



## Thinking by Molecule, Synapse, or both? — From Piaget's Schema, to the Selecting/Editing of ncRNA

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This paper is part of a project to find *sound biological explanations for human mental abilities*. The project soon split into two streams: .

**STREAM 1** focussed on realistic *coding-mechanisms for memory* — an area in which lab-work can only scratch the surface, hence rigorous interdisciplinary theory was invoked instead. This led to strong suggestions implicating *molecular coding* — ideas consistent with the theories of *Piaget* and *W.R.Ashby*.

**STREAM 2** emerged as the parallel problem of *intercommunication* — but its solutions then unexpectedly also offered explanations to other “unrelated” problems! (Moreover, lab-testing of *these Stream 2 ideas* is much more feasible, so we might now hope for such future investigations).

**A summary of the whole project is now available:**

<http://www.ondwelle.com/OSM12.pdf> . 1-1-10

<sup>†</sup> The only changes in this 2008 version are: (i) “Housekeeping” details, mainly on pages 1&2, notably updating the email address. (ii) **The addition of a new lengthy Supplement**, which discusses theoretical and philosophical issues relating Piaget to Kant, etc (iii) I had previously suggested the new word “acton” for the postulated *most-basic physical element* underlying Piagetian “scheme/schema” theory (page 7).<sup>8</sup> As that new word seems confusingly similar to “action,” I have since chosen to change it throughout, from “æton” into “taton” — with implications discussed at length in the new Supplement.

## CONTENTS

<b>Abstract</b> .....	<b>3</b>
<b>A. The Hidden Vitalistic “somehow” in Existing Brain-Models</b> .....	<b>3</b>
<b>B. Brain Dynamics Reconsidered</b> .....	<b>4</b>
<b>(1) Synapses may well have accomplice-mechanisms</b> .....	4
<b>(2) Hebb’s formal neuron</b> .....	5
<b>(3) Ashby’s choice-limitation, and its topological implications</b> .....	5
<b>(4) Penfield’s repeatable action-sequences</b> .....	6
<b>(5) Piaget’s repeatable “scheme” actions</b> .....	6
<b>(5a)</b> Analogy as a guidance about the nature of tatons.....	7
<b>(5b)</b> Action schemata as the basis for mental activity.....	7
<b>(5c)</b> Critique of mutationism.....	7
<b>(5d)</b> Circular or Linear (1D) order of action-coding.....	8
<b>(5e)</b> The possible involvement of RNA .....	8
<b>(5f)</b> Hereditary innate knowledge .....	8
<b>(6) Repeatable action-sequences of ethology</b> .....	8
<b>C. Brain Mechanism Reconsidered</b> .....	<b>9</b>
<b>(7) The search for physical 1D code-sequences</b> .....	9
<b>(8) Coherence and Equilibration in the Mind — and in Science</b> .....	9
<b>(8a)</b> Simple loops.....	10
<b>(8b)</b> Stability, half-life, & perturbations .....	10
<b>(8c)</b> Maths and self-consistency.....	10
<b>(8d)</b> Abstraction as a physical model with some rigid constraints.....	10
<b>(8e)</b> Piaget’s three types of knowledge .....	10
<b>(8f)</b> Science — its coherence and equilibration .....	13
<b>(9) Past trends toward claiming “schèmes = RNA structures”</b> .....	13
<b>(10) Need for separate Space-based and Time-based coding?</b> .....	15
<b>(11) ncRNA “junk”, which does not code for protein</b> .....	16
<b>(11a)</b> Background .....	16
<b>(11b)</b> The ncRNA Revolution.....	16
<b>(11c)</b> Editing — its implications for psychology and pathology.....	16
<b>(11d)</b> Intron and Exons — an unavoidable side-issue.....	17
<b>(11e)</b> ncRNA as regulators? — Controlling what? .....	18
<b>(11f)</b> Editing as a rival-or-associated regulatory tool?.....	18
<b>D. Technical Complications for a Molecule-based system</b> .....	<b>19</b>
<b>(12) Need for infra-red (IR) signalling</b> .....	19
<b>(13) Forgetting-versus-remembering</b> .....	19
<b>E. Some unexpected bonus-explanations arising from this approach</b> .....	<b>19</b>
<b>(14) Birds and bees — small brains, but some surprising intelligence</b> .....	19
<b>(15) Why such meagre neuron-regrowth within mammals?</b> .....	20
<b>F. Conclusion</b> .....	<b>21</b>
<b>Appendix: Piaget’s “Schème / Schéma” concepts</b> .....	<b>21</b>
<b>REFERENCES</b> .....	<b>23</b>
<b>Supplement of Feb. 2008 — Philosophers of Knowledge, and the historical context for “Tat”</b> .....	<b>27</b>
Summary of the <i>Piagetian</i> basic theory (sometimes elucidating beyond Piaget’s own account) .....	27
(E) “Taton” as the non-chemical name for those hypothetical “irreducibly basic” scheme-elements.....	27
(F) “Action first” — The “Tat” quote from Goethe — Was he a Piagetian a century before Piaget!?.....	27
Kant’s “Cop-out”. — The “a-priori intuition” as a mere shifting of the problem.....	29
The trick-ingredients from Freud/Piaget/Ashby, which make the Kant system viable .....	29
(H) The brain is one “epistemological system”, but there are others. Do they all use the same basic strategy? .....	30
Knowledge-mechanisms within tangible nature .....	30
Four different epistemological domains .....	31
A physical embodiment for Piaget’s Elementary “Scheme”? — The same question again. ....	32
Extending the Limited “Philosophers’ View” of Epistemology.....	32
Finally, a brief guide to reading Piagetian Literature .....	33
<b>REFERENCES</b> for the 2008 Supplement .....	34

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### **Abstract**

Synapses as we know them seem to offer no possible detailed explanation for higher mental abilities unless they are augmented by other more-active mechanisms. Obviously these mechanisms could well be molecular, but we need to be more specific than that.

Piaget tackled the problem from a different angle, applying psychology and epistemology (the theory of knowledge); and this paper seeks to extend his approach using whatever interdisciplinary insights science has to offer.

Guided by the findings in other epistemological problems (notably the explanation for evolution in terms of “DNA knowledge”), it seems that the basic physical element of thought-dynamics must probably be some sort of stringlike coding — like most of our data-storage, and like DNA itself. By elimination, it was concluded that these elements must be RNA, thus vindicating earlier largely-forgotten work by Hydén and others.

Quite independently, molecular biologists have meanwhile made the amazing discovery that about 97% of the cell's useful output of RNA does *not* generate protein (previously thought to be the principal role of RNA). Instead they have suggested various regulatory roles for some of this ncRNA, but there is clearly also scope for thought-related roles — and indeed we might see thought as just another form of regulation.

Additionally it is now clear that RNA is routinely “edited” — thus changing the meaning of the code, and often in an indispensable way. In fact this has important mental consequences, for whatever reason. Finally, science itself is an epistemological endeavour, so it seemed appropriate to comment occasionally on common misconceptions about “scientific method” and policies concerning it.

### **A. The Hidden Vitalistic “somehow” in Existing Brain-Models**

We have long assumed that the mind/brain will eventually be explained in terms of *synaptic mechanisms* between nerve-cells — these being taken as the basic elements of thought and cognition. But even if that is true, how could it really help us explain psychological phenomena?

Names like [Hull \(1930\)](#), [McCulloch & Pitts \(1943\)](#), [Hebb \(1949\)](#), [Blum \(1962\)](#), and [Edelman \(1987\)](#) come to mind — and more recently other neurophysiologists have made some further ground-breaking discoveries about synapse-dynamics — of notable importance to their specialized area of interest, ([Fields, 2005](#)). But even when the synapse itself has been fully explained, will that go far enough? Clearly it plays a vital part, and it might perhaps suffice-on-its-own to explain how a slug or woodlouse can manage to learn a simple discrimination through strengthening certain pre-existing random connections; but let us ponder some other example-cases:

(i) Consider single-celled animals<sup>1</sup> which obviously cannot have any synapses because that would require at least one extra cell, and yet they evidently have a ready-made behavioural repertoire — and, less crucially, they may also be able somehow to learn new behaviour as individuals.<sup>2</sup>

Likewise (ii) the genetically inherited behaviour traits of *any* animal species must also be encoded by a synapse-free medium, for at least part of its embryonic existence. Moreover:

(iii) This synaptic approach does not seem to have offered us any credible comprehensive model of *advanced human thought processes*. As far as I am aware, any such synapse-based account has far too many “somehow” caveats in it, without even any in-principle explanation. And that loose account is surely just a fig-leaf to cover our ignorance, or our tacit acceptance of vitalism or supernaturalism — which maybe comes to the same thing.

Against this, approaches such as Piaget’s *do* credibly tackle the niceties of advanced mental abilities. However they then leave a different “somehow”-gap at the micro-structure end of the problem-scale — failing to link up with synapses, or with any other material sub-mechanism. Thus it is the purpose of this paper to try close that gap: taking the Piagetian scheme-based system as given, but seeking to find a credible physiological basis for it.

But first let us take a step back — starting again with the synapse-model to see whether we can wring any relevant concepts from it, by generalization or otherwise. For one thing, we might ask whether we might have fallen into a false-assumptions trap concerning synapses — a wrong turn which could then have stopped us from making significant progress? There are several clues which might help:

## **B. Brain Dynamics Reconsidered**

### **(1) Synapses may well have accomplice-mechanisms**

Choice between alternative mechanisms is commonplace within biology and elsewhere. Moreover cooperation and symbiosis are also well-known; so it would not be too surprising to find two different systems-or-mechanisms operating in parallel, doing similar things, but with their own specialities — and maybe each might fill in the occasional “somehow”-gap for the other. For instance, their key difference might be one of scale (such as synapses on roughly a  $\mu\text{m}$  scale being complementary to another well-organized system on a  $\text{nm}$  scale).

Indeed even if the synaptic system does have the predominant memory-role attributed to it, we may still need to find what subsidiary outside-mechanisms aid its development, and exactly what rationale they operate by.

Moreover superficial appearances can be deceptive. The highly-visible Emperor may look as though he is the *one* person for a new ambassador to deal with — but there may well be two or three such persons.<sup>3</sup> Thus there could be an apparently-subservient Shogun who is actually calling the shots — or

<sup>1</sup> I.e. *protozoa* such as *amoeba* and *paramoecium* — (alias *ameba* and *paramecium*).

<sup>2</sup> Piaget’s *Biology and Knowledge* (1967: p.190 §13.2, p.252 §18.1), and Traill (1999 §3a(d)). Note that even if such protozoa could not learn as individuals, they still have a behavioural repertoire which had to be learnt *as a species* — and either way, these repertoires must be performed without any nervous system as we know it.

<sup>3</sup> Weiskrantz (1991) suggests there may be “multiple memory systems”. This might ultimately turn out to be true, but in the interest of simplicity, we will here look only at the *two* possible parallel mechanism-types already suggested:

(i) the traditional-synaptic system, including the autonomic system (sympathetic and parasympathetic hidden adjustments — which together arguably make up a three-or-fourway system on their own anyhow); plus  
(ii) a newly-proposed simultaneous system invoking a necessary stringlike *linear* coding (whatever it is made of, though molecular strings are likely candidates).

Meanwhile we might merely note an obvious *third* bodily communication mode: the Endocrine system,

at least making *some* of the major decisions. But if we don't even know that the Shogun exists, we are poorly placed to judge the local politics.

If synapses “S” need the services of assistant-mechanisms “A” at all (and surely they do), then we would be rash to prejudge the relative power of these two entities — or to assert that S is always dominant over A, when in fact their relative influence might depend on the job in hand. Surely then, it is *doubly important* to do our best to seek out any “assistant mechanisms” and to discover in detail what real roles they may play. Nor should we give up too easily when one hypothesis seems to have been discredited, and yet the overall picture still fails to make sense.

## (2) Hebb's formal neuron

Hebb explicitly suggested that his “formal neuron” need not necessarily correspond to the real neuron, but could instead be some other switching mechanism (Hebb, 1949) — even perhaps mechanisms at the molecular level (personal communication). Moreover we should perhaps take the best of both worlds and consider *both types operating in parallel*: ● some tasks such as pattern-recognition and attention-focus being fulfilled by the traditional synaptic system, while ● other tasks such as abstract thought (hitherto-unexplained) are left to molecular or other types of “formal neuron” — a division of labour and expertise, as just-now discussed in “(1)”.

## (3) Ashby's choice-limitation, and its topological implications

In his in-depth theoretical studies of decision-making systems, Ashby (1952/60, ch.10; 1956/64, sec.7/14-) pointed out that *too much choice is actually debilitating*; and that a skill such as balancing, is gained by learning to *limit or constrain* one's response-repertoire — at least within that task.

Any formal-neuron is faced with the tacit problem of succession-choice: “*which other 'neuron' should I pass the relay-baton on to? — or indeed should I prompt several?*” Too much message-passing may well produce an epileptic seizure, yet a ‘neuron’ which passes no messages would presumably be useless. Maybe the system quickly learns appropriate orderly pathways — though how the sometimes-necessary precision could be acquired is not entirely clear.

Alternatively a fixed sequence-path could have been set up in advance. After all, that is what computer-manufacturers do in designing their electronic-memory organization. Consider instruction #1334 (say): After doing its own task, it is preset to pass control on to #1335 by default (unless it constitutes an explicit instruction to do otherwise) — and so on through #1336, #1337, *etc.* Note firstly that there is no inherent reason why computers should be designed that way. — Manufacturers *could have* allowed programmers the “freedom” of having to nominate a list of immediate next addresses to follow after each instruction, with no “lazy” way out. But it is clearly more practical to have a clearcut lazy default convention, giving a preset next address — and indeed usually just *one* such next-location, as befits a one-dimensional approach.

Secondly it will usually be convenient to express such sequences by having the linked items *physically* adjacent, and that will mean having linear (1D) strings of code-items. And thirdly, such linear strings of physical code seem to be found within nearly all other systems using information-handling: written and spoken speech, DNA/RNA-genetics, TV-images(!), immunology, etc — even though these systems may *also* use other techniques when that seems helpful.

Fourthly, on *theoretical* grounds, one can argue for the advantages of using such 1D coding as the main technique within a knowledge system (Traill, 1999), whilst also using other connective patterns (2D, 3D, random, etc.) as an auxiliary method where appropriate.

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serving as a sort of chemical “postal service” to augment the “telegram-and-telephone” services which concern us here.

#### (4) Penfield's repeatable action-sequences

Cranial surgery on conscious patients offered evidence that the brain held organized ensembles of memory items in a physical form, that these could be stimulated and re-stimulated artificially, even when they seemed to have vanished from the reach of natural recall processes; (Penfield, 1958/1967; Penfield & Roberts, 1959).

Now our ordinary lived experiences have a sort of linear presentation to them, if only because that is how life presents itself to us, given that we have only limited attention at any one time; and Penfield's descriptions of artificial recall have the same linear feel to them. Whether or not that says anything about 1D storage of such impressions is a moot point, but it does at least suggest some sort of systematic structured organization within memory, and not just vague associations.

#### (5) Piaget's repeatable "scheme" actions

Whatever the merits or otherwise of those Penfield observations of the late 1950s, these accounts were mentioned by Piaget in several works<sup>4</sup> — and they did seem to enliven the Piagetian "*scheme-or-schema*" concept<sup>5</sup>:

In his early work, *Language and Thought* (1923), the word "schéma" was rather vague and scarcely different from our everyday usage of "scheme", which I here take as effectively the same term.<sup>5</sup> Such words are hardly used at all in his *Traité* (1949) as he is there much more concerned with the "operation" — the specialized type of "mathematical" scheme which is held to be operating *internally* on other schemes. For the most part then, he had been simply taking the schéma/schème (in its most elementary form) as a given.

So whether or not Piaget was influenced by Penfield during the 1960s, at that time he more-clearly envisaged *compounding* such elements into ensemble-schemes, which might then be re-modelled by assimilation and accomodation, or perhaps split into *smaller* compound schemes.<sup>6</sup>

<sup>4</sup> including *Biology and Knowledge* (Piaget, 1967), plus *Memory and Intelligence* (Piaget & Inhelder, 1968).

<sup>5</sup> Unfortunately "*scheme-or-schema*" entails several relevant types of ambiguity and a rather chequered history (see the Appendix, especially "*Ambiguity I*"). Luckily we can sidestep most of those old difficulties by now entering virgin micro-territory; but four points still deserve mention at this stage:

(a) The supposed distinction between the words "*schéma*" and "*schème*" (Piaget & Inhelder, 1966, 1968; Furth, 1969; R.L.Campbell, 2002 §37), is hardly tenable in practice generally. This "rule" is so frequently broken (especially in English translations such as "*Biol&Kn*" 1967/1971) that it confers no reliable benefit, and *e.g.* Piaget's earliest book on the subject ("*LTh*" 1923, French original) uses "*schéma*" throughout, contrary to the rule. Moreover the intended distinction is usually discernable from the context anyhow.

(b) As far as I know, Piaget does not offer a formal definition until Beth & Piaget (1966 p235): "*The scheme of an action is, by definition, the structured group of the generalisable characteristics of this action, that is, those which allow the repetition of the same action or its application to a new content.*" — Note that this is couched in observational terms, emphasizing overt actions. However the text continues, effectively conceding that we are dealing with a hidden mechanism: "*Now, the scheme of an action is neither perceptible (one perceives a particular action, but not its scheme) nor directly introspectable, and we do not become conscious of its implications except by repeating the action and comparing its successive results.*" Also see Piaget (1967 "*Biol&Kn*", p231): "...units of behavior that can be repeated in a virtually unchanging way and applied to situations or objects of various kinds."

(c) It may help to think of schemes as like *computer subroutines*. For both schemes and subroutines, some may act as feudal dictators, with encodings which can enlist-and-direct others — but the theory in this paper will centre on *simple "lowly" codings* with no such direct power over any of their fellows. (Piaget tended the other way. He was more interested in considering effects from the compounded *complex* cases; but that is another story.)

(d) The main aim of the current paper is to prompt a search for the hidden physical embodiment of the elementary "scheme" abstractions — (just as in the 1950s it proved possible to interpret the abstract "gene" concept in molecular terms, even though much detail still remained to be elucidated).



So what about those “given” basic elements<sup>7</sup> — the bricks which supposedly build up into compound-schemes/schemata? Although such elements are implicit in most of his works, Piaget did not grace them with any separate name, so I suggest the term *taton*<sup>8</sup> — to convey the notion of “element of action-encoding”.

The closest Piaget got to discussing this issue seems to have been in his *Biology and Knowledge* (1967/1972), and even then the *taton*-related comments appear only as “throw-away lines” while he is actually more concerned with other matters. Needless to say, this means that there is no systematic organization to his presentation of *taton*-topics; but I will try to summarize them here below, abbreviating the references into the format: {§[section].[subsection] p[Page in English edition]}. (Meanwhile we may note that Piaget’s own chief objective in this “*Biol&Kn*” was to demonstrate the similarity-or-identity between the growth-and-control procedures of both (i) the recognized physical structures-and-transactions of the body, and (ii) the seemingly intangible psychological transactions — both being seen as parallel epistemological processes, which also have a parallel in the development of scientific knowledge within society.<sup>9</sup>)

Here then are the *taton*-related notions which we might note-or-infer from Piaget’s work:

**(5a)** *Analogy as a guidance about the nature of tatons*

If we accept the *epistemological parallels* just mentioned<sup>9</sup>, then these offer some analogy-guidance about the nature of tatons — see (7) and (8) below, based on Piaget (1967) {pre-§6 p70 (Introduction to ch.3) — §13.2 p189 — §20.6 p331}.

**(5b)** *Action schemata as the basis for mental activity*

*Action schemata* may be seen as the basis for mental activity, (obviously fundamental to any Piagetian treatment): {§1.2 p.6-7 — §1.3 p.8-9 — §3.3 p33 — §5.7 p66-7 — §12.5 p181 — pre-§15 p215 (Introduction to ch.5)}

**(5c)** *Critique of mutationism*

His critique of *mutationism and combinatorial intelligence* — criticizing the idea that genetic change comes from alterations to the DNA within genes rather than the re-arrangement of how their effects are combined. There is some suggestion that Piaget goes too far here (R.L.Campbell, 2002 §95-97), and it will be debatable how much this might apply to tatons and their schemes. But if nothing else, such questions do at least help focus on the *taton*/gene concept as such, whatever the details. {§4.1 p39 — §6.4 p81 — §7.1 p87 — §7.2 p90 — §8.4 p114 — §16.5 p238 — §18.3 p259 — §19.2 p271-2 — §19.6 p289, 293-4 (footnotes) — §19.7 p299 — §20.5 p324}.

<sup>6</sup> That seems to be what he means when he says of Graham Brown’s work: “...that the reflexes splinter off as a result of the rhythm.” (Piaget *Biol&Kn* 1967, p220).

<sup>7</sup> That is the crucial question which concerns us here. Compounding is largely a separate issue which I discuss elsewhere (Traill, 1999), and which pre-occupies Piaget himself most of the time, directly or indirectly. In the present work, such compounding will be of little relevance except when we consider *stability of memory-records* later on.

<sup>8</sup> (replacing the previous suggestion of “acton”, see the new supplement below — p.27). Meanwhile there is a further complication: It is by no means clear that a single unsupported *taton* could ever serve as a Piagetian scheme — producing an effective action either externally or internally. For one thing, it is likely that a *unison-chorus-population* of *tatons* would be needed, if only to stop us being at the mercy of some single bizarre encoding-mutation. Secondly, some minimum amount of compounding might be needed before the ensemble could function effectively. If such turns out to be the case, then perhaps “*schemon*” could be applied to suchlike minimal compounds, or minimal populations.

<sup>9</sup> This question of parallel epistemologies is also discussed elsewhere — e.g. Jerne (1966); D.T.Campbell (1970, and 1974 especially the appendices), R.B.Glassman (1977), Changeux *et al.* (1984); Traill (1999 ch.4); Riddihough (2002); — and section (8f) of page 13 below. Also see the Supplement (p.31)<sup>54</sup> regarding Popper.

**(5d) Circular or Linear (1D) order of action-coding**

This is the notion that mental coding is in sentence-like strings (maybe sometimes closed into a daisy-chain loop). Piaget's evidence and the theoretical advantages: {§11.3 p158 — §11.5 p164-5 — §12.4 p177-8 — §16.3 p231}. Also see Traill (2000, §9.3).

**(5e) The possible involvement of RNA**

Piaget considered the then-fashionable idea, that RNA appeared to be involved in memory-storage in some way, and quite likely constituted the actual medium of memory-storage. One serious trouble was that it was far from clear how it could interact efficiently with synapses — it being generally assumed that these synapses were the one-and-only key to understanding brain-function. {§1.4 p12 — §5 p50 — §7.2 p90 — §8.3 p107 — §13.2 p189-90 — §19.5 p287 — §19.6 p289}.

Piaget may well have continued to bear the possibility in mind, but he evidently spent no more public effort on it. He did advocate experimental work...“into...even biochemical conditions of memory and learning (with the necessary intervention of RNA etc.), but such is not at all my line of country.” — and he evidently left it at that. {§5 p50}. Likewise “...confining ourselves to observable data, with the sole aim of finding out what knowledge is, not biological mechanisms.” {§20.5 p327}.<sup>10</sup>

**(5f) Hereditary innate knowledge**

— hence species-based instincts and behaviour traits. {§8.5 p117 — §11.1 p150 — §15.2 p219-20}. Also see his references to his “Three Forms of Knowledge” which includes hereditary {§19}. (The other two types are: learned {§21}, and logico-mathematical {§20}. All three are discussed further below in subsection (8e), complete with further references to *Biol&Kn*.)

**(6) Repeatable action-sequences of ethology**

Meanwhile ethologists were developing similar ideas, although their terminology and traditions were sometimes different. Their approach was to study animal behaviour in the wild, and try to determine the basic units and stereotyped patterns of behaviour — plus similar units-and-complexes for perception.<sup>11</sup>

E.g., to quote from Rose *et al.* (1972, p20) regarding what Piagetians might call *perceptual schemes*: “if a response is produced by a limited number of sign-stimuli, then there must be some mechanism within the animal ... This was recognized in the 1930s by Lorenz and ... von Uexküll, and they called such a mechanism ‘ein angeborenes auslösendes Schema’, a term which was later translated by Tinbergen as ‘an innate releasing mechanism’ (IRM).”<sup>12</sup> — However Rose *et al* then point out, in effect, that this description applies only to simple hereditary schemes which trigger an observable response, whereas a broader definition would be more applicable in their context.

Correspondingly, on the output (“motor”) side, there was the ‘fixed action pattern’ (FAP).

(In this present paper, our main interest will be in exemplars which are: (i) elementary, (ii) hereditary or mutated from a hereditary template, and (iii) with an imaginable significant output, even if it is not directly observable. In this we may differ from ethology's aims and practice, if not its actual terminology.)

<sup>10</sup> Newton had had the same problem regarding gravity: Based on observation plus mathematization, he formulated gravitational effects spectacularly well — but he famously declined to suggest a mechanism, writing “*hypothesis non fingo*” (“I do not formulate any hypothesis”), presumably implying that this next step was beyond him.

<sup>11</sup> These are surely just Piagetian motor and sensory schemes under a different guise — complete with the elementary/compound distinctions. Indeed the ethologists doubtless have prior claim to some of the innovations, and Piaget did cite them frequently by the time of his *Biol&Kn* (1967).

<sup>12</sup> Piaget too (1967 “*Biol&Kn*”) makes several references to Tinbergen, Konrad Lorenz and “IRM”.



## C. Brain Mechanism Reconsidered

### (7) The search for physical 1D code-sequences

In principle, the traditional actual neurons *could* form such strictly linear sequences — but they seem unlikely candidates in view of: (i) their profuse connectivity, (ii) the conceptual difficulty of explaining how the strict 1D connectivity could be inherited or otherwise set up, and then maintained, (iii) the lack of observational evidence for any such widespread 1D neuron-organization.

Crick (1989a) offers an interesting critique; e.g. “Neurons are slow, operating in the millisecond time range, and typically have many hundreds or thousands of inputs. Although many of them produce action potentials or ‘spikes’ whose distribution in time is not completely random, there is no obvious sign of precise pulse-coded messages.” — One could add that such attributes might actually suit tasks such as muscle-control, fine-tuning, or spatial pattern-recognition; but they are likely encumbrances for other mental activity such as logical reasoning.

In fact, the only plausible candidates for such stringlike entities all seem to be *molecular*<sup>13</sup>: DNA itself, RNA, PNA,<sup>14</sup> and protein — but which, if any? Reasonable straightness-despite-varied-elements could be one important criterion, which would tend to rule out protein (never mind any other objections to it).<sup>15</sup> Stability is perhaps an even more important consideration, and this would surely need to be controllable, allowing for *long-term memory* at one extreme, and *prompt forgetting* at the other (lest our minds be swamped with junk-information). That probably rules out DNA (which is identified with inherent high stability, and is also relatively inaccessible within its histone complex (Alberts *et al.*, 1983) — leaving us with RNA, the unlikely PNA, and perhaps protein occasionally.

It thus seems sensible, on these grounds alone, to adopt RNA as the best candidate for embodying the postulated second memory system. Maybe someone will later come up with a better suggestion, but meanwhile RNA seems worth pursuing as the most reasonable working-hypothesis.<sup>16</sup> But note that we could also contemplate a hypothetical *mixed system*: — E.g.(i) maybe using some protein. — E.g.(ii) perhaps allowing some reverse-transcription from RNA, yielding Long-Term-Memory as non-inheritable DNA, and thus freezing some aspects of an evolving RNA system. Nevertheless it seems prudent to concentrate on RNA at this stage.

### (8) Coherence and Equilibration in the Mind — and in Science

What is it about self-organizing dynamic systems which stops them from disintegrating? That was the key question posed by the original science of Cybernetics<sup>17</sup> (before that term came to be over-used by the popular media<sup>18</sup>). The answer lies ultimately in feedback systems such as:

<sup>13</sup> which would **also** instantly solve the inherited-scheme problem! — a matter of no small importance when one considers the problem of explaining how any synaptic coding of inherited behaviour traits could be reliably “pre-wired” from the orthodox DNA/RNA instructions for protein-manufacture.

<sup>14</sup> E.g. see Böhler, Nielsen, & Orgel (1995) — or papers and advertisements in *Nucleic Acid Research*.

<sup>15</sup> Protein is well known for its lumpy structures — secondary, tertiary, and quaternary (McGilvery, 1979 ch.3) — though it *can* form reasonably straight structures if its coding is sufficiently monotonous (which would however presumably conflict with any information-storage role). On the other hand, the supposed straightness of RNA is often compromised when it pairs up with “anti-sense” sections of itself — sometimes forming important non-protein 3D structures, notably ribosomes (Alberts *et al.*, 1983 p204; McGilvery, 1979 p.75).

<sup>16</sup> Note that this line of reasoning is totally different from the usual RNA-advocacy (which is based on lab-tests for RNA etc.). The present argument is a search for *any structure* which might conceivably fulfill certain technical requirements which seem (on informed logical grounds) essential for explaining advanced intelligence. RNA then becomes involved, simply because it seems to be the only credible candidate.

That is to say: Here we have *function first, then candidate*. In contrast, the lab approach will offer us some candidate, but we are then left wondering about its precise function.

<sup>17</sup> Cybernetics, feedback, autoregulation and related concepts, are liberally invoked by Piaget in his *Biology and Knowledge* (1967): {§1.4 p10, 12 — §3.3 p35 — §5.4 p61 — §14.3 p210 — §19.6 p293-4, 296 — §19.9 p305}.

**P** → signal which soon supports **Q**, → signal which soon supports **P**, the source itself.

*(8a) Simple loops*

Note that such a simple loop can be augmented by further loops, especially if they form a neat pattern in three-or-more dimensions. *E.g.* think of a “skeleton cube” made up of 12 matches, and yielding six short-path loops (each bordering one face). Such patterns could lead to *high stability*, provided the signals of the separate loops are compatible within their shared pathways. See [Thagard \(1992\)](#), and [Traill \(2000\)](#) for different approaches to the same theme.

*(8b) Stability, half-life, & perturbations*

For any real physical system, its *half-life* will depend on the ongoing battle of this self-sustaining feedback against any *disruptive perturbations*. Of course many would-be systems simply lack any adequate feedback-support, so they do not even endure long enough to be considered as “systems”. That elimination (like Darwinian evolution) explains why we are likely to find “cybernetic success stories” everywhere we look — until there is a new crop of novel perturbations to upset existing “permaculture” steady-states!

*(8c) Maths and self-consistency*

Mathematics is the art of devising sets of symbolic procedures which form logical loops — loops like the above, but which are meant to be *unfailingly* self-consistent (“perfectly coherent” and immune from perturbation) within that system — and in which every elementary action is reversible. That is what Piaget is talking about when he refers to a “group” — and meanwhile any incomplete-but-orderly system of this type is a “groupment”, translated as “grouping”. ([Piaget, 1949, 1952](#); [Beth & Piaget, 1966](#)).

*(8d) Abstraction as a physical model with some rigid constraints*

Such mathematization is an *abstraction*, but what does that actually mean? Let us take “abstract” as implying a *model*<sup>19</sup> of reality where some of its elements<sup>20</sup> are made permanent by *definitions-or-axioms* — stipulations which are imposed from outside the system, and leave no room for unformulated perturbation or any other awkward unintended variables. It should then be reliably clear whether the model-as-a-whole is self-consistent within its ideal setting — and if not, mathematicians will normally reject it in its existing form. But meanwhile note that, because real systems and mathematical systems are all subject to the same selection criterion of requiring at least some degree of self-consistency, mathematics and reality will often be good mirrors of each other in some respect. And when we need to understand our world, this may be the best-or-only ultimate means toward this goal, whether as individuals or as an enlightened society.

*(8e) Piaget’s three types of knowledge*

Piaget’s three types of knowledge — hereditary, learned/acquired, and logico-mathematical, see [Piaget \(1967 Biol&Kn ch.6\)](#), and subsection (5f) above.

(i) HEREDITARY. If we accept the proposed memory-role for RNA, then the mechanism for *hereditary-memory*, via DNA-to-(RNA/taton), is so obvious that we scarcely need to elaborate much on it here — though of course it is still hypothetical, and the tatons would presumably still need to assemble themselves into effective *scheme ensembles* (“*schemons*”, see the end of footnote 8 of page 7). (“Taton” is defined in the same section.)

It may also be worth noting that the ambiguous phrase “*la constuction de modèles cybernétiques, théoriques ou concretes*” (page 76 in the French original) was evidently translated the wrong way — implying misleadingly that cybernetics was *alternative* to both theory and the concrete, whereas it is actually *applicable to both*. {§5.4 p61}.

<sup>18</sup> See [Traill \(1999\)](#), endnote “f”.

<sup>19</sup> A model which is ultimately physical, whether in our *minds* or within our *social* knowledge-encoding.

<sup>20</sup> I.e. objects or functions or relations, though they would not all be fixed. *E.g.* in the usual interpretation of “ $y = mx+c$ ”,  $y$  and  $x$  are not fixed, but the relationship is; and of course so are the constants.

(ii) **LEARNED.** The next question of how new individual-learning might be *recorded* presents certain problems discussed at length elsewhere (Traill, 2000)<sup>21</sup> but the conclusions were briefly: • that natural knowledge-acquisition (at least in its early stages<sup>30</sup>) seldom-or-never uses “Lamarckian” procedures like tape-recording. • Such procedures require the intervention of a *designer* (an “alien” from a different epistemological domain!<sup>25</sup>); and nature cannot usually depend on such largesse (though society often can). • This leaves us with Darwinian-like trial-and-error, which is inherently robust but “wasteful of coding”. • If mental coding were molecular (rather than just synaptic), then that would increase the coding capability by many orders of magnitude, making Darwinian wastage much more feasible (and possibly also take some pressure off the synaptic systems which may well be performing other supportive tasks). Also see Changeux *et al.* (1984).

Thus, to express the idea radically: • If you “listen” to me, you yourself are re-creating my speech amongst your own pre-existing schemata — *and re-creating much else besides* (of things I might have said, but didn’t), extraneous material which you must then waste or recycle unconsciously. • Thus you have not “written” my ideas into your mind, but rather *selected* them from existing coding (schemata which are already available to you in some form, perhaps including random mutations) — *i.e.* assimilating and accomodating in real-time. • This seems to be in accord with both Piagetian and Darwinian principles, *and* to be technically feasible given present knowledge of the topic.

— At least that would have been the likely scenario in your infancy when your word-and-symbol function had just developed beyond the sensori-motor M<sup>0</sup>L into the M<sup>1</sup>L stage. However, as an adult, the situation *could be complicated* by M<sup>2</sup>L and M<sup>3</sup>L stages which enable you to marshall your lower M<sup>n</sup>L ideas in some more intricate way, which could affect the argument; (Traill, 1999 ch.8; Ashby, 1952). Here any M<sup>2-or-3</sup>L levels would effectively be “aliens” — agents from a comparatively independent epistemic domain — like the individuals within society, two paragraphs back. But despite such complications, we may nevertheless expect random-or-arbitrary Darwinian trials to govern such activities at some level or levels: M<sup>top</sup>L or whatever — and at a rate much faster than the unaided Synaptic System could offer.

In any case, none of these scenarios offer much encouragement to the Lamarckian “tape-recorder” model of memory-recording — though they do not necessarily rule it out as a subsidiary device, and indeed society (aided by individuals) does offer it as an extra. After all, industry does market tape-recorders!

(iii) **LOGICO-MATHEMATICAL.** That leaves the somewhat mysterious *logico-mathematical* type of knowledge, discussed in Piaget’s *Bi&Kn* (1967) in numerous poorly-connected places, notably: {§1.2 p6 — §2.1 p14-15 — §3.3 p35 — §4.3 p47 — §5.2 p54 — §5.3 p59 — §5.6 p65 — §5.7 p67 — §6.1 p74 — §11.6 p167 — §18.4 p265 — §19.7 p299 — §20 p305 (heading) — §20.5 p321-2, 327 — §21.2 p336 — §21.3 p339-341}.

And what are we to make of claims like the following? — “...it is characteristic of logico-mathematical operations that they have an internal necessity attributable to their complete reversibility (and therefore not physical)” — ({§1.2 p15}, my underlining).

This sounds uncomfortably like *vitalism* — or like the very human temptation to attribute magical spirit-powers to arcane mathematical symbols like: ∫, ∂, ∞, ∞<sub>0</sub>, and √-1. However I believe we can briefly • explain the “necessity”, *and* • re-interpret the “physical”:

(α) *Necessity* of the “mathematical” component. As we saw in subsection (8c), (page 10): only certain feedback configurations within an ensemble will allow that aggregate to last long enough to be counted as a system; — and this effectively applies to mathematical systems too (as well as to the “physical”). That all suggests that there will be a reasonable chance of fortuitously matching the two

<sup>21</sup> For a traditional synapse-only viewpoint of such problems, see Dingman & Sporn (1964), with an emphasis on the absence of ribosomes at the synapse, *etc.*

types faithfully, and hence of allowing the one to predict and explain the other — at least until they become unduly complicated.

But we can take the argument further. If this coherence-test is broadly interpreted to cover both internal *and* external feedback, it is arguably the *only* criterion for learning-success (at least at the initial stages of any epistemological/learning process when there is no deliberate tutor). That then is a key issue in explaining the origin of knowledge (or of life), starting from nothing but random flotsam *plus* the universal reality of “mathematical” coherence-properties. (Traill, 1999 ch.4; D.T.Campbell, 1970; Thagard, 1992).

(β) So, can-or-must a mathematical system be *physical*? Let us first try to work out what it actually is, and only then worry about its supposed physicality:

Of the “[...√-1...” symbols, it seems that each is code for a list of sub-actions combined into one composite action (a compound schema). But let us take the simpler example of “2”. The recipe would then be *somewhat like* this: “Put one cake into this box, and then put another cake in as well. Now watch how Tom yawns, and then yawns again. Next find what is common to these two activities — and *abstract away* (take pains to ignore) *every other feature*.”<sup>22</sup> (And what happens when we systematically fail in such editing tasks? Could that be the basis of delusional psychosis?)

On the one hand then, mathematical concepts seem to be like other schemata — essentially action-patterns, dubiously physical<sup>23</sup> in themselves, but arguably captured and maintained as physically-embodied encodings (using *tatons* within the individual mind, and/or *word-based explications*, mainly in writing, within the separate-but-intercommunicating domain of society-at-large).

On the other hand though, any such schema contains a constraint which insists that certain features are guaranteed to be unchanging<sup>24</sup> and hence reliable and infinitely repeatable — forbidden to accommodate, while still enjoying equilibration within itself.

Anyhow, let us return to the main point of this paper, the search for that hypothetical “*taton*” introduced in section (5) on page 7. Thus the importance of the above discussion is to show that we might reasonably expect all three types of Piagetian knowledge to be explicable in terms of the *taton* elements. Of these three, the mathematical case is complicated by an extra contribution from the inevitable nature of dynamic systems nomatter whether they reside within the observer or the observed. In the simpler cases, this free source of templates makes it so easy to continually re-invent the wheel that minimal hereditary instructions are needed on such points, and hence we are tempted to see some vitalistic guidance behind it all. If we can resist that vitalism-temptation, then the 1D *taton*-model appears to be both feasible and promising. (That appeal to equilibration-criteria applies to any type of schema-activity based on the here-proposed molecular system. One might ponder whether it could also apply to the traditional *synaptic system* taken alone; — that is not our concern here, though the evidence discussed below in subsection (15) does offer some meagre support.)

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<sup>22</sup> In many circumstances we would not need to be told to abstract away the extraneous. Our natural tendency to equilibrate (seek internal coherence amongst relevant schemata) would eventually tend to do the job for us — and probably at an unconscious level. As Mach once remarked, such cognitive scaffolding was forgotten about long ago, (if indeed society was ever conscious of it); and in default we pride ourselves as having a sort of transcendental cleverness — somehow above the mere physical.

<sup>23</sup> Such *schema-actions* (as such) are probably not material in the sense of being composed of atoms, but arguably they are made up of dynamic signals analogous to radio-instructions. But are these wave-based signals “*physical*”? I would say “yes”, though arguably that is merely a semantic matter.

<sup>24</sup> immutable — either because of a natural overwhelmingly stable equilibration (like the *group* properties of solid objects (Piaget, 1949)), or by conventional fiat shared by the users of that schema, and maintained over time unless deliberately and explicitly altered by common consent — which effectively creates a new-and-different mathematical concept anyhow.

**(8f) Science — its coherence and equilibration**

*Science* is a *social* institution, and (as such) not directly the province of individuals. In fact there seem to be at least four different knowledge-gathering domains (epistemological systems): • The brain of the individual. • Science as the “brain” of society. • DNA and genetics as the “brain” of species survival; and • The immune system as the “military intelligence” of the body’s defence system. — Jerne (1966); Piaget (1967 {§3 p70 — §13.2 p189}); D.T.Campbell (1974); R.B.Glassman (1977); Changeux *et al.* (1984); Traill (1999 ch.4); and Riddihough (2002) — as already mentioned<sup>9</sup> on page 7.

Moreover there are good reasons for supposing that all these domains operate by the same *formal strategy* (though sometimes *embodied very differently*, and on vastly different time-scales), thus providing credible analogues for each other, provided we take care to account for the special circumstances of each.<sup>25</sup> (*Ibid.*)

In that case we can understand why Piaget expected our *Scientific Method* to apply equilibration criteria (alias “internal coherence”) in its attempt to reach the truth. However during much of the 1900s, the ruling empiricist-positivist-behaviourist doctrines frowned upon any evidence other than direct observation (even though we now know that observation itself actually has to depend on the “taboo” process of equilibration/coherence!). So Piaget’s restrained attack on such doctrines was no mere aesthetic distaste, but a well-founded objection. — {§4.3 p47-48 — §12.2 p175 — §21.2 p338}.

In principle he has since been vindicated. As R.L.Campbell (2002 §02) puts it: “For nearly 30 years, his ideas were completely out of favor in behaviorist-dominated American universities ... But Piaget outlasted behaviorism, and by 1960 his ideas were being jubilantly rediscovered...”. However, despite this Kuhnian revolution<sup>26</sup> which supposedly ousted the excesses of strict empiricism and positivism, I am not convinced that the Kuhn/–/Piaget lessons have yet penetrated into all the many depths of academic policy-making.

**(9) Past trends toward claiming “schèmes = RNA structures”**

As mentioned above in (5e), Piaget himself once looked as though he might be considering this RNA/scheme identification (1967 {§13.2 p190}). There he cites some unidentified rat-investigation by Hydén, and three other studies implicating RNA in memory: Babich *et al.* (1965a, b), and Fjerdingstad *et al.* (1965); but also a “fail-to-replicate” report by Gross & Carey (1965). However I am not aware of any clear indication that he seriously considered RNA as *the key* main agent; and he probably never even questioned the assumed-monopoly of the synaptic system — and this acquiescence could have obstructed any equilibration of any ideas about such submechanisms. In any case, he seems not to have taken the matter any further himself.

Beyond that, I mention just a few of the many other main works on this topic: Egyhazi & Hydén (1962), Gaito (1963, 1966b), Hydén (1967a, b, 1977), Cupello & Hydén (1978). Also collections • Gaito (1966a), and • Ansell & Bradley (1973) including, notably, E.Glassman & Wilson (1973).

<sup>25</sup> One important special circumstance is the conspicuous overlap between individuals and society — which notably allows *real designers* (from the individual domain) to intervene in the otherwise Darwinian trial-and-error workings of society. However any close look at history will show that such intervention has often made surprisingly little-or-sporadic impression, so we should not overemphasize this overlap effect. (Tolstoi’s *War and Peace* makes relevant comment on such matters — not to mention the prophets of the Old Testament!)

<sup>26</sup> Kuhn(1962) — though this was just the best-known of numerous critiques on the conventional wisdom about Scientific Method. The actual “revolution” went on till about 1978, by which stage it had gained lip-service acceptance. (Traill, 2000 chs.1-2).



(Meanwhile the topic also had a considerable following at a less serious level, with something of a cult-following regarding experiments on flat-worms (*planaria*)<sup>27</sup> — with its own periodical *The Worm-runners' Digest*, see McConnell's anthology (1965).)

Given all this evidence it is hard to believe that RNA is not involved in memory-processes in *some* way, though it is perhaps less illuminating on just what that role might be. In any case the topic had mostly lost its appeal by the 1980s, though it may now be staging a comeback with an altered emphasis — see section (11), page 16ff.

Just what caused this fall from grace? My first guess is that it was a mixture of (i) The economic-politics of funding,<sup>28</sup> (ii) Competition from other suddenly-more-fashionable topics such as genetics, which was then seen as comparatively unrelated. Also (iii) unrewarded attempts to fit RNA theory to the workings of evolving *synapses*, in a subsidiary role as a supposed manipulator of those synapses — see Crick (1984, 1989a, 1989b) for critiques.<sup>29</sup> This perhaps led to a misunderstanding in which *all features* of the “RNA-governs-synapse” model were rejected simultaneously — thus arguably “throwing the baby out with the bathwater”. In contrast to that, of course, the present paper envisages a significantly different memory-role for an RNA-type encoding as a *text-like storage/emitter/receiver in its own right* (a rather self-contained second system); and in that case we would not be surprised to find it ineffective in alternative menial roles.

Moreover all such debates as (iii) are complicated, perhaps tacitly, by the problem of how learning could come to be encoded or “written down” — and we have seen (in subsection (8e.ii) and its references), that RNA arguably offers a more credible Darwinian-selection basis for realistic natural encodings of this sort.<sup>30</sup>

My second guess is shaped by a science-journalist rumour. This has offered its own plausible explanation for the neglect of RNA-based memory, and suggests that the collective thinking ran like this (based on a dubious-but-unshaken premise): “*All sensible brain-theorists have intuited all along that the neuron/synapse was REALLY the basic memory unit. But there was that problem of the doctrine that adult neurons never reproduce, which meant there was inadequate mechanical-scope for changing the memory code synaptically — so one was forced to look instead to RNA coding as a second-best alternative. However it has since been shown that the ‘irreplaceable neuron’ doctrine is false.*<sup>31</sup> *That means we can now all thankfully go back to the neuron/synapse model, and forget those memory-molecule works of investigators like Hydén.*”

<sup>27</sup> also alluded to by Piaget (1967 *Biol&Kn* p189): “*The famous question of the Michigan planarians*”.

<sup>28</sup> About 1978 there was a crisis in economic theory. Stagflation was not supposed to happen, and yet its presence was all too evident, (Stein, 1982). In the resulting policy-panic, that presumably led funding bodies to tighten the research purse-strings overall — so any long-running projects which did not really seem to be getting anywhere were likely to be doomed.

Indeed I had previously noted a similar loss of interest in the early 1980s, in long-running questions of *myelin geometry* — a separate-but-related topic discussed in Traill (1999, 2000, 2005).

<sup>29</sup> The situation has altered a little since then, with the discovery of 7H4 — an RNA which evidently does have some synapse-interaction role, (Velleca *et al.*, 1994). (These authors had expected to find a protein agent, but were surprised to find RNA instead.)

<sup>30</sup> This Darwinian account does not necessarily kill off the “Lamarckian tape-recorder” model as a parallel alternative, provided it can find a *resident “designer”* in a sufficiently-different domain — and Piaget’s “operation” schemata might just offer this service during *later* development. Such schemata might well manipulate some of the recently discovered RNA-editing processes discussed below in section (11), especially (11f).

<sup>31</sup> This is discussed below in section (14). However although this capability now clearly exists, nature actually uses it much less enthusiastically than we might have expected (see section (15)). Thus the “thankful” return from flirting with RNA-coding just might have been premature.

Indeed Hydén and his associates themselves clearly held grave doubts about the direct memory potential of RNA — doubts expressed in conference papers such as those in [Ansell & Bradley \(1973\)](#).<sup>32</sup> But then their doubts existed within the context of my first guess, especially “(iii)” *the writing problem*. Perhaps all these factors played their part, and it is probably prudent to bear them all in mind if one is seeking to debate their implied conclusions. But let us now move on to other considerations:

### **(10) Need for separate Space-based and Time-based coding?**

Life-experience is largely based on the objects *and actions* which we encounter. We usually notice the objects but often overlook the “less tangible” actions; and yet Piagetian theory depicts the actions as more basic. (We could likewise see this as a contest between *nouns and verbs*). Anyhow my point here is that *both* mind-sets are important within psychology and elsewhere — the static “noun” and the dynamic “verb”, and I offer that as an analogy in support of a postulated similar dualism within RNA-like coding, as follows:

First note that *ribosomes*<sup>33</sup> are the body’s “machine-tool makers” at the micro level — but on their own they lack instruction. The accepted elementary-textbook role for most RNA is to act as messengers (“mRNA”) to instruct those ribosomes on just how to produce *specific pieces of “hardware”*; and the ribosomes then do this (according to the given mRNA recipe) by assembling specific protein strings which then buckle and fold in predictable ways thus producing “lumps of solid machinery”. These protein lumps obviously have their identity in 3D space, with *x,y,z* coordinates, and made up of amino-acid “bricks”. That then fulfils the “noun” task.

But what about the “verbs”, with their time-coordinate: *t* ? One new difficulty here is that such coding-systems must operate in “*real time*” — coping with rapid signals as they arrive, with no scope to pause for “research” or for marshalling raw materials reliably. On the other hand, there might now be no need to marshal amino-acid “bricks” for protein production. In fact the input/output would now presumably consist of “immaterial signals” instead, such as an ordered sequence of infra-red quanta or their waves — requiring energy sources only — no “bricks”.

Thus, in place of the relatively cumbersome ribosome, one might envisage (say) a simple phonon-excitation travelling along the RNA (perhaps a double-stranded “hairpin” fold), stimulating an ordered sequence of well-timed quantum emissions according to the coded sequence on the RNA. These synchronized emissions would then collectively<sup>34</sup> comprise the verb-like product. Such a system would seem to be very close to the sought-for “*taton*” — the physical element-embodiment of Piaget’s scheme (introduced above in section (5), page 7).<sup>8</sup>

As a variant on this theme, the emissions might also (perhaps incidentally) generate ephemeral optical interference patterns, such as those needed for hypothetical *holograms* ([Pribram, 1966](#)), or for the postulated “*moat*” *barriers* around growing myelin ([Traill, 1999, 2005](#)).

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<sup>32</sup> The usual rationale for this doubt was the lack of direct evidence — though such logic is reckless, since it merely expresses the indecisive “not proven” verdict as in Scottish law. More plausibly, the real cause for doubt may be that there had been no coherent suggestions as to mechanism — especially the vexed question of the supposed “write-down” mechanism — problems which the present paper is seeking to solve, at least in principle. (I might add that similar doubts should also be applied to the traditional synaptic account; but the obvious existence of *some* empirical detail does make provisional acceptance easier, despite the logical gaps.)

<sup>33</sup> As already noted, ribosomes are actually made from specially dedicated RNA — “rRNA” — but that need not affect the present argument.

<sup>34</sup> It is unlikely that memory would ever routinely depend on any *single unsupported* molecular encoding; see the second part of footnote 8 (on page 7).

## (11) ncRNA “junk”, which does not code for protein

### (11a) Background

It is well-known that protein is assembled according to coding sequences held on DNA, and transferred via RNA; (e.g. Miller, 1973; McGilvery, 1979 ch.5; Alberts *et al.*, 1983 ch.5). However only some of the DNA coding ultimately results in protein, hence much of the intermediate RNA-coding produced was, until recently, generally assumed to be mere random waste left behind by the evolutionary process. (“A minor inefficiency of no great significance”). Anyhow this “waste product” was known as “ncRNA” (non-coding<sup>35</sup> RNA) — or simply labelled as “junk”. However this knowledge was based on studies of bacteria, and it turned out that the situation in higher animals is *drastically different statistically*, especially in humans:

### (11b) The ncRNA Revolution

Brannan *et al.* (1990) raised the possibility that a certain particular gene (H19) might generate a *non-junk endproduct* which was RNA and not protein. Then within eleven years, Mattick (2001) was claiming that in fact more than 97% of the human genome was yielding this sort of *usable* ncRNA-endproduct. This 97% is clearly a major turnaround from the previous 0–3% assumption, and it certainly calls for a much more serious consideration of RNA and its likely applications. (Significantly perhaps, some specific RNA agents are found only in the brain; e.g. at least four “snoRNAs”<sup>36</sup> in the mouse, and two in humans (Cavaillé *et al.*, 2000).)

As a further unexpected phenomenon of likely interest to psychology, there is a considerable amount of selective “editing” performed on the emerging RNAs. The four Watson-Crick nucleotides (symbolized by A, G, C and U) are open to methylation, or de-amination, or other alterations — effects which change the coding to something a bit more exotic. For instance “A-to-I” editing changes Adenine to Inosine; and there are many other biochemical possibilities, e.g. see McGilvery (1979 p79-81). Also see (11c) and (11f) below for some likely applications of such editing.

### (11c) Editing — its implications for psychology and pathology

Moreover such edit-changes have evidently become quite important during evolution. Thus if the relevant edit-enzyme (ADAR) is missing, “invertebrates show ... behavioral defects, [and] ... mice die...” (Levanon, Eisenberg *et al.*, 2004), citing Tonkin *et al.* (2002), Palladino *et al.* (2000), Wang *et al.* (2000), and Higuchi *et al.* (2000). Other psychology-related effects of edit-change reviewed by Levanon *et al.*<sup>37</sup> are: *epilepsy in mice* (Brusa *et al.*, 1995), *depression* (Gurevich *et al.*, 2002), *amyotromic lateral sclerosis* [ALS, a motor-neuron disease] (Kawahara *et al.*, 2004), and *gliomas* (Maas *et al.*, 2001). Mattick (2004) adds “..., autism and schizophrenia”.

Obviously the *behaviour defects* could plausibly be attributed to a specific schema-template deficiency (perhaps providing poor raw material for the building of complex schemata);<sup>38</sup> and the *mouse-deaths* might perhaps also be blamed on the failure of Piagetian schemata<sup>39</sup> — if they are

<sup>35</sup> It is vital to note that “non-coding” here refers literally only to *coding for protein* — the task originally assumed to be the only worthwhile purpose for coding (apart from known structural applications: rRNA and tRNA, which were considered separately). Following subsections should make it clear that, despite the “n-c” label, such RNA regularly does carry important coding — though obviously for tasks other than protein-making.

<sup>36</sup> “sno-” means “small nucleolar-”. Also note that “imprinted” (in the paper’s title) differs from the psychology/ethology meaning and refers instead to a genetic-identification “switch” in the genome, carried from individual parents.

<sup>37</sup> and a later update-paper by the same team: Eisenberg *et al.* (2005).

<sup>38</sup> see the discussion of “tatons” as basic elements of schema-coding — in section (5), (page 7).<sup>8</sup>

<sup>39</sup> maybe schemata or tatons at a *pre-sensorimotor* stage — M<sup>-1</sup>L — whose failure was predicted to be “totally disabling” (Traill, 1999).

indeed embodied as RNA segments. (Note that these examples point to *both ends* of the “psychosomatic” range.)

Perhaps then we should not be surprised that some such specific effects are apparently peculiar to primates (Eisenberg *et al.*, 2005) — maybe related to Piaget’s higher stages of development and/or the recursion which may be involved therein (Traill, 1999 ch.8). However, given present unitary assumptions about the nervous system, it is perhaps not too remarkable that no-one in this biochemical field seems to have suggested any RNA role for *mental* activity as such — except perhaps as the agent for influencing synapses, (Fields 2005).

*(11d) Intron and Exons — an unavoidable side-issue*

Gilbert (1978) reviewed *Cell* 12(1) and nine other 1977(+) papers which had revealed the surprising finding that a *gene* was not only a “sentence-rather-than-a-syllable”, but that this sentence was usually interrupted by intrusive extraneous material — like rude interjections during a speech. Gilbert accordingly coined the terms “intron” for the intruding sections, and “exon” for the underlying gen-text. See also Chambon (1981), and Alberts *et al.* (1983: pp203, 414-421).

But why were the introns there at all? Some possible reasons might be: (α) They may just be blemishes with no intrinsic significance. Maybe the genetic system could easily deal with such disruption (just as computers often unobtrusively cope with files whose parts are “fragmented” across the hard-disk and separated by extraneous junk). And if the system is robust in this way, then why worry? That excuse might perhaps be valid if such intrusions were rare; but as they are actually common despite the evolutionary pressure-for-efficiency, this “tolerated benign parasite” role seems improbable — and even computers sometimes need a “de-frag” to tidy-up such aimless disorder.

(β) As above, but now seeing the introns as actually carrying useful late-addition coding which had to be slotted in somewhere — and maybe one place was as good-or-bad as another.<sup>40</sup> Or (γ): The intron-intrusions might actually be serving a *useful purpose as intrusions* (Chambon, 1981, p55 col.3) — thus effectively serving as punctuation. Present indications are that both these last two explanations are probably true simultaneously, so the introns would often-or-mostly serve this dual role — *extra ncRNA-code “within a comma”*, so to speak.

For historical reasons, it is easy to assume: — [i] that introns are the exclusive source of ncRNA (and no other meaningful RNA such as mRNA), — [ii] that exons are the source of all other coding (but never of ncRNA), and — [iii] that we can identify the difference between introns and exons independently of these criteria (notably by observing the excision of introns and the *splicing* which then closes the gap in the exon).

**IF** these assumptions hold good, then there is no *practical* problem if we talk loosely of introns when we really mean ncRNA, or vice versa. However such tidy assumptions have been seriously challenged, so we might need to use more care. (Darnell, 1985; Tycowski *et al.*, 1996; Hurst & Smith, 1999).

For our present purposes in this paper, we are primarily concerned with the role-type of the RNA (nc-, m-, or whatever) — but it seems largely irrelevant to bother about just where these come from, which is why I have tried to avoid talking about introns and exons. Certainly there is a high correlation, so we can properly look to introns as a likely source of most ncRNA, as long as we do not press that notion too far. And maybe the time will come when the evolutionary history of such things might come to offer important clarification — especially related to gene-proximity and cross-talk. Meanwhile, however, I shall probably continue to emphasize the molecule-elements themselves (rather

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<sup>40</sup> There is some suggestion that new segments of DNA information will tend to be reasonably close to existing sites with which they need to cross-talk. Moreover, to complicate matters, this proximity may extend to “adjacent” chromosomes since there is now a case for believing that there is a preset daisy-chain sequence for chromosomes in their natural state, thanks to initial work by A.I.Shchapova; (Lewin, 1981).

than their intron/exon provenance, which I temporarily see as a confusing distraction in the present context).

**(11e)** *ncRNA as regulators? — Controlling what?*

If indeed 97% of the genome consists of ncRNA as mentioned in subsection (11b) above, and if we agree that this is not just junk, then we should obviously try to identify more clearly what roles it may be serving. Here are some opinions expressed more-or-less confidently (and more-or-less explicitly) by various workers in the field, listed chronologically:

- [a] *ncRNA* → **synaptic regulation**: Velleca *et al.* (1994)
- [b] *ncRNA* → **exon-ncRNA control**: Mattick & Gagen (2001)
- [c] *ncRNA* → **gene-or-transcription regulation**. Mattick (2001), MacIntosh *et al.* (2001), Dennis (2002), Hare & Palumbi (2003), Mattick (2003), Pang *et al.* (2005) — whereas it had previously been assumed that protein would serve this sort of role, e.g.: Alberts *et al.* (1983, p449, p455), and Changeux *et al.* (1984).
- [d] *ncRNA* → **mRNA (messenger RNA) control**: Morse *et al.* (2002)
- [e] *ncRNA* → **mitosis-and-meiosis regulation**. Hall *et al.* (2003)
- [f] *ncRNA* → **precise-structure regulation**. Mattick (2004) — who points out that this probably becomes necessary in the face of escalating complex-connectivity. It could also be associated with optical interference patterns as a scaffold for the structure (Traill, 2005, 1999).

At this stage there seems no compelling reason to dismiss any of these suggestions. They may well all be true for some subset of the vast population of ncRNAs. Further investigation will no doubt continue.

Meanwhile there is now a case for suggesting a further role for ncRNA — another case of RNA controlling other RNAs — and embodying such examples as Piaget’s “operation” schemes acting upon the more menial sensori-motor schemes. Or to express it more generally: M<sup>n</sup>L elements exerting control over M<sup>n-1</sup>L (or other M<sup>n</sup>L elements). This offers the potentially radical step of moving beyond “bodily” metabolism into the realm of mental mechanisms (or what some might see as “psychic” metabolism); thus:

- [g] *ncRNA* → **taton**<sup>41</sup> **underlying the Piagetian schema**.

— though we might contemplate the possibility that the bodily/mental division might later come to be seen as overrated, such that the mRNA code for construction work, and the ncRNA code for muscle-twitch (or for introspection) might all have a close formal similarity. As noted earlier in (11c), any such formal similarities would have obvious implications for our possible understanding of psychosomatic phenomena.

Meanwhile, recent researches have uncovered yet another relevant complication, which we should perhaps look at separately:

**(11f)** *Editing as a rival-or-associated regulatory tool?*

In addition to ncRNA as a possible regulator, subsection (11b) also mentioned “editing”, a tampering with the nucleotides (typically converting “A-to-I”). Again then, we should try to identify what roles this may be serving, and here again are some opinions expressed chronologically as in the last subsection:

- [h] *Editing* → **gene-expression regulation**: Paul & Bass (1998)
- [i] *Editing* → **chemical-neurotransmitter receptors — control of**: Maas *et al.* (2001)
- [j] *Editing* → **dsRNA protection (stabilizing double-stranded RNA)**: Levanon *et al.* (2004), Eisenberg *et al.* (2005)

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<sup>41</sup> For “taton” definition, see page 7.



And, as with ncRNA, the newly acknowledged coding possibilities may also be applicable within the extended Piagetian mental-theory discussed earlier — though this time the likely implications are less clear; thus:

- [k] *Editing* → **selection of tatons**<sup>41</sup> *underlying the Piagetian schema?*
- [l] *Editing* → **adjusting the communication between tatons of a Piagetian schema?**
- [m] *Editing* → **pseudo-“tape-recording” onto tatons** — the unlikely Lamarckian case (which is probably nevertheless assumed by most lay opinion, though obviously not yet with ta tons in mind).

#### **D. Technical Complications for a Molecule-based system**

##### **(12) Need for infra-red (IR) signalling**

Recall the discussion in section (10) about “*verbs, real-time, phonons*” and other dynamic entities, distinct from static “bricks”. This raised the question of how signals could travel efficiently and promptly between the postulated *molecular* sites. Crick’s critiques, quoted above in section (9), apply even more strongly for coding at the molecular scale, and the traditional millisecond neural-“spikes” would be quite unsuitable for routine molecular use — though clearly there would have to be some sort of interface at some stage, if there are indeed two systems as postulated here.

It turns out that there are many cogent reasons for suspecting that the intermolecule signal-carrier would mostly be short-range infra-red (IR), chiefly travelling through fatty tissue such as myelin — the insulation around axons. Here the myelin would be serving as a coaxial fibre-optic cable in addition to its recognized task of permitting saltatory conduction for the synaptic system.

This issue has been much discussed elsewhere (Traill, 1988, 1999, 2000) so I will not pursue the matter further here, except to note that it has, in itself, suggested unexpected solutions to some long-standing problems in other fields (Traill, 2000, 2005), and that it involves some non-trivial physics.

##### **(13) Forgetting-versus-remembering**

Clearly we need both Long-Term Memory, and Short-Term Memory — and perhaps every gradation in between according to context (Gaito, 1963, 1966b; Pribram, 1966; E.Glassman 1969; Glassman & Wilson, 1973; Lea, 1984). Indeed this need seemingly extends right down to the *instant forgetting of meaningless trivia* (especially in the Darwinian “listen-and-mostly-discard” example of section (8e.ii)). It is therefore somewhat encouraging to note that the different forms of RNA together offer a whole miscellaneous range of stabilities (Mattick & Gagen, 2001), and that such properties are likely to be affected by molecular *editing* (Morse *et al.*, 2002), RNA *strand-pairing* (Hall *et al.*, 2003; Levanon *et al.*, 2004 p1003ff); and special complications within primates (Eisenberg *et al.*, 2005).

Some systematic control would also be needed, and one would hope that stability would normally increase automatically according to the degree of coherence (equilibration) achieved by the scheme ensemble concerned — in accordance with the principles discussed in section (8). It remains to be seen whether any such detailed mechanism can be brought to light.

#### **E. Some unexpected bonus-explanations arising from this approach**

##### **(14) Birds and bees — small brains, but some surprising intelligence**

J.L.Gould (1984) reviews the behavioural repertoire of bees, and then evaluates their apparent memory-capacity requirements: — “*The real pressure for space comes when the animal begins to encode pictures. ... We must consider problems both of storage and of access. ... Conservatively, then, a low-resolution snapshot map of the first 1 km of the 12 km flight range would require 6000–60,000*

cells. This begins to sound like a lot, unless we can suppose that a cell can store points in more than one array (superimposed storage).” (pp.172-174).

One might take this to mean (a) that it stretches credulity to claim that a purely cell-and-synapse system could deliver the observed performance, and (b) that some *unspecified substructure* within the cell might be able to handle the complexity of the “superposed storage”. Moreover Gould does not seem to have allowed for some probably-needed “housekeeping circuitry” to organize and monitor the rest of the system — and such extras would probably not be trivial. Obviously such item-counting logistical difficulties would disappear if a significant fraction of the coding and activity were held at molecular level.

“Bird brain” is a traditional insult, but birds themselves tend to have an intelligence comparable to many a larger-brained mammal. Now suppose that many important mental codings are actually Piagetian schemes, and that these are embodied as string-like molecules, (see sections (5) and (7) above) — so that we have a dual system (Synaptic *and* Molecular) — then there may be some scope for re-apportioning responsibilities between these two cooperating systems.

Next consider that the typical bird life-style calls for weight minimization (as with aircraft, obviously), and that a typical bird’s *brain* to use molecular mechanisms instead of synaptic devices whenever there was a choice, since molecules are vastly lighter than synapses — and occupy much less volume. Likewise if I had to work in a very small office, I would more keenly prefer miniaturized microchip equipment whenever I had the choice.

But now consider seasonal complications in that office. Suppose my Summer and Winter activities were quite different, and that my space was too cramped to store any macro-equipment not currently useful — then I might well have to discard any currently-unused non-miniaturized surplus, especially if I knew I could replace it later whenever necessary. Well that seems to be what canaries do with some of their neurons at the end of the breeding season: (Nottebohm, 1981, 2002)<sup>42</sup> — or see Kiester & Kiester (c2003) for a concise journalistic summary.

Thus it could be that, in some respects, the brain neurons might represent an ephemeral computer “RAM” — with coding-or-configuration reconstructable from the more permanent “ROM” information held in DNA-or-RNA coding, augmented by new re-learning. In short, the basic neuron restoration may constitute a “rebooting” of part of the Synaptic System — using information held in a different subsystem, plus the knowhow stored within any operator who intervenes from outside.

### **(15) Why such meagre neuron-regrowth within mammals?**

The recent excitement over the discovery that there is some neural-regrowth in mammals, should not blind us to the fact that this regrowth is still fairly marginal and site-specific; (Gage, 2002; E.Gould & Gross, 2002).<sup>43</sup> Clearly this adult neurogenesis does exist, but it is hardly the prolific activity that we might have expected. So the basic question (raised by these authors themselves<sup>44</sup>) still remains: Why

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<sup>42</sup> This first attracted great skepticism, and then great interest — mainly because, for many species including man, this project disproved the long-held doctrine that neurons could not be replaced in adults. This is clearly important, but that particular “*Synaptic System*” debating-point has only an indirect bearing on the issue of this present paper which focuses instead on the hypothetical alternative, the “*RNA-like System*”.

<sup>43</sup> Meanwhile Rakic (2002) raises some procedural and definitional doubts.

<sup>44</sup> Nottebohm (2002, p624, p627) also suggests that this is all tied up with problems in our notions of Long-Term Memory — and that we should be seeking a new theory of this LTM.

The notion of the coherent *signal-structures* (“schemons” built up from “*tatons*”, see sections (8: page 9ff) and (13: page 19) above) might offer a way forward here — though of course, that remains to be seen. In principle, such a model could at least explain various degrees of collective stability — depending on degrees of apparent confirmation (internally and/or externally).

does nature have this reluctance to regenerate neurons? Why indeed, *especially* now that we know the task is actually possible?

Without repeating their comments, it is perhaps helpful to consider additionally how the situation would stand if the two mental systems (Synaptic AND the supposed RNA-based systems) were both on offer together, as suggested earlier. There would then be less need for them *both* to be flexible, so that adaptability task might well be left chiefly to the system with the less cumbersome units — and that would surely be the molecular system, supposing it actually exists. After all, as Nottebohm (2002) points out, neurons take what would usually be an inconveniently long time to re-integrate into any existing circuit; and they are, in any case, geometrically awkward items to reproduce. So, if there happens to be an easily mass-produced alternative (which is also *well organized* within individuals of that species) then why tinker needlessly with the bothersome neuron structures?

And returning to the office analogy to make a different point: If I now move into a warehouse with ample space, it will no longer trouble me if some of my equipment is relatively bulky<sup>45</sup> — not even if much of it is lying idle. That *ample-brain-space* may often be the case for mammals, though one would have to question it in such cases as the mouse!

## F. Conclusion

**(16)** As we saw in section (9), Crick could see little evidence of systematic order in the “spike” pattern of nerve-signals, and we could surely say the same about synaptic connectivity — all very important no doubt, but seemingly no basis for precise or advanced thought.

Piaget took a different initial course, and if we follow his ideas to their logical conclusion, we end up with the theoretical expectation that much of memory must be embodied in stringlike coding — and, considered realistically, that leads us to macromolecules with RNA as the chief suspect. It then appears that this idea fits in with various other phenomena with their own better-known histories, as discussed here.

Independently meanwhile, biochemistry and genetics have recently discovered that RNA constitutes an unexpectedly important class of coding-entities in its own right (*i.e.* not just a messenger and toolmaker), and that it offers a *vast coding capacity* which is but poorly understood at this stage. Biochemists themselves are now largely preoccupied with exploring RNA’s new-found role as a controller of metabolic activity. No doubt that is one important role for such “ncRNA” (formerly considered as “junk”), but there still seems to be ample scope for envisaging an additional role for some of it, as hypothetical “*taton*” elements physically underlying the *Piagetian schema*.

Or indeed, one might eventually generalize the schema concept to *include* the biochemical regulation, or *vice-versa*. Either way, that would vindicate the opinion that “psycho-” and “somatic” can often be closely related, and even share some of the same basic mechanisms.

## Appendix: Piaget’s “Schème / Schéma” concepts

**Ambiguity I.** By 1966, Piaget had claimed to reserve the word “*schéma*” for *figurative* cases — those mental processes involving 2D-or-3D perception, even though these are arguably built up from more elementary 1D elements such as simple motor schemes used in outline-tracing. Thus the alternative word “*schème*” is left for all other cases (Piaget & Inhelder, 1966, 1968; Furth, 1969) — including those simple general-purpose elements, presumed to be essentially linear (1D) which are the main focus of attention in this paper. Meanwhile, according to this “rule” there seems to be no acknowledged word to cover all cases; and as a further complication, Furth points out that most translators had hitherto been unaware of the asserted difference between *schéma* and *schème*.

<sup>45</sup> relatively bulky — like a typical neuron when compared to a “mere” molecule.

This verbal-distinction may well be a lost cause. In any case it may be unnecessary, given that basic 1D elements are arguably involved in each case. Thus I will here largely disregard the “*scheme-schema*” distinction, and rather follow whatever is customary in the literature concerned if that is convenient. (See footnote<sup>5</sup> on page 6 for further discussion, especially in subsection (a)).

**Ambiguity 2** regarding “*schema*”. Bunn (2003) implicates Head (1920) as originating the term; while the Oxford English Dictionary (1989) mentions three perhaps-relevant sources: (a) Kantian Philosophy, (b<sub>1</sub>) Neurology (Head, 1920), and (b<sub>2</sub>) Piagetian Psychology — citing translations of Piaget (1923 *LTh*, and 1932 *MorJCh*). In fact the dictionary treats (b<sub>1</sub>) and (b<sub>2</sub>) together; but I query that because Head was actually using the term for something rather more specialized: His “*schema*” was a sort of passive template (effectively a static vector) marking a muscular rest-position, whereas the Piagetian *schema-or-scheme* is an active sequence of instructions — especially in his later work. Of course we might eventually interpret Head’s version in terms of Piaget’s, but at present that attempt would seem to be a distraction.

(Bartlett (1932) is another candidate. as implicated by Pribram (1966) and Cameron *et al.* (1966). However I suggest that while his important contribution was to point out that memories were stored in schematic summary, his own concern was not with our present problem of dissecting the nature of schemata themselves.)

Actually the Piagetian term is much more in tune with Kant’s abstract usage in his Critique of Pure Reason (Kant 1781 A136 / 1787 B175 / 1993 p142; Nitsch 1796 p103). Initially Piaget’s “*scheme/schema*” was scarcely more discerning than common loose usage (*LTh* 1923, *JR* 1924, *ChCW* 1926, *ChCPhC* 1927a, *IstYL* 1927b, *MorJCh* 1932, *OI* 1936, *CR* 1937). However, after about 1946, he was clearly seeking to develop Kant’s explanation of “*synthetic a-priori*” concepts like Causality (Kant A8,B12,p36), Number (B15,p38), Conservation (B17,p39), Space (A21,B35,p49), and Time (A29,B45,p54) — concepts which (i) “*synthetically*” assert something non-tautological, and (ii) seem to do so “*a-priori*” without having any known recourse to empirical evidence.

Of course Piaget’s contribution was to show how early experience did actually generate the ontogenetic component of such concepts (e.g. just look at his “*...chez l’enfant*” titles). Meanwhile any phylogenetic components might be explained in Darwinian terms (via experience of the species), which of course brings us back to DNA/RNA. Either way, there is now scope for explaining such concept-encoding in material terms — and that is our present concern here.

Anyhow Piaget was apparently the first to use this “*scheme/schema*” term and the concept within psychology, despite claims that Head (1920) may have had priority — and even if his early use of the term hardly differed from its use in everyday speech.

**Ambiguity 3.** The dilemma is this: Should we use the term for (i) the postulated *immaterial* signal emanating from the postulated RNA-sequence, or else for (ii) the *structural coding* on the RNA which supposedly generates this signal? As long as we appreciate such distinctions, we could continue to live with the ambiguity and rely on adjectives or context. However it might be prudent to formalize the distinction: Thus for the IMMATERIAL SIGNALS supposedly transmitted we might and coin a word such as “*transig*” (or “*insig*” and “*outsig*” when we want to imply absorption or emission respectively). Meanwhile we might use a word such as “*taton*” for the stringlike PHYSICAL ENCODING (without necessarily committing ourselves as to its RNA/DNA/-like nature) — or “*actRNA*” if we do decide it does have an RNA basis, and for hypothetical discussion with biochemists. (Regarding the “*taton*”, see section (5) and its footnote<sup>8</sup>).

**Ambiguity 4.** This entails making the distinction: “*elementary schemes*” versus “*compound schemes*” (of which figurative *schemata*<sup>5</sup> would presumably be a special case). I have suggested<sup>8</sup> that the abovementioned “*taton*” should always imply the elementary 1D case (on its own); while a compound ensemble of such elements may be called a “*schemon*”, especially if it is collectively self-supporting. — Such distinctions are discussed in Traill (1999), but without the neologisms.

**Ambiguity 5.** Piaget (1967 “*Biol&Kn*”, ch.6) distinguishes three types of knowledge, of which at least the first two involve schemes. See subsection (8e), above.



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1800 = date 1<sup>st</sup> published in the original language, 1900 = English edition referred to.

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Supplement of February 2008 —  
***Philosophers of Knowledge, and the historical context for “Tat”***

“In the beginning was the deed / *Im Anfang war die Tat*”

Summary of the *Piagetian* basic theory (sometimes elucidating beyond Piaget’s own account)

There are arguably eight key defining-notions associated with the Piaget-orientated view of how the mind/brain operates. We may list them, starting with the most familiar:

(A) The notion of developmental **stages** (here also tentatively associated with Ashby’s control hierarchy — a “staircase” series of levels, each of which somehow exercises authority over the level below). Each level will usually embody concepts with its appropriate degree of sophistication.

(B) Concepts will consist of **complexes (often called “schemata”)**<sup>46</sup> — usually dependent on sub-concepts in lower levels (like subroutines in computer programming), and often controlled by superior schemata in higher levels (if there are any).

(C) These schemata owe their stability and verisimilitude to processes of (internal and external) coherence-seeking called **“equilibration”** — between:

(D) those more elementary elements of (presumably encoded) action, generally called **“schemes”**.<sup>46</sup>

(E) Some of these schemes are presumably **irreducibly basic**, so it would be helpful to have a specific name for these irreducibles (and two suggestions have been “acton” and “taton”, see below).

(F) One key learning strategy is **trial-and-error** — “Act first, and evaluate later” — *selection between pre-existing* schemes (perhaps in a large semi-dormant population) rather than having them “written onto” like recording tape.

(G) It seems likely that such basic elements will *usually* consist of **one-dimensional coded strings**, — such as the RNA-molecules briefly considered by Piaget in 1967.

(H) The mind/brain is a “learning system” or “epistemological system”; but there are at least three other types of undesigned learning system which likewise self-organize, ultimately from scratch. — As this is a seemingly-impossible task, it is likely that they will all be using **the same formal strategy**.

Most of these items have been dealt with in detail elsewhere, but the following ones invite extra comment — to which I will also add some potentially interesting side-issues:

(E) “Taton” as the non-chemical name for those hypothetical “irreducibly basic” scheme-elements

In the first edition of this work (2005), I suggested the word “acton”, (page 7 and its footnote<sup>8</sup>). The main problem was that this word is confusingly similar to “action” — especially in the presence of spell-checkers! So now the proposed word is “taton” — given that “Tat” is the German word for act-or-deed. As it happens though, that word “Tat” has a history behind it, a history which may be of some interest — and some indirect relevance for those concerned with epistemology, since it has been used to embody the ideas relating to “(F)” and its “trial and error”:

(F) “Action first” — The “Tat” quote from Goethe — Was he a Piagetian a century before Piaget!?

In a London-bound train after a conference in the 1970s, the late H.G.Furth drew my attention to the quote from Goethe’s stage-play *Faust*: “In the beginning was the deed” — “Im Anfang war die Tat” — contradicting the start of John’s Gospel, which says: “In the beginning was the word” (λογος). Anyhow the “Tat” quote turns out to be fairly well known<sup>47</sup> because it does nicely illustrate the basic Piagetian (F) doctrine:—

That our perception is *not passive reception (like a tape recorder)*, but rather **the active initiative/Tat comes from us** — and is then blessed or damned by its consequences.

<sup>46</sup> Note that the use of these terms “*schema/schemata*” and “*scheme/schemes*” are not always consistently distinguished from each other — not even by Piaget himself, and even less so after translation; so it is always prudent to check the context. See footnote 5 on page 6.

<sup>47</sup> E.g. see Zelazo (1999, p.95), though the emphasis there is on *responsibility*.



For years I simply saw this quote as a joke-mnemonic, deliberately taken out of context — rather like those bawdy memory-aids used by anatomy students. But what did Goethe actually mean by it? Was he somehow ahead of his time? After all, he was a polymath and notable scientist, and an avid reader of the works of that key contemporary epistemologist, Immanuel Kant (1724-1804), whose most relevant book, *Critique of Pure Reason*, appeared in two contemporary editions: “A” (1781), and “B” (1787).

The short answer is that Goethe almost certainly was addressing this epistemological issue — but cautiously and obliquely. For one thing, Faust first turns from “word” to “Sinn/sense” (which probably means “sensory-input”<sup>48</sup>), before dismissing it. Next he also rejects “Kraft/power”.

It is perhaps significant too that Adelbert von Chamisso (1781-1838) had also published a short *Faust* of his own in 1803,<sup>49</sup> which likewise raises similar issues — and indeed presumably predates the 1803 completion of the relevant “Tat” part<sup>50</sup> of Goethe’s work (*Faust, part one*), which was not actually published until 1808. It is probably fair to say that this Goethe-formulation is more coherent and concise, though here is not the place to debate priorities nor the finer nuances. Suffice it to say that such discussion was very much “in the air” within the German Romantic movement<sup>51</sup> in the wake of Kant’s *apparent* solution to Hume’s paradox<sup>52</sup> — a supposed-solution invoking “*transcendental*” or “*a priori*” or “*instincts-of-the-heart*” basics such as the Space-Concept — taken as *axiomatic*, and not requiring the explanation that Hume (or later Piaget) would insist upon. (See below).

So it seems that Goethe had, in principle, taken this important “Tat” step beyond Kant’s ideas. However it is one thing to have a vague poetic<sup>53</sup> idea (even if it is well based scientifically), but it is another matter to sort out its implications and at least *some* of the logistics for how it might be implemented. That is presumably part of the explanation for why Goethe was so tentative in his suggestion; and it also partly explains why he did not pre-empt Piaget. Nevertheless Piaget clearly had mentors from this period, whether he realized it or not.

A further twist to the story is that Goethe apparently exchanged correspondence with Erasmus Darwin (1731-1802) who had early ideas on evolution which were later formulated more clearly and systematically by his grandson Charles Darwin (1809-1882). This raises intriguing possibilities, given that Goethe was (amongst all his other accomplishments) a biologist of note; and also given that evolution can be interpreted as a form of epistemology — i.e. knowledge-acquisition, but this time within unthinking nature (now known to be DNA) rather than within humans or their institutions — *see below*. However this supposed connection is mere speculation on my part, as I have not yet studied the details.

<sup>48</sup> — i.e. perception/observation; — though a less-relevant alternative meaning might be *the Platonic inbuilt inescapable “form”*.

<sup>49</sup> Adelbert von Chamisso (1803/c1911) — “Faust”: pp.355-365:

E.g. “Was ist des Denkens, was der Sinnen Welt? | Die Zeit, der Raum, die Allumfassenden, | Und ihre Schöpfungen, durch die sie werden? | Was ausser ihnen, das Unendliche?” (p.358)

or the five lines quoted by Orosz (2002) — p.363 in the abovementioned 1911 edition:

“So wie die Sprache, wie des Wortes Schall | Dir Mittler des Gedankens ist und Zeichen; | So ist des Sinns Empfinden, der Gedanke selbst | Dir Sprache bloss und eitles leeres Zeichen | Der ewig dir verhüllten Wirklichkeit.”

<sup>50</sup> For such timing, see Wayne (1949), p.19 — translator’s introduction to Goethe’s *Faust*, English edition.

The “Tat” passage occurs about one page after the beginning of the second scene in Faust’s study: p.33 in the German edition cited, or p.71 in the English Penguin-edition.

<sup>51</sup> Moreover it seems that Aristotle had already covered some of this ground more than 2000 years earlier!

He evidently dismissed the word/λογος option as an unfeasible thought-basis.

<sup>52</sup> David Hume (1711-1776) — see his “*An Inquiry Concerning Human Understanding*” (1748).

<sup>53</sup> A key approach by the German Romantic movement of this period — (Orosz, 2002).



Kant's "Cop-out". — The "a-priori intuition" as a mere shifting of the problem

The original Humean problem was essentially this: Any learning system needs some prior knowledge (perhaps implicit in its structure) before it can even start learning. But then how can it get that initial prior knowledge, needed to "pump prime" the whole process? The situation seems impossible, and yet learning clearly does take place in the real world!

(This causal dilemma thus resembles the problem over the creation of life: Life begets life; but if that is always the case, how could the process have started in the first place? In fact we shall see that this similarity may be more than mere analogy:— There is a case for identifying the two cases as two manifestations of the same formal epistemological process, although in two dissimilar "domains".)

Kant invoked a privileged status for the pump-priming part of the process. This was seen as "transcendent" or "going beyond" — and this was welcomed by those Romantics who ultimately trusted ineffable spiritual powers such as "vitalism", and distrusted any attempt at mechanistic explanation. (This mistrust was partly deserved, but some of us might believe that they took it too far). But, welcome or not, this "solution" did not really answer the scientific question. — It merely shifted it into a more confined discussion-area, gave it some fancy names, and hoped that no-one would notice!

Surprisingly though, this did turn out to be half-right! — so it did eventually lead to further progress along Freud/Piaget/Ashby lines:

The trick-ingredients from Freud/Piaget/Ashby, which make the Kant system viable

It is unlikely that Freud gave much thought to Kant, but his revolutionary recognition of *the unconscious* — thinking processes which are hidden from our own awareness — set the stage for understanding the mysterious Kantian "transcendent" notions, such as the supposedly eternal God-given mental "categories" of Space and Time perception, etc..

Piaget's contribution here was to show that these categories were not actually pre-set, but *had to be learned in infancy* — so they were not transcendent in the strict Kantian sense, though we *adults* might be excused for thinking that they were. Such ambivalence begins to make sense when we re-cast the problem in terms of successive levels of development — successive stages in which basic epistemological problems have to be worked-on anew each time. And each new stage usually manages quite well by (rashly) assuming that the salient conclusions arrived at in previous stages *are* fully reliable — that they are *effectively transcendent*, even though their achievement will often have been unconscious — arriving "undocumented", *to be taken on trust*.

In fact that works exceptionally well for concepts of **Space** (Piaget 1949, 1952; Beth and Piaget, 1966; Traill 1976, 1978). This is because 3D solid objects do obey very clearcut geometrical laws which are comparatively easy to arrive at, so there is little doubt about the correct eventual outcome. (The *learnt* capability of dealing with 3D space is thus virtually a foregone conclusion, so it can then safely be treated by practical adults *as if it really were* an inbuilt mental truth). The transcendence-assumption is thus benign in practical terms for mere objects. To that extent then, Kant's formulation was right *in certain practical terms*, so it was-and-is possible to gloss over the fudged theory for those neater clearcut cases — the cases which gives pragmatists the illusion of infallibility, thus encouraging them to apply this fudged theory to problems which are likely to defeat it.

Hence such assumptions may not be so benign when it comes to successive layers of hard-set conclusions whilst trying to understand more diffuse systems — as in *social matters* for instance! (i) To start with, there is usually no clear indication that there is any clearcut "right" answer to social problems, or neat conceptual frameworks for formulating them; and (ii) even insofar as we could master that difficulty, there will still be the problem that such messy configurations will be more difficult to learn, and to conceptualize such as to generate neat-and-reasonably-correct mental models of reality. Then (iii) there is no guarantee that different learning-systems, (be they individuals, or social groups) will reach compatible conclusions about social reality — given that there is no infallible

criterion for “rightness”. Hence there will be a pressing need to negotiate these incompatible views — a task which can go disastrously wrong, as we know all too well!

But coming back to basic principles of epistemology:— By adding Ashby’s concept of formal hierarchies of control, it becomes feasible to use very primitive abilities to master a simple environment. For the moment, let us say this is the basic 3D world, elucidated at the “sensorimotor” or “M<sup>0</sup>L” level, thus generating apparently infallible M<sup>0</sup>L schemata which then function as a basis for “M<sup>1</sup>L” (the next level up) *as if these schemata really were transcendent* in the Kantian sense.

The process can then start again at this higher level, using these new M<sup>1</sup>L schemata to build up concepts at the M<sup>2</sup>L level, and so on into the increasing conceptual abstraction (M<sup>3</sup>L, M<sup>4</sup>L, ...?) which accounts for human intelligence — surpassing that of other creatures whose hierarchy-building is curtailed at (say) M<sup>2</sup>L, or by some other such limitation.

But, from the viewpoint of this Kant-discussion, the most interesting question is “What happens at the very *bottom* level? What provides the transcendent basis then? Indeed, haven’t we just shifted the problem yet again, leaving it ultimately unsolved after all?”

To start with, I suspect that Piaget may have been unduly conservative and empirical(!) by taking the sensorimotor stage of the *newborn* as basic. To fit in with this assumption, it is convenient to *define* M<sup>0</sup>L as corresponding to this sensorimotor level, but let that not close our eyes to the possibility that there may actually be other levels *below* it — levels perhaps developed prenatally *and/or* long ago in the genetic history of the species. To be consistent, we may accordingly label these levels (if they exist) as M<sup>-1</sup>L, M<sup>-2</sup>L, ..., etc.; (Traill, 1999, p.65).

But let us consider the bottom level “M<sup>bot</sup>L” — whatever its actual scale-position might be — as a special case which can terminate the apparently-endless hunt for the (relatively) transcendent. The solution may now be in sight if we provisionally accept two ideas:

(I) That, when **each given** developmental-step (or task within it) is made **simple enough**, it should be possible to make progress by trial-and-error, along lines suggested by Darwin and again later by Ashby. A choice between 4 (or perhaps even 100) alternative candidate-solutions is manageable — and one can then do it again and again; whereas the prospect of solving the total problem from amongst (say) 4<sup>4</sup> or 100<sup>4</sup> choices is virtually hopeless. On reaching down to the “M<sup>bot</sup>L” level, one will hopefully be able to make do with simple primitive encodings which could either be found from random chemical interaction, or much more likely as “waste junk” initially left over from other metabolic processes. In either case, we can think of these most primitive schemes (“tatons”?) as the real Kantian “*a priori*” or “*transcendent*” elements of the system — and at the same time each is a “found object” — or “*objet trouvé*”, to borrow a term from the visual arts (Traill, 1999, p.24).

(II) Linear molecules like DNA, RNA, PNA, and protein are feasible candidates, and it has already been argued above (from page 15 onwards), that **ncRNA** is by far the most likely final candidate to embody these encodings — the “tatons”. So now it might be informative to re-read Kant with this in mind! — and meanwhile do some new laboratory investigations! — *Theory and practice*.

Note that, if this account is indeed valid, then it offers a solution to the vexed problem of “*infinite regress*” which still engages philosopher-epistemologists like Hans Albert (1968/1985) and Susan Haack (1993). Anyhow let us now return to one other item-for-discussion from the list on page 27:—

(H) *The brain is one “epistemological system”, but there are others. Do they all use the same basic strategy?*

#### Knowledge-mechanisms within tangible nature

Despite appearances, *conscious human minds* are not the only self-organized compilers of knowledge, though we may call these mind/brains “case (a)”. We should also consider: (b) one’s *immune system* which must learn-from-scratch to distinguish “friend” from “foe”, and what to do about the foe.

— Also (c) the *genetic code*, which originally had to learn from scratch how to cope dynamically with its environment and thus emerge into life-as-we-know it.

#### Four different epistemological domains

In all three above cases there is-or-was the *primordial almost-impossible task* of self-organizing without any safe blueprint or “proper” learning mechanism — and it is not immediately clear how they do it (as we have just seen in the discussion over Kantian views). In fact though, we do know about Darwinian explanations for (c); and there is also the less known *equivalent explanation for (a)*, via Piaget’s genetic epistemology — an account in terms of the abstract “scheme-or-schema” entity, first raised by Piaget (1923). (This abstraction is interpreted using material biological hypotheses (pp.3-21 above); and that does at least makes the possibilities easier to understand; see below).

Going further, there is a strong case for also distinguishing (d) *society-as-such* from the individuals within it. (For one thing, society-as-such is an unconscious inhuman entity, and it could not really get started as a knowledge-gathering entity until its would-be members evolved speech for it to use. It is less clear how much it is “self-organized”, but see below.) This now gives us **four largely-separate epistemological domains**,<sup>54</sup> even if there is some limited scope for intercommunication between them.

**Table S — See text**

	properties ↓	(a <sub>0</sub> ) mind/brains (at birth; J.Piaget)	(b) immune system	(c) genetic code	(d) society-as-such (Scientific Method)
1	Approx. time for significant change	seconds-days	days-weeks	generations-millenia	weeks-centuries
2	Basic physical coding-element for trial-&-error	abstract “schemes” (coded action); <i>ncRNA?</i> (see p.15,ff)	proteins <u>strings</u> (antibodies)	DNA <u>strings</u> ; (Rare exceptions — also stringlike)	language: <u>strings</u> -of-words, incl. premises
3	“external coherence” testing; (experiment)	physical <i>and</i> sensory (scheme-driven) actions; selected according to result	huge repertoire of codes, selected via exposure to antigens	large repertoire of genetic diversity + “natural selection”	large repertoire of ideas (memes?); tested experimentally & “politically”
4	internal coherence testing. (“Coherence” in its traditional sense).	“equilibration”. Reject “schemes” if poor ally-network within brain (Traill, 1976) <sup>Mon18</sup>	individual survival?	species survival?	hermeneutic-discussion; + logic & maths. Reject “fallacies”. Do they fit with the pre-existing ideas?
5	Scope for passive “tape-recording” of experience — (rather than selection). [Lamarckian]	<b>None!</b> — unless via intervention by some other epistemological domain	<b>None!</b>	“ <u>Lamarckian</u> ” <b>None!</b> — unless artificial & “ <u>Genetically Modified</u> ” via domains (a) or (d).	Debatable, but probably via individuals (a): as <i>scribes OR tape-recorder designers, etc.!</i>
6	Scope for <i>selection</i> (as substitute for “writing down”) [Darwinian]	<b>Yes:</b> Recycle the rejected scheme element or complex	<b>Yes:</b> Build up defence-stocks, but <i>only</i> of those codes deemed necessary	<b>Yes:</b> Blind “ <u>Darwinian</u> ” selection of those surviving	All too often <b>Yes!</b> — Blind selection-by-rule, despite logic! Debatable.

<sup>54</sup> This exact list-of-four domains was suggested independently on at least two occasions (Popper, 1975/1994, and Traill 1999, Ch.4) — though both acknowledged prior thoughts in this direction by the immunologist Niels Kaj Jerne (1911-1994), and others also had a say.<sup>9</sup> I find it a considerable relief that Popper had the same view on one particular aspect here: the distinction between *individual minds (a)* and the “*mind*” of *society-as-such (d)* — since I blame the confusion between these two for some of the errors in social policy over the years.

Moreover, it is argued (Traill, 1999, Ch.4) that they must probably all be using *the same formal strategy to defeat the “regress problem”* — though via very different physical mechanisms in most cases, and on very different time-scales. If that is so, then we have the advantage of being able to make useful inter-domain comparisons in the search for missing explanations, bearing in mind that (c) (the genetic-code domain) is the most studied and best-explained so far. — See the above table.

A physical embodiment for Piaget’s Elementary “Scheme”? — The same question again.

In the above table, cells 2b-2c-2d each claim that the epistemology for that particular column depends on having some sort of stringlike coding system as its basic element. It thus seems likely that Piaget’s abstract elementary-schemes (see cell 2a) will also be stringlike. (In fact this had already seemed likely due to their analogy to elementary computer-coding as a means of producing reproducible organized action patterns; (Traill, 1978). Moreover such 1D codings have some intrinsic advantages when it comes to: storage, simplicity, duplicating, active “read-out”, adaptability-as-substructures, and as a means of coping with sequencing and exhaustive-searches.)

As already mentioned, a plausible list of candidates for such stringlike coding seems to be limited to: Specially-interconnected-neurons, DNA, RNA, PNA, and protein. By elimination, the most likely candidate was RNA (also supported by supposedly-discredited circumstantial evidence from the 1960s and 70s) — and then new findings of 1990-onwards made ncRNA seem highly probable (Mattick, 2003, 2004; and p.16ff, above) — if we do indeed accept that stringlike coding must be involved.

Extending the Limited “Philosophers’ View” of Epistemology

Note firstly that current philosophers say little or nothing about *possible mechanisms* which might perform the knowledge-building tasks, though they do discuss the problem abstractly. In a sense there is nothing wrong with that except that they then fail to encounter certain practical problems which (in their attempted solution) actually offer helpful extra clues. Likewise they pay little heed to the work of Piaget (primarily a biologist) even though he too claimed to be an epistemologist. — E.g. there is no relevant reference to Piaget in any of these three influential works by philosopher-epistemologists: Albert (1968/85), Thagard (1992), Haack (1993).

Secondly, even Piaget himself stopped short of proposing an actual physiological mechanism (claiming this was beyond his competence), though he did toy tentatively with the temporarily-fashionable notion of RNA (Piaget, 1967; and §(5e), p.8, above). Instead his *scheme* concept went only halfway toward a material embodiment — like Mendel’s still-abstract *gene*-idea of the 1800s. Incidentally, as a biologist, Piaget must have recognized the parallel between his “*scheme/action-first, then learning*” (a), and Darwin’s “*change-first, then selection*” (c), and indeed his account was quite likely partly borrowed from Darwin (perhaps unconsciously) — but then he was often lax in giving references (perhaps for strategic reasons, given the antipathy to theorists during most of his lifetime). It is thus uncertain how much he was aware of the implications of the above table.

Thirdly, most discussion of epistemology has centred either on (a: the individual mind), or (d: society in some form, often the scientific community) — or else some ill-defined combination of the two, which can lead to confusion. In fact, it seems likely that the best approach will be to differentiate clearly between separate domains; and wherever they overlap or intercommunicate, this should be explicitly considered.

There is the added complication that (a: the adult human mind) needs to be considered as a *hierarchy of subdomains*, with the infantile “a<sub>0</sub>” (sensori-motor) level at the bottom,<sup>55</sup> and extending upwards as a<sub>1</sub>, a<sub>2</sub>, a<sub>3</sub>, ... — corresponding to the “developmental stages” which Piaget is best known for. In fact, it is likely that the formal Darwinian strategy discussed above applies *internally* to each

<sup>55</sup> Or perhaps more correctly “a<sub>bott</sub>” — which may actually be “a<sub>1</sub>” or “a<sub>2</sub>” (corresponding to M<sup>-1</sup>L and M<sup>-2</sup>L) — see the above discussion on Kant and the Ashby hierarchies.

subdomain/level separately; (Ashby, 1960). Thus if we seek **simplicity** (and that seems appropriate for the study of *basic principles* of epistemology or anything else), **it will often be prudent** to confine our attention exclusively to *sensori-motor* ( $a_0$ ) activity, largely ignoring the other  $a$ 's — or else use other comparatively straightforward domain-columns, and that probably means (*c: genetic evolution*) as the only sufficiently-simple alternative. However that *c-case* does at least give us one extra domain to illustrate the basic epistemological principles, and this time the mechanisms are much better known.

It may be helpful to re-work that last point again in different words:—

*Table S* depicts four different epistemological domains, but insofar as the mental domain “(a)” is now to be considered as composed of multiple levels (...  $a_0$   $a_1$   $a_2$   $a_3$  ...  $a_n$  ...), it would seem consistent to consider each of these “ $a$ ”s as a *separate domain*, in some sense. Certainly there will be interaction and overlap, but then that applies to the main four domains of *Table S* as well. This offers a *modular view* of the whole epistemological venture — and it also sets the stage for the complex issue of how such domains interact, e.g. by one domain offering “design” to another. But that’s another story, to be dealt with later.

Finally, a brief guide to reading Piagetian Literature

Piaget’s own works are prolific and verbose, and they often seem to avoid controversial issues and references (perhaps wisely, as noted earlier, given the predominance of overenthusiastic positivism-and-empiricism during his lifetime — antithetical to deep theory such as that related to Kant). Moreover two of his more relevant works (1949, 1952) are apparently not available in English, though some of the related discussion appears in Beth & Piaget (1966). Perhaps the best starting-source is Furth (1969) or the anthology edited by Gruber and Vonèche (1977).

For present purposes (concentrating on basic activities and theory) it may be best to avoid work written by-or-for educationalists, as these are understandably more concerned with macro-applications such as the developmental stages beyond infancy. (Indeed most of Piaget’s own later works were thus inclined). More recent development of his basic ideas can be found on the Ondwelle website: <http://www.ondwelle.com>



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