessary to change the infant's "drive mode" so that, for the time being, c no longer provides a satisfying goal, but that a_2 (say) becomes the state to be sought after; — it being assumed that the c and a_2 would not lose their coded identities during this switch-over. Furthermore it is likely to be just a matter of exercisc for the infant to be able to cope with *any* recognizable starting-point around the loop (Traill, 1975).

So now our infant has met most of the requirements necessary for the formation of a mathematical group — at least for two dimensions: (–1) *Substrate of states and operators,* yes, (0) *Parameter set = state set,* generally dubious; (1) *Closure,* partially yes (on boundaries of two-dimensional object-perception loops); (2) *Identity*, yes (if only by inaction); (3) *Inverse*, partial yes — as for closure; and (4) *Associativity* remains dubious along with "0" on which it depends. This is not perfection, but arguably the most important properties are there to an adequate extent,

[25] Viz. *input* "sealed units" for Sensori-motor; or *object schemata* for the Operational Period.

[26] "Space" may be understood here as sometimes meaning geometrical space, but also sometimes meaning other sorts of topological space.

thus giving sufficient capability for the infant to stumble onto reasonably correct models of real objects if he “accepts” fulfilment of group-like properties as a “worthwhile goal."

A related consequence of the higher order schemes is the connecting up, into sets, of those schemoids for different perceptual modes which happen to display a functional correspondence. 69c2

It remains then to probe a little more deeply into how, in *mechanistic* terms, such sets of boundary-tracing schemata (linked by a higher schemoid) come to acquire the extra stability just implied in the “goal” concept. Two general possibilities come to mind: (i) that these “higher order” schemes acquire stability in essentially the same way as for lower order schemoids-indeed it would be conveniently economical to suppose that the two types are identical except for the sorts of entities which happen to be code-referenced in their respective “lists.” (ii) It was suggested (Traill 1975 and 1976 (A1 above)) that such structures might be “tested” more-or-less regularly or continuously for *closure*. Presumably some sort of exercise, perhaps during one of the sleep modes, and perhaps associated with one or more of the EEG frequencies, would attempt to pass control from one element of a set to the next. If the set did have mutual closure, then this “control” would be kept “within the family,” but not otherwise; and stability could then derive from this ability to hold control — possibly by replication during such exercises.

These two suggestions appear to be compatible however, and indeed the second could be used to help explain the growth in local tape-population density which could then promote the first.

A3.10 **Stages 5 and 6 (Approximately)**

It should be borne in mind that Piaget’s “stages” are *observational* categories. As such, they need not necessarily be a safe guide to underlying unobserved developments. In the early stages, when presumably not too many things are going on simultaneously, the correspondence between observed, and non-observed events is likely to be reasonably satisfactory; but at later stages we would be wise to exercise greater caution in the interpretation of the observations. Thus stages 5 and 6 (the second year) are justifiably considered as the closing stages of sensori-motor development, but might they not also be the clandestine starting-phase for the following *operationol period*? After all, it was supposed above that the initial sensorimotor development was also clandestine.

In particular it seems likely that the experiments with sequential displacements of objects should logically be associated with the following *operational* period, because they take some form of *object* and its internal schema as basic elements.[27] Also the same sort of argument applies to observations concerning obstacle removal. In fact, it rather seems that these are a 70c1
recapitulation of the stage 1 activities, but at a higher level — trying to form coherent patterns of *operation* whereby objects can be moved around, rather than trying to form coherent appreciat-ions of objects themselves.

Admittedly the “objects” as perceived in stages 5 and 6 are probably two-dimensional so that some incomprehensible anomalies are likely to confront the child in due course, but they nevertheless provide a very attractive approximation to reality for an individual who has hitherto had to cope with very much less. Small wonder then if he “goes off half-cocked” into the mysteries of the operational period. [Nor are adults immune from such incaution, despite the proverbial warning that “a little knowledge is a dangerous thing.” Indeed for phenomena less well defined than permanent objects, we may not be *able* to do better than use our inadequate knowledge, however dangerous this may be. Such is politics for instance. Moreover, by no

[27] instead of a constituent object-*property* and its internal “sealed unit” signal.

means all material things can be dealt with neatly as "rigid, permanent objects," as Green and Laxon (1970) admirably illustrate in the title of their article.]

But to return to the closing stages of the sensorimotor period as such: Despite any "pre-mature" excursions into the next period, the child will meanwhile also be building up alternative two-dimensional schemata which will turn out to have some interesting common features; and ultimately these are likely to be resolved in the formation of schemata which capture the properties of three-dimensional groups. The process is likely to be broadly analogous to that for producing two-dimensional schemata, though with some additional complications:

"Neat" geometrical groups are more difficult to design in three dimensions than in two; thus, for instance, 60º rotations about one axis (perpendicular to the plane) generate a perfectly satisfactory group ("C_6"). But if we try mixing 60º rotations about two perpendicular axes simultaneously then the result is, at best, haphazard (until we invoke formal trigonometry). However, a judicious restriction to using mainly 90º rotations can circumvent such embarrass-ments, and at the same time provide the proper "feel" for the three-dimensional nature of an object.

So better late than never, by the end of his second year the child will probably have sufficient grasp of the true three-dimensional nature of objects for him to be able to make good some of the perplexities he may have fallen into in the meanwhile.

A3.11 Subsequent Recapitulation (Recursion) 70c2

It has already been suggested in the above discussion that the *operational period* will follow substantially the same course, but at a different level, using objects and object schemata rather than their precursors. It has also been hinted that the following *formal operations period* might follow the same pattern as well, using something like the entities of set theory or logic, in place of objects. One might even logically postulate a fourth period to include meta-mathematics; and the very fact that we talk about algebras (plural) rather suggests this.

Anyhow, the pattern which at least the first two periods were supposed to follow consisted of two phases in each case: An initial phase in which some basic facts about how one elementary state or element (appropriate to the respective period) can be operated on so as to produce another particular state or element. This is then followed by a second phase in which such information is classified and systematized into generalized rules or groups, allowing for maximal efficiency in problem solving (within the limited understanding of that period), and by the same token providing schemata to be used as constant elements for the following period, if any.

A3.12 Summary and Conclusion

There is a prima facie case for believing that mental functioning can be explained mechan-istically, in terms of large populations of linear biochemical codes which are capable, in some circumstances, of communicating at a distance. On this basis, an attempt is made to solve some of the problems in "designing" such a system by offering an analytical account of various phenomena characteristic of sensori-motor development.

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Part B

TOWARD A THEORETICAL EXPLANATION OF ELECTRO-CHEMICAL INTERACTION IN MEMORY USE: THE POSTULATED ROLE OF INFRA-RED COMPONENTS AND MOLECULAR EVOLUTION

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Now available in full as a separate document:
[www.wbabin.net/physics/traill8.pdf *or* www.ondwelle.com/MolecMemIR.pdf]

SUMMARY OF CHAPTERS

B1. Introduction: the search for a rigorous structural theory of memory

This chapter poses some basic questions about how behavioural psychology could possibly be rigorously explained in terms of realistic biological mechanisms — in particular questions about selective communication with different chemical memory-stores.

As long as we insist that physiological action-potential is the basic indivisible unit of such communication, then any rigorous explanation proves to be disturbingly elusive. However there turn out to be some persuasively corroborative arguments that there may be an important and rich fine-structure to these and other neural phenomena — involving frequencies above 10^{11} *Hz* — and this would make the explanatory task much more feasible.

B2. How could bio-electrical messages be converted into meaningful and specific chemical coding — and be retrieved ?

It can now scarcely be doubted that mental activity involves both electrical communication and chemical storage. The prime question then is, what connection could there be between these two types of phenomenon? This leads to an evaluation of the role that infra-red and sub-picocecond events might play in providing such a link.

However we also need to be able to offer biologically feasible mechanisms for both the initial encoding of memories and their subsequent selective retrieval. Thus suggestions are offered to explain selective triggering of particular chemical stores, and the consequences which are likely to follow — as motor action or further internal triggering. But the most enigmatic feature of all is the laying down of memory in the first place; and here it seems necessary to postulate a system of trial-and-error at molecular level, so that the encodings of both action and perception can be judged by their results — in line with Piagetian concepts of mental development at the behavioural level.

B3. Transmission properties for various frequencies of electromagnetic signal within nervous tissue, and the special case of saltatory conduction

This chapter provides a corroborative cross-check by making an independent analysis of the (saltatory) transmission-characteristics for myelinated axons presented with all frequencies from *1 KHz* to beyond 10^{14} *Hz*. Factors considered include: breakdown of circuit-theory assumptions for the very high frequencies, free transmission within the myelin dielectric, reflection at the co-axial boundaries, limited protection against water's absorption bands, evidence for signal-blockage, and possible artefact results. The conclusion is that audio-frequencies and infra-red frequencies are both suitable for such transmission, but not the frequencies in between.

B4. The second-lowest mode of transmission in co-axial myelin ($H_{I,0}$): optical dispersion of infra-red

This chapter argues that existing myelinated segments could also act as dispersive media, similar to those postulated in chapter B2, by using "higher vibrational modes" within the myelin.

B5. Evolution of communication methods: suggested extensions to Bishop's two stages

This chapter speculates on the evolutionary development of communication systems in animals, and suggests that the basic elements of infra-red transmission may be very ancient and primitive.

B6. Other related architecture in brief: glia, paranodal regions, and cell-body interior

It seems likely that infra-red components of a signal will be "steered" into their *own* reception sites, which need not have any close connection with synaptic sites. Instead *para-nodal regions* and glia are likely to be involved. Transmission of infra-red is likely to be limited to distances of about cell-size except when they can find a lipid optical path.