Short papers and letters
On the 'Linear Micro-element' Theory of Mental Mechanism, and related questions of scientific method

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Robert R. Traill
email: rrtraill4@dodo.com.au — *Tel.* (61)-3-9598-9239

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The occasional changes made in this March 2007 edition are either (i) obvious from dates or technology changes etc., notably updating references and links **&/or** (ii) within square brackets "[...]", **&/or** (iii) within footnotes marked thus "*text*[J]" using an upper-case letter (J in this case), **&/or** (iv) trivial editing.

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CONTENTS

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* Chapters concerned mainly with questions relating to *scientific method.* .

Preface to the 2007 Online Edition

"1976 stuff — that must be way out of date!" Well maybe! But then *maybe not*, since no-one else seems to have properly answered these mind-questions in the mean time*!* — and, by chance, some later experimental evidence has been unexpectedly supportive.

What then are these mental-mechanism questions with no accepted solution yet?

In my view then-and-now, the key questions centre on the *physical nature* of Piaget's "scheme" (seen as an element of *action* and/or *thought*).[A] Thus: — What plausible tangible candidates are there for the role of "scheme-element"? Might they be neurons-or-synapses? How might they record-*and*-retrieve memory? Might some of them be inherited? Could they intercommunicate efficiently using action-potentials? If not, then how else? Then what might be the secondary logistical complications arising from any of these decisions? and can we notionally solve those problems too? And (when we think we are on the right track):— Are we sure that the whole ensemble of ideas really forms a theoretically-coherent[B] whole?

Perhaps surprisingly we get a similar list from Steven Rose (2004, p.215)[C] who has long had rather more confidence in synaptic-transmitter mechanisms as having *the* key role.[D] Yet now: — [a] "Hebbianism is not sufficient ... it cannot account for the ways in which ... the putative memory traces are disassembled and redistributed." [b] "Nor can it account for the renewed lability of memory following a reminder..."; [c] "We have no idea how recall occurs,..."; and [d] "Nor do we understand how chicks, and humans, derive a coherent image from...distributed cues, the, so-called, binding problem."

Some cases of early partial-solutions

My own two-fold impetus came from the works of Ashby and Piaget, who both built upon the notion of a hierarchy of control. Ashby went to some pains to investigate working hierarchical

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[A] These questions all relate to *epistemology*: the study of what knowledge is, plus how-in-detail it can be acquired, stored, and applied. Note that the aim here is not to be content with abstract pseudo-answers, but to insist on describing *actual plausible mechanisms* (on the basis of existing interdisciplinary knowledge) even though their existence may be "merely conjectural", and even though such mechanism-descriptions will doubtless remain incomplete, at least in the short term.

[B] Questions of "coherence" are discussed in detail in P.Thagard (1992), *Conceptual Revolutions*, Princeton U.P. — and in Traill (2005c) www.ondwelle.com/OSM03.pdf .
[See also the related software: http://cogsci.uwaterloo.ca/JavaECHO/jecho.html]
In fact this *coherence-test* is arguably the best criterion we have for truth, given that part of the coherence-pattern must involve the outside world, though that is not sufficient in itself.
[Note too that here we are applying epistemology to the *social* domain, though the same principle should also tend to hold in the *brain* (our actual main concern) — as well as in two other separate domains: the *immune system*, and the *genetic code*; see "Four learning-systems...": (Traill 1999, Ch.4), *Mind and Micro-Mechanism*. Ondwelle: Melbourne. — www.ondwelle.com/BK0_MU6.PDF].

[C] S.P.R.Rose (2004) "Memory beyond the synapse". *Neuron Glia Biology*, **1**, 211-217. — Re-published online (2005) as www.open.ac.uk/science/biosci/research/rose/Memory%20Beyond%20Synapse.pdf

[D] *E.g.* as in the following, (listed chronologically):–
Rose, S.P.R., J.Haywood (1977). "Experience, Learning and Brain Metabolism"; in A.N.Davison: *Biological correlates of Brain Structure and Function*. Academic Press: London. [ch.8, p249-292]
Rose, S.P.R. (1981) "What should a Biochemistry of Learning and Memory be about?" *Neuroscience*, **6**(5), 811-821
Patel S.N., S.P.R.Rose, & M.G.Stewart (1988). "Training induced dendritic spine density changes are specifically related to memory formation processes in the chick, *Gallus domesticus*". *Brain Research*, 463, 168-173
Rose, S.P.R. (1992) *The Making of Memory*. Bantam/Transworld: London

models, and the related information theory; while Piaget (and colleagues such as Inhelder) did experimental work on human development, especially with the very young, and especially regarding the development of the implicit mathematical concepts which we adults unconsciously take for granted. However there is no need to repeat further details here.

Such ideas of control-hierarchies and homeostasis were current at the time — usually in connection with "General Systems Theory" (or "Cybernetics" as it was then called, before the term was coopted by techno-enthusiasts!). Of particular interest is the report on the Alpbach conference[E] of 1968, and I shall briefly mention three of its papers — works which I had not read at the time, but might be seen as some sort of corroboration for the account presented here.

Piaget and Inhelder (1969). Amongst other things, they critiqued the empiricist notion: "that reality can be reduced to its *observable* features and that knowledge must limit itself to *transcribing* these features". (My emphasis here and in the next paragraph). They further likened this to the unsound parts of Lamarckian theory, thus: "Lamarck's theory lacks the basic principles of an endogenous possibility of mutation and recombination and ... of an active capacity for self-regulation." — (pp.118-119).

Likewise "in every field — from physics to psychology...— the essence of scientific knowledge consists in going *beyond what is observable* in order to relate it to subjacent structures." (p.126); — "since...it is *only the underlying structure that is explanatory*." (p.148). One might feel that Chapter V (p.21 below) is also in tune with this message.

And yes, they do also discuss the "scheme" and related issues at some length (pp.128-140), and perhaps rather more concisely and coherently than in some of their other texts. However that discussion is all directed at how the *abstract* schemes apply to *macro*-psychology, and it says nothing about our present topic of what schemes might actually be physically. That may seem an odd omission in view of the above quotes about "underlying structure", but then it is seldom possible to tie up *all* the loose ends in one go!

Koestler (1969). Here he emphasized the evolutionary-efficiency of organizational-hierarchies — and of their components which can relate flexibly to both superior and inferior units in the system. These components he calls "holons", and promotes them as a wise compromise between extreme autonomy for each unit ("atomism"), and extreme inextricable union for the whole system ("holism").

Thus (p.197): ❛The concept of the holon is meant to supply the missing link between atomism and holism, and to supplant the dualistic way of thinking in terms of "parts" and "wholes", which is so deeply engrained in our mental habits, by a multi-level, stratified approach. A hierarchically-organized whole *cannot be "reduced"* to its elementary parts; *but* it can be *"dissected" into its constituent branches* of holons, represented by the nodes of the tree-diagram, while the lines connecting the holons stand for channels of communication, control or transportation, as the case may be.❜ [My emphasis, RRT]

Note (i) that this tallies neatly with other hierarchical explanations; and:

Note (ii) the potential importance of his distinction between *"reduction"* versus *"dissection"*. This distinction offers a means of pacifying those overly holist critics who complain unjustly that *X*'s explanatory dissection of a complex systems is "*reducing it to its parts, and thereby claiming that the whole is 'nothing-but-its-parts'*." Often *X* has not intended to support this "Nothing-

[E] Koestler A. & J.R.Smythies (1969) *Beyond Reductionism*; Hutchinson: London. — And within it:
[ch.4] • Hydén, H — "Biochemical Approaches to Learning and Memory"; pp. 85-117
[ch.5] • Piaget, J., & B.Inhelder (1969). — "The Gaps in Empiricism"; pp.118-159
[ch.7] • Koestler, A. — "Beyond atomism and holism — the concept of the holon"; pp.192-232

But-ism" at all; — but obviously one usually does need to do some *dissection* (theoretically if not physically) before one can make a coherent explanation in substructural terms.

Some politicians like Mrs Thatcher might occasionally have such odd **atomist** beliefs, and perhaps some engineers and accountants as well; but I find it difficult to believe that any serious modern biologists (presumably in accord with Koestler etc) could take that extreme atomist view. It also surprises me that any would have to defend themselves against such a bizarre charge from the rival **holists**. However one can see how this latter extreme of *an over-bias towards holism* might be largely a result of science's residual *bias towards excess-empiricism* — (as criticized in the above Piaget paper). In short, this mistaken view causes epistemological problems here and elsewhere.

This matter is discussed further in *"Notes on Reductionism and related matters"* [a new (2007) addendum to this batch of republished Brunel monographs]: www.ondwelle.com/OSM07.pdf

Hydén (1969). Hydén and his colleagues had done extensive assay-work showing impressive correlations between learning and associated RNA measurement, as summarized here in his paper (pp.88-95). Such work was to lose favour by about 1980, for disputable reasons as discussed in Traill (2005b, §(9), pp13-14; www.ondwelle.com/OSM02.pdf); yet his account warrants further study, despite his own growing doubts by 1973 (*ibid.*).

He did offer some good arguments in favour of macromolecules as memory-store (p.88), whilst also discrediting any "tape-recorder fashion" for encoding them (p.89). It is thus perhaps surprising that *he did not consider fast-Darwinian selection* as a viable alternative, as that could have solved much of his impasse — fitting in with his thoughts about glia (pp.86, 94-95) and "easy access to the genes" (p.87).

He was probably also on the right track in contemplating "a secondary system for information processing" as possibly bridging "the gap in knowledge between electrophysiological and biochemical data on brain cells". In fact, he considered ideas by Adey and Elul "that there exist pathways for electrical currents outside the neurons in the extra-cellular spaces" (pp.95-96). This comes fairly close to the thoughts mooted in the new panel on page 20, below — but then any non-physicist would almost inevitably *overlook the very arcane next step into high-frequency cable theory* (which had been very controversial in the 1855-1920 period, even for engineers!).

Hydén thus led the way in important respects, despite two missed opportunities; so some aspects of the present project may be re-inventing his wheel. Meanwhile his paper also offers a further service. It reminds us, despite any "secondary system", that the traditional action-potential system does indeed learn (even if the second system does it faster or with greater precis-ion), and he discusses likely mechanisms for this *primary* system (pp.98-99).

Notes about the present text

As far as practicable, this account follows the text of the original 1976 collection. Any variations should be obvious, but a list of revision-identifiers appears in small print at the bottom of the title page — e.g. note the use of *capital letters to indicate the new* footnotes, while *old* footnotes retain the original numerals or asterisks etc. Diagrams have been re-drawn in digital form, but following closely to the originals.

The main practical change is the liberal insertion-or-updating of references to "future" works, including web-links where possible. However any fully-new references have been left either in-text or within the footnotes. They have *not* been added to the pre-existing "References" sections, though my own self references (new and old) are re-compiled into a final convenience-list, p.33.

R. R. Traill

Melbourne, 5 April 2007

Original Introduction

The *linear micro-element theory* emerged initially as a part of an attempt to formulate Piaget's view of mental development in some sort of "mechanistic" form. To this was added the further requirement that any such model should at least make sense in biochemical and physiological terms. The main purpose then, was to reconstruct a reasonably specific and detailed model as a basis for specific criticism and/or modification, and also as a more tangible "heuristic" (right or wrong) on which to build one's concepts of a hitherto vaguely defined process.

It remains to be decided just how valid this particular theory might be, but meanwhile it has proved possible to elaborate the existing formulation further such as to encompass a wider range for phenomena than those originally contemplated; and this may perhaps be taken as an encouraging sign. Anyhow, this volume constitutes, in part, a record of such subsequent developments in the theory (excluding the more physiological by-products — which have however already been outlined in Monograph 15).[G]

The question of validating such models brings us into the realm of *Scientific Method* — the second topic dealt with in this volume. The case developed here may be stated in a weak or strong form. The weaker claim is that where there is a dearth of experimental evidence specifically relating to an area of interest, it is nevertheless legitimate to "re-design" (into our model) whatever seems likely to be going on covertly; — provided we pay due regard to indirect evidence in related disciplines (formulated with sufficient precision for us to be able to narrow the range of "feasible" solutions), and provided we always bear in mind that we may have happened to hit on the *"wrong"* design. It is theoretically possible, though most unlikely for the current study, that such a wrong design would escape detection indefinitely in the face of new indirect evidence — as long as we do not close our minds to such new evidence.

The stronger statement asserts that (strictly speaking) we can never do anything other than follow this method of constructing knowledge — simply because there is no such thing as *absolutely direct* evidence. Of course some evidence is much more direct than others, but however good it may seem to be, there will always be a logical flaw in it somewhere; and this flaw will consist of some act of "re-design" — as if to say "it must be this way if it is to make sense". (Often this flaw may be traced to our infantile learning experiences with geometrical objects — or even to genetically coded "learning experiences" of our species). If this is true so that evidence can never be wholly satisfactory, then it seems unjustifiable to postpone indefinitely any attempt to formulate a particular topic on the pretext that there is "no direct evidence" — since, in practice, this excuse would now imply the use of an *arbitrary* cutoff decision-point between "direct (enough)" and "indirect".

It is the above concept of "re-designing unseen reality (into our model of reality)", or "that's the way it must[1] be if it is to make sense" which is intended by the more formal term: *internal closure*.[F] Although I have laid some stress on the importance of internal closure, it should not be imagined that I am claiming it as a cure for all ills. For one thing, it merely complements experimentation and certainly does not replace it. Then paradoxically, its very neglect is a consequence of its own inappropriate over-use elsewhere! An apparently successful (i.e. internally self-consistent) system of beliefs based on internal closure within an initial set of "data", can be very resistant to revision when the body of data grows in volume or precision — as Galileo and many others have discovered to their cost.

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[1] Of course, the word "must" should not be taken too literally!

[F] Here "closure" is almost synonymous with "coherence" used in later writings, although *"closure"* stresses a two-dimensional (2D) arrangement of linkages, whereas *"coherence"* implies (3+)D; — *i.e.* having a (virtual) interlink-structure which looks more robust when represented by a digraph diagram. In fact this verbal-connection was raised in a 1976 letter, see page 32, below — (in Chapter VII).

This locking-in to false models explains Science's aversion to the overt use of internal closure. But it seems that the problem is ultimately inescapable, and this aversion itself seems to be a locked-on case of internal closure — though perhaps at a higher level of abstraction. To this dilemma there is no panacea, though it may prove to be of some value to make the problem explicit.

I had originally intended to group these papers according to the two themes. However as this area of study is an evolving one, with interaction between the two topics, it seemed better to present the papers chronologically, and distinguish between the themes by asterisking those dealing with Scientific Method. (As a further chronological guide: the two *Kybernetes* papers[G] and "Monograph 12" should precede Chapter I; and "Monograph 15" comes between Chapters IV and V) — [see new note-panel on page 20 — where this "Chapter IV½" might have been inserted]:

R. R. Traill

October 1976

[G] The two *Kybernetes* papers later formed *Part A of the Thesis* (1978/2006), now available online: www.ondwelle.com/Mol-Intel-A.pdf (2006) — though the *references* are currently in a separate collective file (for Parts A, B, and C together): www.ondwelle.com/Mol-Intel-Refs.pdf (Alternatively see the BURA website where the same material is available within a single document). The two papers were both actually written *within 1975*.

Monograph 12: (1976 Feb). *Thinking as Mental Model-Building: a Piagetian-cum-mechanistic explanation of the 'engram'*. Brunel University Cybernetics Dept. — from Nottingham BPS conference (Philosophy of Psychology seminar, 7 April 1975) — *now* available online as: — www.ondwelle.com/OSM04.pdf (2007a).

Monograph 15: (1976 Apr) *The Gulf between Behavioural Psychology and Fundamental Physiology: a systematic attempt to bridge the gap*. Brunel University Cybernetics Dept. — from York BPS conference (Workshop on Memory, April 1976) — *now* available online as: — www.ondwelle.com/OSM05.pdf (2007b)

[RRT, April 2007].

Sensori-motor Development of Object Concepts: a mechanistic hypothesis

Chapter I

Display Paper presented at the Tenth Annual Conference of the Australian Psychological Society, La Trobe University, 16-22 August 1975. #11/75
This paper summarises, pictorially, the ideas inherent in the author's three cited references, and also foreshadows later papers which postulate a physiological role for infra-red information-transmission: "Brunel Monograph 15" (April 1976)[G], and hence "Brunel Monograph 24" (1977/1980).
[The latter is now available online as *Thesis Part-B*: www.ondwelle.com/MolecMemIR.pdf — references included]
The main topic concerning "linear micro-elements" is further elaborated in Chapter VI of this volume, [page 21] [and was later (1978) developed further within *Thesis — Chapter C5*: www.ondwelle.com/Mol-Intel-C1-5.pdf and *Section C6.7*: www.ondwelle.com/Mol-Intel-C6.pdf — plus references: www.ondwelle.com/Mol-Intel-Refs.pdf
OR see the Brunel University Research Archive (BURA) website where these three texts are all in the same 203-page document]

ABSTRACT (from pp 62-63 of the Conference Proceedings)

Assuming a mechanistic basis for mind, we are faced with the problem of spelling out, in detail, just how the basic mechanisms could work. Moreover any such explanation must be constrained by what is feasible (given existing physiological, biological and psychological knowledge).

Instead of Hebb's adaptable synapses, let us take RNA-like codeable "tapes" as the basic raw material for memory. Mental constructs like "objects" are seen as models which are built up piecewise during interaction with the real world; and these "pieces" are encodings (often heavily replicated) within a huge "tape"-population.

Coordination between individual tapes is seen as essentially biochemical cross-feedback, but achievable at a distance through the intermediary of electrical signals.

Such signals would apparently need to be made up of infra-red frequency components. Calculations suggest that at just such frequencies, the *myelinated* axons become able to 'mail-sort' any complex signal components into time-and-space 'pidgeon holes'; whereas lower frequencies remain unmodified and unsorted.

During Piaget's sensori-motor stages 1 and 2, genetically produced blank or pre-set scheme-"tapes" become modified by mechanisms similar to genetic recombinations, and then eliminated whenever they are non-adaptive. (This appears to be consistent with classical and operant conditioning). Subsequently such schemes appear to become categorized within cross-referenced "lists" in "higher-order schemes". When such systems achieve mutual corroboration (akin to mathematical "group closure") they become inherently stable as "schemata".

This paper considers the modelling of geometrical objects, but essentially the same process is believed to operate, with varying degrees of success, for less well defined concepts (e.g. see [chapter II, following] for the cases of *self-identity* and *superego*).

Also, a recursive recapitulation of the whole process "at higher levels" is seen as explanation for Piaget's *Pre-operational/Concrete-Operational Period* and *Formal Operations Period*.

Notes from handout

A. Postulated "building blocks" from which the system is to be developed.

- Linear "strings" or "tapes" of coded information (stored chemically) are seen as basic elements — rather than Hebb's adaptable synapses.
- Such elements are seen as being powerless individually — normally depending on extensive replication of a given species of element before any behavioural manifestations will be detectable. (Such a replicated species, acting in coordination, offers a basis for Piaget's "scheme").
- The program-items codeable into such linear sequences will include instructions to *"call"* specific motor activity into effect, or to modify input-processes such as focussing, or to call

other such linear sequences into effect or into readiness (Possibly like computer "sub-programming", but probably more flexibly — allowing for simultaneous reading, etc). Furthermore, such call-codes should be capable of being qualified by an auxilliary code — distinguishing between (i) actual executions, (ii) calling into readiness, and (iii) a mere "symbolic" reference to such a potential call (to action, or to another "tape").

> **Technical considerations**:
> In view of the type of chemical storage thought to be involved, it seems likely that the signals required for such "calls" will start and finish as electro-magnetic complexes in the infra-red range of frequencies. If they can also be *transmitted* in the same form, then so much the better.

- If a "tape" is to be "callable", then it must presumably have one or more distinguishable labels implicit in part of its coding sequence. As we have seen, it should be callable by other "tapes"; but it should also be callable (in many cases) by specific patterns of sensory input.

- Compared with normal chemical transactions, very large distances will often be involved. Efficient "calling" seems to require that signals be transmitted *as* infra-red, along *myelinated segments* of axon. The effective memory-centres may thus be the Nodes of Ranvier (and the glia within range of the end of the myelin sheath), as well as [neuronal] cell-bodies. Transmission down the next segment might be a faithful copy or it might not.

> **Technical considerations**:
> Circuit theory is really only an approximation, applicable to lowish frequencies.
> For infra-red, myelinated axons should be considered using radio-theory or optics instead. In principle, such coaxial cable can carry signals of any frequency, if the constituent materials are appropriate.

Given the expected range of frequencies, and the dimensions of existing myelinated axons, it would be technically feasible for a call to be emitted as a carefully-shaped pulse, and then received remotely as a specific key-like sequence of pulses or waves. This might suffice to "catalyse" the postulated effects.

B. Postulated sensori-motor development of the object-concept — using the "building materials" just discussed

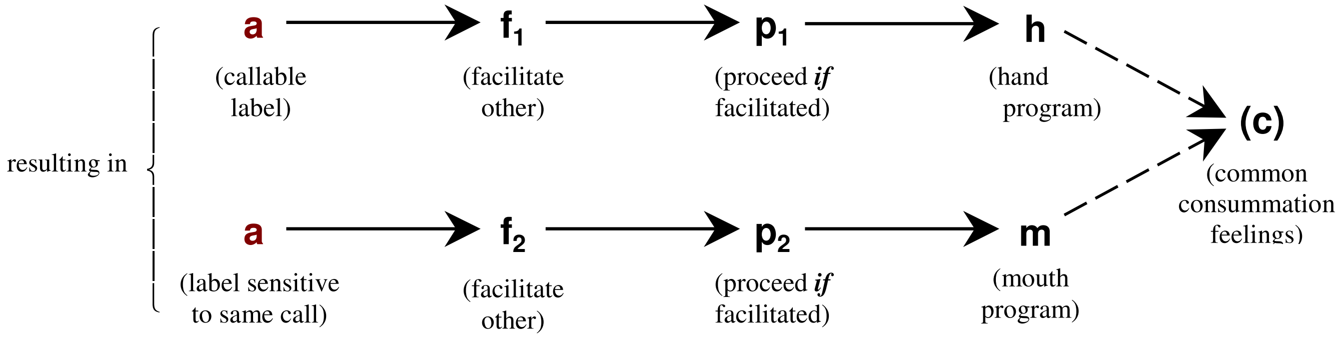
FIRST SUB-PERIOD

Stage 1 (not directly observable till later):

fortuitous pairing, eg **a** ------------> **b** ------------> **(c)**
(recognisable feature) (action) (satisfying consequence)

and/or fortuitous *substitution*, starting from hereditary "blank" schemes:

eg



Stage 2 (also clandestine).

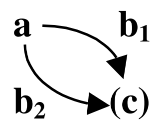
The more frequently used schemes acquire stability; perhaps because they are more heavily replicated, allowing for positive feedback supports to develop more readily — at least somewhere within the population. Such stabilised entities are then referred to as "schemata"[H].

SECOND SUB-PERIOD

Because of the increased stability, it becomes feasible to keep references of such entities without the "object" itself vanishing; so now we may expect the development of "symbolic" references to them. This means that it is possible to set up higher-order schemes which *list* other schemes, thus forming a *set*. Notably:–

$$\left\{ \begin{array}{l} \mathbf{a} \dashrightarrow \mathbf{b_1} \dashrightarrow \mathbf{(c)} \\ \mathbf{a} \dashrightarrow \mathbf{b_2} \dashrightarrow \mathbf{(c)} \end{array} \right\}$$

which gives a slight advance towards [mathematical]-group structure :



Next add new starting-points elsewhere round the loop, adding more directed topological links.

When "drive" changes, **c** may also act as a starting point, so that true reversibility then becomes possible. (This gives a two-dimensional group, and hence a "feel for 2D objects").

Develop further for 3D objects (stages 5 and 6)

Use object schemata as basis for next period (Operational), just as simple input or action codings served as elements to the sensori-motor

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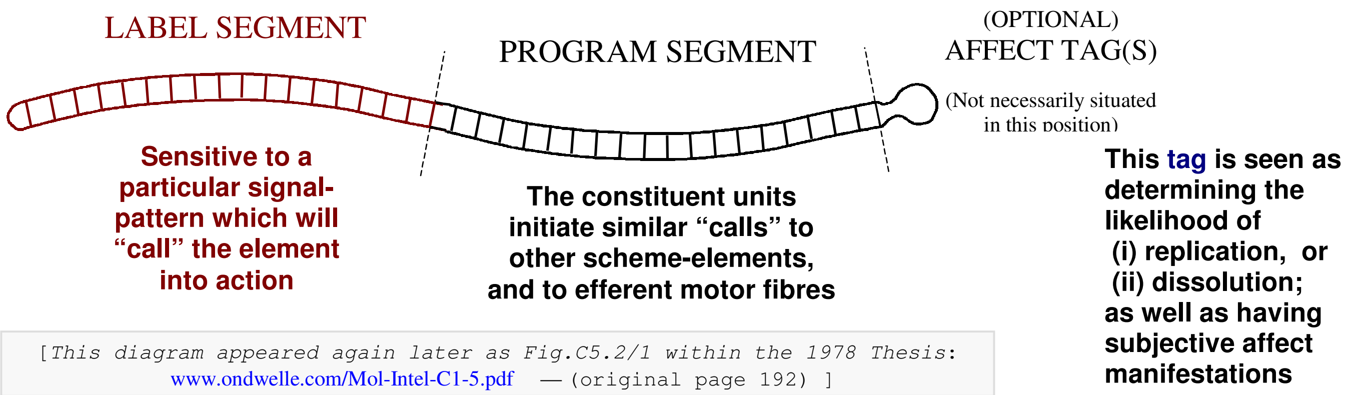
The Poster-Display (as presented):

THEORETICAL: Piaget/Neurophysiology

- Piaget's *scheme* is seen as simply a gross, *summated*, behavioural manifestation of *many replicated code-elements*. These would be of molecular size, and act in co-operation. (Synaptic effects (Hebb 1949, Eccles): relevant but *secondary*).
- Activity of these elements would be started, co-ordinated, and executed utilising high-frequency components in neural-network signals. These would encode highly specific call-signals capable of activating other scheme-elements — or motor tracts.
- Likely "anatomy" of such an RNA-like scheme-element (simple type):



[*This diagram appeared again later as Fig.C5.2/1 within the 1978 Thesis:* www.ondwelle.com/Mol-Intel-C1-5.pdf — (original page 192)]

——————— oOo ———————

Types of scheme/schema[H] *element*

Some are hereditary (and more-or-less infinitely replaceable, when used up or dissolved).
{Basis for **hereditary schemes**: cf. Bruner, etc.}

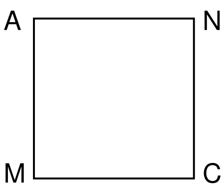
Some are spontaneous "mutations" of the above. These become tagged with affect according to experience. {Basis for **ordinary schemes**}

Some are associated into a more-or-less permanent self-sustaining "compound"
{Basis for **schemata**}

Any of these types would be capable of forming "higher order" elements, thus effectively forming a *set* of "listed" lower-order schemes/schemata. (An important role for higher-order elements would be to form "lists" of lower elements — initially indiscriminate, but dropping those members which were insufficiently "similar" to other members of the list).

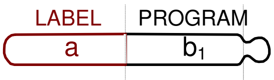
——————— oOo ———————

Eg Development of the scheme for a two-dimensional "object" such as a square:

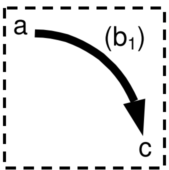


Let a,c,m,n = the internal codes which come to be associated with the four "real" states A,C,M,N.

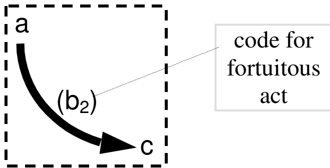
(i) Fortuitously mutated and executed scheme-element:



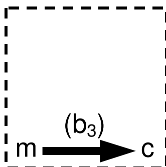
such that

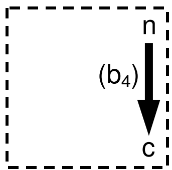


(ii) Similarly

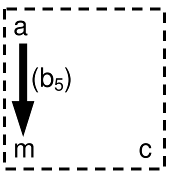


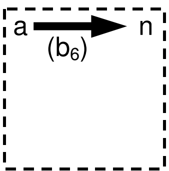
And **(iii)**



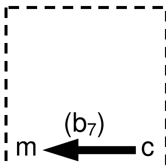


Hence **(iv)**



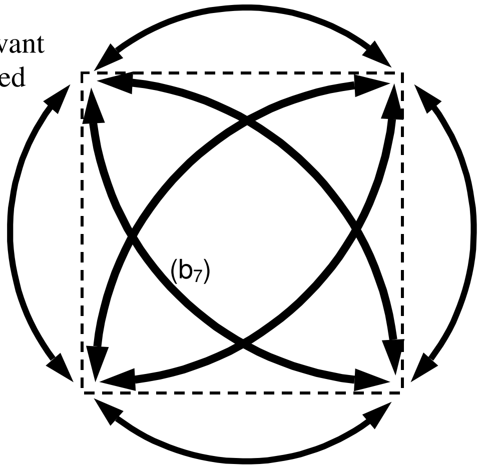


And **(v)** *reversibility* becomes possible — given a change in the drive state (to m, say).



[H] There is some disorder in the Piagetian distinction between "scheme" and "schema" (plurals: "schemes" and "schemata"), see discussion in www.ondwelle.com/OSM02.pdf (Traill, 2006b — *footnote 5* on its page 6, and in the Appendix, pp.21-22).
Piaget himself (usually!) took *schema* as "figurative", which is arguably close to the usage here.

And so on, till eventually all relevant "group" moves have been mastered (incorporated in the set):–



METHODOLOGY
These two papers (Chapters I & II) are theoretical, and quite possibly un-testable in any *direct* sense.

★ Under what circumstances is such an approach justified?

This question of Scientific Method is discussed here in Chapters V and VII.
(RRT, 1976)

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Self-construction of Personal Identity: an extension of the mechanistic mental object hypothesis

Chapter II

Display Paper (as a sequel to the then adjacent display, now Chapter I) at the Tenth Annual Conference of the Australian Psychological Society, La Trobe University, 16-22 August 1975. #12/75

This paper sketches the way in which the ideas of the previous paper might be extended to wider, non-geometrical concept-formation and dynamics. It is thus a forerunner of Chapter VI in this volume [page 22], and of the related 1978 *Thesis (Part C)*, then still in preparation:
www.ondwelle.com/Mol-Intel-C1-5.pdf *ff.*, especially §C6.3 in www.ondwelle.com/Mol-Intel-C6.pdf

ABSTRACT (from pp 61-62 of the Conference Proceedings)

The linear-element population model of the Piagetian scheme was described in the previous paper, where it was used to explain the formation of object schemata. To this, the complications of non-rigidity and breakableness of objects is now added, followed by a postulated mechanism for affect phenomena and some indication of its formal role within the terms of the model.

The self-concept is seen as being a further development on the same fundamental lines, but with crucial differences in detail, including:–

1. ***"Autonomy"*** which implies: goal representation in advance; effective availability of schemes or schemata capable of producing consummation; availability of "wrong choice" schemata; and alos the availability of an effective choice between them. Where (e.g.) parents "use their children instrumentally", this can prevent the child's higher-order ideas (schemata) from developing normally, resulting in an "unreal" concept of autonomy.
(The *superego* is also seen as at least one schema (imago) of roughly comparable salience. The "unreal" concept of self is seen as arising from the creation of a *common* self-superego schema, the inconsistencies of which may be "resolved" non-adaptively by a split into an "autonomous inner-self" and an "outer" perfunctory-behaviour/body/environment schema — Laing's concept of psychosis).

2. **Intensity of *affect*.** In general, affect is seen as an essential ingredient for making complex *subjective* decisions (dependent on context, and without any overall precise model). It is envisaged as operating via "tags" on the linear scheme codings (like episomes in virology), and grossly influencing categorization processes amongst the "Scheme-tapes" concerned. This influence will depend on the "drive-or-sleep state".

3. ***Consciousness***. Here a mechanistic explanation becomes more difficult! Some tentative suggestions are offered.

The Poster-Display (as presented):

THEORETICAL: Freudian/Neurophysiological

(First see Chapter I, above)

- A scheme need not necessarily have perfect group-like structure (as in the 2D-object at the close of Chapter 1, above) especially if aided by favourable affect tags. Thus *less well defined concepts* can also form; (E.g. Erikson's "Basic Trust" or "Mistrust").

- An *ego schema* would also be of this "fuzzy" type. Further it is seen as having essentially the same qualitative status as any other fuzzy schema, except perhaps some specialization in its affect tags.

- This *ego schema* is seen as developing from hereditary scheme elements, and first forming as "the set of actions which produce reasonably reliable results". This "higher order" schema would normally acquire *positive affect* and embody a concept of *available power* (autonomy).

- Any serious and sudden disillusionment about any capabilities closely associated with the ego-schema may thus lead to a negative affect tagging of the ego-schema as a whole — with pathological consequences; e.g. Kimmel (ed)(1970).

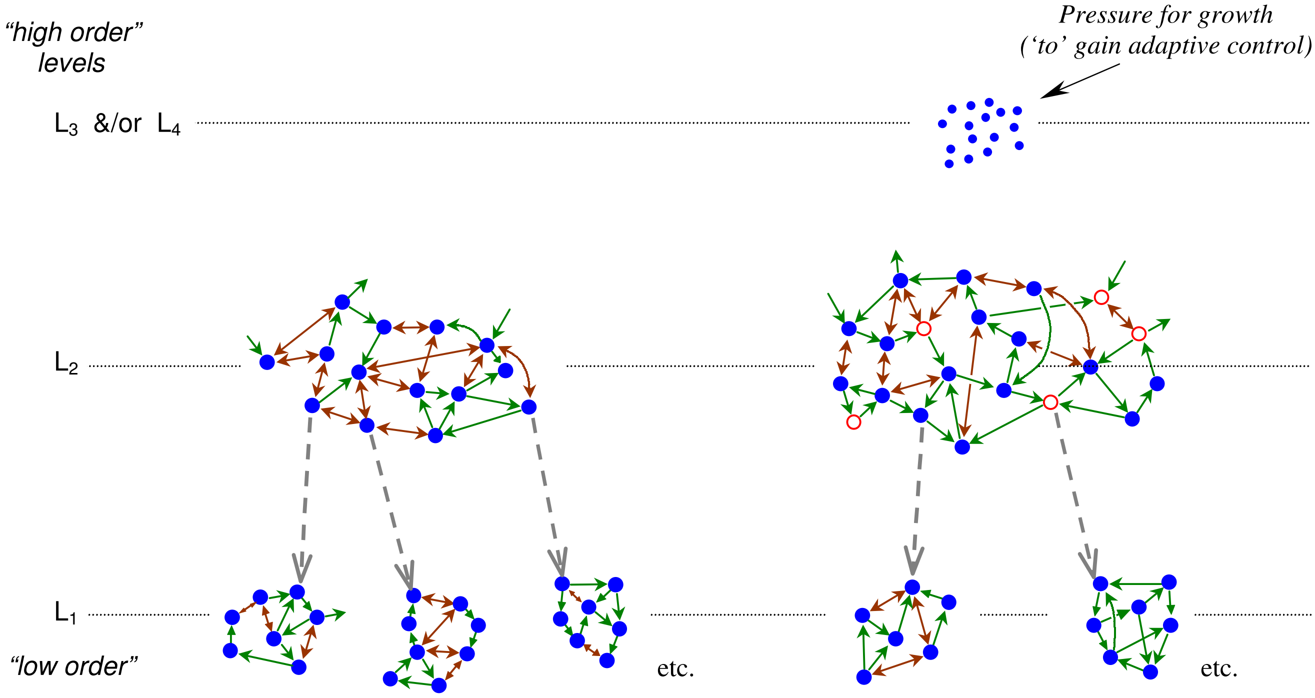
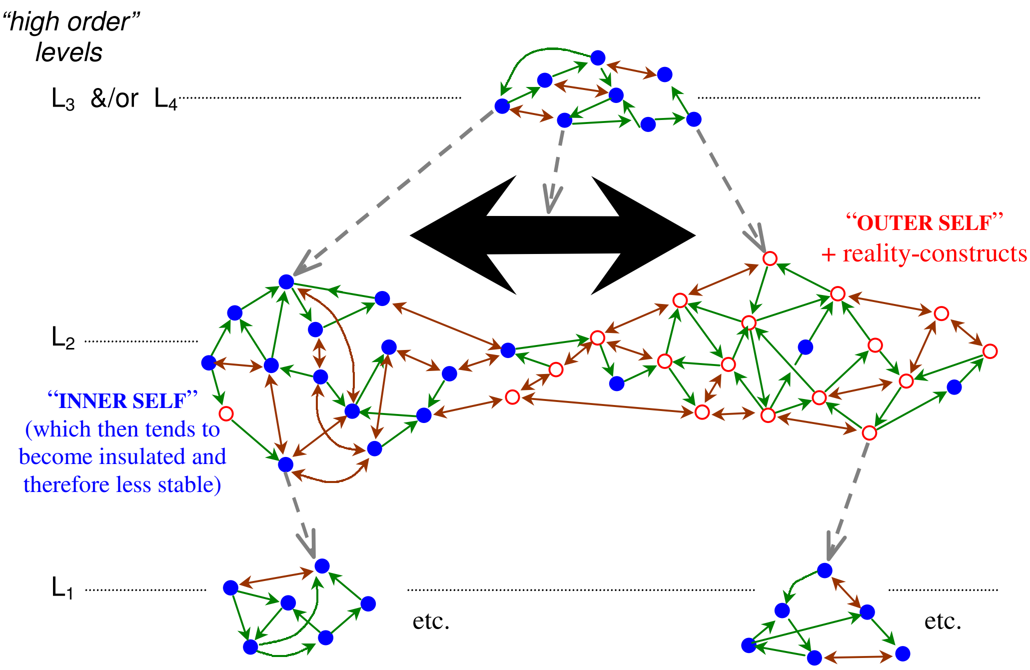


Fig (i). Idealized unperturbed(!) ego-schema. Likely to be unstable due to lack of use — (i.e. lack of predictive "external closure")

Fig (ii). Normal moderately perturbed ego-schema, with some re-adjustment (cf. Festinger's Cognitive Dissonance).

Conditions favourable to development of a "higher order" level — to aid this process.



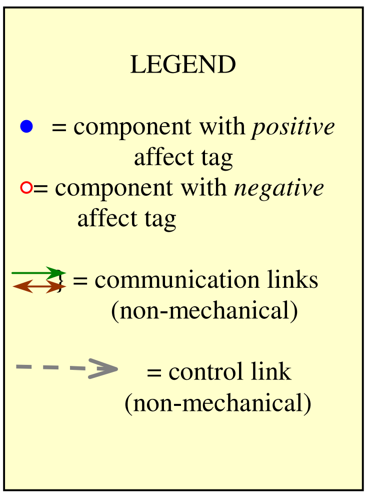


Fig (iii). Traumatized ego schema with start of adult schizoid "solution" to the problem; (Laing's conceptualization).

- "Neurotic solutions" to trauma perhaps entail a "protective coating" around the negative-affect elements "○") — achieved spontaneously (i.e. homeostatically *within* the same level and thus difficult to access from a higher "more conscious" level).
- Superego is seen as one-or-more schema (imago) embodying the perceived properties of another "person". Provisionally this may be thought of as being at L_3 level (rather than L_2). [I]
- *Consciousness may* be explicable in terms of a high*est* order schema (or scheme) having a privileged access to the communication network — (and hence attention ?).

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Wolman, B.B. (1970). *Children Without Childhood*. Grune & Stratton: New York and London.

[I] **Concerning "L_1 ... L_4 ...".** In later writings I have used a different notation for these levels: The basic **L_1** level (usually taken to be the essence of Piaget's "sensori-motor" developmental stage, as in the newborn), I have chosen to re-label as either just "L", or equivalently as "M^0L".

The **L_2** level above L is a *meta-level* (ML), or "M^1L". Likewise **L_3** = "M^2L"; **L_4** = "M^3L"; ...

The apparent complication has this advantage: It helps to fit in with the *"meta–"* terminology of philosophy and mathematics, and thereby emphasizes the capacity for the recursive control which was much discussed by Ross Ashby (http://en.wikipedia.org/wiki/William_Ross_Ashby), especially in his book *Design for a Brain* (1952/1960/1966).

In either case it is convenient to have an index-number n (for M^nL *or* L_{n+1}), such that n=0 tallies with Piaget's sensori-motor level. This also makes systematic provision for the possibility that there may also be overlooked *lower levels below* sensori-motor. Any such lower levels can now be referred to as $M^{-1}L$, $M^{-2}L$, etc. for any tedious subprocessing which Piaget did not contemplate. (If such sub-levels do exist, they may well be analogous to the unseen subprogramming which underlies today's elaborate software: machine coding of which most users are totally unaware). RRT, 2007

A Defence of the Linear Micro-Element
Chapter III

Relevant extracts from a letter (dated 26 September 1975) to Dr N E Wetherick concerning his comments on the draft of the paper "Analytical Theory of Sensori-motor Spatial Development", which later appeared as (i) the second *Kybernetes* paper ((1978): **7**, 61-71); (ii) as Chapter A3 of the *Thesis*; and in 2006 (iii) online within www.ondwelle.com/Mol-Intel-A.pdf — [5]

#14/75

Dear Norman

Thank you very much for taking the trouble to sort through the issues raised in my draft article[5]. I think I can make some sort of defence on most of your points, ...

Your points seem to be:
(1) How translate elec. signals to RNA (or whatever)?
(2) How RNA... might replicate, (via DNA? If so, how?)
(3) How translate RNA... to electrically mediated action?
(4) What exptl evidence is there (or can there be)?
(5) What facts are inconsistent with my theory?
(6) What's so terribly wrong with current plastic-synapse theories?

(1 & 3) Translation between elec/Chem: Firstly, I have not come across any existing theory which adequately explains this in full Phys/Chem rigorous detail. (E.g. I have just gone carefully through Pribram's 1971 book[4]; and granting him his holograms etc, he still does not seem to have tackled the mechanics of this vexing last stage of the process). Secondly, I have an idea that I may be on to an explanation in terms of much higher frequency components ($\gg$*1000 c/s*) of electromagnetic radiation. This sounds a bit far-fetched until one thinks that (i) this is what one might expect from the basic molecular sources, and (ii) the accepted phenomena such as *1 m.sec* "spikes" should be explicable as *envelopes* of high-frequency phenomena (like the audio-frequency transmission in radio) — nor need this render the "spike-concept" redundant — just less-than-complete. See [*"Technical considerations"*: in section A3.3 [5] — and above in Chapter I, section A]. I have half drafted a paper going into this in more depth [Draft-Ref.10/75][J], and I have done some calculations for a second one concerning some unexpected embryological implications — control of the dimensions of the myelin sheath.[K]

(4 & 5) Offhand I rather think that my theory can be regarded as subsuming existing theories, but I wouldn't like to be dogmatic at this stage; Pribram writes about *two* types of phenomena, why not *three* (especially if the third also happens to be a building-block for the others)? Anyhow such compatibility would (if true) dispose of point "**(5)**". As for testability **(4)**, this is a problem — see ["Methodology" panel at the close of Chapter I in this volume]; though if we accept the infra-red hypothesis as an integral part of the theory then this just might be testable,

[J] later[7] re-written (1977) as *Thesis Part B*, alias Brunel Monograph 24 — www.ondwelle.com/MolecMemIR.pdf Also see the sequel: • Traill (1988), "The case that mammalian intelligence is based on sub-molecular coding and fibre-optic capabilities of myelinated nerve axons". *Speculations in Science and Technology*, **11**(3), 173-181.

[K] This account eventually appeared as Chapters 6&7 of the 1999 book: www.ondwelle.com/BK0_MU6.PDF and was developed further in the online paper (2006a): www.ondwelle.com/OSM01.pdf .

e.g. (i) micro-fibre optical tests, (ii) imposed signals using I.R. lasers, (iii) fluorescent "indicators" of neural transmission (already reported[3]) might be explained in these terms.[L]

However, on philosophical grounds, I would not accept that exptl "untestability" (sic) necessarily means non-existence. See draft 13/75 [since superseded by Chapter V of this volume, p.21]. I suspect that a non-realization of this has had a serious stagnating effect on science in general (espec Psych & Physics[2]).

(2) Replication of RNA. Good question! The route via DNA which you allude to does seem to be a practical possibility as there is an enzyme which mediates a reverse copying (from RNA to DNA). Personally though, I think that the following sounds "more biological": under favourable conditions (of course) the RNA (or..)-to-be-copied is "read out" into electrical signals and almost-simultaneously used to "write" new RNA(or..) codings. This does seem rather fanciful, but given a dispersive medium it is remotely feasible — though it seems difficult to see how the new coding could be in the same "language" (but maybe this would not matter).[M] Moreover this is arguably no more fanciful than the reversal enzyme system (which would involve a two-stage process, and that common office-problem of what to do with the "permanent" DNA record of the transaction; to me it seems more biological to dispense with dubious records and stages, and neat observable delays, — instead things tend to be done "on the run" with scant regard for the observer and his modelling problems). Mind you, this still leaves a lot unsaid; I shall have to give it further thought. What do you think?

(6) What's wrong with the plastic-synapse? In brief, I have found it hard to see how, on its own, this could be built into a thoroughgoing explanation of the more precise types of behaviour. I suspect it on a number of grounds, none of them (yet) conclusive, but collectively contributing to an overall skepticism. So far I have never attempted to commit these suspicions to paper, but it should do me good to start right now:- (a) OK, evidence exists that such changes do occur (cited in Pribram[4]), but this says nothing about how important the changes are; they may just be long-term re-adjustment of weightings, or applicable to only certain types of memory, or even necessary but not sufficient.

(b) As suggested above, no-one seems to have explained in detail just how the read/write coding could work by this method. Pribram postulates "induction" (p43) as a byproduct of a specific induced mitosis; but how is the mitosis induced? Elec/RNA?!

[L] [A surprising new "test-possibility (iv)" arose recently when an anomaly in *insect behaviour* seemed explicable by invoking IR as a component within *their* nervous systems, involving chitin rather than myelin (Traill, 2006c, bottom of p.13 – top of p.15): www.ondwelle.com/OSM03.pdf . This would be comparatively easy to test, though its human-significance could only be suggestive pending further work. RRT, 2007]

[M] [**A better suggestion which arose later**: Note that SPECIES-EVOLUTION faces the same formal problem: *"How am I to write down the lessons I learn?"* It seems likely that the main solution in both cases will be: No actual "tape-recording" at all, but rather a multitude of randomly generated internal suggestions, most of which are then scrapped when they do not fit with reality — i.e. Darwinian selection in *both* cases, though at vastly different speeds! (If synapses really were *the* key memory-mechanisms, they would probably not be sufficiently numerous-or-organized to offer a practical mental-system of this Darwinian sort, *unless* aided by some auxiliary system. **However** any significant *molecular-coding base* could offer a vastly bigger pool of "suggestions" from which to make rapid selections).
(Traill, 1999, Chapters 2&4): www.ondwelle.com/BK0_MU6.PDF ;
(Traill, 2000, Chapter 5): www.ondwelle.com/BK1_V28.PDF .
Then again, close inspection shows that Piaget's much earlier *"action first" interpretation* had also implied this Darwinian trial-and-error within infant-learning; see www.ondwelle.com/OSM02.pdf]
RRT, 2007

(c) Brindley G S (1969) “Nerve net models of plausible size that perform many simple learning tasks” *Proc. Roy. Soc. Lond. B*, **174**, 173-191. This paper goes into calculations concerning information storage capacity, and I do not find the results sufficiently convincing, bearing in mind the staggering capacity we have for remembering “trivial” incidental details of everyday life, and the redundancy which must be needed for such a robust system made of chancy elements.

(d) Recent electron-microscopy has shown up a wealth of structural detail, down to roughly molecular levels of scale; and it doesn’t seem to me to make biological/evolutionary sense for such structure not be seized upon for functional purposes. Moreover if discrete information is to be stored, why use a “floppy” thing like a synapse rather than a more compact and predictable element at molecular level? (Of course Pribram would say that memory may have continuous “analogue” elements *as well*, and perhaps these would be retained more appropriately in an adaptable synapse. OK, *nihil obstat*, but I am more immediately interested in the discrete elements).

(e) Presumably synaptic modification would be a comparatively slow process, and meanwhile memory would be retained (according to the orthodox view) in reverberations around neural loops or such-like.[N] This would seem to be even more grossly wasteful of facilities, indeed it is difficult to see how such a procedure could retain sufficient precision in the details without gross corruption (especially given a reasonably limited number of neurons) or indeed just how an appropriate loop could be allocated and reserved during use — unless perhaps a molecular-based steering mechanism were used (in which case it might obviously be better to forget the wasteful loops altogether and adapt the molecular mechanism — maybe evolution did just that).

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References, Chapter III

1. Brindley, G S (1969), “Nerve net models of plausible size that perform many simple learning tasks” *Proc. Roy. Soc. Lond. B*, **174**, 173-191.
2. Bunge, M (1973), *Philosophy of Physics*, D Reidel: Dordrecht
3. Cohen, L B, *et al* (1974), “Changes in axon fluorescence during activity: Molecular probes of membrane potential”, *J Membrane Biol.*, **19**, 1-36
4. Pribram, K H (1971), *Languages of the Brain*, Prentice-Hall: Englewood Cliffs, N J
5. Traill, R.R. (1978). “Analytical theory of sensori-motor spatial development”. *Kybernetes*, **7**, 61-71. — [Then still “in press”. Now available online[O] within *Thesis Part A*.] #7/75
6. Traill, R R (1976), *Sensori-Motor Development of object concepts: A Mechanistic Hypothesis*, (Chapter I of this volume) #11/75
7. Traill, R R [1977/1980], *Toward a theoretical explanation of electro-chemical interaction in memory use: the postulated role of infra-red components*; — later published as Monograph 24, Cybernetics Department, Brunel University (1977/1980); and as *Thesis, Part B* — www.ondwelle.com/MolecMemIR.pdf #17/75

[N] Early computers used acoustic reverberation-loops through mercury or stiff wires — with the signal being delayed by that medium, next *“tidied up”* at the end of that journey, and then repeatedly sent through again. Not surprisingly, that technique has *long been obsolete* for most purposes. [RRT, 2007]

[O] *Thesis, Part A*: www.ondwelle.com/Mol-Intel-A.pdf (Chapter A3) plus references in the separate file: www.ondwelle.com/Mol-Intel-Refs.pdf —— *or* see the BURA website (Brunel University Research Archive) for a 203-page file which contains both.

A Critique of Computer-Validation as a test for Brain Theories

Chapter IV

1st chapter on scientific method

Relevant extracts from a letter (dated 9 December 1975) to Professor W A Scott, discussing aspects of: (1) testing by "external closure"; and (2&3) two aspects of testing by "internal closure". #15/75

On reflection, I would like to query your apparent criterion for the validity of mental-mechanism-theories. If I understood correctly (from private conversation), you were concerned that such a theory should be *modellable on a computer* — though I am not now clear as to just how much importance you placed on this, or what other criteria you migh seek (additionally or alternatively), or whether you were talking exclusively of digital computers. Indeed perhaps you really meant "exhaustive formal specification" — in which case I would probably agree with you.

Arguably there are three types of criteria for such models which purport to represent reality:–

(1) **Empirical criterion**: Are the basic "relatively static" elements of the model "indubitably" identifiable with the real world? This is a philosophical minefield, which I fear modern physics has run foul of — carrying many other disciplines with it; (Bunge (1973); Traill (1976a, Part II)[P]).
For one thing I would suggest that there is no such thing as absolute indubitability; and more importantly, that there are many cases where there is not even a reliable common-sense guideline for distinguishing between practical dubitability and indubitability. Secondly too few experimentalists think sufficiently in terms of systems-theory: how an apparently static configuration can actually have a dynamic base.

(2) **Digital-Dynamic Criterion** (Discrete-element approach):
Can we construct a model dynamic system whose long-term emergent properties (given appropriate input) will agree sufficiently with a chosen natural system? I would agree with this criterion *if* any of the following conditions seemed reasonable:–

(i) If the brain were to operate entirely in terms of discrete on-off elements (which were "big enough" to be "observable " in some sense).
I no longer believe this is true for brain models: *Whole-neurons* have been discredited as elements since about 1960; *synapses* are dubious contenders until their positioning and transmitter-release can be shown to operate in an entirely discrete manner; and evidence in favour of *some* use of *continuous* (*field*) effects is now formidable — see (3).

(ii) If we were to understand the processes sufficiently well so that we could "abstract the essence" of mental activity by using "functionally equivalent" digital subroutines to represent comparatively large-scale mental subsystems. There seems to be some following for this view amongst biologically-naive computer-buffs; but I am pretty sure that they are mistaken at this point of time — though the situation might change eventually.

(iii) If digital computers could approximate any required continuous phenomenon with sufficient *efficiency*. I used to think this might just be possible, provided a sufficiently "small or ablated" brain were considered acceptable — or provided one were content to make piecemeal models and cope with the difficulties entailed thereby. At present however I believe that wave phenomena are much more important *and microscopic* than has commonly been supposed, so I see absolutely no prospect of *adequate* digital modelling (unless via (ii)). This might not stop me using programming language to *describe* theories formally, but I would first abandon all thought of ever running such a program on a real digital computer.

[P] also available as *Chapter A2* in *Thesis, Part A*: www.ondwelle.com/Mol-Intel-A.pdf (plus references in the separate file: www.ondwelle.com/Mol-Intel-Refs.pdf — *or* see the BURA website for a 203-page file which contains both)

(3) **Field-Dynamic Criterion**: (continuous-"element" approach — or hybrid continuous *and* discrete). I will not bother to dispute the practical legitimacy of discrete elements (especially for memory and symbolism) though one might argue that even the sub-atomic particles of physics are "really" *more* waves than particles. Suffice it to say that we may accept many discrete elements, but that our dynamic model is not likely to be adequate unless it can also model very large quantities of wave-phenomena; e.g. for hologram activity (Pribram 1971).
There is some chance that traditional mathematical analytical techniques might serve here as an important part of the model (and perhaps one could usefully program this *discrete symbolism*). Anyhow *if* some workable and comprehensive medium can be constructed along such hybrid lines, then this would be the way to validate theories of brain function. I would certainly agree that mere flowcharts which do not explain in detail how decisions are made and propagated (etc), do not really get to grips with the problem.

References, Chapter IV

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[Now also available as Chapters.A1&A2 in: www.ondwelle.com/Mol-Intel-A.pdf
plus references in the separate file: www.ondwelle.com/Mol-Intel-Refs.pdf
or see the BURA website for a 203-page file which contains both— RRT, 2007] 4/75'partII

[*"Chapter IV½" — an option to digress into physics and physiology*]

[Logically, the next chapter could be *The gulf between behavioural psychology and fundamental physiology: a systematic attempt to bridge the gap*. (Paper presented at the workshop on Memory during the annual conference of the British Psychological Society, York, April 1976). However:–

Originally this had already been published separately as <u>Monograph 15</u>, Cybernetics Dept., Brunel University, (1976); — and it now seems less confusing to maintain this separate identity, especially as it is a comparatively long "short-paper". Moreover it is now easily accessed online as www.ondwelle.com/OSM05.pdf (2007). (It is also cited above on page 7, on the same location-issue).

In any case, as well as discussing the current topic (Piaget on behaviour, and the physical nature of his "scheme"), the paper also initiates a digression into somewhat separate issues concerning possible infrared quantum signals within cell-tissue — *a parallel-but-distinct line of enquiry* which has since come to include

- <u>Brunel Monograph 24</u> (1977/1980) / *Thesis, Part B* (1978) www.ondwelle.com/MolecMemIR.pdf;
- The journal article: *"The case that mammalian intelligence ..."* (1988). Full ref.[J] on p.16 above.
- Book A (1999): *"Mind and Micro-mechanism",* Chapters 3 and 7: www.ondwelle.com/BK0_MU6.PDF
- Book B (2000): *"Physics and Philosophy of the Mind"*, Part II: www.ondwelle.com/BK1_V28.PDF
- *"Strange regularities in the geometry of myelin ..."* (2005a) www.ondwelle.com/OSM01.pdf

Thus there is some logic in "filing" this paper separately anyhow.] RRT, 2007

On Modelling Reality: The 'camera fallacy' in our approach to scientific method, and sometimes to mental functioning

2nd chapter on scientific method

Chapter V

Display paper presented at the York conference of the British Psychological Society, 2–5 April 1976.
This is an outline argument against the currently widely-accepted view that a theory is not worth even considering unless it is "directly" testable. #1/76

ABSTRACT
[from *Bulletin of the British Psychological Society*, **29**, 222-223; (June 1976)]

If we are not to accept a theistic view that knowledge and structure are ultimately given by an unfathomable authority, then it seems to follow that we can only explain knowledge and structure in terms of homeostatic self-organizing systems. Moreover these systems must always, in the long run, evolve entirely without any infallible guidance from 'above'.

One unpalatable consequence of this is that the rules of logic must ultimately be accepted as only empirical, and not absolutely transcendental as is often assumed (also see Gödel, 1931).

But more important is the disagreeable consequence that the status of such fundamental concepts as 'object' and 'observation' can no longer be taken for granted at 'face-value'. This is now reasonably well understood in Piagetian circles with respect to *child* development; but the same principles apply equally to the acquisition of knowledge by a *scientific community* — and this is *not* so well appreciated. (In both cases, the objective is to build or improve models of reality.)

In this light, current doctrines on Scientific Method deserve close scrutiny. Scientific experimentation corresponds (partially) to spontaneous scheme-exercise in the child, and each is certainly essential in its own domain. But such necessarily limited sampling-transactions with reality are not sufficient on their own; *internal testing* of the self-consistency of one's model of reality is also indispensable (and *depends* on the implied *assumption* that the reality outside is also discernibly self-consistent). In science, such internal testing is done by *certain types* of theorizing.

Because these issues have been poorly understood, and because some types of theorizing (such as Hull, 1943) contribute little or nothing to internal closure, all non-trivial theorizing has tended to fall into disrepute and there has been an overemphasis on experimentation — some of it quite inconsequential due to an inadequate theoretical basis.

Poster

What *ultimately* creates the meaningful structure in . . . ?

		"Democratic" answer	"Authoritarian" answer
	a photo ?	the camera (✘)	photographer + camera designer (✔)
	a computer design ?	the computer (✘ ?)	programmer + hardware designer (✔?)
Epistemology { individual	a mental concept ?	**self-organizing brain (?)** ?	God (?), "Spiritual Self" (?)
Epistemology { social	a scientific law ?	**self-organizing scientific method (?)**	Einstein (?). Freud (?), God (?) ?

• If this is the correct answer (for a mental concept), then how is such self-organization achieved ?

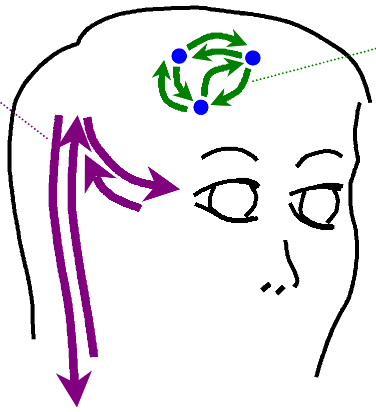
Piagetian answer (for the individual) includes:–

(1) The evolutionary development of a material medium (brain) capable of performing *adaptively* according to the following —

(2) **Transactions with 'reality'**
(= Reality-testing/sampling or "external closure" testing):

Absolutely essential, but not sufficient because
(i) a *total* sample is impossible (especially with respect to the future).
(ii) At least some aspects of reality, such as the 'law of nature', cannot be directly observed but must be inferred or *'intuited'*.

Indeed this arguably applies to *all* non-hereditary concepts.



(3) **Selection according to internal self-consistency**
(= 'equilibration' or 'internal closure').

This is the key to the "democratic" self-organization of concepts, without any *absolute* method of validation — ever!

- So what about the 'self-organization' in scientific method in fields where there is (as yet) no reliable authority ?

It is suggested that the growth of scientific knowledge happens in a manner which is *functionally equivalent* to the growth of knowledge in the individual. Thus it may be counter-productive if we become over-awed by experimentation (external closure, (2) ↖) at the expense of interdisciplinary tests for self-consistency (internal closure, (3) ↗)

Discussion

In practice, this overemphasis on experimentation (in non-clinical psychology) seems to take the form of failing to accord credibility to any theory for which there is no immediately foreseeable experimental test — (due to a misinterpretation of Popper (1934) ?).

Such a criterion does have value in combating metaphysical excesses. Yet it is difficult to see how physics and chemistry could have reached their present state of sophistication if such a rule had been rigorously followed in those disciplines; and indeed this was nearly the fate of Planck's quantum postulate, until Einstein formulated further internal corroboration, using other existing data.

It is therefore proposed that an alternative criterion should also be effectively available:–

> "Is there one or more method of corroborating the model using its internal structure (and taking interdisciplinary data into account)?"

Or equivalently:

> "Does the internal 'logic' of the postulated system have a mathematical-group structure in two or more dimensions?"

Or equivalently:

> "Given the current state of knowledge about the supposed micro-constituents and their properties, is the postulated system *capable* of accounting in detail for two or more *diverse* sets of macro-observations (and at least be compatible with all other reliable macro-observations)?"

It still remains, perhaps, to formulate this in more practical terms.[Q] For instance, how much internal corroboration should be required, and in what circumstances?

Anyhow, in the absence of anything better, such an approach can be attempted in such ‘impossible’ areas of study as the ‘no-mans-land’ between psychology and physiology, where experimentation is very slow in producing results with sufficient detail to give an adequate explanation of the basic mechanisms (Traill, 1976b [Monograph 15]).

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[Q] Since then, Professor Paul Thagard and his team have done just that — offering their “ECHO” computerized coherence-evaluation system. See: P.Thagard (1992) *Conceptual Revolutions*, Princeton University Press; and the related website with working examples (http://cogsci.uwaterloo.ca/JavaECHO/jecho.html).
Also see my own discussion of this and related matters in: www.ondwelle.com/OSM03.pdf

[R] *Thesis, Part A* (1978): www.ondwelle.com/Mol-Intel-A.pdf — plus references: www.ondwelle.com/Mol-Intel-Refs.pdf —— *or* see the BURA website (Brunel University Research Archive) for a 203-page file which contains both.

Mechanistic Modelling — of consciousness?

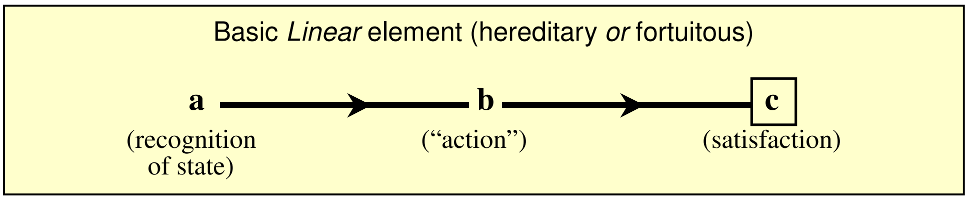
Chapter VI

Paper presented to the Philosophy of Psychology Group, during the York conference of the British Psychological Society, 3 April 1976.
This paper elaborates further on the "linear micro-element" theory put forward in chapters I and II within this volume (and in references cited therein), and also helps prepare the way for *Thesis Part C:*
[www.ondwelle.com/Mol-Intel-C1-5.pdf , www.ondwelle.com/Mol-Intel-C6.pdf ... *ff.* ——
or see the BURA website for a 203-page file which contains all these Part C files and references]
#5/76

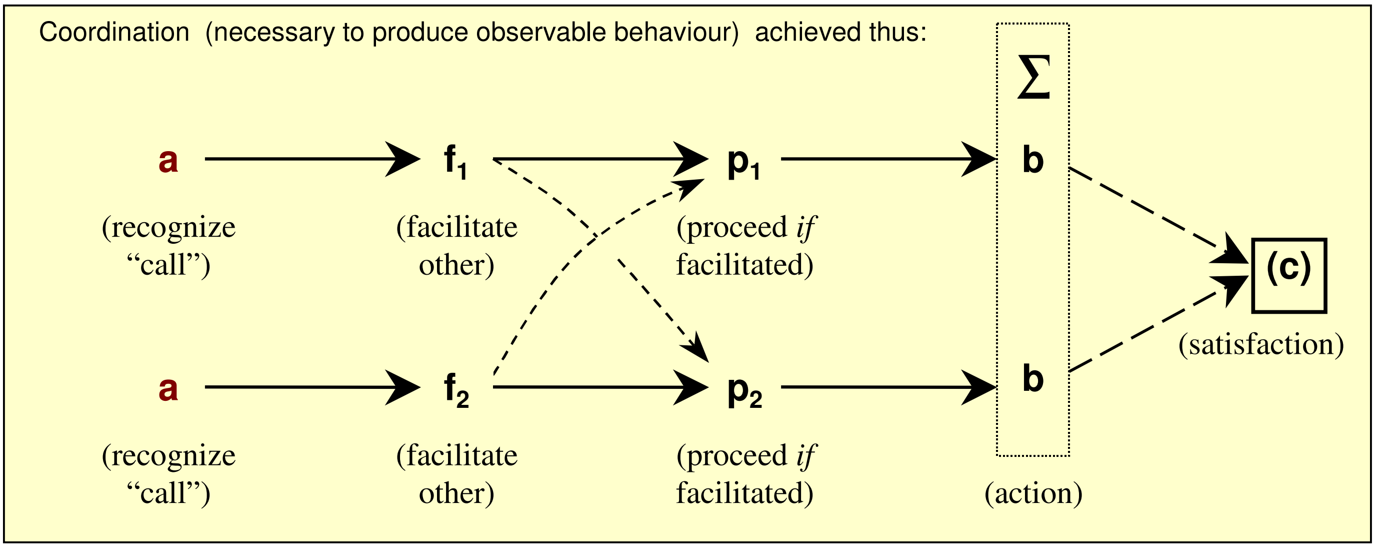
A. Basic concepts.

— See also Chapter I above, — and Traill (1976c: Monograph 15 www.ondwelle.com/OSM05.pdf)

(Fig **A₁**) Elements interacting with reality — [from page 9, above]



(Fig **A₂**) Consider each such linear element as encoded on a linear molecule and replicated many times. [S]



——————— oOo ———————

[S] Diagram modified from page 9 — and used again later in *Thesis, Part C* (Fig. C5.2/2, original page-number 193), where it is discussed in greater detail: www.ondwelle.com/Mol-Intel-C1-5.pdf + references in the separate file: www.ondwelle.com/Mol-Intel-Refs.pdf ; — *or* see the BURA website for a 203-page file which contains *both*.

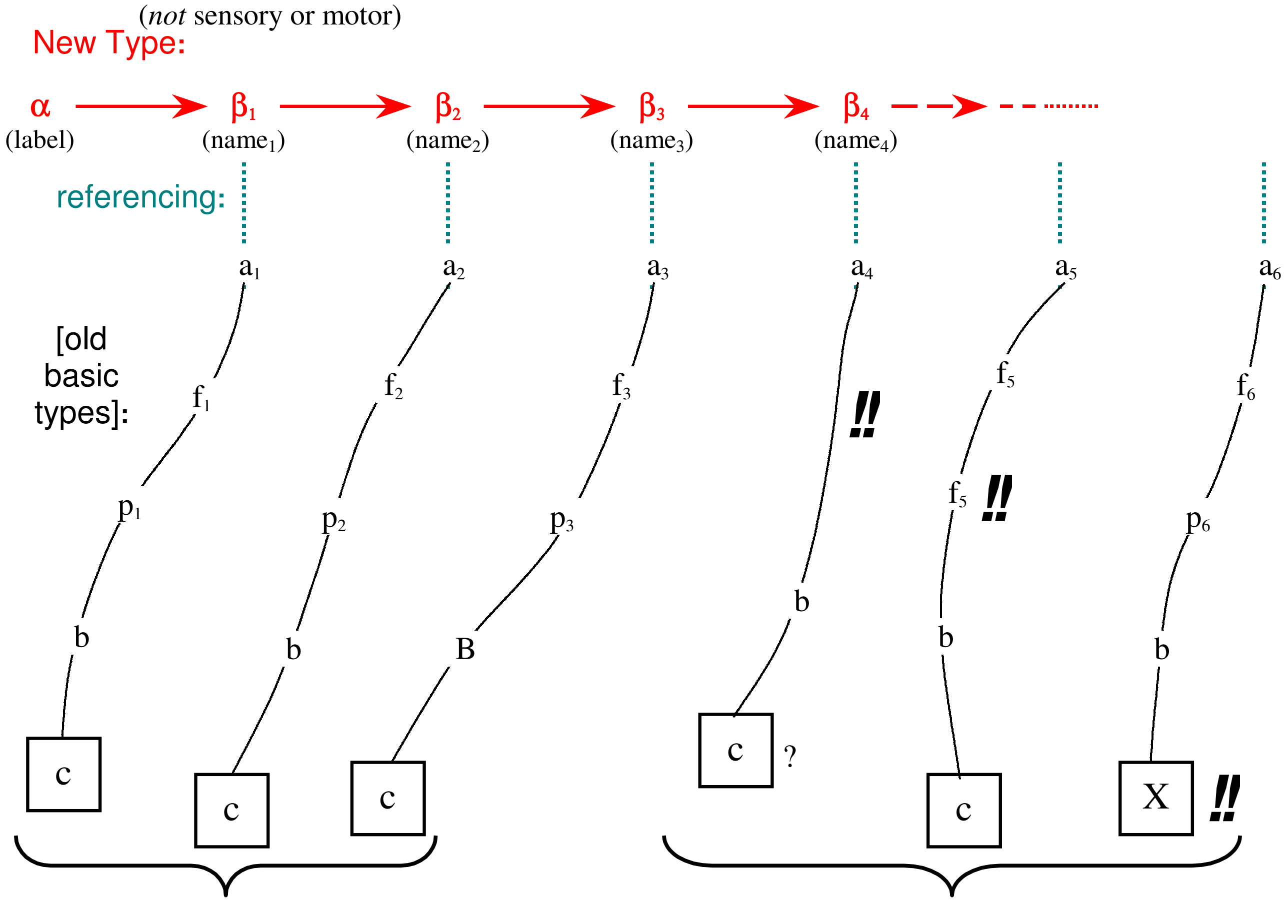
B. Elements stabilise allowing categorisation.

(B_1) *Stability* is acquired by *coordinated-and-successful* element-types.
(Perhaps because "satisfaction" brings an "approval-tag"
which protects the element from enzyme attack?
See Brunel Monograph 15 (Traill, 1976b), Fig.(V)b.**)**

Another possible stabilising mechanism would be for the component elements to carry out "practice-exercises" (during a suitable sleep-mode[T]). Any elements "left out in the cold", as not being sufficiently involved in any cycle of 'internal closure', would also tend to become detached and/or dissolved. (See *Thesis, Chap.A1*, §1.5, last two paragraphs. [www.ondwelle.com/Mol-Intel-A.pdf — (+ references: www.ondwelle.com/Mol-Intel-Refs.pdf *or* use BURA)]).

There may be many such stabilising mechanisms, possibly working in collaboration, but just one or other of the above two would probably suffice for the development postulated below; (and this development could plausibly account for a third stabilising influence: see below, under (C_2)).

- Such stability makes it feasible to form useful *categories* (*lists*) of similar types of element.
- This *listing* requires a new type of (control) element which has ***internal reference***
  (*not* sensory or motor)



? acceptable according to the arbitrary criterion

(Fig $\mathbf{B_2}$)

? eliminated as "not belonging"

[T] This possible significance of sleep modes was later explored in §8.6 of www.ondwelle.com/BK0_MU6.PDF (Traill, 1999)

[This diagram appeared again in 1978 (slightly modified) as Fig.C5.2/3 within: www.ondwelle.com/Mol-Intel-C1-5.pdf (original-page 200) RRT, 2007]

In Fig.B_2, we may suppose that basic elements (of the original type) come to be "listed" in the α-β_1-β_2-β_3··· element, by means of "*names*" (β_n, which are plausibly inhibited calls to the basic element-labels in question: a_n). There would be an arbitrarily set expectation ("intensive definition") as to what the membership criterion should be for the particular list; (embodied, perhaps, in the properties of the first member?). The resulting list would amount to a tentative "extensive definition" of the set's common property.

Ill-matched members, such as (a_4······) would plausibly be cast out for failing some sort of uniformity-of-behaviour test (another type of internal closure?)

In appropriate lists, in which the criterion for membership turned out to be irrelevant to the apparent needs of the organism as a whole, would presumably be dissolved (at the α-β_1-β_2··· level, *only*) through failure to gain enough support from internal or external closure — at this new higher level. Such dissolution could well follow the same basic procedures as used for dissolving meaningless (a_n···)-type codings.

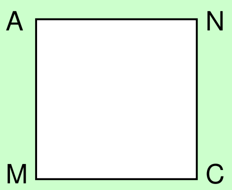
The third form of stabilisation for the (a_n···) elements (referred to above) would be likely to arise as a consequence of their involvement in "kindred-spirit" lists — as a further type of internal closure or (equivalently?) because they are now "embedded" within a larger communication-"structure".

——————— oOo ———————

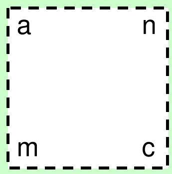
C. Development into representation of a *Mathematical Group* [U]

(Fig.**C1**)

Consider task of "learning the geography" of a square



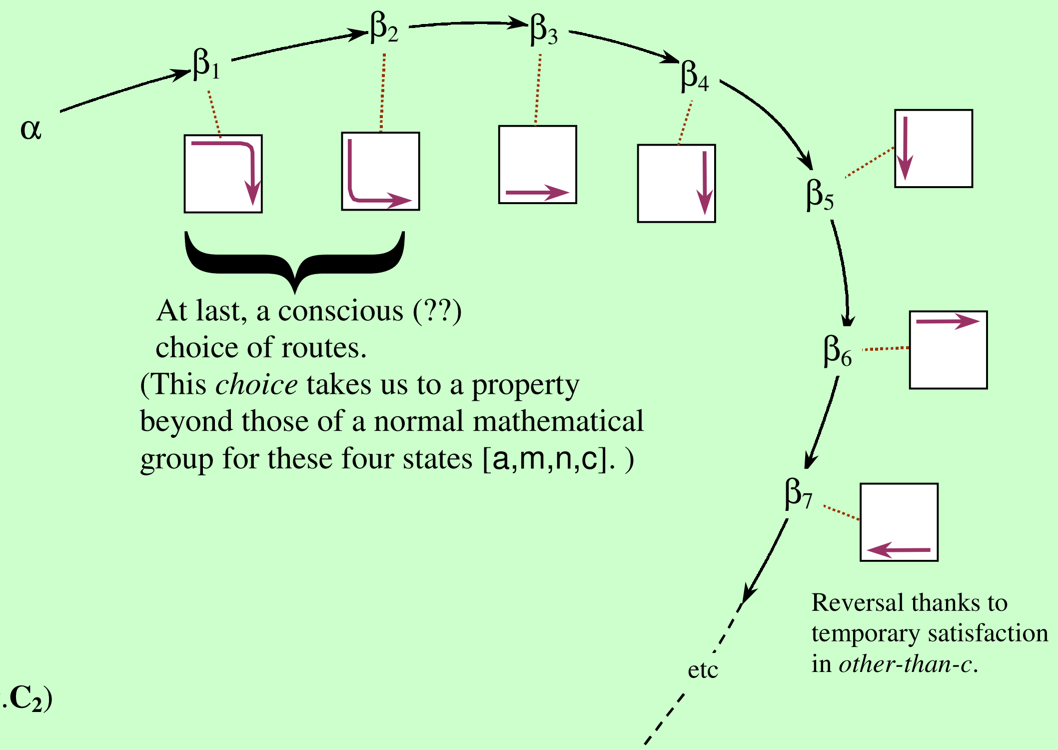
internal representation being:–



(*parts* only)

At least initially this is essentially a topological problem — learning to grope one's way round the cycle A-N-C-M-A, back again, and variations on this theme like how to take short-cuts or how to compound moves.

[U] This summarizes an explanation offered in Chapter I, above. Some diagrams are essentially the same. Also see the *Kybernetes* **5** paper (= *Thesis* Chapters A1 & A2: www.ondwelle.com/Mol-Intel-A.pdf +*refs.* — *or* see the BURA website for the 203-page file which contains both).
The topic later appears again in a somewhat different treatment, as Chapter 8 within the book *Mind and Micro-mechanism*: www.ondwelle.com/BK0_MU6.PDF (Traill, 1999), Ondwelle: Melbourne.

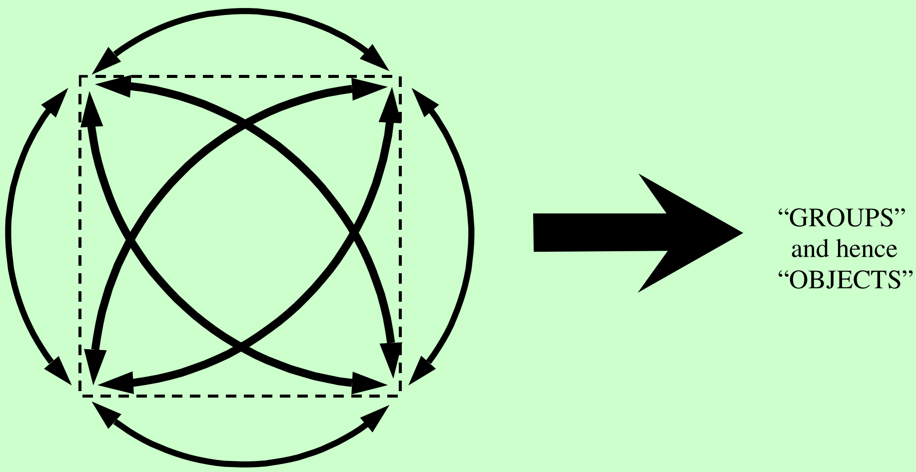


(Fig.**C$_2$**)

The formation of a list for elements of the type: "Transactions involving changes in my relationship (touch, eye-fixation, ...) with this configuration in front of me".

This list of relevant transitions, when it is complete in some internal-closure sense, constitutes the embodiment of a mathematical group. As such, it would be sensible to suppose that it would acquire more stability than it previously had as a mere kindred-membered set.

(Fig.**C$_3$**) ... so eventually all transition possibilities become listed.



Arguably, this development effectively promotes the (α_n...) strings from being mere *scheme*-elements of the "higher level" into *schema* elements of the "higher level". Arguably, too, this completes a cycle producing the basic concepts of permanent objects, which can now be used as a solid foundation for re-capitulating this whole process at a higher double-level again:–

D. Recursion

(D_1) Now objects have a special significance as such (*cf.* only primitive recognition previously; a, m, n for instance).

Mental concepts for objects etc now constitute a new "toehold on reality" allowing a new learning cycle to begin — using these new stable structures as basic elements, in much the same way that primitive (inherited?) perceptual and motor elements have been used hitherto.

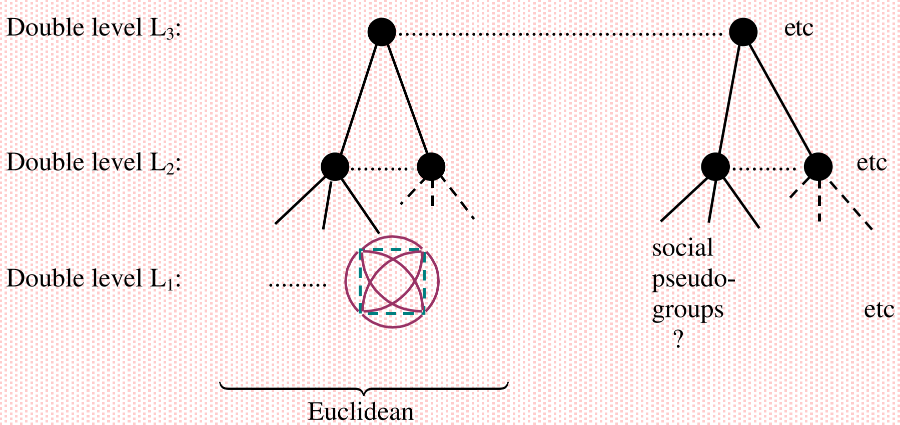
(D_2) This allows the whole process to repeat itself, but at a "higher level", (L_2).
{(L_2) = Pre-operational = listing of objects and other group-like concepts of similar status. The "Operational Period" leads to new "super-groups" allowing the child to make conscious intentional transformations in objects, etc.}

Note the two-stage nature of this recursive level ("Pre-operational" then "Operational") in the formation of sets (which may then become groups; — a third stage?) This is formally equivalent to the procedure postulated for the previous Sensori-Motor period.[V]

(D_3) The process may repeat again to give *abstract thought*: "Formal operations". (L_3)

It looks as though this might be yet another double-level of development "above" that of the Operational Period, and making use of its output-constructs.

(D_4) Thus:



So we seem to have something very like a recursive process which produces what amounts to a hierarchical pyramid structure — stating from the base, and working its way upwards. There may well be irregularities and departures from this (especially in pathological cases?) but this seems to be the basic pattern.

——————— oOo ———————

[V] These are terms used by Piaget to identify various developmental stages. E.g. see *footnote-25* (page 12) in the abovementioned[U] 1999 book: www.ondwelle.com/BK0_MU6.PDF — or H.G.Furth (1969). *Piaget and Knowledge*. Prentice-Hall: New Jersey.

E. Consciousness

(E_1) Could consciousness be "the highest existing level"?

How does consciousness relate to the pyramid structure? If it is *part* of the structure, it would arguably "be at" the current top level, and would therefore need to "move camp" whenever a new double-layer happened to materialise. But plausibly "consciousness" does not appear at all until the Operational Period, and then arguably it might remain primarily associated with L_2 (the corresponding double-level).

It might, however make better sense to think of consciousness as some sort of communication-device-or-centre, *separate* from the pyramid-structure itself, and having a more-or-less stable and unchanging status throughout the development process.

(It would be plausible to suppose that the highest existing level in the hierarchy would have privileged access to such a consciousness-centre).

(E_2) Limits to Introspection?

It seems likely that introspection would be limited to those parts of the total structure which were: subservient to **c**onsciousness and/or subservient to the current-**h**ighest-level. It would seem to follow then that one or both of these constructs would itself be more-or-less hidden from direct introspection and so constitute a "black box"; — though there are, of course, methods for inferring the contents of a black box.

(E_3) Is a child's consciousness homologous to an adult's?

Our answer to this will presumably depend very much on which broad model of consciousness (see "E_1") we choose to accept; and vice versa. E.g. If we take consciousness to be essentially separate from the pyramid, then we would be inclined to accept adult and childhood consciousness as being qualitatively equivalent (and vice versa).

(E_4) How is the ego involved, if at all?

The concept "ego" is intimately bound up with the notion of "self-identity"; — in fact the two terms were (rather rashly) used as interchangeable equivalents in Chapter II above.* One could, I suspect, make out a good case for claiming that consciousness is also intimately related with "self-identity"; and one might plausibly go on from there to explore the idea that *ego* and *consciousness* are very closely related.

However that may be, I would suggest that the problem of how to relate "consciousness" to the hierarchical pyramid, also applies to "ego"; though I suspect that the best *answers* might differ for the two cases. Seen from the present context of the hierarchical pyramid, the psychoanalytic literature's use of the term "ego" appears to be ambiguous — implying different variations of relationship to the pyramid (as in E_1 for consciousness).

Provisionally it might be useful to adopt working definitions which take (i) *consciousness* as being essentially separate from the hierarchy and (ii) *ego* as being bound to level L_2 — as suggested in [Chapter II above — (Traill, 1976b)].

(E_5) And Superego?

If *ego* is to be associated with the double-layer L_2, then the *superego*, which develops later, should probably be placed somewhere higher in the pyramid: L_3 for instance, as suggested in Chapter II above. But clearly this idea need further investigation and explication.

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Acceptance of the Validity of 'Internal Closure': Is this the basis of a 'school of philosophy' ?

3rd chapter on scientific method

Chapter VII

Extracts from letters to Professor M.P.Haggard in answer to his query about "the school in philosophy of science that you most closely ally with...". This query arose in the context of Chapter V above: {"On Modelling Reality", York conference}. #7/76

[At that time, I had had little exposure to schools of philosophy as such — so it is debatable whether I actually answered the question! Nevertheless I did take the opportunity to find some interesting ***precedents*** within psychoanalytic theory and elsewhere. Since then, Paul Thagard has also cited other cases in his book *Conceptual Revolutions* (1992), — and in the software at his ECHO website http://cogsci.uwaterloo.ca/JavaECHO/jecho.html , as discussed in my recent paper www.ondwelle.com/OSM03.pdf (Traill, 2005c). RRT, 2007]

(Second letter) 12 May 1976

Dear Professor Haggard

On second thoughts, I suppose that the most correct answer to your question (about philosophical schools of thought) would have to be that I have regarded myself as a rabid Piagetian — making capital out of his (poorly defined) concept of "equilibration", which he presumably borrowed from psychoanalysis (e.g. see Fenichel (1946/1971), *The Psychoanalytic Theory of Neurosis*; p.32; copy enclosed)* Both the Freudians and the Piagetians *act* upon a philosophy of knowledge-acquisition-aided-by-"internal-closure" — though they do not seem to have "sold" the philosophical angle explicitly enough — at least not in English.

Yours sincerely,

R. R. Traill

——————— oOo ———————

{ Editorial comment (RRT *15-6-1976*): Such analogies should not, of course, be relied on blindly. E.g. a jigsaw is a *static "system"* (endogenously speaking); whereas the patient of a psychoanalyst is a *dynamic system* subject to a rather low-resolution modelling porocess; and then again real systems studied by the "hard" sciences are *also dynamic but* subject to *hopefully-precise* modelling (in some sense).

However, the dynamic aspects or fine detail may be considered as "additional jigsaw pieces" which must *also* be fitted into the picture; this makes the task more difficult — but any answer is then more likely to be correct. }

* " Freud once compared psychoanalysis to a jigsaw puzzle, in which the aim is to construct a complete picture out of its fragments (550). There is but one correct solution. So long as this is not discovered, one can perhaps recognize isolated bits, but there is no coherent whole. If the correct solution is found, there can be no doubt as to its validity, for each fragment fits into the general whole. A final solution reveals a unified coherence in which every hitherto incomprehsible detail has found its place. And, also before this happy point is reached, dynamic-economic changes in the state of the patient are decisive for derermining whether or not the procedure of the analyst is adequate.[1]

[1] Many problems merely touched upon in this chapter are discussed at some length in (438). "

— O.Fenichel (1946/1971), *The Psychoanalytic Theory of Neurosis*, Routledge and Kegan Paul.

(Third letter) 30 June 1976

Dear Professor Haggard

Some more thoughts about schools of philosophy related to my approach.

(1) Perhaps "structuralism" is of some relevance here, (after all Piaget falls into this category, and has written a book with this title). Consider the following pseudo-definition which I have just come across: "One could say a structure is a combination and a relation of *formal elements* which reveal their *logical coherence* within given objects of analysis". (My emphasis)
— J.Ehrman (1970) *Structuralism*. Doubleday: Garden City, NY. (page ix).

"Logical coherence" seems to be much the same as "internal closure";[F] (in fact this might form the basis of a satisfactory definition of "logic").

I think the thing that disturbs me about this school is that they seem to be rather undiscerning in their choice of "formal elements". I prefer to *also* see a way of explaining the existence and stability of these "elements".[*] This seems to lead to an infinite regress, but only if one is totally purist. In practice, one must be content with "*adequate*" explanations at each level, until axiomatization does become "reasonably" indisputable at one's lowest level.

(2) Further to that quote from Fenichel, I enclose a copy of an interesting double page from Freud: (1914/1957) *Collected Papers*, **IV**, 34-35.[‡] —— *"On Narcissism: an introduction"*.

(3) Obviously writers on *Cybernetics* (alias "General Systems Theory", or as I like to put it "The whatever-it-is about any dynamic system which enables it to hang together for longer than random expectancy") will also be relevant; especially Ross Ashby (see references in my .....)[W].

Yours sincerely,

R. R. Traill

‡ " One dislikes the thought of abandoning observation for barren theoretical discussions, but all the same we must not shirk an attempt at explanation. Conceptions such as that of an ego-libido, an energy pertaining to the ego-instincts, and so on, are certainly neither very easy to grasp nor is their content sufficiently rich ; a speculative theory of these relations of which we are speaking would in the first place require as its basis a sharply defined concept. But I am of the opinion that that is just the difference between a speculative theory and a science founded upon constructions arrived at empirically. The latter will not begrudge to speculation its privilege of a smooth, logically unassailable structure, but will itself be gladly content with nebulous, scarcely imaginable conceptions, which it hopes to apprehend more clearly in the course of its development, or which it is even prepared to replace by others. For these ideas are not the basis of the science upon which everything rests : that, on the contrary, is observation alone. They are not the foundation-stone, but the coping of the whole structure, and they can be replaced and discarded without damaging it. The same thing is happening in our day in the science of physics, the fundamental notions of which as regards matter, centres of force, attraction, etc., are scarcely less debatable than the corresponding ideas in psycho-analysis.

. . . .

In the complete absence of any theory of the instincts which would help us find our bearings, we may be permitted, or rather, it is incumbent upon us, in the first place to work out any hypothesis to its logical conclusion, until it either fails or becomes confirmed. "

[*] (and their substructure too if necessary, and so on).

[W] the two *Kybernetes* papers (then available as pre-publication copies), whose references are now in www.ondwelle.com/Mol-Intel-Refs.pdf . Also see http://en.wikipedia.org/wiki/William_Ross_Ashby

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