Part C

DEVELOPMENT AND APPLICATION[1]

Developing the hypothetical concepts more deeply —
and applying them to explain a number of poorly
understood psychological phenomena, including
dysfunctions of the system which result in
clinical neurosis or psychosis

"A molecular theory of brain organization, with explanations of
neurosis, psychosis, and normal functioning".

Chapter C1

A Broader Strategy for Research — *seeking coherence amongst well-defined elements*

SUMMARY OF CHAPTER

This chapter serves two purposes, though this may not be immediately apparent. The manifest topic is its critical dissection of Scientific Method, seen as a *social* thought process; but the same discussion will later be seen as also being significantly relevant as an analogy in an examination of the thought-processes of the *individual*: because both entail Epistemology.

The case is put that, although modern science may be prepared for progress and change in the laws and theories which it directly studies, it is less prepared to question its own *rules of procedure* which take the form of "Scientific Method". And yet (it is argued), these rules should themselves be open to scientific scrutiny under the rubric of Epistemology. Seen in this light, it may be argued that present methodology places too much emphasis on "direct" experimental testability and too little on the development of "theory" in the form of internal structural logic; indeed I suggest that it is not strictly legitimate to separate these two processes.

Accordingly, we might expect to accelerate scientific progress if we can properly redress this apparent imbalance. But to do so efficiently we will need non-vague statements concerning whatever we take to be the basic structure of the system concerned.

C1.1 Observability and experimental testability

After a very extensive review of the work on schizophrenic thought, Chapman and Chapman (1973) suggest, amongst other things, that a tightening-up of experimental method might produce some new, more valid, bimodalities — and hence an improved insight into the relevant variables; (page 337). Such statistically based experimental work clearly offers an important second dimension to augment the earlier clinically-based conclusions, but it is open to question whether statistical experimentation is really adequate for the task — unless it is assisted by yet other approaches.

Even if we look no further than Chapman and Chapman's proposals, it is clear that a great deal of painstaking work would be needed, and that the value of the resulting payoff would be uncertain and incomplete. Moreover Elsasser (1958, 1961) makes the same point in a rather more systematic way by extending the physicist's concept of *indeterminacy* into the biological field. Thus whereas a physicist must ultimately choose between precision in the *position* of a particle — and precision in measuring its *momentum*; a biologist must similarly choose how much he is prepared to interfere with (or destroy) the *individual* subject, in the interests of "knowing about" something which no longer exists in that form — or alternatively he must choose between the precision of his experimental categories, and the possibility of finding exemplars to fit such exacting requirements.

The latter (statistical) formulation of the experimental constraints implies that we must expect diminishing returns from our efforts to find new insights from statistical surveys — however well designed. This does not preclude progress, but it suggests that sooner or later we will simply run out of resources in one way or another. Of course much can be expected from the intensive investigation of individual subjects, but clearly we will then have to cope with the "interference/destruction" formulation of the indeterminacy principle, as well as any problems we may have in generalizing our results.

Assuming that such arguments are valid, then should we despair of ever reaching an "adequate" understanding of the details of mental functioning, and other complex processes?

Of course it is true that for some time to come, the psychological sciences will be able to draw on the aid of non-psychological techniques in the form of physiological and pharmacological probes; (a semi-salvation for which there is no equivalent in particle physics). But in the end, will we still come back to the problem of an insurmountable knowledge-impasse arising from indeterminacy considerations? Well, it would seem that if we accept normal criteria for "objective measurements", then we will indeed reach such an impasse at which our "objective" procedures can take us no further.

[It has, in fact, already been implied (in Chapter A1, above) that we have been too generous in our criteria of "objectivity" — that we exceed the bounds of objectivity much earlier than we suppose, and that there may indeed be no such thing as objectivity (leading to "episteme") at all! It is not intended, however, to labour this point here; though it may perhaps lend urgency to our present considerations. The main point is simply that *sooner or later* experimental observations will fail to provide all the answers.]

Pure Behaviourists and many Physicists would have us believe that we should go no further; that any dabbling in what is not objectively testable is at best futile, and at worst obscurantist or occult — and in all fairness one must admit that such dangers do exist. In contrast however, two points might usefully be made without having to delve too deeply into the philosophical issues involved. Firstly, provided we place any reliance on our deductive processes at all, there is a case for *indirect* testing of postulates — even to the extent of testing by means of *remote* logical implications which may not yet be apparent to us, and which are not likely to become apparent if we do not first "take the plunge" and "exercise" the postulates in an *apparently* untestable state. Secondly, even if such postulates were absolutely untestable, they might nevertheless have a perfectly legitimate role to play as heuristic devices — methods of condensing an unmanageable mass of facts into a more parsimonious and *manageable* "mental model" (which is arguably the defining criterion for "understanding"). The classical example of such progress is Newton's formulation (via Kepler) of Tycho Brahe's mass of astronomical observations. But we should not scoff at the very real (if inaccurate) heuristic achievements of the Ptolomaic system as an aid to the practitioner in the absence of anything better; its main defect, given the circumstances, was that it was *treated* as absolutely factual.

Eysenck's attack on psychoanalysis, (for instance in his chapter "Little Hans or Little Albert", 1965), is of some interest in this context. The very term "Psycho-*analysis*" implies an attempted explanation by means of a 'model', the basic elements of which are unobservable (and therefore, in some sense, untestable). Whether or not one agrees with Eysenck that the practice of psychoanalysis is a waste of time and money, there does seem to be a good case for conceding that hypothetical analytical constructs such as 'super-ego' may well be immune from all experimental proof and disproof — by any normally accepted laboratory criterion. So what value, if any, is there in such intangible concepts? And have they any epistemological justification at all ?

Well, insofar as such concepts form part of a *self-consistent*, *quantitative*, and (perhaps) *dynamic* model, then to that extent we may argue in terms of justification by 'internal closure'. In this case, such criteria are only vaguely met — but it would be an overstatement to claim that they are not met at all. It is easy to recite the slogan that "they explain everything, and predict nothing"; but neither part of the statement is rigorously true, so it would be unwise to entirely dismiss such semi-logic. (Roughly speaking, we may think of such semi-logic as giving tolerably usable "conclusions" as long as we do not try to use extended chains of inference. Related concepts include subjective thought, intuition, and primary-process thinking; though these terms are usually applied to the individual, and our immediate concern is with *collective* thought within the scientific community, (see Chapter A2, above). Anyhow the point is that although there is some substance to Eysenck's criticism, nevertheless there may be some *formal* justification for entertaining vague constructs like 'ego' and 'super-ego'. However the case would be much more

convincing if a more precise theory (more rigorously testable, by internal closure at least) could be formulated. The question then is, can such a plausible model be devised if we ignore the traditional constraint of the need for *experimental* testability — for the time being at least ?

One apparently valid strategy for improving the internal consistency of a theoretical system is to look for new distinctions and variables, or roughly the same type and scale of magnitude as the old ones, but with boundaries or definitions revised to accommodate the data more closely. Thus phlogiston, having been shown to be a "negative substance", was more conveniently replaced by the concept of 'the absence of oxygen'. Similarly a geo-centric planetary theory can be conveniently re-formulated as a heliocentric theory — or a relativistic theory. In other cases we may replace a linear scale by a logarithmic scale (Ellis, 1966), or rotate the axes. This then is the type of theoretical development which one might hope would follow on from any significant advances in Chapman and Chapman's proposed experimental programme, as referred to in the start of this Section. However it is far from clear that this would be likely to produce any important departure beyond our present vague concepts of mentation. Tempting though it is to hope for a Copernican or 'oxygen-versus-phlogiston' revolution, such hope is ill-founded for the complex type of "inhomogeneous" system found in biology (Elsasser, 1958); whereas in physics, "the study of homogeneous [or identifiably discrete] systems", such hope is demonstrably often justified — and the situation is also similar for elementary chemistry.

C1.2 Induction. deduction, and internal testability

But there is at least one other strategy which is also available to us. Even in Physics, where such macro-formulations as Boyle's law did make significant inroads into the subject, it was still found eminently worthwhile to re-interpret these and other phenomena in terms of (unobservable) micro-particles — hence the molecular kinetic theory. So *if* we managed to dream up a *plausible* micro-structure for *mental* phenomena, then it is not unreasonable to suppose that a significant advance in understanding might accompany this development. (To be "plausible" in this sense, a model would not need to be experimentally testable in the usual direct experimental sense — at any rate not initially, and perhaps not at all; but it *would* need to be able to be compatible with reliable evidence from all relevant disciplines — so that it fits in with the 'internal closure' of theory in general. This is by no means a 'soft option', even if it is somewhat soft*er* than the task of "observing the unobservable"; and indeed the more precisely such a micro-model can be formulated without violating internal closure on an interdisciplinary basis, the more likely it will be that we have stumbled onto *the* correct solution (*episteme*), though we can never know for sure whether we have actually done so (Popper, 1963/1969; see Part A, above). Just as the false testimony of a murderer (if explicit enough) is unlikely to stand up against logical analysis and forensic evidence (if it is thorough enough), so a highly explicit theory is unlikely to survive a thorough logical and interdisciplinary examination if it happens to be false. And this would seem to apply even if we consider only the internal closure or 'equilibration' based on previous theories of a similar status, without necessarily relying on 'objective experimental testing'; — a dispensation which is fortunate if there is, strictly speaking, no such thing as a genuinely objective experimental test, (Chapter A1, above).

It is a matter of some considerable practical importance to clarify what would actually be entailed by our "stumbling onto" a suitable micro-theory. a perfectionist strategy in which nothing but ideal solutions are accepted, and no record is kept of failures or partial successes, would take such a long time to achieve its goal that we should (if possible!) dismiss it as a workable technique. Ashby (1960) illustrates this point (in Section 11/5) with a simple example in which 1000 on/off switches have to be set to a particular combination by *random-based* testing, each test to take one second. Of the three strategies considered: ● the perfectionist all-or-nothing method would be expected to take more than 10^{301} seconds, (i.e. 2^{1000} seconds, or

142

143

$3 \cdot 5 \times 10^{291}$ centuries!); ● a serial-test of switches, holding on to the partial successes (assuming that these are manifest) would take 500 seconds; while ● a parallel-but-individual testing of all switches simultaneously would take only one second. Unfortunately the third technique will often be inoperable, so we will frequently have to be content with a technique similar to the second method; but heaven help anyone who tries to solve a complex problem using the first technique alone (or as a sizable component in a mixed strategy)!

For our present problem of developing scientific postulates which are intended to represent reality, the situation is naturally somewhat more involved. For one thing, in Ashby's example both the rules of the game and the nature of the three strategies are presented to the reader ready-made; and even if he has any difficulty in understanding them, he is not likely to see this 'personal' problem as relevant to the example. However in science there is ultimately no infallible guide as to what the rules or options might be, nor is it even indisputably clear just what the objectives are. Even supposing that the switches do operate in a *reliable* on/off manner (and in a genuine scientific enquiry there is no infallible authority to tell us whether they do or not), how are we to know for certain that these (or any) switches are relevant to the problem? They may be mixed in amongst other irrelevant switches — or switches which change the rules applying to the others, and so on. Moreover without "*relevant*" experience to guide us, might we not just as well expect that the recitation of magical formulae, or the divination of blemishes and dust-particles on the console (or anywhere else!) would in some way be predictive or instrumental in solving the problem. Of course there *are* ways of obtaining reasonably credible tentative answers to these *meta*-problems; but the point to be made here is that such answers should not merely be taken for granted.

(The above reference to confusion over the significance of dust-particles and blemishes was metaphorical, and referred to the *social* question of scientific method; but of course such confusions are encountered literally in schizophrenic patients, and as we shall see below, this is probably no mere coincidence).

C1.3 Discrete modelling of "continuous" reality

We also encounter a second complication as we move from Ashby's example to most cases of scientific research, at least in the macro-phenomena which we can "observe" (in the normal sense of the term). As Weiss (1969) puts it: "Nature presents itself to us primarily as a continuum". Even though our scientific problems may well amount ultimately to operating 'on/off' mechanisms at some micro or ultramicro level, it will probably be beyond our power to treat such mechanisms like normal switches (or perhaps even to prove or disprove their existence using normal criteria) so that we may have no option but to use gross statistical manipulations and/or measurements in practical transactions. (Moreover we cannot be certain that the basic elements of any system will really be discrete and atom-like, though it seems likely that such elements will at least be meta-stable centres of concentration within a continuum).

Despite such continuous properties manifested by the reality which we set out to study, it seems to be the case that most or all "*satisfactory*" scientific explanation needs to be expressible in discrete terms. Smooth curves, for instance, are "most satisfactory" when expressible in terms of finite series of polynomial, trigonometric, or other standard functions. Fortunately however, natural phenomena are generally "well behaved" in this respect — exhibiting properties which *can*, in principle, be simulated extensively at the observable level by an appropriate choice of discrete elements at the micro-level in the model, whether or not these correspond to the *actual* micro-structure of "the real system".

Alchemy was a mystic *art* which, as it developed into Chemistry, acquired some semi-satisfy-ing explanations when its regularities at the observable level were summarized *approximately* by

Mendeleev's periodic table; but adequate "understanding" had to wait for the atomic theory — to provide a micro-structural basis. As a surprising contrast, the explanation of gravity has rested at the observable-scale of formulation (despite Newton's own misgivings!), presumably because the inverse-square formula is so reliable here that we become conditioned not to press too hard for a "proper" explanation. And the same applies even more startlingly to that extraordinary formulation of our own century: the wave-particle dualism concept which is expressed in strange abstract formulae. This formulation has produced spectacular results like atomic energy, to be sure, but no satisfactory "explanation" in the sense of fundamental "understanding". (Popper, 1963).

It would probably be fair to say that the alchemists were slow to produce proper understanding because they lacked

(i) an adequately systematic methodological approach, and

(ii) an adequate feel for the underlying micro-structure which might conceivably exist within their chemicals;

and the same might be said of Medicine before Pasteur and Koch. In both cases, this shortcoming may be seen as a failure to conceptualize adequately in discrete terms — indeed we may suspect that such a rudderless groping for structure is a nearly-inevitable precursor in any new field of enquiry. In such an environment, superstition is likely to surround the particular area of ignorance — perhaps despite general enlightenment on other topics. That is to say some sort of ad hoc structure is likely to creep into unstated assumptions, as a means towards papering over the nagging cognitive dissonance. While such ad hoc structures remain unrecognized as being only provisional, they are likely to form a serious obstacle to the elucidation of truth however valuable they may otherwise be as "stop-gap" theories.

But note that this danger of 'getting stuck with' a misleading model of reality (without realizing what has happened), is not confined to the models we construct of chemicals or neural elements; the danger also applies to the methodological rules which we use to constrain ourselves from using hopeless strategies like Ashby's first example. But these methodological rules are not infallible, and they too can serve as superstitious obstructions to truth-seeking — even if they simultaneously prevent us from setting up other obstructions lower down the hierarchy. In fact it seems likely that the "methodological superstitions" would be the more insidious in that they are less likely to be noticed for what they are. If this *is* the case, then we would do well to spend some effort to re-check the status and credentials of our rules for Scientific Method from time to time, or perhaps even relax them occasionally in the spirit of (controlled and monitored) adventure. It might even be fair to say that this is the chief value of responsible *clinical* evidence, leading to such fresh insights as those of Freud, Laing, Tinbergen and Konrad Lorenz, and the 'diary'-observations of Jean Piaget; so those who have criticised such work on the basis of 'laboratory criteria' have not necessarily helped the cause of science as much as they might suppose.

This of course brings us back to the point (raised in the middle of Section C1.1) that the current doctrine favouring direct-testability is open to criticism. To elaborate a little on the earlier comments: it seems that, in practice, this doctrine means that only those novel theories which can be 'fully developed' within the space of one paper (or perhaps one book) are likely to be taken seriously; (where 'fully developed' means concluded to the point that decisive experimental tests are feasible). Not only does this tend to exclude useful heuristic developments, but it also tends to rule out those theories which cannot be 'fully developed' within the arbitrary limits imposed by the single paper (or book) format, no matter how well the theory fits the alternative criterion of internal consistency (Traill, 1976c). This may therefore be taken as an argument in favour of *altering the rule* so as to include the internal consistency criterion — *and* for keeping such rules under surveillance generally.

C1.4 Apparently-continuous modelling of apparently-continuous reality: "Scientific Impressionism"

Let us first consider the implications of apparent-continuity in the "reality" under study. One approach is to adopt the Newtonian technique of discrete-symbolism-for-continuous-phenomena and hope that reality does match the models provided by integral and differential calculus. Any success here will automatically provide empirical predictive power; but it is open to question whether this really contributes to our fundamental *understanding*.

Alternatively we may do our modelling stochastically, using a large population of micro elements or events, such that a statistical simulation of macro-reality may be hoped for, whether or not we wish to claim any validity or homomorphism for these *micro*-aspects of our model. For instance, suppose that we had no clear concept for a circle or how to reproduce one; how then could we approach the problem of communicating the concept between us? Well, we could say: "it's a bit like a square (in all its orientations), a bit like a pentagon, …". If we were then to superimpose line-drawings of all these approximations, this would ultimately result in a tolerably identifiable *impression* of a circle.

It is here suggested that our concepts of ego, superego, schizophrenia, and indeed most of the concepts used in the psychological sciences, are of this impressionistic type. Similarly we might perhaps say that the various alternative theories of psychoanalysis, used pluralistically by clinicians, are analogous to the square and pentagon (etc.) of our above example. Thus none of them would be strictly correct, yet a judicious choice might result in the clinical use of a particular approximation which is optimally suited to the particular patient at a particular time. And a suitably mixed "superposition" of these theories might sometimes give a good overall "feel" for the real phenomenon as a whole.

We do ourselves less than justice if we decry the value of such "unscientific" techniques (which are significantly similar to 'primary-process thinking'), but neither should we be content to rest on our laurels. If there is any prospect of building a model which is sharper than our present impressionistic one, a model capable of incorporating the various "straight-line figure" approximations into one improved "circle" formulation, then it would seem prudent to try. (Planck's reconciliation of Wien's law and the Rayleigh-Jeans law is perhaps a more pertinent example, (Slater, 1955, Chapter 3)). If such a formulation cannot at present be supported by evidence or immediately forseeable experimentation, then this is unfortunate — but not catastrophic so long as we do not forget its *tentative* status, and so long as it shows 'internal closure' within its interdisciplinary implications (like its close cousin 'secondary process thinking').

It was to this end that an unobserved ultra-micro structure was postulated for the Piagetian impressionistic concepts of "scheme" and "schema" (Part A, above); and also further postulates were introduced to improve the interdisciplinary internal-closure by removing some more "somehow"'s from the general formulation, (Part B, above; and Traill, 1976b). It is now proposed to apply these concepts to more general activities of the human brain, both in the hope that the results will find practical application, and as an effort towards a further extension of interdisciplinary internal closure.

Chapter C2

A Brief Review of the Neo-Piagetian Linear-Molecule Theory of Brain-Function

SUMMARY OF CHAPTER

This chapter serves mainly to draw together the main ideas put forward in the present work so far, in preparation for their impending use in postulating the more elaborate structures and processes which are presumed to give rise to behavioural phenomena.

We start by reviewing the essential points of the "linear micro-element" basis postulated for memory, which differs from the pure neuron-synaptic view in the much greater information content which it permits, and in its ready-made organization into linear sequences — and yet offers a new basis both for precision and for flexibility. We also recall the notions of how each individual may reconstruct reasonably faithful models of basic reality by seeking grouplike properties amongst his elementary sensori-motor concepts — seen as physical encodings. This leads to the question of elementary "symbolic" thought (without full external action), and a likely role for sleep.

In addition the question of "levels" of mental organization is brought up again, and formalized a little more by the introduction of the "$\mathbf{m^n l}$" notation.

C2.1 The fundamental physical basis

The essential point is that synaptic changes are no longer seen as all-important elements for memory or for hereditary reflexes, but are augmented or even replaced by supposed coding activity at the molecular level. Molecular coding is thought of as a linear sequence of coded sites — as in DNA, RNA, or linear protein — which are set up in such a way that they can be *rapidly* "read out", thus activating other such molecules or triggering specific items of motor activity. Individual molecules would probably not be capable of producing manifest observable activity on their own; but could only be effective if and when they found "agreement" from a sufficient *population* of similar molecules, with which they would also have to have some sort of coordination mechanism — probably along the lines of Monod and Jacob's biochemical coordination model, "Model IV" (1961).   (Traill, 1975b, 1976d).

Sites along the molecular strings were seen as falling into two main types and a subsidiary type. The *label* type would await appropriately coded signals from outside the molecule before they would do anything, and in general they would probably also need to have been primed by an internal signal within the molecule before they would start to "listen" for the external messages; so they would normally serve as 'and'-gates thus: "Have I recently received a nudge from my neighbouring site, *and* have I the specific go-ahead from outside? If so, then I must nudge my other neighbour to set him going in a similar way." Phonon or exciton transmission seemed to offer a sufficiently rapid method for relaying the "nudges", and the external messages were seen as emission- and absorption radiation, which implies infra-red for macromolecules of the type envisaged. (See Chapter B2, above).

Whereas the first main type of site served as a tuned[28] receiver for specific external signals, the second was seen as a *transmitter* of such signals (along with the sense-organs, via their proximal representatives). The second type would also normally pass on the "nudge" without question. The transmitted signals were seen as having various functions: motor, controlling

---

28 It should not be imagined that this is a simple tuning in the sense of a fixed-frequency sensitivity like a radio morse-receiver. It will instead, almost certainly depend on *patterns* of signal input which will be more like ordinary (complex) speech waves, highly dependent on sequence (like a combination lock), and rather less definable in terms of sinusoidal functions. (See Chapter B2 above, and again later on in Section C6.7).

internal activities such as sense-organ parameters and aspects of attention, "subprogramming" calls to other relevant types of molecular coding, and synchronization of the population of similar molecular codings. (Whether or not this account is correct, it does at least bring into the open the question of explaining just how chemical memory-traces and electrical signals might conceivably interact in detail).

The concept of infra-red signals being transmitted across appreciable distances leads to some troublesome problems due to the formidable absorption characteristics for water in this region of the spectrum. One conceivable solution would be for the infra-red coding to become "translated" into and out of orthodox action-potential coding, though it is by no means clear how this might be achieved on the basis of conventional theory, and it seems inevitable that huge losses of information would be entailed by such a procedure. An alternative suggestion (Traill, 1975b, 1976b; also Chapters A3 and B3, above) arises out of a close analysis of the physics-theory of saltatory conduction of electromagnetic signals along myelinated nerve-fibre segments.   It emerges from this study that such dielectric pathways would be eminently suitable for conducting infra-red along the distances envisaged, *provided that* a moderate amount of protection from the absorption effects of water is available. It would seem that there are several plausible ways in which this moderate protection could be provided, and moreover the idea can plausibly be reconciled with action-potential phenomena by considering the latter as massed effects in which infrared-scale pulses or waves are an important component. The infra-red hypothesis may therefore be provisionally accepted on the basis of its internal consistency (see Chapter C1), pending external evidence more convincing than the fragments currently available.

C2.2 Learning and the sensori-motor period

As for the learning process, it is remotely conceivable that experience is "recorded" on such linear molecules in the manner of a tape-recording. But until some plausible mechanism is suggested, we would do better to take Piaget's concept of *action first, then learning* to its logical conclusion and suppose that *all learning* must first await spontaneous elaboration or "genetic crossover" at the molecular level — on a trial-and-error evolutionary mutation basis; (Traill 1976b; also Parts a and B, above). Any such embryonic idea which appears to be adaptive will then be "reinforced", while others will be re-dissolved or mutated further. Just how such reinforcement might take place is a matter of some importance, and two possible mechanisms have been offered, though so far even the internal-closure support for them (in the public-knowledge domain) leaves much to be desired. Provisionally however, we may accept them both as working hypotheses.

The first mechanism is seen as a rather abstractly described seeking-after-internal-consist-ency, or a trend toward "mathematical groups"; (Traill, 1975b; and Chapter A3, above). This means a striving towards '*internal closure*', so its successes-or-failures appear to entail their own "pleasure-or-pain" in what is usually taken as a metaphorical sense, but is here taken rather more literally.

The second mechanism involves normal concepts of pleasure and pain; and thus, in the main, serves the cause of '*external closure*' — i.e. keeping the mental models more consistent with the reality outside. For this, we suppose that there is a supplementary type of molecular site which can become (reversibly?) appended to the molecule according to the 'satisfaction state' of the organism at about the time that the particular molecule was last 'read'. Those molecules with a 'positive' tag would tend to be immune from attack and/or be eligible for replication, while a 'negative' tag would amount to an "outlaw"-label; (Traill, 1975b). These tags might also be involved in subjective feelings of pleasure or pain — or alternatively there might be *other* similar tags for this purpose, which would normally operate in parallel, but could fall out of line under some circumstances thus producing anomalous effects.

Linear codings of this sort are seen as accumulating sufficiently for them to produce observable behavioural effects, in which case they would appear to coincide, collectively, with Piaget's concept of a "scheme" — either hereditary or learned. They may also be closely related to the concept of Short Term Memory, though that is of less immediate relevance. With continuing use, such an ensemble appears to acquire stability and permanence for reasons which are not entirely clear, though increasing localization and the concentrations of mutually-supportive cross-feed (from similar elements within a closely-knit population) do offer one likely explanation; (see Section A3.8, above). Anyhow, such a stabilized and quasi-permanent scheme is referred to as a "schema" (plural "schemata") following Piaget's terminology, even though his usage is confined to what we shall regard as a *similar consolidation* during the following "Operational Period" (Furth, 1969). Here we shall use the same term for both periods, and also for the two sub-periods within each, for reasons which should soon become apparent. (For both these periods, and others, it is tempting to relate the schema concept to the notion of *Long* Term Memory; but as before, there is no need to labour the analogy here).

Mental development of the first type (the one seeking internal group-structure) depends on some degree of lawfulness or consistency in the external environment which it is trying to model. Similarly we might expect that any "introspectively directed" attempts to classify or control one's own thought-elements would be doomed to failure until such time as these thought elements showed some potentially-discernible pattern. With the consolidation of a sufficient number of the primitive schemata (such as: "Given situation-perception a, and desire-state (c), then the thing to do seems to be action b"), then it becomes feasible to classify such schemata into *sets* — and ultimately into *mathematical groups* (Traill, 1975a; and Chapter A3, above).

155

C2.3 Internal reference 'by name': sleep, mathematical sets, and the "m^{n}l" hierarchy; implications for decision-making behaviour

Given a substantial repertoire of pre-coded patterns of activity, including integration arrangements between them similar to 'subprogramming', then it would only take some minor modification to the key 'call-sign' coding (which would normally activate the ensemble) to *suspend* the actual execution of the action implicit in the ensemble. Such a modification serves to convert the 'call-sign' into a detached 'name' which is then available for use as an abstract symbol for the action which has now been deferred; and this is accordingly a likely material basis for a primitive mental 'concept'.

It is of some importance to consider the nature and implications of such suspensions of execution. Presumably they must often be convertible so that 'at the flick of a switch' certain abstract thoughts can be converted into the real action to which they correspond; but if so, then we would do well to have some idea about how such 'switches' might be controlled. Without going too deeply into this question, we might list several possibilities:- a comparatively minor *modification to the call-sign* might distinguish the 'symbolic' from the 'real', (and such a modification might be analogous to a grammatical change of vowel as an indicator of tense). Alternatively '*tags*' might well be involved in the modification of the actual encodings (as opposed to the call-signs which are presumed to activate them).

Then again *sleep* would almost certainly be involved in some way, possibly by changing the *overall* criterion as to what is to be executed and what is to remain symbolic and abstract. This raises some intriguing and complex possibilities relating to the different modes of sleep — and perhaps also various drug-induced states. Consider for instance: (a) the suppression of general muscle tonus during REM sleep, (b) the increased incidence of REM sleep with position in the phylogenetic scale, and (c) its dominance (to the exclusion of "orthodox" sleep) for neonate mammals, (Jouvet, 1967). We will come back to some of these issues in Section C6.4, and again

156

subsequently as part of the discussion of the psychotic symptoms arising from sleep deprivation, (Section C8.4).

Anyhow, the development of name-codings[29] for stable primitive action patterns now makes it feasible for 'higher-order' scheme-elements to evolve. These may be thought of as linear *lists* containing 'name-codings' for lower-order schemata (and hereditary schemes).

This new development constitutes the first step up an extended hierarchical ladder, so it would be well to introduce a naming-system for the various levels. What seems here (rather naively as we shall see) to be the basic level containing the elementary schemes of sensori-motor development — will be referred to as the "m^0l" level. (The terminology here is intended to be roughly compatible with that of mathematico-logic/linguistics, in which *L* represents the basic "language", *ML* represents the "meta-language" within which **L** may be discussed, *MML* stands for the "meta-meta-language", and so on. In fact though, this traditional notation will be seen to correspond a little more closely to the somewhat broader categories inherent in the "M^nL" notation, which will be introduced below in Section C3.2). The *new type* of scheme-element which we have just introduced as "lists" for the basic elements will be referred to as being at the "$m^{1/2}l$" (sub)-level, where the fraction conveniently implies a qualitative difference from both m^0l and m^1l (which roughly corresponds to the traditional "**ML**"); and of course the fraction also implies the postulated position for $m^{1/2}l$ as being *between* m^0l and m^1l.

The setting up of such $m^{1/2}l$ lists or *mathematical sets* might plausibly take place by a process such as the following[30]. Firstly we need some mechanism for producing a "blank note-pad" on which the acquired lists can be "written"; where we may assume that these "note-pads" are linear scheme-elements similar in principle to those of m^0l, but containing internally-directed references to the m^0l schemata or hereditary schemes rather than to externally-directed actions.   Such blank scheme-elements might be provided ready-made as part of the hereditary legacy, or they might be the result of arbitrary accretions combined with a selection process, and/or they might be produced by *converting* some of the m^0l elements either by 'inactivating' their references into 'names' — or by some sort of (gradual?) replacement of sites along the linear string.

So suppose that, by some such means or other, we now have blank or "inadequately coded" $m^{1/2}l$ linear elements available in sufficient numbers; how could these be developed into usable mathematical-set concepts such as "the set of all schemata which seem to lead to my hunger being alleviated (given initial conditions 'so-and-so')"? In view of the postulated mechanism for the m^0l scheme elements, it seems likely that the $m^{1/2}l$ elements will similarly contain some sort of coded *expectation* about the properties of its listed 'members'; and such an expectation would amount to an intensive[31] definition of the set. Conceivably this expectation might simply be implicit in the existing list, at any given instant, as some sort of consensus property. But pending a satisfactory explanation of how such a system might work in detail, we may consider the alternative that the $m^{1/2}l$ elements contain an explicit coding of expectation — albeit arbitrary. (This might result, for instance, from the 'promotion' of m^0l elements — as these will presumably already contain a coding for consummatory expectation, even if this is not qualitatively appropriate as it stands). It is not clear just how many members there could reasonably be on a

Linear-molecule theory — C2.3 Internal Ref — m^nl

---

[29] These should *not* be thought of as being linguistic names in any literal sense; indeed we have so far been discussing pre-linguistic infants, in their first year

[30] More detailed explanations will be given in Section C5.2 and in Section C8.1, but this present generalized account will suffice for our immediate purposes.

[31] In Piagetian usage, an *intensive* definition is a *description* of set members against which any potential member can be assessed to determine whether it is actually allowable as a member or not; whereas an *extensive* definition amounts to saying "all objects within this boundary" (or "all phenomena within this time-interval") etc.

given list element, though it is tempting to see a connection with the concepts of 'chunking' and 'the magic number *7 ± 2*' (Miller, 1956a, 1956b); but the effective list at *behavioural* level might gain extra scope using a *population* of *less complete* elementary lists, used redundantly.

But given that these $\mathbf{m}^{\frac{1}{2}}\mathbf{l}$ elements have some coded "expectation" concerning any members to be listed on it; how could it come about that the members *are in fact* of the type "expected"? (I.e., how is it that the "extensive definitions" happen to coincide with the "intensive definitions"?). At first sight, one might expect the list to "search" (somehow) for suitable members whose 'names' might then be added onto itself; but this is another case of an anthropomorphic model which we would do well to set aside until a detailed explanation of it, in mechanistic terms, should happen to be forthcoming.

Meanwhile let us suppose that 'names' of members are added to the list more-or-less at random, and that it is then the job of 'the system' to *correct the resulting errors-of-commission*. It would not be too difficult then, to imagine some sort of automatic rehearsal procedure (perhaps during one of the sleep-modes) in which the coded "expectation" for the list was to be compared with the relevant properties of the named members. Whenever there was a mis-match we might suppose that the whole list might be liquidated, or else all 'names' could be erased from the encoding, or perhaps only the name of the offending member would be erased; (these alternatives being in order of ascending efficiency). Once again it is worth commenting that the huge redundancy possibilities which result from the present molecule-based model, make it feasible to contemplate such apparently inefficient mechanisms and to use them as an alternative to anthropomorphic or teleological "ghosts in the machine".

One consequence of this development of set-constructing ability is likely to be the capacity for using elementary strategies for problem solving. Consider the general situation (Siegel *et al.*, 1964; Wetherick, 1977) in which the individual must choose (repeatedly) between two alternatives which we will call *A* and *B*. If *A* happens to be the appropriate choice in, say, 75% of choices (while *B* is correct for the remaining 25%), then the "rational" strategy would be for the Subject *always* to choose *A* — that is once he has had enough experience of the situation to appreciate the nature of the random factors involved. However this "rationality" requires a comparatively sophisticated mental organization, so some animals are unable to operate at this level; and those that can, such as humans, will not necessarily do so under the given circumstances — for one reason or another.

In the absence of this rational strategy, the individual seems to use the less efficient technique of frequency-matching; that is to say he ends up selecting *A* for about 75% of his choices, distributed in an approximately-random fashion — thus matching the frequency inherent in the phenomenon itself. In terms of our present molecular model, this can be explained in terms of the degrees of reinforcement for the two types of action and their respective molecular encodings. Those coded elements instrumental in the choice of a will receive more support, and will consequently come to be represented by a larger population of molecular encodings, while the pro-*B* elements will also receive modest amounts of support and will thus build up their own relatively-modest population. In any actual choice then, the two populations will presumably compete to provide the effective program for action; and it will literally be "the luck of the draw" as to which one will succeed in any given instance. But note the important point: *no special set-organization appears to be necessary* for this process.

Fish apparently operate at this primitive level, but rats are capable of evolving toward the "rational" strategy of always choosing that single alternative which has the best average payoff (Wetherick 1977, citing Bitterman), and this superior capability seems likely to be due to an ability "to envisage something that is not present in the immediate perceptual environment". But for the rat to be able to realize on this inherent ability, it needs to be "allowed to approach the

correct discriminandum even if it chose the wrong one initially" — and it would seem that some sort of set-organizing mechanism must be required if these *two* exploratory probes are to have their information *integrated into one* usable scheme, this being some sort of an appreciation of the selection situation as a whole rather than the mere blind impulse that a certain response should be made. Anyhow it would seem likely that Wetherick's *second level* corresponds to the $m^{1/2}l$ level proposed here, and indeed his *third level* which "involves ability to identify differences and similarities between present states of the environment and past states ... [and hence relates to] language" seems to be identifiable with the m^1l level and its presumed power to manipulate the m^0l and $m^{1/2}l$ elements (as discussed below).

Note that even human subjects may be caught out using the primitive frequency-matching strategy which we have just considered (Siegel *et al.*, 1964), though here the result is bound to be more difficult to interpret because of the confounding influence of the third level (m^1l), and probably other levels as well — as we shall see. However one plausible explanation for Siegel's results is that the subjects used the primitive strategy because something in the experimenter's instructions, or the cultural expectations implicit in the situation, led them to believe that this was what was required of them. Thus possibly they operated under active (m^1l ?) schemata, which served to *suppress* any relevant $m^{1/2}l$ activity, so that the primitive m^0l-based "lottery"-approach was left as the one which actually operated on the problem.

The notion of a negative instance raises some complex issues which we will not go into fully here, but we should bear in mind that a concept and its specific negation actually have much in common (Freud, 1900). This suggests the need for a set-like structure to link them both, and probably an extensively defined set-structure at that — though perhaps this would take time to evolve itself. Similar things might also be said about conjunctive concepts (Bruner, Goodnow, and Austin, 1956), and it is likely that their reported increased difficulty with *disjunctive* concepts arises due to a need to call on further organization higher up the m^nl scale.

Anyhow provided that extensively defined mathematical sets (or "lists") can be established for any conceivable common-criterion (intensive definition), then it becomes feasible to explain a possible mechanism for the mental development and retention of "mathematical groups", or more normally — group-*like* concept-structures. We may now turn to this topic and elaborate on the ideas expounded in Chapter A3 (above), and illustrated in Traill (1975b).

C2.4 Trend toward mathematical groups in the later sensori-motor period

This turns out to be a matter of formidable complexity if we try to trace the probable course of real-life developments in any detail. For our immediate purpose it will suffice if we look only at a few simplified or idealized cases; but it should be born in mind that a real brain system must cope with the complexity as it actually occurs, thus making heavy demands on its structural capabilities and redundancy reserves. Anyone attempting to build a computer model to simulate such activities would also do well to ponder the "hardware"-implications quantitatively before becoming too involved.

The most convenient example, already discussed in some detail (Chapter A3, above; Traill, 1975b), is the geometrical-spatial task of learning how to find one's way around a (topological) square in which the "corners" correspond to recognizable and distinguishable events or phenomena, and at least one of these is inherently rewarding — and so worth trying to attain. Voluntary reversibility was seen as arising as a consequence of a change of the individual's "drive state" (e.g. from hunger to thirst) in such a way that a *different* corner of the square became attractive. In this way, all the appropriate m^0l schemata could develop, thus enabling the $m^{1/2}l$ "lists" (relevant to transactions with the square) to achieve exhaustive completeness — or something like it.

Linear-molecule theory — C2.4 → mathematical groups

But there are many ways of conceptualizing a Euclidean square, and likewise there are many ways of viewing other similar "topological quadrilaterals", both literal and metaphorical; and the same argument applies in principle to the learning task for each of them. Even if we look no further than the "corners", it will be of practical value to distinguish different subsets taken from the complete set of possible moves between one corner and another. We may usefully keep a list of schemata for "diagonal moves" (only), or for "clockwise moves", or for "routes towards corner *x*", etc.[32].

Yet there is still another, less obvious, complication. Our view of an *observed* square as having group properties is partly conventional[33]. Consider what happens when we have to scan the "corners" consecutively *and* we also judge each corner as having an absolute time-based property, then we can never return to precisely the same corner again (though we might be prepared to accept "an approximation", more-or-less consciously). Thus, depending on our criteria, a conventional group-like thing will not always be perceived as a group.

So it would seem that even the "ideal" group-like nature of rigid geometrical bodies is, to some extent, just a construct of the mind — made for the sake of pragmatic convenience. But in any case, for other more "fuzzy" macro-manifestations of structure such as *Mr X's personality*, it becomes quite obvious that any group properties which we might attribute to such "things" will always fall short of perfection; (and this will apply to our social "scientific knowledge" models, as well as to our concepts as individuals). In fact this presumed strategy of "aiming at groupness, but being prepared to settle for the best available approximation" seems to work very well on the whole. Errors will often be irrelevant for practical purposes (even if their existence comes to be recognized), or they may simply be corrected as the occasion demands. There may however be problems when one such partially-correct grouplike model becomes enmeshed in another, and when the resulting structure has become instrumental in maintaining the stability of the individual or the society. Thus trivially-inappropriate associations may give rise to neuroses or (socially) a Galileo may have trouble correcting a mistaken cosmology when it becomes enmeshed with religious doctrine due to historical and political reasons rather than "rational" reasons.

original page numbers: 163, 164

---

[32] Moreover we will probably need a *list of these lists* if the individual is to keep track of the fact that the subset lists are fundamentally related to each other — unless this could be done in some other way such as overlapping memberships see Section C8.1. Then there may be some value in associating these schemata with those for the concepts of "rectilinearity" and/or "perpendicularity" — almost certainly more difficult and sophisticated than would appear at first glance. Next there might be concepts for "midpoint of side" or "middle of square", and so on — though *not* indefinitely. Thus, for instance, there would normally be little point in developing a (non-numerical) schema for a concept which amounted to "17.87% of the way along a side".

[33] Such a view is based on pragmatic grounds which are suggestive of the truth, but do not absolutely prove it. (This is a "hair-splitting" distinction in the present example, but in general it will *not* be).

Chapter C3

Abstractions, or Thoughts about Thought-Constructs: Is the Process Recursive ?

SUMMARY OF CHAPTER

Within the discussion of mind-brain organization, this chapter introduces the mathematical concept of recursion, i.e. the re-using of the same basic organizational procedure repeatedly (but at different levels), so as to build up a hierarchical organizational structure while also allowing simplicity of design and standardization of basic resource units. This is seen as a possible explanation for Piaget's stages, and it also seems to fit in with Ashby's ideas of double-or-multiple feedback loops.

C3.1 *The concept of mathematical recursion, and some variations on this theme*

As an explanation of the recursion concept, consider the mathematical function "factorial n" (written as "n!"), where

$$[0! \ = \ 1],$$
$$1! \ = \ 1,$$
$$2! \ = \ 2 \times 1,$$
$$3! \ = \ 3 \times 2 \times 1,$$
$$4! \ = \ 4 \times 3 \times 2 \times 1,$$
$$. \ . \ . \ . \ . \ . \ . \ . \ . \ .$$
$$n! \ = \ n \times (n\text{-}1) \times (n\text{-}2) \times \ … \ \times 2 \times 1.$$

A function such as this can readily be specified by a recursive algorithm, which in this case would be:-

(A) Take the value of *n* and 'write it down' in some accessible place called 'memory' (specific to *this particular* entry into the algorithm, see below).

(B) Make sure that *n* is neither negative nor fractional; otherwise give up and signal that an error has occurred.

(C) 'Write' "1" into another position called 'answer' (which must also be specific to this particular recursive "level").

(D) Test whether 'memory' now contains "0", and hence choose whether to do E or F:-

(E) If *n*, the number in 'memory', has a value of *1* or more, then multiply the 'answer' by this number, and also by *(n-1)!* — which will entail a *recursive* repetition of the whole procedure "at a different level of operation" (with a new value for *n*), and there will ultimately be many such levels whenever there are high values of the original *n*. When this task has been done, the next task is determined by 'exiting' back to wherever the immediate request for a factorial evaluation came from — whether within the same recursive series or not.

(F) If the *n*-value in 'memory' is zero, then accept 'answer' in its present form (= 1) as being the result for *this* recursive level, and pass this result back to wherever the immediate request for a factorial evaluation came from (either the "abstract" previous level of recursion, or the original "real-life" problem), and also return to this same place to find the next instruction.

Equivalently, this algorithm may be written in ALGOL:-

integer procedure factorial(n); **value** n; **integer** n;

begin

    integer answer;   **comment** The "integer n" sets up a new memory-location for each successive recursive entry into this subroutine, and stores the nominated value in it ready for use. The answer location is also set aside, but with no reliable pre-set value;

    if n<0 **then goto** ERROR; **comment** n has already been made an integer;

    if n=0 **then goto** F;

    E: answer:= n * factorial(n-1); **comment** recursive call;

        factorial:=answer;

        goto EXIT;

    F: factorial:=l; **comment** Basic simple case, no (further) recursion;

        goto EXIT;

    EXIT:

end of factorial subroutine (this time round);

The details of this particular example are not important here, and indeed their relevance is somewhat limited; but there are nevertheless several useful concepts to be drawn from the example, or from others like it (Barron, 1968). (a) Firstly the method may be seen as a "trick" whereby a complex operation, of unpredictable magnitude, can be carried out by a compact repertoire of not-very sophisticated actions: simple multiplication, subtract 1, identify 0 and error conditions, keep basic records, transfer results, and the ability to "pass the buck recursively". It has been suggested above (in Section A3.11) that a similar sort of recursion between levels may be operating in connection with Piaget's stages of development; though here the limited repertoire of available actions would include those involved in *set* and *group* construction, and also the ability to "handle" such constructs — or at least those produced at lower $\mathbf{m^n l}$ levels. It is not suggested that the brain can necessarily cope with a denumerably infinite number of levels, as *seems* to be the case with the factorial-function example. In both cases there will necessarily be a practical limit to recursive processes; though in the case of the brain it is not yet clear how such limitations would be likely to operate. It will perhaps be evident from the system proposed above in Section C2 that an important difference between it and the "factorial" algorithm lies in the method of "passing the buck" to the next level. In the brain-model, the lower level is "at a loss" and is only rescued if-and-when the upper $\mathbf{m^n l}$ "interferes" arbitrarily in the right way.

(b) In both cases there is an indispensable "ground-floor" level which comes closest to being in contact with the "real world outside" — on an actual transactional basis. In the factorial-algorithm, this level is the one which actually performs the final stage of the calculation and passes the result back for the benefit of the computer program in which it is embedded — this being its "outside world". In the case of the brain-model, as we have seen in Chapter C2, the lowest level was postulated as being $\mathbf{m^0 l}$ — the basic part of the sensori-motor double level. (Later on, in Section C5.4, it will be suggested that the basic level is actually lower than this — at an "$\mathbf{m^{-1} l}$" or "$\mathbf{M^{-1} L}$" level).

(c) In both cases there is also, in some sense, an upper level. Obviously some degree of flexibility is called for here, so it may not always be clear where to locate this "top floor" when the system is diffuse — as envisaged in the brain model. In the "factorial" case, the "top" will always consist of a mere allocation of the answer "1" (in response to the problem: "evaluate *0!*");

167

168

but in view of the postulated *downward* direction of initiative-taking for the biological brain, the significance of the current top level will presumably be radically different for the two cases. In fact it has been tentatively suggested (Traill, 1976d) that the top level may be intimately involved in the phenomenon of *consciousness* for the case of the brain model; and after all, the "top" is seen as a unique source of initiatives which exert decisive control over some activities of the other levels.

(d) a purely recursive process may be thought of as re-using the *same material mechanisms* for each successive level of its "calculation", as is clearly implied in the above ALGOL example. However it would be perfectly possible to re-write such a subroutine such that different mechanisms are used for different levels. For instance it would not be unreasonable to treat 0!, 1!, 2!, 3!, and 4! (say) as special cases in which it is, on the whole, easier to simply "remember" the answer ready-made (as being 1, 1, 2, 6, and 24, respectively). The calculation for higher values would then terminate with the "evaluation" of *4!* as being at the top of the hierarchy instead of that for 0! as was used above in the pure case. This sort of change in methodological procedure according to hierarchical position or complexity is fairly common in a non-computer daily context, and may perhaps be illustrated below by the second way of writing out the following formula for an infinite series:-

$$e = \frac{1}{0!} + \frac{1}{1!} + \frac{1}{2!} + \frac{1}{3!} + \frac{1}{4!} + \frac{1}{5!} + \dots$$

$$e = 1 + 1 + \frac{1}{2} + \frac{1}{3!} + \frac{1}{4!} + \frac{1}{5!} + \dots$$

And moreover it should be recognized that there are practical advantages in such a corruption of the purity of the formula; — though there are disadvantages as well. Ultimately the "efficacy" of such a mixed methodology must be judged according to the context of its likely use.

In the brain-model as described so far, we have considered two supposedly separate levels (active within the sensori-motor period): $\mathbf{m^0 l}$ and $\mathbf{m^{\frac{1}{2}} l}$ — the first dealing with internal codings for direct primitive interactions with the outside world, and the second dealing with sets and groups of the former. It would not be unduly surprising lf these two levels of activity were to find their physical embodiments in linear elements which were strictly allocated to the one *or* the other, and not freely available to both — in the short term at least. Nevertheless, it would seem likely that these two types of coding should be spatially close together for easy communication and control; and being *populations* of elements, this would presumably mean that the two types of element would be intermixed within whatever regions of the brain happen to be involved.

C3.2 Estimating the likely scope and nature of recursion in the brain

It was suggested previously (in Chapter A3) that consecutive recursive levels correspond to the *sensori-motor*, *concrete operations*, and *formal operations* periods respectively. It was also suggested that each of these levels has a two-tier subdivision; so that in each case, the lower tier was concerned with the relevant type of "primitive" element (as for the $\mathbf{m^0 l}$ level) while the upper tier was engaged in forming sets and groups (as for the $\mathbf{m^{\frac{1}{2}} l}$ level). Logically then, there would seem to be at least six such sublevels in use in adults: $\mathbf{m^0 l}$-and-$\mathbf{m^{\frac{1}{2}} l}$ (sensori-motor); $\mathbf{m^1 l}$-and-$\mathbf{m^{1\frac{1}{2}} l}$ ("concrete operational" in the wider sense, comprising "pre-operational" and the narrower sense of "concrete operations", respectively); then $\mathbf{m^2 l}$ and $\mathbf{m^{2\frac{1}{2}} l}$ (for formal operations).

169

170

This postulated hierarchical series raises several questions as to whether recursion is really involved, and if so, then in what form. There is no immediately available means for supplying a confident answer to such questions, either from experimental data, *or* from 'internal closure'. But if our general approach is correct then it will become a matter of considerable interest and importance to clarify such matters; mainly because many *malfunctions* are likely to be selective as to the physical mechanisms which they will affect, so that any mechanism which is shared by two or more levels is likely to cause *multiple symptoms* whenever it fails to work correctly.

For the sake of having some definite ideas to criticise, the following suggestions are offered as tentative working hypotheses. Any evidence for these postulates is no more than impressionistic; but the exercise should at least help us to a clearer concrete view of the problem, and it could plausibly lead us fortuitously into a model which turns out to offer unsuspected internal closure.

Let us first take the three double-levels as units and consider the relationships between neighbours. Now, according to our earlier tentative postulate, the $\mathbf{m^0l}$ and $\mathbf{m^{\frac{1}{2}}l}$ combined-level will collectively be the one most directly concerned with interactions with the outside world (whereas the others are supposed to confine their control activities to internal entities). Also it is tempting to locate the $\mathbf{m^0l/m^{\frac{1}{2}}l}$ elements within the phylogenetically older parts of the brain, and the higher double-level elements within the neocortex (which, roughly speaking, starts to appear in animals which are capable of transcending sensori-motor limitations (Diamond and Hall, 1969). Anyhow on the basis of such impressionistic leads, let us postulate that (i) the $\mathbf{m^1l/m^{1\frac{1}{2}}l}$ double-level (i.e. $\mathbf{M^1L}$)[34] shares a common material substrate with the $\mathbf{m^2l/m^{2\frac{1}{2}}l}$ (or $\mathbf{M^2L}$) double-level, but that (ii) the more basic and primitive $\mathbf{m^0l/m^{\frac{1}{2}}l}$ (or $\mathbf{M^0L}$) double-level has its own separate substrate, thus disturbing the "purity" of the supposed recursive activity. However even if the substrate *is* different, this does not of itself mean that the basic mechanisms of the different substrates will necessarily be different though of course that is a possibility; and in any case, drugs and other disturbing agents might well affect these substrates differently.

But this picture may well be too simple. Bearing in mind that we are dealing with a "parallel processor" (unlike the strictly sequential activities of a digital computer), and considering too that the initiative is supposed to come from the "top", it is quite plausible to suppose that there may be several *alternative* substrates for any given level. We need not go into the ramifications of this here, but we should not lose sight of the possibility and its implications for potentially antagonistic mechanisms.

Neither should we assume that organization *within* any given level will be straightforward. Indeed we probably need look no further than the problem of developing the schemata for "three-dimensional solid object" *on the basis of* previously acquired schemata for two-dimensional figures (Section A3.10, above), to see the need for some sort of recursive-like procedure within the $\mathbf{m^{\frac{1}{2}}l}$ level. Moreover the above remarks about parallel processing would seem to be just as apposite here.

As for consciousness, any suggestions can be little more than intelligently speculative at this stage. However it seems likely that, as a presumed source of "initiative", it will have something to do with the "top level" whatever that is, (Traill, 1976d). This might reside in whichever level is currently developing, though anyone who has tried to carry on office-work whilst the business is gradually moving premises might be inclined to doubt the wisdom of such an activity. Instead, for the sake of the present discussion, let us postulate that: (iii) There will be a comparatively

---

[34] At this stage it is advisable to introduce extra terminology:

"$\mathbf{M^0L}$" as a collective name for the $\mathbf{m^0l/m^{\frac{1}{2}}l}$ double-level

"$\mathbf{M^1L}$" as the collective name for $\mathbf{m^1l/m^{1\frac{1}{2}}l}$; and

"$\mathbf{M^2L}$" for $\mathbf{m^2l/m^{2\frac{1}{2}}l}$.

171

172

Thoughts about Thoughts — C3.2   scope of brain-recursion

stable top level, whose workings will not normally be accessible to the other abovementioned levels; but it will itself behave, in some sense, as controller over them as the occasion demands presumably the "source of initiative" which was considered above. One might perhaps think of this postulated controller as being a sort of $\mathbf{M}^{\infty}\mathbf{L}$ level[35] a sort of *local approximation* to an "all-seeing" transcendental being. If such a "centre" does exist, then perhaps the reticular formation would be a promising candidate for the post.

Pure recursion, in the sense of re-using the same basic "hardware", may well not extend throughout the whole $\mathbf{M}^{\mathbf{n}}\mathbf{L}$ scale; but provided that it does exist at the higher levels, this will have important consequences for the nature of abstract thought. It would seem to make possible, in principle, a boundless series of levels of abstraction beyond mere "Formal Operations" — as implied in Section A3.11, above. Just how far this could be taken in practice is a moot point however. "In principle" one might well be "able to build a card-house up to the ceiling", but to actually do so is quite another matter. The importance of "effort" and "ideal conditions" becomes increasingly intrusive the further one goes. Even in computer-run mathematical recursion one first encounters economic constraints, and ultimately technological constraints on the practical size of a recursion process; while in "biological recursion" one must expect much greater problems in view of the less formal and redundant organization inherent in such systems.

173

---

[35] Concerning "$\mathbf{M}^{\infty}\mathbf{L}$", this issue is discussed in more detail in Section C6.4 (paragraphs 4 and 5), and is also raised again in Section C8.2 (paragraph 36). Meanwhile we should not take the numerical implications of the "∞" too literally — but only its topological implications. Indeed, to avoid misunderstanding, I propose in future to use "$\mathbf{M}^{\mathbf{top}}\mathbf{L}$" in place of the "$\mathbf{M}^{\infty}\mathbf{L}$".

Chapter C4

Ashby's Adaptive Brainlike Mechanisms — but with added Self-organization

SUMMARY OF CHAPTER

This chapter provides a summary and discussion of Ashby's successive brain-simulation models, of increasing sophistication:-

(i) a simple non-learning but reactive system;

(ii) an "ultra-stable" system, with an extra feedback loop to allow it to randomly alter its response strategy if it has not achieved its goal by a certain time; and

(iii) the "accumulator of adaptions" whose "gating mechanism" allows it to store its earlier strategies — in case the original situations should re-occur. This latter development seems to provide a third feedback loop, and is somewhat suggestive of a recursive process.

However it seems possible to envisage *a more-complete ensemble of self-organizing procedures* for such systems — a capability which seems essential within the real biological world, and which is developed in further detail in the next chapter.

C4.1 Evolutionary pressure toward self-organizing systems

Biological systems would seem to have, as a vital distinguishing characteristic, the ability to "actively" counteract a moderate range of exogenous disturbance-or-attack on their integrity as physical entities. By definition, such an ability will have survival value, so it must be expected that Darwinian natural-selection will promote the evolution of increasingly sophisticated systems of this type.

Ashby's "homeostat" or "ultrastable system" (1960) is a promising paradigm for understanding such mechanisms, but his account still depends partly on "ready-made" components (as he himself points out in his Sections 17/10 and 17/11). Let us therefore work carefully through his standard cases, starting with the simplest, in an attempt to postulate an unaided sequence of development in plausible biological terms rather than using the short-cuts of a computer-programmer aided by his "god-like" insight.

C4.2 The trivial cases of unstable and metastable systems

In the extreme case of an arbitrarily assigned set of entities with no coherence at all, then the set scarcely even qualifies for the title of "system". If disintegration does go on without interruption but is somewhat delayed by a modicum of cohesion, then we may talk of an *unstable* system; while temporary freedom from disintegration, pending some set of triggering stimuli, will characterize the *metastable* system.

In all these cases, the ensemble is likely to go into irreversible liquidation if left on its own without outside maintenance, so their interest to the biologist will be somewhat limited. Nevertheless two points should be made about them:- Firstly they may be viewed as a first easy rung up the evolutionary ladder; and secondly these concepts may be useful for describing *sub*systems which form part of a larger system capable of providing the necessary maintenance or restoration — especially if it can aid its own overall stability in some way by so doing.

C4.3 Inanimate stability and stereotyped "responses" to "stimuli"

Any isolated (or "*closed*") ensemble will eventually end up in a stable static state, or in a regularly recurring cycle (Ashby, 1956/1964) — except in the freak borderline case of "neutral equilibrium" in which all feasible alternative configurations have the same energy state. But meanwhile, even for non-isolated (or "*open*") sub-ensembles, there will also often be a spontaneous formation of locally stable subsystems — coagulations within the erstwhile

homogeneous or randomized substrate. Crystals or droplets will tend to form and disrupt the homogeneity of the medium — though this will depend on the overall conditions within the system: temperature, pressure, and relative concentrations, (Goel *et al.*, 1970; Goel and Leith, 1970).

The important point here is that these subsystems *do not need to be designed;* they will occur spontaneously given certain commonplace conditions, and they will tend to survive moderate fluctuations of the environmental conditions also, without any deliberate maintenance from outside. Any would-be creator does perhaps need to design a suitable *substrate* (unless such a substrate is fortuitously provided, ready made), but after that he may just let things happen in their own way.

176

In its most basic form, such stability will manifest itself in ways similar to those described in the equilibrium-paradigms of elementary physics: a temporary displacement will fail to upset the system permanently, and it will return to its equilibrium state spontaneously. (In the present context of naturally occurring systems, it would be better to look at distortions to the shape of dew-drops or the energy-state of an atom — rather than the usually cited examples of pendulum or cube-on-a-level-surface; but the stability principle is essentially the same). The essential point is that such systems will react to specific "stimuli" by giving a specific response — perhaps allowing for statistical variation if necessary; they thus correspond to Ashby's (1960) non-learning but reacting systems, depicted in Figure C4.3/1.
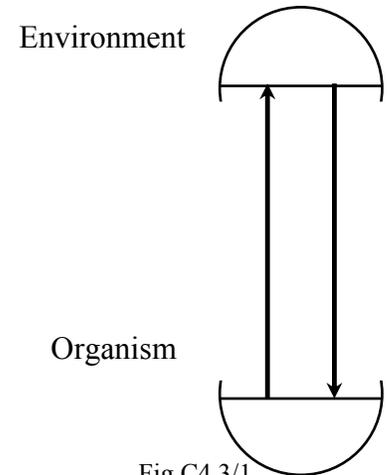

Environment

Organism

Fig C4.3/1
Ashby's Fig 7/2/1 depicting
a reacting but non-learning system.

177

One point which does not seem to have been made clear in Ashby's treatment until six chapters later, is that the "response" will not necessarily appear straight away, nor will it necessarily emanate from the stimulated part of the system; nor indeed will it necessarily show any preconceived temporal or spatial *distribution* — given that the system itself will be somewhat extended, and so capable of a multi-faceted response. a system (such as an atom) *may* simply fend off the stimulus (as when an atom causes an incident alpha-particle to "bounce off"), but it may alternatively bide its time (before re-emitting the same particle — or some other, if we choose to accept that it will still be the "same" system afterwards); and of course the disturbance may travel through the "body" of the system and eventually produce one-or-more responses at remote parts of the system. In short then, the "stimulus-signal" is likely to be subject to spatial and temporal *dispersion* (Ashby, 1960, Section 13/14 *ff*) whenever the system is polystable in Ashby's sense (Section 13/2) of its having richly intercommunicating parts with many equilibrium positions.

If the system has a more meagre claim to polystability, by having a definite-but-more-limited intercommunication path and a more modest repertoire of equilibrium positions, then the effect may be less extreme — but it may be more useful and orderly. It may well be that this is one of the advantages which would favour the survival of *linear* molecules such as RNA if they were to be generated spontaneously. Anyhow, a mechanism of this sort has been suggested, in a more biological context, as a basis for the reproducible precise gross behaviour of animals, (Traill, 1976e).

C4.4 Systems able to switch to alternative response patterns

Unlike Ashby (1960, Section 7/20 *ff*), let us confine ourselves for the moment to systems which might reasonably be expected to form spontaneously within a realistic evolutionary time-

178

scale — and using only elementary trial-and-error techniques. Orderly limited-dispersion molecules (such as RNA) would appear to answer this description and also serve as a suitable medium for the initial development of the adaptable-response mechanisms which we are about to discuss. In general we will be talking about collective systems comprising ensembles of linear strips which maintain communication linkage between them, yet representing distinct formulae for action or response. It is not important at this stage to speculate seriously about whether they are *physically* connected or not, though it seems convenient to think of them as joined end-to-end in the nature of chromosomal strips — even if this is less likely from an evolutionary viewpoint.

There would seem to be two ways in which adaptability could be built into such collective systems. Firstly we might expect to find sites along individual strips which were capable of taking on several different functional states — as "reversible mutations". In most cases these would doubtless be no more than on/off "switches", determining whether that strip would be available for participation in any ongoing activity; and of course this is likely to be of vital importance, even if it is only a beginning. It is also possible that some such sites might serve as multi-purpose switches or gearsticks, with their functional role changing according to the "switch-position"; but it should be recognized that the chances of structural economy occurring spontaneously would be comparatively remote as if an object selected for its knife-like properties, from amongst randomly fabricated metal shapes, were to be *found* useful for its comb-like properties *as well*. Anyhow, to the extent that this type of mechanism is adequate, there would be little need for the strips to form into ensembles involving higher $m^n l$ levels.
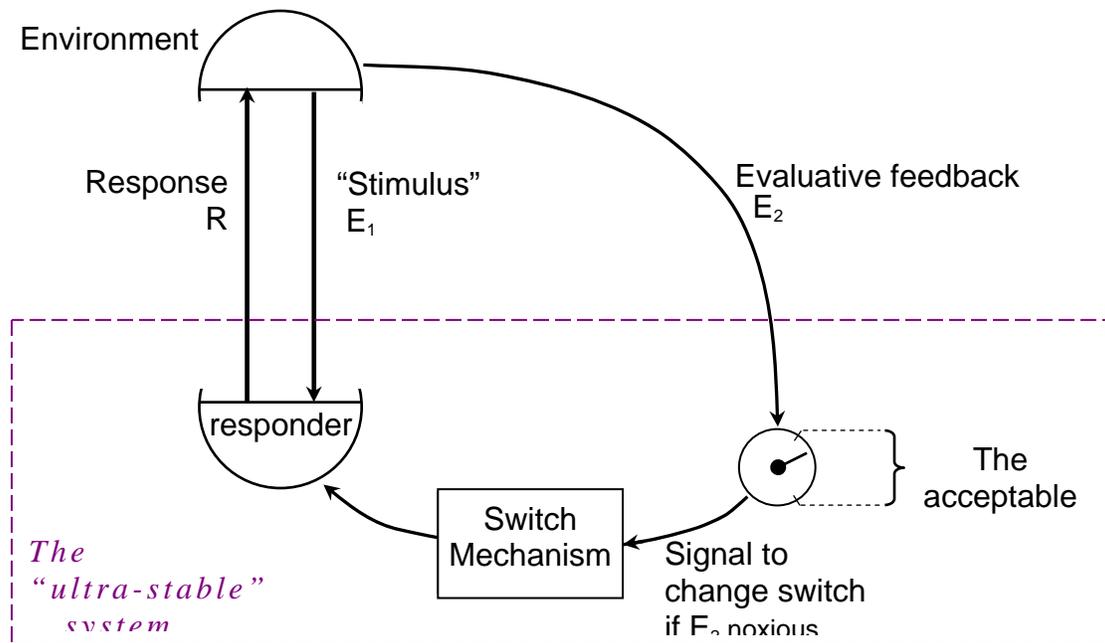
179

Fig C4.4/1.   Ashby's concept of the "ultra-stable" system,
capable of adapting its functioning response-pattern (usually
reversible) according to changes in the environmental *pattern*.
(Diagram adapted from Ashby's Fig 7/5/1, 1960).

The second source of adaptability would be the choice between whole alternative strips within the ensemble. The actual selection between these alternatives is likely to depend rather critically on subtle nuances in the overall signal pattern, probably involving optical interference

phenomena (Chapter B2, above; and Section C8.1, below). Such context-dependent mechanisms are of course thought to operate within the genetic code: if the developmental stage is right (so that it is emitting an appropriate pattern of signals) *and* if the cell is appropriately placed within the signal-pattern, then the cell's DNA will have its "liver-cell-development" strips switched on — or whatever is appropriate, if anything.

We may now turn to Ashby's "ultra-stable" system, and look at it in the light of the above considerations. To start with, we should notice that there is a degree of complexity creeping into the system which is now arguably beyond what we can expect to be created spontaneously with any sort of reasonable frequency; Fig C4.4/1.   Accordingly we should expect that some sort of self-replication mechanism would need to be one of the "actions" to evolve in the repertoire of such systems, at about this stage of development though perhaps slightly later. This would make it possible to store partial successes in the learning of survival-technique, in the manner of Ashby's cases 2 and 3 (1960, Section 11/5), already described in the present work (Section C1.2). Anyhow, such replication (together with mutation) would help to explain the postulated existence of *ensembles* of limited-dispersion molecules within a totally indifferent or hostile environment which contributes no guidance for the creation of the system — other than a stiff dose of fortuitous reality.

There would seem to be no great problem in postulating an RNA-like basis for such ultra-stable systems; see Fig C4.4/2.
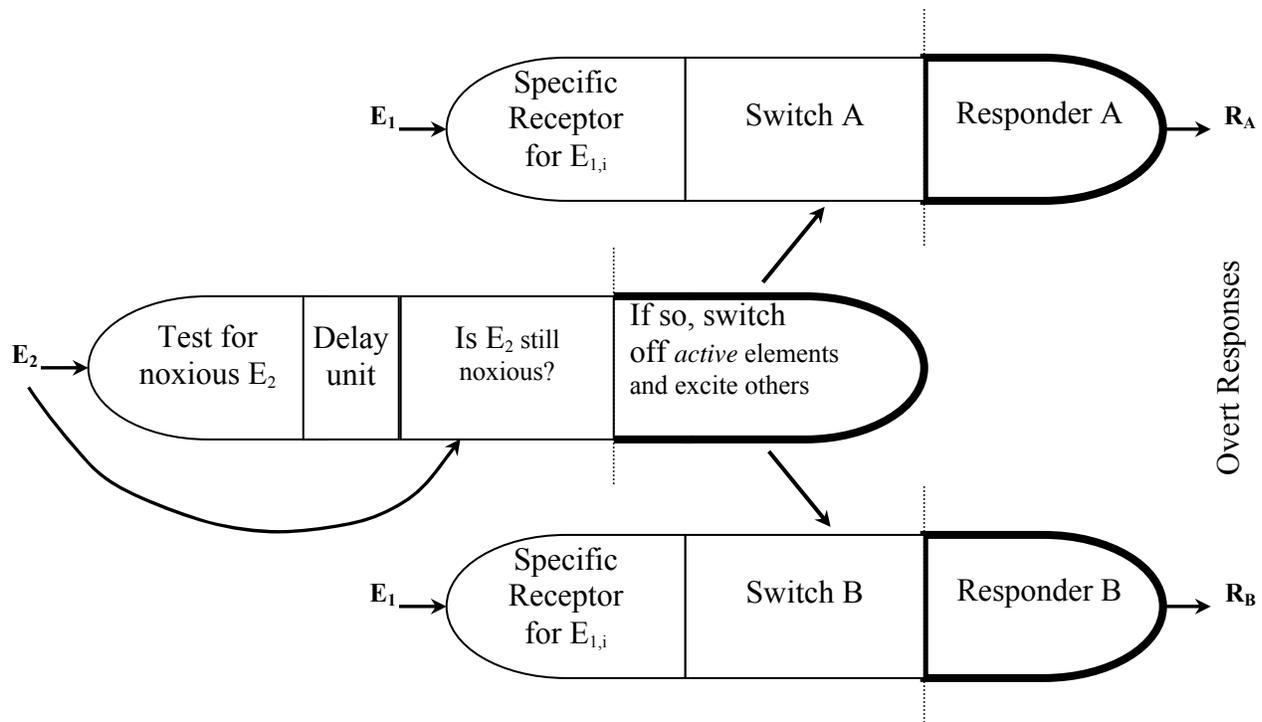

Fig C4.4/2.   Schematic diagram to suggest a plausible ensemble of linear elements capable of functioning collectively as an "ultra-stable" system (as in Fig C4.4/1).   Three types of linear element are shown here (and in practice each of these is likely to represent a whole population of coordinated similar elements). Those on the right correspond to the primitive paradigm discussed in Chapter B2 (above), but with switching-gates as part of their labels.   The other element is also formally similar, but serves to re-set the switches; and this will occur in accordance with the pattern in a different category of inputs — the "evaluative stimuli" E_2. The "program" part of each element is heavily outlined, while the other parts, to the left, constitute the "labels".

C4.5 **Purposeful switching to alternative response patterns**

The system depicted above in Figures C4.4/1 and C4.4/2 operated on the principle of "let's do *anything* in an attempt to alleviate the present discomfort" — much as you or I might do in a *panic* situation where our existing knowledge-and-experience seemed to be invalid. But given some sort of patently appropriate body of knowledge such as we could normally expect for human subjects, we would expect something more systematic than a mere random change of parameter-settings. As Ashby puts it, at the end of his Section 10/2 (1960): "If the reader feels the ultrastable systems as described so far, to be extremely low in efficiency, this is because it is as yet quite unspecialized; and the reader is evidently unconsciously pitting it against a set of environments that he has restricted in some way not yet stated explicitly …". Note that if the environment *is* totally capricious, unpredictable and unconstrained, then one might just as well go right on and panic — as in Figures C4.4/1 and C4.4/2 — there being no basis for anything more logical! Indeed there would not even be any basis for deciding what was a "suitable delay" before trying some other panic measure. Nor, come to that, would our genetic repertoire of panic measures (fight, flight, freeze, etc) necessarily help at all! The very fact that such repertoires *are* fairly standard, reflects some degree of consistency in the "laws of nature" in the environment; and it also reflects the ability of surviving species to acquire a genetic "knowledge" about general approaches to coping with such situations in practice.

One important constraint commonly found in natural environments, is the tendency for events to present themselves in consistent patterns: "the recurrent situation" (Ashby, 1960, Sections 10/4 to 10/7). In these circumstances there is some point in acquiring the ability to transcend mere random panic reactions to noxious stimulus E_2, so that now it becomes sensible to respond systematically with a *specific* "switching" action — corresponding to whatever has been successful in the past. In Fig C4.4/2 there are only two alternative courses of action indicated, so it represents the trivial case in which there can be no choice once one sets out to change the switch-setting. But in general there will be many possible actions which one might take if "Switch A" is currently on and due to be changed; and one can imagine that initially there will be many linear codings for all the different competing actions which might be taken to try to escape from this situation. It then becomes easy to see that we will get the right sort of adaptive learning process if we postulate a Darwinian selection of the "fittest" amongst these coded linear elements; and moreover the explanation will then be fundamentally similar for both learning within the brain of an individual animal *and* for phylogenetic learning of hereditary traits within the species. (Cf. Chapters A2 and C1, above).

Another constraint likely to be found in the environment is the distribution of times for such patterns of stimuli-and-consequences to manifest themselves. If we imagine that the initial set of codings also varied as to the number of "delay elements" along their length (or some other equivalent "clock" mechanism), then we can see a way in which the organism-or-species might learn some "idea" of how long it should persevere with one line of endeavour before abandoning it as unpromising; (cf. Ashby, 1960, Section 17/10). Once again the process may be interpreted as Darwinian.

However such constraints are not so straightforward as would first appear. Such recurrent situations will often not recur immediately, and in the mean time other unrelated situations will frequently intervene. Thus the constraints in the environment will not usually present themselves as one ever-repeating pattern of events, with all the essential elements discernibly similar; they will instead appear as a *variety* of *sub*-environments with such properties. So the constraints will be there alright, but if the organism is to take advantage of these constraints then it must be capable of adapting to the prevailing sub-environments. This means, firstly, that it must be capable of *storing* enough of what it has learned so that it can turn back to it later on — without having it "over-written" by any unrelated learning which has occurred in the interim. Such is the

Ashby + Self-organization — C4.5 Purposeful Response-switching

problem considered by Ashby in the remainder of his Chapter 10 (1960) under the sub-heading of "the accumulator of adaptations", and he depicts the solution as shown here in Fig C4.5/1. The various sub-environments are represented as P_1, P_2, and P_3, while the corresponding alternative sets of storable response-determined experience are represented by S_1, S_2, and S_3.
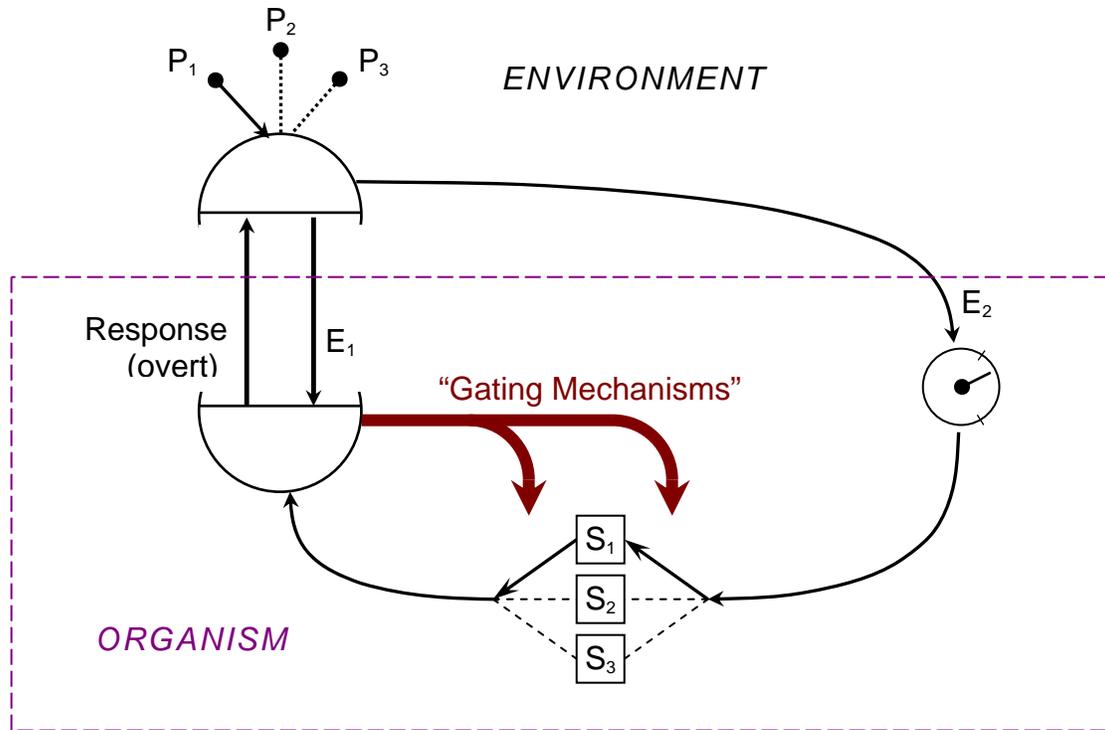

Fig C4.5/1.   The "accumulator of adaptations" to various different "sub-environments" (P_1, P_2, P_3), as depicted in Ashby's Figure 10/9/1, (1960).

Ashby's account gives rather scant attention to the nature of the "gating mechanism" which selects which S_i should be used by the organism; nor does he elaborate greatly when he returns to the topic in Chapter 16. However it seems that we can make the system rather more mechanically plausible, without violating Ashby's conceptualization, by re-drawing the diagram as in Figure C4.5/2. Here the gating mechanism is depicted as operating under the influence of a *third* sub-set of the general input from the environment, E_3, (instead of Ashby's arrangement — which arguably amounts to the same thing — of using "a part of" E_1). When seen in this way, it becomes easier to envisage the mechanism as being a "higher level" recapitulation of the same basic linear micro-element mechanism postulated in Fig C4.4/1 and discussed further in the next chapter (Chapter C5). It also suggests the possibility of further recursion into even "higher" levels provided further distinctions (E_4, E_5, ... etc.) can be made between different aspects of the input. This question also incidentally highlights the problem of just how these subdivisions of the input are to be made — a point which we will take up in Chapter C6.

184

185

Ashby + Self-organization — C4.5 Purposeful Response-switching

ENVIRONMENT

E_3 (major)

E_2
(medium)

Response E_1 (minor)

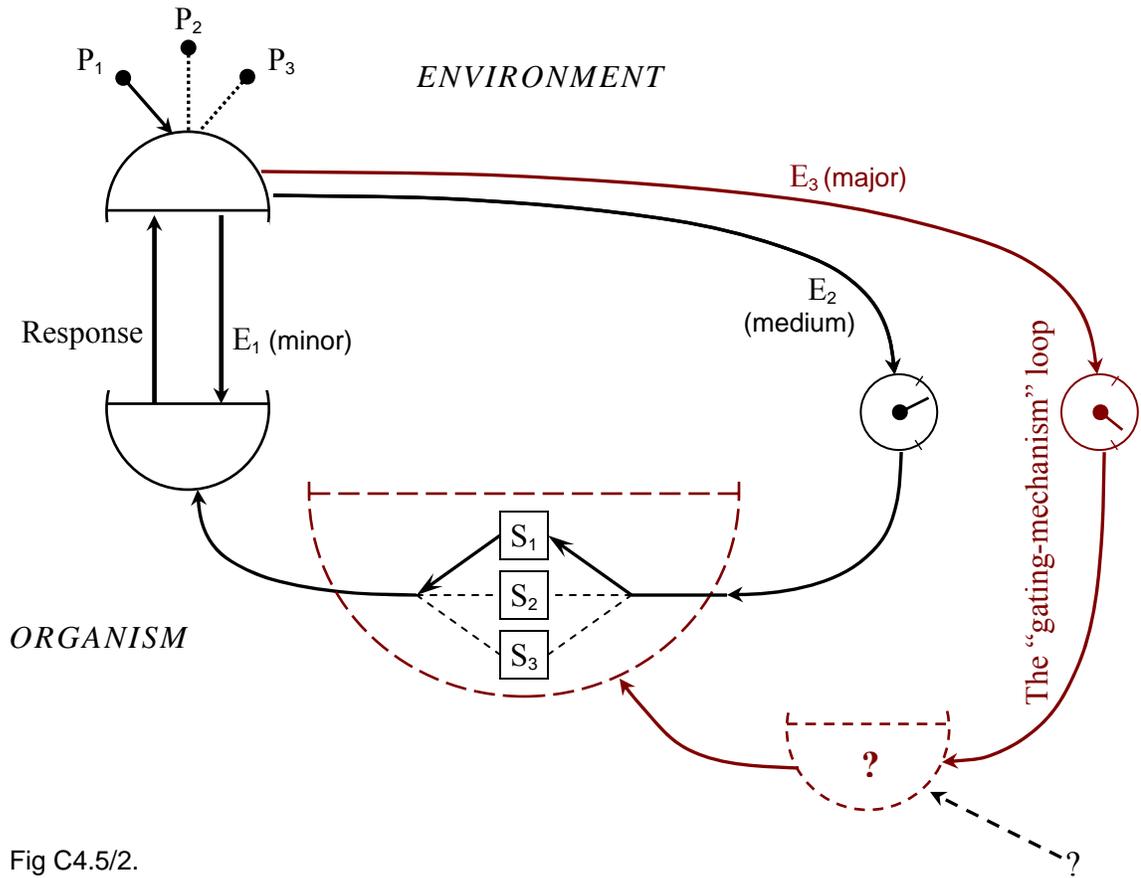The "gating-mechanism" loop

S_1

S_2

S_3

ORGANISM

?

?

Fig C4.5/2.
A variation of the diagram of Fig C4.5/1 to emphasize the potentially recursive
implications of Ashby's "gating mechanism", and to suggest approaches to
explaining its operation.   When there is a mismatch between P and S, then the
S will be altered (randomly or purposefully) by the gating-mechanism loop.
However this concept is developed further, starting at Section C5.1(3), so we
should regard this E_3 loop as provisional only.

Ashby + Self-organization — C4.5 Purposeful Response-switching

Chapter C5

Toward Fully Self-Organizing Versions of Ashby's Systems, Using Populations of 'Linear Micro-Elements'

SUMMARY OF CHAPTER

This chapter sets out to reconcile Ashby's model with the molecular or 'linear micro-element' theory of memory which has been our main concern so far. In general this means introducing largish *populations* of structures capable of acting *collectively* in a manner similar to those outlined in Traill (1975b, 1976d). As it stands, Ashby's model is still too "computerish" and not biological enough (as he himself implies in his sections 17/10 and 17/11 when he lists five unexplained features as lacking provision for self-adjustment). The linear micro-element theory offers an explanation in essentially Darwinian terms, because the molecules (the presumed micro-elements) would be profuse enough for extensive trial-and-error procedures to work economically.

C5.1 Some acknowledged defects in Ashby's exposition

Like most working models, Ashby's model is presented ready-made with many of the important problems of existence already built into the system before "testing" begins. It is not easy to refrain from imposing one's observations of gross structure onto a model, and there are some advantages in taking such a short cut:- Displaying some limited-but-important principles (as here); Short term prediction; and/or Making-do when there is no guide to possible substructure. Indeed most model-builders (Dutton and Starbuck, 1971) impose even more structure onto their models — these being mathematical stereotypes which are then fitted, as best they can, by measuring the "relevant parameters" and inserting them into the model. In such cases the basic structure of the model itself is not regarded as being subject to continuing amendment (let alone evolution), though it may be seen as being "on trial" in an absolute pass/fail sense.

However it would seem that, for biological systems, their very structure must be largely self-constructed — with no explicit guidance, though with some implicit shaping from an indifferent or hostile environment. (One might quibble on the detail of this proposition — after all education is explicit guidance in some sense, or so it would seem — but self-organization nevertheless seems to be the more important factor, especially embryological development of physiological structure which presumably lays the basis for feedback-loops like those in Ashby's diagrams. In any case, one can probably answer the "education" argument insofar as it is applicable — by regarding education as communication within the species, and asserting that it is this wider system which is self-constructing in the long run — even if this is not quite true for individual members of the species).

Ashby's first two self-criticisms — problems potentially solved in the above discussion

Ashby is more-than-usually aware of the desirability of incorporating such self-organizing principles into models which purport to depict biological reality. Indeed he himself lists five ways in which his model falls short of this total objective (1960, Sections 17/10 and 17/11):-
(1) The duration of a trial, before abandoning it as "a failure", was set *by him* in the light of experience — and not by the system itself. However it has been suggested above (Section C4.5, paragraph 3) that it *is* feasible for systems of this broad type to adjust such timing-parameters, at least in the case of a 'linear micro-element population' realization of Ashby's general concept.

(2) Ashby decided for the systems: *which ranges* of the "essential variables" (E_2) were to be considered "*good*" thus entailing a non-action "response", and which were to be considered "*bad*" so that they lead to attempts to change the input by altering the configuration in its own parameters. To simply identify such mechanisms with biological pleasure/pain activation is an

187

appropriate start, but it still leaves the problem: "How did the body come to decide that a burn should be 'painful', while replenishing a physiological deficiency should be 'pleasurable'?". Indeed the problem is heightened when one considers pathological cases in which this formula fails to hold. Of course at a relatively trivial level one can *learn* that *object A* is pleasurable or *object B* is painful. this is the familiar paradigm of classical conditioning which has been interpreted in terms of the linear micro-element model by invoking the concept of genetic "cross-overs" between segments of the linear-elements (Chapter A1; Traill, 1976b). Similarly one can learn the same sort of differential between one's own actions; this being the case of "operant conditioning" which may be explained in the same general way (*ibid.*), in what amounts to a Darwinian explanation entailing the competition for survival amongst the linear elements. It is possible, for instance, that those producing "painful" consequences fail to acquire the tag (or switch setting) that would enable them to be replicated; or they might be tagged "for demolition"; or they might simply fail to become incorporated into a self-supporting "schema" of mutually intercommunicating elements as an "internal closure" systems (Chapter A3; Traill, 1976e). *However* it would seem that the *ultimate* sense of pleasure/pain, from which the above phenomena derive, must be arbitrarily inbuilt into individuals in the first place. When such settings help survival, the setting is likely to survive along with the individual and be transmitted genetically; whereas a "wrongly-connected" individual will probably not survive; — Darwin again.

Ashby's third self-criticism — What generates the "gating mechanisms"?

(3)[36] Ashby points out that he has not made any suggestion as to how the organism might acquire its *gating mechanism,* shown here in Figure C4.5/1. However, if we re-interpret this concept as a "metasystem" (E_3 in Figure C4.5/2), making recursive use of the same basic type of mechanism as in E_2 and E_1, then it begins to look as though a plausible explanation for this aspect of the self-organization might be forthcoming.

Some details, of how such a system might work, have been given in Traill (1976d). But first we should re-examine Figure C4.4/2, the base level mechanism, and re-interpret the role of such a subsystem when acting "on a higher plane". For the base-level case, the routine interactions with "the outside" are R (the set of responses) and E_1 (the set of "non-evaluative" stimuli) — both implying contact with the environment *outside the organism itself*. Let us now consider *another* such system in which the E_1 and R interact with a *different domain*. Instead of interacting with the outside environment, we shall suppose that this new system will interact with a "pseudo-external" domain consisting of the stable elements of the original *system*. (Thus it would become capable of interfering in the activity of the original base-level system — as a god-like "meta-system"). What form, then, should such interference take?

Figures C4.5/1 and C4.5/2 portray this "gating-mechanism" interference as controlling the activity of the switch-setters (S_1, S_2, S_3, ...) by some means which is not clearly specified, though it seems likely that it might involve changing the sensitivity of the environment-oriented system to the evaluation-signals (E_2) — presumably by blocking off communication pathways on the left side of Figure C4.4/2. This could make sense in the light of Ashby's discussion, in his Section 11/5, concerning the tremendous practical benefit to be gained from *holding onto partial solutions* while other unsolved parts of the problem are explored further. Thus if we had a large population of environment-oriented sub-systems of many different types — corresponding to many possible transactions with the environment — then it would be sensible to be able to exercise some sort of control over the incoming "criticism" via E_2. Otherwise the criticism is likely to be received at inappropriate points, with destructive results.

---

36 We will not get to items (4) and (5) until Section C5.5.

Not-yet-resolved aspects of the problem which now require deeper investigation

This still does not explain how the organism comes to know *which* base-level sub-systems *are* to be left open to the prevailing E_2 from the environment. To some extent this will doubtless be trivially genetic or structural, and not readily open to modification. Thus emergency responses such as pain or visual reflexes will often have a private, genetically-determined pathway; and such an arrangement is not really open to orthodox re-education, though it may well be *overruled* or else functionally isolated in some way, in some cases. Rather more subtly, it has been suggested that such stability may be partly due to the closing of a communicational loop (or a "more complete" topological enclosure than a mere loop) — in the form, perhaps, of elements such as those on the right of Figure C4.4/2 giving off responses which included a component which could activate other members of the group; (see Chapter A3; and Traill, 1976e). Such groups were then envisaged as being capable of acting collectively as new complex elements on which higher systems of organization could be based. This is a point to which we will return shortly, early in the next section.

But leaving aside these more automatic cases, we are left with the previously-mentioned situation in which one "higher" subsystem interferes with the parameters of a "base-level" subsystem; (or more correctly, one parallel *set* interferes with a lower parallel set). How then does it "know" which type(s) of base-level subsystem it "should" interfere with, and in what direction? Indeed how does it "know" that it should interfere in anything, or take any action at all? The answer, according to this model, is that it generally *does not* know any of these things *a priori;* (or if it does, then the knowledge was gained genetically — by orthodox natural selection amongst its ancestors). It is postulated instead that, as in the case of the base-level codings themselves, the higher subsystems develop from a large number of more-or-less arbitrarily coded elements which then have to compete against each other for survival under the prevailing conditions. It is rather less clear what form this evaluative feedback (higher-level E_2) should take, but we may tentatively suppose that it is governed by essentially genetic-based mechanisms, as for the lower level. The main difficulty would seem to be how to distinguish adequately between various painful evaluative verdicts, where these are "appropriate" to only one of the two respective levels. How then could we tell which of these two levels should be amended for each of the various painful feedbacks? Once again this is likely to be ultimately explicable in terms of trial-and-error among competing units — genetic and/or learning elements. In particular, acceptable trial-times are likely to be very different for the two cases; (Ashby, 1960, Section 17/8; see also Sections 3/15 and 8/15).

The problem of distinguishing between different levels of evaluation will be discussed in Chapter C6. Other points relating to Ashby's list will be continued in Section C5.5, after two longish digressions:-

C5.2 **Exploring details of linear micro-element versions of such mechanisms**

It seems desirable, at this stage, to take the time to reconcile the picture presented in Figure C4.4/2 with that portrayed earlier (Chapter A3; and Traill, 1976e), in which the development of representations of sets and groups was explicitly emphasized. For both versions, the basic element is the "RNA-like linear microelement", envisaged as being approximately as shown in Figure C5.2/1:-
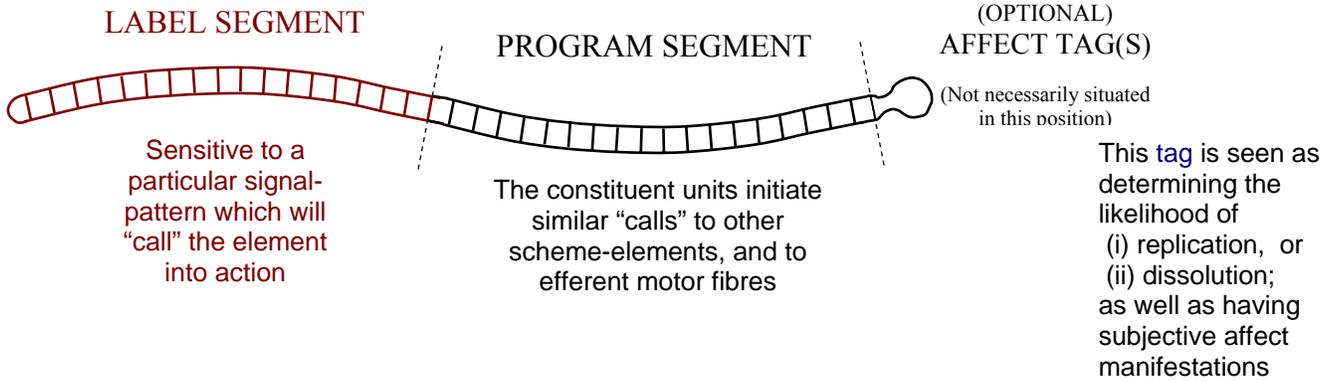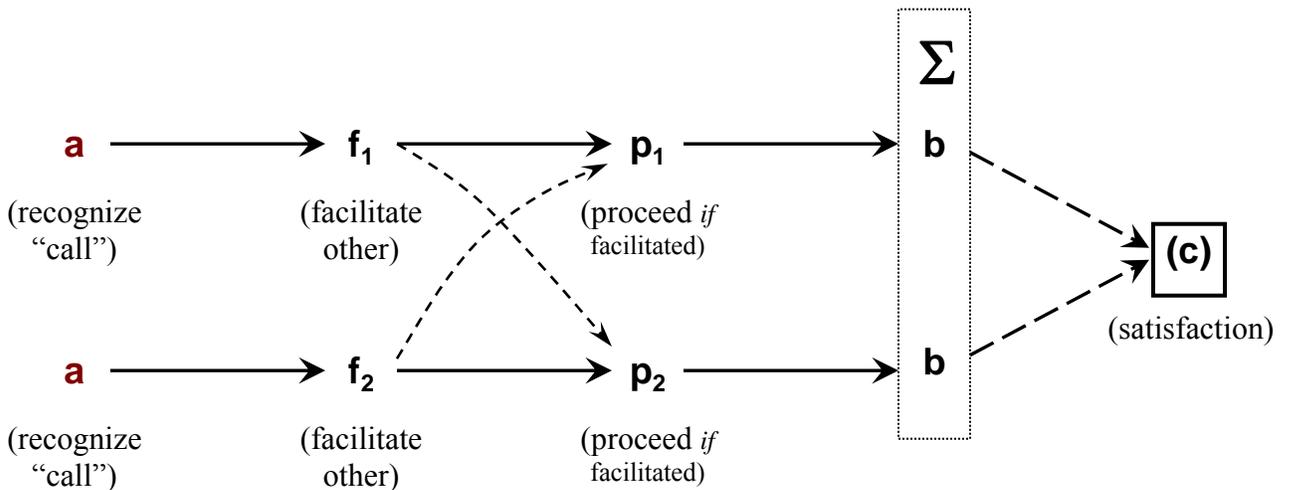
LABEL SEGMENT

PROGRAM SEGMENT

(OPTIONAL)
AFFECT TAG(S)

(Not necessarily situated
in this position)

Sensitive to a
particular signal-
pattern which will
"call" the element
into action

The constituent units initiate
similar "calls" to other
scheme-elements, and to
efferent motor fibres

This tag is seen as
determining the
likelihood of
 (i) replication, or
 (ii) dissolution;
as well as having
subjective affect
manifestations

Fig C5.2/1.   Supposed "anatomy" of an RNA-like linear micro-element (of the simple type,
lacking any "AND-gates" within the program segment).   This is offered as the physical
counterpart of the functionally defined "scheme-element" (Part A, above).
This diagram is taken from Traill (1975b), with slight modifications to accord with
developments in Part B, above.

It will be evident that the three sub-parts in Figure C4.4/2 conform to this general pattern —
though with some doubt concerning the roles of "switches" and "affect tags". Also it is not
immediately clear that these functional units in Figure C4.4/2 are actually envisaged as compris-
ing physical *populations* of linear micro-elements (of a slightly more complex type involving
synchronizing signals) as depicted in Figure C5.2/2.

$$a \longrightarrow f_1 \longrightarrow p_1 \longrightarrow b$$

$$\Sigma$$

$$(c)$$

a

(recognize
"call")

f_1

(facilitate
other)

p_1

(proceed *if*
facilitated)

b

(satisfaction)

a

(recognize
"call")

f_2

(facilitate
other)

p_2

(proceed *if*
facilitated)

b

Fig C5.2/2.   Illustration of how the individually-insignificant micro-elements may collectively
cooperate, under favourable circumstances, to produce significant behaviour.
Only two elements are depicted here, but the concept should be thought of as applying to a
somewhat larger number — probably involving a threshold concept, in that only when there
is a sufficient active population with sufficient unanimity and synchronization, will the
behaviour actually take place.   The diagram is taken from Traill(1976d), but alternativie
versions may be found in Traill (1975b and 1976b), and in Section A3.4 (above).

Synchronization and specificity of elements

This question of cooperative activity amongst the postulated elements is arguably another situation for which categorization of elements into sets would turn out to be essential. It would seem to be invariably necessary to involve largish numbers of such elements, and often it would seem to be important to orchestrate various different types in one collective complex action; but clearly it will be crucial to be highly selective and orderly in the involvement of these various elements — in higher animals at least. If such specific selection is to take place, then there must inevitably be some effective mechanism whereby the elements are allocated to *sets*: either by *intensive definition* (relying on shared common labels, tags, or other distinguishing features on the elements themselves); *or* by *extensive definition* (using externally imposed boundaries or other physical constraints on the physical elements); — or by using some combination of both methods.

193

For instance, in the straightforward case depicted in Figure C5.2/2, the two-or-more parallel elements would presumably both be called into action when they both "recognize" the same signal at about the same instant; here the possession of the correct "recognizer" or "label" serves as an intensive definition of the relevant set — and if the signal has only a restricted distribution confined to a local area, then there is also an extensively defined criterion to delimit the (sub)set which is to be activated. (Further details on this postulated process of recognition are given in Sections B2.3 and C6.7, and in Traill (1976b); but briefly, the label segment is seen as a series of sites receptive *individually* to specific pulse-or-wave configurations — *and* to the prior excitation of the previous site, if any; so that the *chain* of sites will only collectively accept signals with the "correct" temporally extended pattern, probably in the infra-red region of frequency components).

194

Another example is afforded by the postulated activity of "affect tags" (see Figure C5.2/1, above). Generally speaking, these are thought of as auxiliary labels which *intensively* define their respective elements as "good, bad, or indifferent": thereby influencing the likelihood that these scheme-elements (or the schemes of which they are part) will be called into action. Alternatively or additionally they may also influence the chance that such elements will be annihilated as unwanted cellular rubbish. However we may also suppose that such tags will often be *context-dependent* in their significance. Thus if an animal is *angry*, or *fearful,* or *sexy*, then quite different sets of scheme-elements will be "switched on" or at least made more sensitive to the prevailing signal traffic, while other sets of elements will be inactivated for the time being. In this way then, it will be possible for the important and far-reaching sets of "good" and "bad" (or "appropriate" and "inappropriate") to be *superimposed* reversibly, briefly, and flexibly, *right across* any other existing set-memberships of the elements — and without necessarily altering those other membership categories in any direct way. Such tag-activation would presumably be performed overall by endocrine hormones; or more selectively by means of the Autonomic Nervous System — implying a location-based *extensive* component to the definition for sets of elements in this case. [Indeed, broadly speaking, the transmitter-release from normal synaptic activity of the *Central* Nervous System might *also* be seen in this comparatively lowly role, while the *detailed* information could all be coded within the postulated infra-red signals!]

195

Stability of element-ensembles

Yet another instance of the usefulness of this conceptualization of sets has been the proposed explanation of the nature of Piaget's schema, and how it comes to be formed; (Part A, above; Traill, 1975b, 1976d). To quote from Chapter A3:-

> "*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. self-consistency …) …".

One supposed mechanism producing stability of this sort was sketched, in outline, in the third-last paragraph of the previous section (Section C5.1). This concept brings together a number of ideas; notably the notion of "seeking out" of *internal closure*[37] as a collective property for related scheme-elements — and this is closely identifiable with the well-known concept of Gestalt (English and English, 1958). Essentially this entails building models which will tend to have a *mathematical group* type of structure; — a strategy which is likely to pay off because many phenomena in our environment do seem to have such a structure, especially for solid objects and discrete phenomena. [It may well be that we are less well equipped to deal with concepts involving continuity — at least until we devise some discrete way of symbolizing them. Thus Bridgman (1927) wrote "The mind seems essentially incapable of dealing with continuity except in negative terms." (page 94); while Ashby (1960, Section 17/9) writes "But when the whole system is not so divisible it remains merely a fearfully complex whole, not capable of reduction, …".]

Other ideas inherent in the notion of group-structure modelling of this sort are concerned with the likely mechanics of such model-building and maintenance. a useful guiding biological "intuition" for this is the principle that physiological structures tend to develop appropriately *by virtue of* the stress laid upon them.[38] In the present context this encourages the notion that the survival of individual scheme elements may be promoted if they happen to be part of a communicational network which is continually "exercising" its collective self by echoing lowish-intensity signals around its network. (Such activation might, for instance, ensure that the elements did not fall into a state of particularly low energy — perhaps centred at a particular site such as a "tag" — in which state it might be vulnerable to dissolution as "rubbish").

The formation of meaningful ensembles

Here we have been talking about the *maintenance* of group-structured schemata, but we need to go into the matter a little more deeply to explain their *formation*. In fact these structures are seen as being comparatively sophisticated types of schemata, so we should now give some attention to the rather more elementary structures which have the properties of sets (i.e. lists), defined *extensively* in their essentials, but which have not (yet) achieved group-status. (It is supposed that the group structures must, in fact, progress through this mere-set stage during their development. After all, to the mathematician, a group is simply a specialized *type of set*).

The problem for the organism is to gather together, into some readily controllable centre, the access to those scheme-elements which are all characterized by some common property. Or, in other words, the task is to translate intensive definitions into extensive definitions — except perhaps for the trivial case in which the formal *label-segment* constitutes the intensive definition (in which case there is arguably no need for an extensive definition since the members are already "on tap", as in the first example following Figure C5.2/2, above). Anyhow, this process of translation of other distinguishing features into physical structures representing extensive definitions is explained, as in Traill (1976d), using Darwinian and genetic concepts as follows:-

---

[37] Here "closure" is effectively synonymous with "coherence", as discussed later in Traill (2005c) — www.ondwelle.com/OSM03.pdf — and in philosophy-orientated works generally; [RRT 2006].

[38] There is an obvious advantage in such a biological strategy in that the animal is taught by *the environment which is actually present* rather than some out-dated formulation of what the environment should be; and moreover it makes for tremendous savings in the amount of information needed to be transmitted genetically. Galbraith (1977) makes a similar point when he compares the regulation-bound Spanish colonial administration with the pragmatic approach of the British administration in India. And returning to biology, Ashby (1960, Sec. 18/4) writes: "it is *the mouse* which teaches the kitten the finer points of how to catch mice."

196

197

First we need to consider how *any* sort of arbitrary list could find physical representation in terms of the linear micro-element model. It scarcely seems likely that in such circumstances the elements should become physically enclosed within vesicles containing representatives of the set's members, but some functional-equivalent to this is required. Perhaps such elements could somehow become "moored" along a master linear element which would form a "spine", like the side-chains along an aliphatic carbon-chain? Unfortunately it is difficult to see how such a structure could replicate effectively — unless the side-chains were quite short — and replication is probably an essential means for "amplifying" successful combinations of this sort. In any case, if the listed elements were of any appreciable length (and some of them might well be), then it would be wise to do what sophisticated computer "sort" programs do: *i.e.* deal only with the "names" (label codings) of such elements — not the elements themselves, as these can be accessed subsequently by using the label-coding if necessary.

Thus we may start by thinking of the list as a master linear element, with its own separate label segment "α", followed by a series of names (β_1, β_2, β_3, etc.); and this is the arrangement as portrayed in Traill (1976d). However there is a possible difficulty with this too *if* we are thinking in terms of adding and removing names from the list, because it is hard to see how this could be done without breaking the chain. For the moment then we will have to contemplate a compromise, which seems to mean short side-chains (for the names only) short enough to be encompassed in the replication process *as* side chains, *or else* able to undergo a conformational change such that they "slip into line" for replication purposes. Of course if we are prepared to accept these master elements as having a substantially fixed membership once they are formed, on a Darwinian trial-and-error basis, then the name-codings can be considered to be entirely linearly arranged without any need for side-chains at all.

Next we should consider how such sets come to have relevant and helpful entries in their name-lists. According to Traill (1976d) there is "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?)".   Candidates are then tentatively accepted (or perhaps merely considered) pending acceptance or rejection on the basis of the arbitrary criterion; while the survival of the list itself, as a whole, depends on the relevance of the arbitrary criterion to the apparent needs of the organism. (Such possibilities are re-evaluated below in Section C8.l(a), but the above account will serve for the present discussion).

Several practical problems arise out of this suggestion, and they will need a more thorough investigation in the hope of finding a more rigorous yet plausible mechanistic explanation. The first problem is how the arbitrary "membership criterion" is to be encoded; and this leads into the second problem of how candidate members are to be selected on such a basis. Another associated question is to consider what communicational and spatial connections there might be between the "list-of-symbols" on the one hand — and the actual items represented by those symbols, on the other hand.

It was suggested in the earlier work (Traill, 1976d), that possibly the first-named item on the list might also serve as the paradigm criterion by which all subsequent candidate members might be judged. At first sight it is difficult to see how this could conveniently be executed if the list contained only a "name" for the paradigm concerned; it would seem to be too cumbersome to be constantly invoking this name to access fully-fledged exemplars at relatively remote sites whenever a new candidate member was being assessed. Rather it would seem probable that any such criterion would need to be physically attached to the list — in full relevant detail, and not by "name"; (though it is possible that such attachment could be intermittent and reversible — depending on how the list was being used or replicated at that time, and this would possibly be related to the various sleep modes).

→ Full Self-organization — C5.2 Details of linear-micro

Figure C5.2/3 sketches the general idea of how a "higher order" linear element might constitute a list; but it gives no clear answer to the problem of criterion-specification, so we should now attempt to remedy this deficiency.
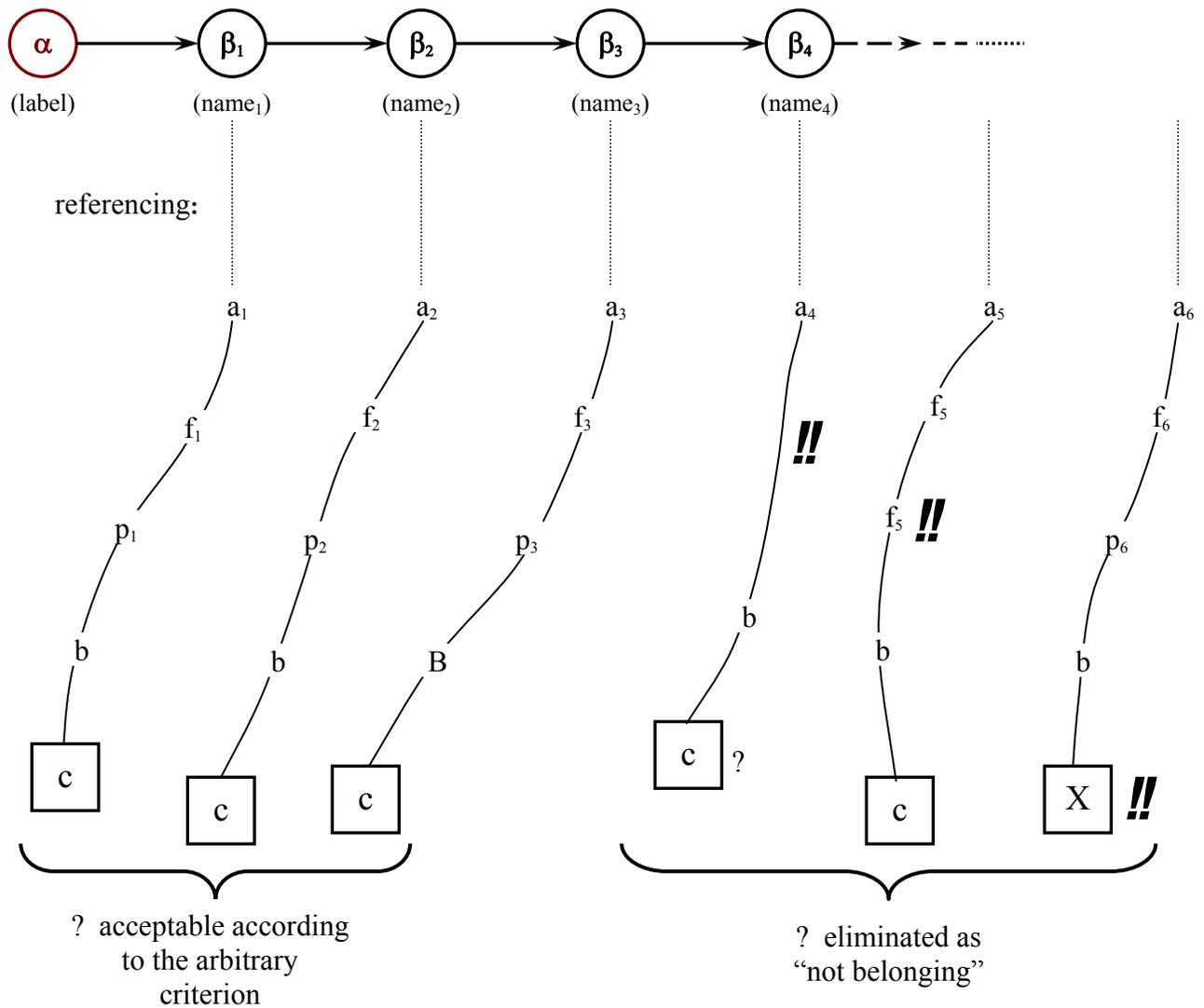
Fig C5.2/3.
Slightly modified version of a diagram from Traill (1976d) depicting the postulated "higher level" linear element (α–β_1–β_2–…) serving to construct, physically, an extensively defined set or list. This set is shown as consisting, momentarily, of six linear elements. Three of the element-names (β_4, β_5, and β_6) "should not be there" because the codings of their corresponding elements do not accord with the arbitrarily preset intensively defined criterion for the set, so these member-names are seen as about-to-be-ejected. The detail of this arrangement is criticized in the text, and a subset of simpler structures proposed instead.

Practical procedures for establishing ensemble-"lists" — each with its consistent criterion

Suppose then, that such a criterion segment (γ) were to be fitted somewhere into the higher-level linear element in Figure C5.2/3. How then could this effectively influence the membership of the (extensive) list? For the moment we may also suppose that such an *element* will contain *only one* member-name, β_1, (any other names being held by other similar elements in a population all having the same label, α). Thus, if α is activated, the β_1 will also be activated in turn, sending off a "call" to labels of the type a_1. As a consequence of this, at least some of the "base-level" elements will be activated — either with overt behaviour, or perhaps asynchronously or subliminally so that there is no overt effect. Anyhow, it may be supposed that such activation will produce some sort of reproducible and locally-recognisable signal activity capable of being monitored at the "criterion" segment (γ) of the "higher level" element, enabling this element to be tagged according to whether or not it is consistent between its *intensive* component (γ), and its *extensive* component ($\beta_1 \rightarrow a_1$ …, etc.) which provides the *feedback* on which the comparison can be made with γ; see Figure C5.2/4.

Special (sleep)-modes of operation might be involved here in several ways. In particular, the suppression of overt behaviour is one likely result. Also the monitoring and tagging procedures might well be impossible during the normal "noisy" activity of the awake state, so this might supply the reason for any such suppression.

In the version of the theory just described, in which each "higher element" of the set held only one name (β_i), our extensive definitions have lost the *ordering* of the names in that list — as compared with Figure C5.2/3 where they are clearly ordered. So does this matter? In fact this loss will arguably make our model more realistic in several ways:- To start with, it is more credible to imagine the spontaneous mutation of linear elements into usable re-codings of the type α–β–γ, rather than the more unlikely longer sequences of the type α–β_1–β_2–β_3– …–γ. Secondly, we arguably do not usually start our concepts of sets-of-objects by thinking of them in terms of ordering; and indeed such ordering concepts take some degree of experiential sophistication before they develop, (Inhelder and Piaget, 1964, Chapter 9, p 247). Mind you, this might be explained as an initial random ordering of the β_i units among the different elements making up the relevant higher-order *population* of elements (α … β_i … γ), though one might expect a speedy selection process in such circumstances.

Then obviously, it is difficult to see how the "γ" could effectively monitor the *simultaneous synchronized* feedback from all the β_i…a_i connections within the set; whereas with a separate physical label (α) for each β_i, there is at least a chance of moderately-asynchronous-but-adequate feedback — especially if other activity is reduced by using a "sleep" mode. Finally if the β_is were to be added and subtracted as implied in Figure C5.2/3, then there are further difficulties such as how to stop the whole physical sequence from breaking inappropriately and losing the γ segment, along with the distal β_is. (This assumes that the γ is at the far end away from the α — as it probably *would* be in view of its *post hoc* role and the supposed transmission of the internal signal along the element away from the α).

200

201

202

→ Full Self-organization — C5.2 Details of linear-micro

Note that these considerations do not conclusively rule out the possibility of two or more β_is in series, as depicted in Figure C5.2/3. It is conceivable that the membership criterion codings might be located remotely in a detached β_1, either initially or as a later sophistication, and that other linearly-ordered members might then be added; though it remains to be explained how the detailed operation of such a system could take place, and until then we should regard it as suspect. (One of the attractions of such an arrangement would be the ready explanation it would offer for temporally-ordered sequences of behaviour *if* these are deemed to be important at this
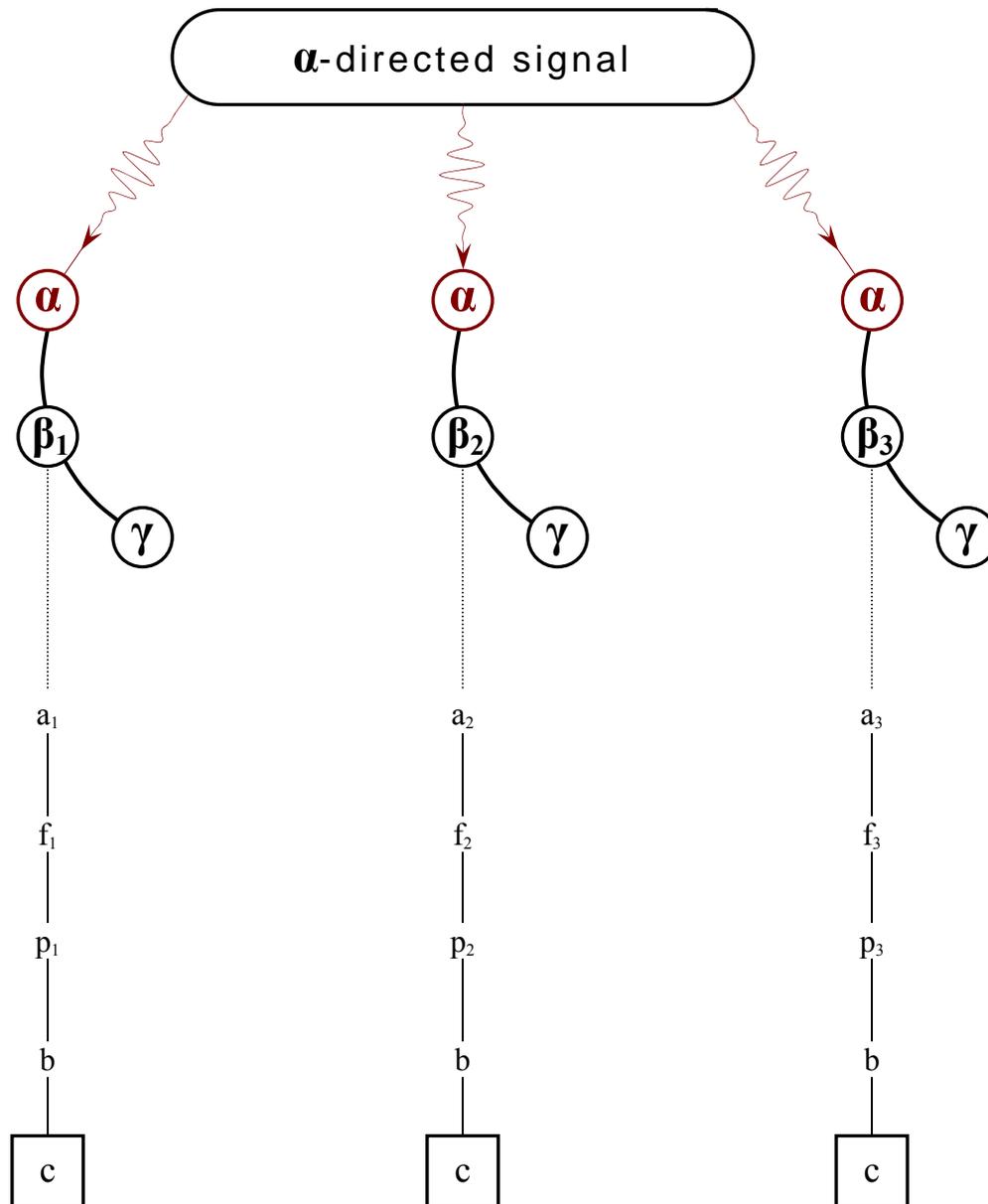
(201)

Fig C5.2/4. A variation on the theme of Figure C5.2/3.
Here each β_i is held by a separate "master" element which nevertheless acts in unusually close cooperation with its fellows; — a well-controlled intensive linkage (rather than a genuine "tethered extension"). Only one subsidiary element ($a_i - f_i - \ldots$) is shown for each β_i, though in general there would probably be many available and the reference would presumably not distinguish between those with the same "i" value.

higher level. But even if they *are* important, they could be explained alternatively in terms of differential time delays or by chain-reactions between such elements, or by parallel lower-level linear-elements — though admittedly such explanations would themselves seem to be rather cumbersome). However, until we are pressed to look again at such possibilities by unexplained inconsistencies, it would seem best to opt provisionally for the simpler *basic* mechanism outlined in the previous four paragraphs — on the "Occam's razor" principle.

(Actually neither of these two proposed techniques seems to be entirely satisfactory if it is to be *the* method of organizing sets. Accordingly we will later be looking at yet another suggested technique, which will be depicted in Figure C8.1/4, below; and there may well be other possible arrangements of a similar nature. Moreover it is quite conceivable that some or all of these alternative methods might be used concurrently as a collective pluralistic system).

A likely topic-segregation role for cell-membranes (and other brain subdivisions)

Extensive definition can, and does, take another form; (arguably co-existing with the one we have just discussed, and quite possibly also *collaborating* with it). An extensive definition of a set is most commonly seen as the *drawing of a physical boundary* around the relevant members, thus isolating them from non-members; so what could be more natural than enclosing supposedly similar molecular members within a restricting cell-membrane. For much of the brain (other than the "association areas") there is ample evidence that function is localized in a systematic way (e.g. Hubel and Wiesel, 1962; Thompson, 1967, Chapter 11); but it has not been clear in detail just how such specialized cells operate, and it has been even less clear how they come to be in such methodical positions with such systematic functions. Of these, the question of operation might be explicable if we invoke the above molecular theories; and the question of construction clearly raises problems of embryology and development. It is quite possible, however, that such molecular theories are applicable to embryology, and that embryological considerations are pertinent to theories of molecular encoding; so it might well be profitable to be on the look-out for unifying principles here.

204

C5.3 **Embryological considerations, and their relation to the extensive definition of linear micro-element sets**

This is not the place to embark on a full discussion of the likely mechanisms of embryological development; but it does seem appropriate to sketch in certain ideas which would seem to have some immediate bearing on our present problem. Trinkaus (1969) explains, with some understandable misgivings, the commonly accepted notion of how the cells of a developing organism may be guided into position by the supposed process of *chemotaxis* — extreme sensitivity to gradients of chemical concentration, which are presumed to be somehow set up and maintained in just the right way at the right time in a reasonably robust way. I have yet to see a reasonably detailed working through of just how such a mechanism might operate; but, in any case, it has seemed to me for some time that a much more likely explanation might be found in terms of one of the war-time navigational aids — code-named "Gee", (Crowther and Whiddington, 1947, pp 53-56).

This system ("invented by Mr. R. J. Dippy and developed by his team at Tele-communications Research Establishment")[39] involves the emission of a signal from one centre *A*, which then stimulates the emission of secondary signals from two other "slave" centres *B* and *C*. Provided that the time taken for each path remains consistent, and provided that a suitable periodicity is maintained by *A*, then the pattern of crossing secondary signals forms a grid network on which meaningful navigational moves can be made. One could, for instance, follow

205

---

[39] and eventually developed into our present-day Global Positioning System ("GPS") — [RRT 2006]

one set of wave fronts with axons from one set of neurons, and the other wave fronts with other neurons — thus forming such extraordinary structures as the optic chiasma, whose criss-crossed architecture is so baffling to explain in terms of chemotaxis. In "Gee", the network lines were hyperbolic; but it is not too difficult to envisage the formation of other shapes by using larger numbers of slave centres — possibly using distinguishable call-signs, and possibly *partly* depending on chemotaxis or other influences. (Come to think of it, there is a formal similarity between such ideas and the notions of Huygens — developed later by Young and by Fresnel; (Bell, 1947; Jenkins and White, 1950)).

Moreover, in view of the circumstantial evidence from other theoretical fields[40] that infra-red local radiation is likely to be an important biological phenomenon; it would seem that there are now reasonable grounds for taking seriously the idea that the "Gee" principle might be crucial for embryological and developmental processes[41].

An interesting set of cases is provided by the work on regeneration of the optic nerve in amphibians (Sperry, 1943; Sperry and Hibberd, 1968). It is difficult to see how the cut optic fibres could re-connect more-or-less as they had been originally — despite physical obstruction or *rotation* of the target optic-stump unless some system of call-signs (intensive definitions) were in operation. The subsequent re-connection would then constitute a re-establishment of the equivalent *extensively defined* associations, thus emphasizing a particular form of informational redundancy and interchangeability, and therefore robustness in the face of perturbation.

Why then is there no regeneration for a cut mammalian optic nerve? Could it be that once the initial connections have been made, the call-sign system *changes role* from carrying hereditary information (which is now left to the extensive definitions inherent in the connections) and now takes on the task of encoding the results of learning processes? After all, Sperry's amphibia were unable to learn to correct for the rotated eye, whereas human subjects *can* learn to correct for "mirror-vision" spectacles in a way reminiscent of Ashby's homeostats. Such a role-change would also seem to be consistent with the observation that neurons cease dividing mitotically[42] at about the same time as they (presumably) become functional, suggesting that the genetic equipment (DNA etc.), having fulfilled its initial role in producing "extensive" structures, is now free to be used for a different type of intensively-defined coding — arguably the scheme-elements or "tapes" of the "Linear Micro-element Theory".

In this light, it seems likely that any tendency to use "higher order" linear elements (to tether or list the basic elements) might constitute a new alternative method for forming extensive definitions which does happen to be *compatible with the newer non-genetic intensive codings*. We might well consider that this constitutes a first step up the hierarchical ladder (the "$m^n l$" scale of Chapter C2, above), with some formal similarities between the two levels, but using discernibly

---

[40] Cope (1973) on ATP metabolism — and later refences "45-51" cited in Traill (1988 — *Speculations in Sci.&Tech.*, **11**(3), 173-181) on photon-emission from nerve-fibres. — Also see Part B, above, [www.wbabin.net/physics/traill8.pdf or www.ondwelle.com/MolecMemIR.pdf] concerning saltatory conduction *and* molecular spectra (*ibid.*, Table 2.2/I).

[41] — a view supported more recently by theory-and-evidence regarding the control of myelin-growth, (e.g. Traill, 2005a — www.ondwelle.com/OSM01.pdf or at www.wbabin.net/physics/traill4.pdf).

[42] By now (2006), it is known that this adult mitosis-suppression is not always true, though the exceptions are fairly rare and specialized so the argument still seems supportable in general.   See:
• Nottebohm, F. (2002) "Why are some neurons replaced in adult brain?"
J.Neuroscience, **22**(3), 624-628. http://psych.colorado.edu/~munakata/csh/nottebohm.pdf
• Gould, E., & C.G.Gross (2002) "Neurogenesis in adult mammals: Some progress and problems".
J.Neuroscience, **22**(3), 619-623. www.jneurosci.org/cgi/content/full/22/3/619 —
• Traill (2005b, sec.(15)): www.ondwelle.com/OSM02.pdf or www.wbabin.net/physics/traill2.pdf

different mechanisms. If so, then clearly any recursive process we may envisage will not be a totally "pure" one; (see Chapter C3, above).

C5.4 Concept-structure types within each level: another digression in preparation for clarifying Ashby's difficulties

When we digressed at the start of Section C5.2, we had just been in the middle of discussing the third item on Ashby's list of unexplained features: namely, that he had offered no suggested process whereby his "gating mechanism" could be established without deliberate outside intervention.

His difficulty in giving a detailed account of better-than-random tinkering, by the system, with its own parameters may be partly ascribed to his emphasis on *actions* — neglecting consideration of more sophisticated symbolic representations of the real world outside. The main symbolic structures are likely to be: *verb-like* concepts which represent actions (but without executing them), and *noun-like* concepts representing objects (or mental schemata, in the case of abstractions without any tangible external embodiment).

"Verb" concepts may be taken to be made up of linear elements of the straightforward action-type, but with some sort of inhibitory switch which prevents them from actually initiating any action under normal circumstances. ("Abnormal" circumstances may be determined by hormonal, reticular, or Autonomic Nervous System influences — selectively setting such switches on, and plausibly leading to such phenomena as dreams, "acting out", and vestigial or subliminal action. On the other hand, "abnormality" might well consist of an unusually high propensity to use such inhibition:- conscious "suppression" of otherwise spontaneous tendencies, or unconscious "repression" of them — leading to neurosis in some cases).

"Noun" concepts may be identified with the rather more complex "sets" or lists of other elements — often having mathematical "group"-properties — described above in Section C2.4, and also in Chapter A3, and Traill (1975b, 1975c, and 1976d).

The Neo-Piagetian approach to Ashby's third problem

These *Action*, *Verb*, and *Noun* concepts should now be fitted into the context of the supposed hierarchical control-structure discussed in Chapter A3 and in Section C2.3, above. Let us start by considering just one level insofar as that is possible; and for this purpose, let us choose the level predominating in an infant of about one year old — half way through the sensori-motor period. Here "action" concepts refer to such hereditary reflex actions as sucking, grasping, kicking, smiling, and crying — and also learned modifications to these schemes (Section A3.7, above). But it should be noted that none of these action-patterns is a straightforward simple action of a single muscle; in each case there is already a considerable degree of organization, including temporal and sequential considerations. Clearly then, we should not delude ourselves that these actions of the sensori-motor stage lie on the bottom level of the hierarchical-pyramid; but rather there must be at least one other level below them, with its own (genetically determined) set of structural devices. Nevertheless, as adults we tend to take both these levels for granted in our daily life — if we ever notice them at all. From that "common-sense" point of view then, it seems reasonably in accord with everyday thinking to rate them as being of order "0 and –1" respectively; and this fits in with the nomenclature used in Section C2.3 where the simpler phenomena of the sensori-motor period were said to be at the "m^0l" level. Presumably then, we might reasonably talk of individual-muscle activity as being at the "$m^{-1}l$" level — or at least within the broader category of an "$M^{-1}L$" level.

From a formal point of view then, the assignment of the zero (to *n*) for the "m^nl" scale has been done arbitrarily — serving, if nothing else, to emphasize the existence of some misconcept-

208

209

↑ Full Self-organization — C5.4 Concept-structure types

ions built into our culture: comparable to talking about the Sun as "rising and setting". But in any case we would be wise to be cautious in our interpretation of $\mathbf{m^n l}$ values:- Even assuming the present basic theory on hierarchical organization is correct, there is no guarantee that we have identified all the relevant stages and their true interrelations. There may be at least one extra full ("double") level between what we have happened to call "$\mathbf{M^0 L}$" and "$\mathbf{M^1 L}$" (in Chapters C2 and C3); and the hierarchical organization may not be as simple and linear as we would like to believe. After all, family relationships do not always observe neat proprieties of generation-separation — so that aunt-nephew or father-daughter matings are possible, even if they are not regarded as proper; and who is to say that our supposed hierarchy will not break these or other rules? It may do so to some extent under normal conditions — especially if it is physically split up into poorly connected regions; and it may do so even more in pathological cases, as we shall see. It is possible that these irregularities, if they exist, may be connected in an intelligible way with EEG (electro-encephalogram) tracings, bearing in mind that effective trial-times are likely to increase appreciably for the more sophisticated control-loops in Ashby's system (as mentioned at the close of Section C5.1) so the slow waves of the EEG might have a particular association with the higher $\mathbf{M^n L}$ levels.

Anyhow, we were considering the action-codings within that level of organization which has now been labelled as "$\mathbf{M^0 L}$", and for this level we should be clear that we are talking about "actions" as calls to $\mathbf{M^{-1} L}$ structures which *then* activate the actual muscles. Next it will be well to recall some of the details as postulated previously, (Sections A1.3 to A1.5, and Sections A3.1 and A3.2, above).   Isolated linear elements or "tapes" were seen as essentially unstable or at least vulnerable in a competitive environment. Survival for such codings was seen as taking one of several forms: either they were hereditary and so readily replaceable from genetic sources, or they attained a collective stability through joint participation with other such elements, or else they acquired a "tag" of approval which afforded them some measure of protection from dissolution and/or enabled them to replicate. Following Piaget's terminology, the collective effect of the more ephemeral elements (including the replaceable hereditary elements) were termed "schemes", while the collective effects of the stabilized elements were referred to as "schemata". The non-commital term "schemoids" was used to include both cases, and its use is convenient to evade some confusion as to whether the stable *patterns of behaviour in a reflex* should be considered as schemes or schemata; (Section A3.2, above). In the light of our current discussion, we may now reasonably suggest that they are schemes *from the* $\mathbf{M^0 L}$ *viewpoint,* though we might well consider them as schemata at the $\mathbf{M^{-1} L}$ level — if we accept the legitimacy of this terminology for such a primitive level.

On fitting the Ashby and Neo-Piagetian models together

It remains now to identify the dominant "verb" and "noun" types of entity for this stage of development, given this postulated collection of linear elements. In the third paragraph of this section we have already taken the "verb" entities as being reversibly inhibited action entities, and clearly it will make sense for us to stipulate that these should be $\mathbf{M^0 L}$ action entities; however no mention was made there about stabilizing-influences in such cases, and this is something which we should now consider briefly. The predominantly symbolic role of such structures would seem to militate against direct strengthening-through-use, thus arguably weakening the case for both the tag reinforcement method and the passing-of-a-signal-around-a-closed-circuit method. However, as has already been suggested, such active roles may be intermittently restored in some sense during certain sleep modes — for the very purpose of promoting such selective stabilization.

We have already identified "noun" entities as being list-structures of some sort, presumably defined predominantly by extensive means, and preferably having a substantial degree of internal closure within themselves so that they have some claim to being embodiments of mathematical

210

211

Full Self-organization — C5.4 Concept-structure types

groups. It is envisaged that the constituent members of such lists will be inactivated action codings, and we would expect these to be of the $\mathbf{M^0L}$ type, (though it is not clear whether these members would already be intermittently-inactivated "verb" entities before they become listed, or whether it is the listing process which inactivates them. On balance, we might favour the former alternative as representing a stepwise imposition of control; but then this could be construed as an unnecessarily time-wasting strategy (Ashby, 1960, Chapter 11). In fact there seems to be no particular reason why both types of entity should not be eligible for membership on such lists, so let us provisionally accept this to be the case).

[This is a case of what we may choose to call "*the pluralistic principle*": that when there are several ways in which a biological mechanism might work, and they appear to have comparable probabilities, then there is a good chance that all of them are actually operating in parallel — or operating alternately, according to fluctuations of circumstance. Indeed the concept might profitably be extended beyond biology to complex mechanisms in general — including the case of chemical resonance. Even when one of the alternatives is rather improbable, it is probably fair to imagine that sooner or later such improbable methods will be "tried", whatever the consequences might turn out to be. a similar principle may also be stated for cases where the *function* of a structure (such as the urethra in male mammals) seems to have more than one possible use. Often it will be profitable to postulate that it *does* have multiple functions — thus increasing the chance of side-effects if we try to alter any single one of these functions. These two different variations on the general theme might suitably be distinguished by separate titles: the "redundant mechanism" principle, and the "multiple function" principle, respectively].

212

Anyhow, because such list-structures must necessarily await the appearance of the "verb" and/or "action" elements before they can build up and appear as "noun" entities, there is some logic in dividing the control level of the hierarchy up into two parts: lower and upper — $\mathbf{m^0l}$ and $\mathbf{m^{½}l}$. This also seems to tally with Piaget's division of his Sensori-motor and Operational periods up into "A" and "B" subperiods; (and this would hardly be surprising, because it was in fact the Piagetian notion which first suggested the $\mathbf{m^0l}/\mathbf{m^{½}l}$ type of double-stage mechanism — repeatable at higher levels).

Adding a third component —
the coexisting "Textbook Synapse-and-Action-potential" account

We may now turn to the $\mathbf{M^{-1}L}$ level to consider what mechanisms may be involved there, and the nature of their interface both with the $\mathbf{M^0L}$ level and with the outside world. Whereas the basic action elements of $\mathbf{M^0L}$ were seen as sending off their sequential orders in an "intensively defined" form (using specific callsigns to select their targets), the $\mathbf{M^{-1}L}$ action elements must presumably come down to earth and send *their* orders down efferent nerve fibres (each defining an "extension") — and presumably using straightforward Hodgkin-Huxley (1952a, b, c) action potentials, more-or-less in accordance with the accepted view: (Eccles, 1964; Katz, 1967). It seems then that the output side of $\mathbf{M^{-1}L}$ will entail an explanation of how the postulated intensive calls of the linear micro-element theory could become transformed into (amplified) extensive signals destined to bring about muscular changes.

213

No detailed explanation will be attempted here, but some general ideas will be outlined. Firstly, initiation *or modification* of motor signals by "intensively defined" calls must presumably arise through some sort of chemically-induced action at non-myelinated locations:- cell-bodies, dendrites, unmyelinated axons, or Nodes of Ranvier. (Hence all such sites are feasible locations for $\mathbf{M^0L}$ processes, especially when the neuron concerned happens to be an interneuron). Secondly, since we are dealing with highly stereotyped actions, there seems to be no pressing need for the driving "chemical" activity to be located in a central brain; — local reflex centres should be adequate. Much of the neural activity of invertebrates would seem to be of this sort,

and what little learning there is may plausibly be explained in terms of *locally situated* (Horridge, 1962) $\mathbf{M^0L}$ linear action elements, together with the mutations and "crossovers" amongst them; (Traill, 1976b; and Section A3.4, above).

Thirdly, it should be borne in mind that the Hodgkin-Huxley propagation along unmyelinated neural membranes has some obvious chemical aspects. These appear to be predominantly governed by trans-membrane electrostatic potentials, but it is possible that these may be at least *modified* by information emanating from $\mathbf{M^0L}$ action elements: for instance by altering the postulated infra-red components of the Hodgkin-Huxley action-potentials (Traill, 1975b, 1976b; and Part B, above), or simply by altering thresholds. This may be what is happening during the disruption ("blocking") of the resting-state alpha-rhythm EEG tracings, when "thinking" activity begins. After all, it would not be difficult to imagine that such "calls" from collective micro-elements might be in a position to control the release of energy from mitochondria, thus functioning like the grid in a triode valve to produce amplified effects; and no doubt there may be other feasible methods which could be used for such amplification. Incidentally, it is worth bearing in mind that EEG rhythms, as measured, have been located as coming from apical dendrites, (Gray-Walter, speaking at Brunel University, 1976).

Fourthly and finally; as far as I know, there has not yet been any adequate explanation as to what precisely causes the vesicles of Acetyl Choline (or whatever) to rupture and spill their chemical message into synaptic clefts. (One might perhaps be forgiven for supposing that some workers in this field are content merely to *name* this process as "exocytosis", and accept this as a substitute for explanation! But actually, of course, they are presumably just awaiting some "hard" evidence before attempting such an explanation; however such evidence is very slow to appear — as is so often the case in this ultra-micro field of investigation). Anyhow, it seems likely that this exocytosis may be set off by specific types of (coherent infra-red?) "calls" from linear micro-element sources — or else set off by some other agency with at least some features in common with these "action elements".

But the $\mathbf{M^{-1}L}$ level will also have a traffic in *input* sensory signals, and not just the output actions which we have been discussing. Hubel and Wiesel (1959, 1961, 1962, 1963a, 1963b, and 1965) have done much to elucidate the more-or-less automatic stepwise processing performed on a visual image during the passage of its effects from the retinae, through the lateral geniculate body, to the striate cortex and beyond. Such accounts naturally tend to focus on those effects which are observable in the laboratory situation, so we would do well to be awake to the possibility of other ramifications of this process; and we might profitably also give some thought to *how* such a system might develop its structural detail under the joint influence of genetic coding, and interaction with the environment — or lack of it (Hubel and Wiesel, 1963a, 1963b). Nevertheless, we shall leave aside such details here, and consider more generally what it is that this whole string of processes amounts to:-

There seems to be a somewhat arbitrary distinction drawn between the gross layout of the overall picture and the fine textual detail. The gross layout is "mapped" onto the new regions more-or-less intact — maintaining its *extensive* properties; however the textual detail (including dynamic aspects) is "analysed" or "interpreted" into a *different type of extensive definition* — with specific screen-display patterns[43] being "identified" and turning up as action-potentials in

---

[43] many elements of which might justly be described as *intensive* in nature — so the interpretive process seems to be translating in the wrong direction. However, if we consider that the intensive properties (such as *physical* colour patterns) are external to the individual and probably in an unusable intensive form, then it makes better sense to think of such visual impressions as being translated first into a more manageable extensive form — and then, if necessary, re-translated into a different (internal) intensive form. After all, such procedures are commonplace in computer practice, and in the use of coordinate systems .

specific neuron cells (and thus being sorted into positional "slots") . If this input process is to be the reverse of the postulated output process, then we would expect this processed set of extensive codings to be subject to still further processing until it ended up in an internally usable intensive form.

Extending Piaget's ideas into the more elementary domain — (pre-sensorimotor: $M^{-1}L$)

According to the Piagetian view, perception is no mere passive acceptance of whatever manifests itself at the sense-organs, but rather a consequence of *active* internal manipulation of such material (based on the original physical manipulation of relevant parts of the environment). This concept is of fundamental importance — though even today it has not been widely understood. In fact, we might perhaps sum up Piaget's main contribution as an application of this concept to phenomena which we, as adults, tend to take for granted as "basic" — discrete objects, phonemes, the concept of "mother" or even "original sin"! Re-examination from a Piagetian point of view suggests that many of these supposedly basic units are not necessarily basic at all, but built up experientially at an early age (or in one's ancestors) as described in the postulated detail of how an object concept develops (Traill, 1975b; and Chapter A3, above).

But this breakthrough was at the M^0L level, and we have now turned to a discussion of the $M^{-1}L$ level; so does the same principle apply here too? Perception of edges and bars etc., with specific motion and orientations, would appear to be the basic units of $M^{-1}L$; so do we come to an appreciation of their potential value through association with some *action* which we ourselves initiate? Well, while it is possible that something of this sort might take place here[44], it nevertheless seems that there must be a stop to this subdivision process eventually. Sooner or later we must "hit rock bottom" in that our sense-organs will simply be unable to discriminate any further remotely meaningful detail among the signals from the world outside; indeed it might be argued that the outside world will itself eventually reach a bottom limit of structural detail. Whether such basic units of mental concepts are to be found here in the $M^{-1}L$ level (or any lower level) is thus open to question. However as the outcome of any such discussion is unlikely to have any crucial bearing on the issues of this present work, we may leave the question open — but meanwhile provisionally assume that $M^{-1}L$ *is* the lowest level, in which case concepts such as specific edge motion-and-orientation *will* be basic. In any case, it is difficult to see what would be gained by extra flexibility at this stage because configurations like this would seem to be adequately ubiquitous and basic.

Anyhow, whichever one is the fundamental basic level, its "noun/verb/action"-content will be rather different from the other levels. As far as the Central Nervous System is concerned, signals enter and leave carrying codings which have meaning only by virtue of (i) the fibre in which they are travelling (extensive definition), (ii) the pattern of action-potential pulses along the fibre, and perhaps (iii) interaction effects between different fibres; — at least that is the accepted view, and there does not seem to be any pressing need to challenge it as the means for sensory and motor

---

[44] At some stage, development is likely to be governed by what I have elsewhere referred to as "ortho-maturational" processes. These might be regarded as being half way between straight genetic development and learning-process development. Whereas true learning can explore a number of unforeseen possibilities in an open ended and reversible way, ortho-maturational development is essentially genetic but with the implicit "assumption" in its coding that the environment will provide certain recognizable cues at appropriate times. Imprinting is one apparent example, and the development (and maintenance?) of optical pathways into fully functional systems may well be another.

In a trivial sense, of course, all genetic development must be ortho-maturational to some extent. For instance, it will nearly always "assume" that the environment will never manifest a temperature of 100ºC in the organism's own immediate location.

216

217

↑ Full Self-organization — C5.4 Concept-structure types

peripheral communication (though the present work obviously contains different proposals, involving infra-red, for the mechanisms appropriate for *higher* mental processes — including such lowly activities as *channelling* reflex emergency responses). Thus it would seem that any structure in the coding of the entities of this lowest level will have been imposed from elsewhere, so the entities themselves will apparently lack structure of the "linear micro-element" type, which means that the noun/verb/action distinction loses significance. This seems to mean that there is no room here for the abstract difference between such a signal and the "thought" of it. It will operate or not operate, as the case may be, but there is no symbolic contemplation of that particular signal by the animal or person concerned.

Other thoughts about mental hierarchies

It will now be left to the reader to consider whether bacteria, plants, or computers (as usually programmed) could be said to operate at this level of "thought". I suspect that this exercise might not be as frivolous as it appears at first, and that it might shed some light on the historical development of thinking, as well as giving some further insight on what constitutes the "lowest level" (if indeed this concept is valid) and related questions on "how to emerge from a recursive process". However, it seems that a full development of these questions here would take us too far from our main topic which is more concerned with the "higher" levels of thought.

We have been discussing the processing of information of a textural/edge-detection/colour type — information which can be attributed to a small local area on the retina (or corresponding local areas in other sensory or motor systems). But as we have seen, these "points" will often form a gross topological pattern which will be transmitted more-or-less intact right across the brain, and despite the considerable amount of processing which has meanwhile been happening to the "textural" information. I would suggest that it is, in fact, beyond the competence of the $M^{-1}L$ mechanisms to deal with information of this sort — with its almost astronomical variety of potentially meaningful configurations (unlike the limited "textural" repertoire whose information it will also use). a different approach is required in such circumstances, and this is provided by the Piagetian paradigm of: "arbitrary action first, and then see if the outcome seems to tend towards meaningfulness" — as already described for M^0L, the level which will presumably be required here.

Simpler animals will probably make do without such gross holistic analysis. Ashby's remarks (1960), that it is fallacious to believe that a brain must be richly interconnected, would seem to be particularly applicable here. Indeed one might say that the only justification for interconnection within parts of the brain is the need to handle situations in which the meaningful cues are of at least this complexity; and arguably this handling can only be done by using M^0L mechanisms, or higher. In this context, it is interesting to speculate on the significance of various anatomical sites in the brain, and why new structures of various sorts have appeared at particular stages of evolutionary processes. Plausibly M^0L would initially be absent, or present only locally at isolated sites without any need for a *centralized* "brain"; and later it might achieve such centralization in the "old brain" in such structures as the optic tectum. Presumably these old structures were incapable of being modified to cater for new higher levels such as M^1L and M^2L, and so they became something of a historical curiosity (with some vestigial powers) when there arose, fortuitously, a new structure which did have such capability, and clearly the disproportionate size of the *neocortex* in mammals makes it look very like the sort of structure we would have in mind.

218

219

↑ Full Self-organization — C5.4 Concept-structure types

→ Full Self-organization — C5.4 Concept-structure types

TABLE C5.4/I

Postulated schematic organization for the micro-structures in the various levels in the hierarchy of mental coding and control; see text.

(" – – – → " signifies informational dependence, and also subsequent potential for control).

Level	Piagetian Stage at which this level first becomes important	Corres-ponding salient ages	"Noun" / "Verb" / "Action"	Comments	Guess at plausible main site in Nervous System
M^3L	——— ?	?	(etc. ?)	? "Algebra of algebras" ? Mutant "strings" found capable of manipulating M^2L structures (as a generalization of them).	⋮
M^2L	Formal Operations	16+ 11+		Separate algebraic and logic systems. Inactivated "strings", *symbolizing* set-manipulation: the essence of *algebra*. Mutant "strings" able to manipulate "obvious" sets.	Cerebral Cortex (Neo-cortex)
M^1L	Concrete Operations	7+ 2+		"Obvious" sets:- of objects, etc. Inactivated "strings", *symbolizing* object manipulation (etc.). Mutant "strings" able to manipulate M^0L structures.	
M^0L	Sensori-Motor	1+ 0+		List–"strings", incl.obj.concepts: Fig C5.2/3 Inactivated "strings", *symbolizing* action-patterns. Hereditary action-pattern "strings" (+ mutants).	Older brain-sites: tectum etc. + Sensory & Motor Cortex
$M^{-1}L$	(Basic)	pre-natal		Afferent and Efferent Tracts	Peripheral

As for recursion (discussed above in Chapter C3); it would now seem likely that $M^{-1}L$ and M^0L will use different mechanisms from each other, and indeed there may well be at least two different types of mechanism in use for M^0L (corresponding to the supposed alternative sites in the archecortex and neocortex). It is possible however that M^1L and higher levels may all use the same basic linear microelement pool, and that the distinction between the different levels will be maintained autonomously and adaptively using internal devices (such as "tagging") inherent within the system itself. Moreover the "neocortex type" of M^0L might well also be a party to the same sort of method, which would seem to give us the option of a complete recursive arrangement for all but the basic $M^{-1}L$ level. (This option need not necessarily be used for all purposes, as in cases where the archecortex is still significant). It should be noted that such an elaborate recursive system, with potentially flexible roles for the constituent elements, is more *vulnerable to malfunction* than a less sophisticated inflexible system; and we will return to this issue in Chapters C6 to C8.

Table C5.4/I summarizes some of these hierarchical M^nL concepts which we have been discussing. In particular it shows how the postulated fundamental linear action-elements of each level (n≥0) are supposed to depend on the Noun and/or Verb structures of the next-lower level to provide them with meaningfully stable referents. In their turn, these Noun or Verb structures are presumed to refer, directly or indirectly, to the linear action-elements of their own level. The Table also implies the question of whether any recursive build-up can continue beyond M^2L; and if so, then how far? In other words, can we form an abstraction of an abstraction — of an abstraction … (and so on), without losing track of what we are doing, and without delegating our task to some essentially mechanical outside aid?

Of course we must expect that the real brain system will be rather more "untidy" than the comparatively neat arrangement here. Not only are we likely to have such things as old and new types of M^0L system, but it would seem likely that there will be other duplications of (e.g.) the visual gross field into areas "I and II" (Thompson, 1967, p 316 — after Woolsey, 1958) *within the new* processing lobes. Moreover, there will almost certainly be irregularities and exceptions in any such arrangement, so we would be well advised to bear this possibility in mind.

C5.5 Completing the review of defects in Ashby's exposition (as listed by him)

At this stage it should now be possible to give a reasonable explanation of Ashby's "third conundrum" (if we may call it that): how a natural biological system could establish its own "gating mechanism". Let us first remind ourselves of the elementary situation shown in Figure C4.4/1, in which the organism responded to an unsatisfactory response-pattern by a random change into some other arbitrary response-pattern, and (so to speak) hoped for the best! While we may accept this sort of change as occurring *in principle,* it will now be necessary to take issue with it concerning matters of detail in the mechanism:- Ashby is clearly thinking mainly in terms of gross "nuts and bolts" models, as described in his Chapter 8 (1960), whereas our concern here has become one of *populations* of cooperating and competing micro-elements — for which it will not quite do simply to say of *gross* behaviour-patterns that they are "switched" on or off; so further elaboration is needed.

Applying the above discussion to Ashby's third problem
— on explaining gating-mechanisms

In terms of the linear micro-element theory, we may suppose that the "Responder" in Figure C4.4/1 consists *mainly* of linear elements with a specific hereditary action coding[45], though there will also be *minorities* of mutants, thus implying possible alternatives, but whose existence or growth will not be encouraged while the organism remains in a "comfortable" state. Plausibly the most prevalent type of coding (sensitive to *this* E_1) will be the one which will actually lead to action; and in some sense we may say that it is holding the organism's "attention" (Without necessarily implying that this attention will be conscious). If attention, in this sense, means that *its* programme of action is the one which forms the response R; and if it also "gets the blame" whenever E_2 detects that the outcome is unacceptable; then we can see how the response-pattern may become "switched". Thus "getting the blame" will entail a dissolution of at least some of those elements which are "under attention" — either directly, or via unfavourable tagging. This will tend to allow some rival coding to increase its relative numbers and eventually capture attention from the original coding.

[Without actually using the term "attention", this concept has already been discussed above (in the early paragraphs of Section A1.4). There, the linear elements were described metaphorically as being called (by an E_1-type stimulus) "to report to a particular control centre"; though it was pointed out in the footnote that the "control centre" should be interpreted in terms of "*communicational* proximity" rather than the spatial proximity implied by the terminology. However there was no clear indication given as to what form such "communicational proximity" might entail; so let us now specify something more definite. It is conceivable that this could be attributed to *gene-like* switches on the linear-elements (with "on" signifying "attended to"), perhaps dependent on sleep-or-mood state as postulated above for the formation of "verb" elements; but we may be inclined to think that such a mechanism would be too passive, sluggish, and unwieldy as an explanation for "attention". Alternatively we might consider *excited quantum states* (rather than chemical changes) as a somewhat more credible switch mechanism for constituting the attention-state. This would seem to be more clearly reversible, especially under competition, and more readily established under suitable circumstances. Various degrees of stability might be obtained, depending on potential barriers or on a possible propensity for some states to trap any further hyperactivating transmissions which might be available — a form of positive feedback].

So, looking again at Figure C4.4/1, we would apparently do better to think of the "responder" as a sort of consensus ballot-box, activated by a multitude of "micro-responders" — each with its own (micro) switching system of one sort or another, and in danger of liquidation if its performance in office is not up to scratch, for whatever reason! This would be more accurately represented in Figure C4.4/1 if the "responder" and "switch mechanism" boxes were combined, though this would still not quite do justice to the supposed infrastructure. Anyhow, we may presumably identify this combined subsystem with the M^0L level which we were discussing earlier — and moreover this will mainly entail use of the *action* entities of M^0L.

Coming back now to Ashby's "gating mechanism" as depicted in Figures C4.5/1 and C4.5/2, we may recall that its purpose was to *select* a switching-mechanism (S_i) *appropriate to the current situation* or "sub-environment" (P_i). In terms of the present version of the theory, this would seem to mean selecting the *appropriate subset* of micro-elements (from those sensitive to the E_1 in question) and allowing only this subset to respond overtly to the stimulus pattern E_1;

223

224

---

[45] Here we are considering only those elements which are susceptible to the *particular* input stimulus E_1. There would presumably be many others which would remain passive here, but would be roused by other types of E_1.

though if the general "situation" had been different, then some other subset of micro-elements would have been chosen. Such a process may be likened to the selection of a sport-team specifically to suit the prevailing conditions of weather (etc.) on that particular day. It is incumbent on the selectors of such a team to "have in mind" alternative *subsets of criteria,* and to use the right subset for that set of conditions.

It may be that the appropriate criterion is very simple and straightforward. Perhaps the same pool of potential players is to provide a basket-ball team on Monday, and a team of rowers for Thursday; and conceivably the over-riding criteria for the two cases would be: *height* for basket-ball, and *weight* for rowing. Simple criteria like these can readily be expressed by *intensive* specifications:- "Tall men over here please, whoever you are!"; (and this seems to be very much the same thing as applying an "objective test"). But such decisions will normally be more subtle or "subjective", and depend heavily on experience; and this "experience" can be interpreted as a pragmatic evolutionary selection amongst arbitrarily established sets — defined *extensively.*

Whatever the physical mechanism is that holds such successful subsets of M^0L elements together, it will evidently be taking on the role of an M^0L "noun" element — acting as a link to M^1L action-elements, as depicted in Table C5.4/I. We may recall that the mechanism for forming a physical extensive set could be:
 (i) enclosure by a physical envelope such as a cell membrane,
 (ii) the somewhat improbable direct tethering to the M^1L linear element itself, and/or
 (iii) symbolic tethering by having "names" of the member elements attached to the M^1L element — and depending *partly* on intensive definition to give the "names" a meaning. See Sections C5.2 and C8.1.
While the enclosure method (i) would doubtless suit admirably for more-or-less permanent subsets like those envisaged for $M^{-1}L$, any potentially evolving system of subsets would seem to need a more flexible-yet-controllable arrangement such as symbolic tethering (iii).

Anyhow, this provision for control by the M^1L structure now leads us to identify this control-path as the "gating mechanism" postulated by Ashby. From there, it is a comparatively simple matter to use the above expositions to answer his conundrum as to how such gating-mechanisms might arise. We may suppose that the potential M^1L linear elements arise arbitrarily, in largish numbers, by some process of mutation. These will have inbuilt sensitivities to various situations, using input codings of a type which will turn out, in successful cases, to be different from those input patterns (E_1 in Figure C4.5/2) which are used for activating M^0L systems. The new type will be identifiable as E_3 in the diagram, but in any live situation it may well be a matter of some considerable subtlety to distinguish which input cues are to be considered as E_1, and which are E_3 as we shall see in Section C6.5. And then the situation is further complicated by the need to consider *evaluative* feedback-input: E_2 for the M^0L case, to which we may now add E_4 for the M^1L case — thus introducing another feedback loop into the configuration of Figure C4.5/2.

In addition to their specific sensitivities to incoming signals, these elements will also have definite inbuilt codings for "actions" of various types. But in the successful M^1L elements, these "actions" will apply *internally* to the structures of the M^0L level, and not to the motor-effectors as would be the case for M^0L actions.

So the answer to Ashby's third conundrum would seem to be: that the gating mechanism consists of a population of M^1L linear micro-elements, and these evolve into effective systems in an essentially Darwinian trial-and-error way. Provided that we can sort out the tangle of feedback loops satisfactorily, this process can be seen as being a learning procedure within the life of each individual concerned — and essentially operating on the same principles as the basic learning process used to improve the M^0L repertoires of responses. The crucial difference will be the destination of the action signals, and the nature of subsequent derivatives which will presumably

227

arise from the "inactivation" of such codings; but we may also expect other incidental differences such as a new pattern of sources for the input signals.

Before we move on, however, there is a rather important further point which should be remarked upon — albeit with unseemly brevity. If we can have a "gating-mechanism" system to bring orderly "purposeful" operation at the M^0L level, instead of the random re-settings used in Ashby's simpler "Homeostat" models; then why not have a *higher level* gating system to bring a methodical approach to the original one *at* M^1L? Why not indeed? Provided (once again) that we can cope with the extra complications of further feedback loops, we may well assign such control to an M^2L level; and then why not continue recursively to higher and higher levels (until lack of coherent structure in the organization of the outside world renders it unrewarding to put further structure into our modelling system)? It is tempting to suggest that, in principle, that is very much what happens in the human brain; and that it is this very recursive ability which gives to man his superior place on the scale of intelligence. Like most elaborate mechanisms, however, this arrangement lends itself to a new set of possible misfunctions; and this will be our chief concern in most of the remainder of this current work.

At last then, we have disposed of Ashby's third conundrum. Let us now move on and look at his fourth:-

Ashby's fourth problem — maintaining a balance between parts of the system

(4) Ashby was concerned that he had not explained how the body might maintain a reasonable proportional balance between those "parts" of the system which were in equilibrium at any given time, and those parts which were undergoing a change at that time — thus determining the pattern of changes in the next "instant". This would seem to be a much simpler question than his previous one. Presumably, like many well-known physiological devices for maintaining one variable or another within appropriate bounds, the process may be explained in comparatively straightforward *servo-mechanism* terms.

228

Our present theoretical context does rather complicate the issue however. To start with, Ashby was clearly thinking of his system's "parts" as being whole neurons (1960, Section 13/12); and while he might well be right as far as $M^{-1}L$ processes are concerned, we should now keep in mind the idea that for many other processes we are likely to be dealing with excitation-states at more-or-less discrete sites along linear molecules. Secondly, the hierarchical structure proposed above puts a rather new slant on his concept of local stabilities, as discussed in his Chapter 13; — not that this invalidates his argument here, indeed it could well be claimed that the revised interpretation adds weight to it, but it does alter the implications somewhat.

There is a third complication which amounts to a generalization of some points which Ashby himself mentions in passing. Signals cannot be counted on to add up neatly in any given way: arithmetical, algebraic (taking inhibition into account), Boolean logic, or whatever; (Ashby, 1960, Section 13/12; cf. McCulloch and Pitts, 1943). This has become increasingly obvious for real extended neurons in which the important signal components are presumed to depend principally on action-potentials (including refractory-period phenomena), exocytotic rupture of vesicles of transmitter chemicals into synaptic clefts, and other such mathematically "untidy" phenomena. The quantitative situation for linear micro-elements has scarcely been discussed yet, but there is little reason to suppose that the "addition" of signals would obey absolutely neat mathematical rules here either — though almost certainly there will be addition of some sort, if only in the form of mutual facilitation as in Figure C5.2/2, above. The situation could be particularly complicated if "optical" interference patterns between signals should play an important role (Pribram, 1971), possibly involving the coherent infra-red sources postulated for the micro-element theory (Traill, 1976b; and Part B, above).

229

Despite all this untidiness concerning detail, there does not seem to be any great problem here regarding general principles (unlike the fundamental issues raised in Ashby's third conundrum, above). At a practical physiological level, we may reasonably assume that such control mechanisms either depend on some natural self-limiting process around which evolution has "designed" the rest of the system, or else some system parameters are set genetically on a trial-and-error basis. (One component may be unmodifiably hereditary, but other aspects might be subject to learning in the manner previously described using the concept of mutation-within-the-nervous-system). Moreover, at a basic ultra-micro level of resolution, we might reasonably expect to find ultimately that the mathematical untidiness would disappear — though we would then be left with the same sort of untidiness as soon as we attempted to "put the parts back together again". This might then be best dealt with on a statistical basis — perhaps even using statistical mechanics techniques borrowed from physics.

There seems to be no pressing need to develop these ideas here in a generalized way; instead it will suffice to develop specific cases if-and-when they become important. However we may usefully draw attention to some places in which these concepts have already arisen:- There was the stability of mental schemata arising from their supposed "mathematical-group" closure-properties (Traill, 1975b; and the last two paragraphs of Section A1.5, above). There was also the phenomenon of positive reinforcement, the mechanism of which could be of the type sketchily described in terms of "tagging" (Traill, 1976b, Figure (v)b; Traill, 1976d, Section B). However any limit needed here will presumably be supplied by some obvious natural limitation; whereas Ashby's problem was to explain why only a sensible number of "parts" were active at any instant — neither too many nor too few. Of course, it may well be that there is a natural limitation here also; and conceivably this could take the form of a breakdown in effective coordinating communications whenever too many "parts" are trying to function simultaneously creating too much "noise" for each other's coherent infra-red signals, or whatever. And incidentally, a similar type of "jamming" phenomenon might be behind the "displacement" behaviour described by ethologists, in which the animal is "torn with indecision" between two mutually incompatible instincts — so he "ignores" *both* alternatives, and does some other (less pressing) activity! (Tinbergen, 1951).

This half-answer will suffice for our present purposes, so we may now turn to Ashby's fifth and final conundrum (1960, Section 17/11).

Ashby's fifth problem — Which **M**ⁿ**L** level should receive which input? — A topic for Chapt.C6

(5) Under the heading "Distribution of feedback", Ashby poses the problem of how any given disturbing change in the environment will be "steered through" *the appropriate* $E_i - R_i$ path, where E_i is the appropriate "essential variable" and R_i is the corresponding appropriate response. In terms of the linear micro-element theory, part of the answer will now be quite straightforward:- At any given level in the hierarchy which involves learning, the "paths" will be name-call-specified, with considerable precision, *in advance;* but many such specifications will be found to be non-adaptive, and will be eliminated in a Darwinian evolutionary process. However there is another part of the answer which remains anything but clear:- How does such a living system choose *which hierarchical level* to incorporate new information into? In other words, should a given input pattern be associated with E_1, E_3, or E_5, ... ; or as an evaluative feedback E_2, E_4, etc.? This is a major concern of Chapter C6, to which we will now turn.