

Philosophies

Proceedings of ANPA 17

K. G. Bowden, *Editor*

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Alternative Natural Philosophy Association

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The Alternative Natural Philosophy Association

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EDITORIAL

After the excitement of the previous year the last twelve months has been a relatively quiet, yet active one, for ANPA. ANPA17, the 1995 meeting in Cambridge was one of the largest meetings yet, as can be judged by the number of papers in this Proceedings (despite the lack of a written contribution from one or two speakers), and by the fact that we have decided to increase the length of the next meeting, back to four days. The Special Issue of IJGS on the Emergence of Physical Structure is going to print as I type. One of the main achievements of the issue will have been to get Tom Etter to write up his ideas on Link Theory and Quantum Mechanics. Preprints of his paper are making some quiet ripples around the Information Theory and Physics community (including the IBM research laboratories in San Jose). Tom will present a database approach to his work at PHYSCOMP96 in Boston later this year. Meanwhile Pierre Noyes and Lou Kauffman have produced a sequel to their Royal Society paper, on a derivation of the Dirac Equation. Finally, on the year's news, I am sad to announce the death of Seigfreid Wouthuysen of Amsterdam, who although he had only been a member of ANPA for a relatively short time has made a significant contribution to its work.

I thought that this Editorial might be a good place to remind members of the formal structure of ANPA, and announce a few changes. The structure of ANPA is quite complex; we have a President (currently Tom Etter), who serves for two years and a Vice President (I am the only nomination currently) who serves for the twelve month period leading up to his Presidency, an Executive Council and an Advisory Board. The Advisory Board also have executive authority. Both the International wing of ANPA, and the American one (known as ANPAWest and based at Stanford), are responsible to this structure, and the Executive Council consists of members of both organisations (many of whom are also past Presidents). Both ANPA and ANPAWest have an annual Conference (at Cambridge in September, and Stanford in February, respectively), Conference Proceedings and a Newsletter (currently edited by Viv Pope, and Tom Etter and Suzanne Bristol respectively). The length of the Presidency was increased to a maximum of five years and a minimum of two at the last Executive Council meeting. The other major initiative that was put into place at this meeting was a stronger link between ANPA and ANPA West. Discussions were initiated regarding joint membership of the two organisations, which until now have been relatively autonomous. This would mean, for instance, that members would receive the Proceedings of both annual Conferences.

The ANPA Advisory board is currently undergoing some changes. Mike Horner of Neurolab in Geneva (formerly European Strategy Manager for DEC), is now Chairman of the Advisory Board. He has recently been consulting with the other members, as to the best strategic direction for ANPA. Some of the results of these discussions were made public at the last meeting. The members are Professors Geoffrey Chew of the Department of Physics at Berkeley, Michael Redhead, Head of the Department of the History and Philosophy of Science at Cambridge and Chris Isham (formerly Head) of the Department of Physics at Imperial College, London. We thank them for their continued support. The fourth member, Prof. Pat Suppes of the Department of Philosophy at Stanford is currently in the process of retiring, and feels therefore that it is also time for him to relinquish his position with ANPA.

Many thanks to Pat for the help and support he has given us over his term of office. Nominations are currently sought for a replacement (probably American or continental European, in order to retain the international flavour) for Pat, to the Advisory Board.

The Proceedings starts with the 1996 Arthur Frederick Parker-Rhodes memorial talk, given by Yorick Wilks, now Professor in the Department of Computer Science at the University of Sheffield, but many years ago with Parker-Rhodes, and other illustrious colleagues, at the Cambridge Language Research Institute, run by Margaret Masterman. Yorick is a *funny* man (probably not uninfluenced by a stint on Rowan and Martin's Laugh-In in his younger days) and his memories of his time with Parker-Rhodes led to almost constant laughter (mingled with amazement at FPR's achievements), in an atmosphere more consistent with post Conference meetings, than that usually associated with ANPA presentations within the ancient walls of Wesley House Theological College at Cambridge.

We have two retrospective papers, one by Jon Beck, written in 1976, on computers and de Rham's Theorem, and the other written by David Bohm and Ted Bastin in 1967 on early ideas about hierarchies. Publication of the Jon Beck paper was proposed by Mike Manthey who persuaded Jon to rewrite the paper in ANPA format specially for the Proceedings. The Bohm and Bastin paper was discovered in John Amson's attic along with others, some of which are published in the last ANPA Proceedings and in the IJGS Special Issue. I saw the paper during a recent visit to St. Andrew's and thought it would be appropriate to print it in this issue. Also in the Proceedings Rainer Zimmerman, now Professor of Philosophy at Munich, gives an insightful critique of ANPA methodology, Clive Kilmister describes a simple example of the building of a DCS within the Combinatorial Hierarchy, Ted Bastin discusses the labelling of Heterogeneous Objects and Pierre Noyes makes some comments about the status of Discrete Physics. My own paper is a verbatim transcription of my talk on various problems, including the necessity of finding the relationship between conceptual and physical information, an endeavour in which Tom Etter's forthcoming IJGS paper has had much influence on my thinking. I particularly enjoyed Geoffrey Constable and Tony Deakin's talks. I hope they will publish in the next issue of the Proceedings. We also welcome Karel Pstruzina from Prague, who describes his philosophical approach.

This Proceedings is produced by the Print Unit of the **University of East London**. For their support and help I would particularly like to thank Al Freimanis and Anna Bass. I would like to try to continue conformity of *style* for future issues of the Proceedings. Ideally I would like contributions to be submitted in International Journal of General Systems format (I have some copies of their Notes for Authors) or similar. At least, Times Roman, 12 point, double spaced, *single sided, two copies*, is preferred, although I will still accept typescripts in Courier. Main heading capitalised, centred, other headings capitalised to the left. At least a one inch bottom margin for footers; page numbers NOT bottom right. *Only copy in good English will be considered, and remember, this is a formal Proceedings*. Remember also to include your **name** (surprising how many people omit this!), affiliation or address, email address and the **version number** of the draft. I often get sent more than one version of a paper and invariably mix them up! Send copy to *Keith Bowden, 47 Park Avenue, Barking, Essex IG11 8QU*. Please help me by conforming to all this as closely as you can. Finally, let me wish all who will be attending, an excellent ANPA18 meeting for 1996!

Keith Bowden,
University of East London, August 1996

Arthur Frederick Parker-Rhodes: a memoir.

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Abstract: Parker-Rhodes was an original thinker in information retrieval, quantum mechanics and computational linguistics. The paper describes the basic ideas of his work in the last area for an audience familiar with only the others. It tries to bring out his view of the role of lattices and graphs in language structure and the way his assumptions differed from those of his contemporaries. The paper also aims to give some anecdotal feeling for him and his writings and to point to more recent work where his ideas have reappeared in other forms.

Note: a version of this paper was given as a Parker-Rhodes Memorial Lecture at the annual meeting of the Alternative Natural Philosophy Association in Cambridge, September 1995.

Arthur Frederick Parker Rhodes (AFPR from now on: his colleagues and he all referred to each other by initials at the period I am writing about) was a paradoxical man: never in very good health, he nevertheless dressed as a countryman in Cambridge and walked his collie Shep miles to work every day and back. One says work, but life at the Cambridge Language Research Unit (CLRU) was not like other occupations: as a research student one did not know everything that went on, but we were all pretty sure that, when grant money was short, AFPR was not paid because he was considered to have private means. Later, one learned that, as Communist Party members earlier in their lives, AFPR and Damaris, his wife, had been under constant pressure to hand over their money to the Party, but somehow they never had.

I knew him and the CLRU best during the period 1962-9. It was not part of the University at all and housed an extraordinary collection of eccentrics. It survived, in a way that would now seem almost miraculous, on the instincts and energy of Margaret Masterman its Director (MMB), and the grants she extracted from the UK and US governments and later the EEC Commission. Among those then working there are now numbered an FRS, an FBA and a Stanford full Professor, but also some, no less talented and original, who had no academic careers in the conventional sense, and among whom were AFPR and MMB. AFPR would be modestly pleased that he is now enjoying something of a revival among physicists and that a memorial lecture for him is being given, but not wholly surprised. His surprise might be that it is being given by me!

The work of the CLRU at that time, its golden age probably, was in three parts: research on language and computing, including translation, which was the main interest of MMB; research on information retrieval, by Roger Needham (RMN) and Karen Sparck Jones (KSJ); and the Information Structures Unit, under Ted Bastin (EWB).

Some people had interests in two areas: KSJ in the first two, and Stuart Linney (DSL) in the first and last. AFPR was unique at CLRU in that he actively contributed to all three. He also collaborated with RMN to set out the Theory of Clumps, which has become a standard tool for statistical information retrieval and provides functions with which data that may share no common features can best be clustered into coherent sets or clumps.

My own interests were and are in the first area, language computation, where AFPR is perhaps least known. He did write a book on language (Parker-Rhodes 1978) and I recall that I had something to do with pressing him to get it out and the publishers to take it. But it had all the faults of any AFPR work, the very ones that, along with a lack of ambition, made a conventional academic career impossible for him.

He did, of course meet one essential condition for British academic advancement which is to get to yourself into Oxford, Cambridge or Edinburgh and never, ever, leave.

But that is only, as they used to say, a necessary and not a sufficient condition: he also suffered from an inability to revise a draft, an unwillingness to publish anything he wrote, and a blithe ignorance of the role of citations. The combination of these faults is disastrous. With most of us drafts converge, under criticism or self-criticism, and the last draft is better than the first. AFPR, after criticism, would simply go and write another draft, quite different from

the first, so the process never converged. Also, having written a document--and more about those documents later--his normal procedure was to put in it a drawer, for he had made his point by writing it. He was rather like the mathematician in the old joke who, having proved the raging fire cannot reach his bed, goes back to sleep.

His book had some of these failings which may explain why it fell so stillborn from the press; I had hoped the publishers would sub-edit it and give it the shape he would or could not, but no, they simply printed what he sent without examination, and so were the worst possible combination of author and publisher. As to references and citations, he shared Wittgenstein's view that it was unimportant whether anyone had said the same thing before or not: all that mattered was the content of the thought, which is not a view that endears its holder to colleagues. It was said that he only put some random citations into the book because MMB told him you had to so as to get a book published. Once he saw that was part of the obligatory syntax of book writing, he could carry it out.

Before moving to some detailed discussion of his views on language structure, and how they fitted, or did not, with his other views, one should remember that he did not begin from any assumptions one could call philosophical, in the way that some of his colleagues at CLRU most certainly did see their work as an extension of metaphysics by other means. One could say that AFPR was a Quaker, which may in part explain his distaste for abstract thought until, that is, one remembers that Eddington was one too.

If you asked him if he was hungry he would look at his watch, which was for him a joke about the externalization of sensations. Conversely, he often said, when explaining why he never consulted a dictionary: 'If I want to know what a word means, I ask myself'. And he told me once that if he wanted to understand a new subject he wrote a book about it; the remark being in the context of his once having written a history of the world, which I assumed had gone into the bottom drawer. Clearly, anyone who holds the assumptions behind those jokes cannot have a consistent philosophical position: sensations for him were externalized, while at the same time he knew meanings by introspection!

AFPR's original thesis work had been on the statistical growth and development of fungi populations, which had little direct connection with his CLRU interests, except with the statistics in clump theory. But the mycologist in him remained very active: he tramped Nepal with Damaris seeking rare fungi; he loved knowing better than the rest of us what was poisonous and what was not. Tom Sharpe rented a writing hut in the CLRU garden opposite AFPR's, who once said to him, as they looked at a garden fungus, if you believe that to be edible it is surprising there are so few deaths from fungus eating as there are. Although their attitudes to drafting and revision were quite different, the two seem to have got on well.

There was an obsession at CLRU with working in a hut in a garden. I believed at the time it had descended by apostolic succession from Wittgenstein, who had taught MMB briefly, but I could find no reference to hut work in the biographies of him I read later. AFPR had originally worked in the CLRU main building, upstairs and right outside the hidden door to the chapel,

but his thumping on the floor as he thought and typed, humming and moaning, eventually brought about his exile to a hut.

AFPR's intellectual formation was a cluster of diverse interests and influences: Boolean logic was an obvious one, and later lattice theory, to which he was introduced by MMBs interest in it. Michael Halliday, then a lecturer in Chinese at the University and later the major British linguist, introduced AFPR to that language and the notions of formal linguistic syntax, particularly its central notion, present in virtually all syntactic theories, of the way linear items cohere to form longer ones, based on an underlying principle of the substitutability of shorter items for each other, so that some class of larger, including, items remains unchanged with respect to some property, such as "well-formedness".

Here one should add Brouwer's time-centred mathematical intuitionism (Brouwer 1913), which AFPR got from EWB and Clive Kilmister, and perhaps in part from DSL, who knew intuitionistic logic, as well as Spencer-Brown's idiosyncratic notion of logic (1969) based on no more than the ability to make a distinction of this from that (with Brouwer it had been present from past). These notions were important to him, I think, in both his physics and linguistic work.

Associated with CLRU at that period were also cyberneticians, a word now rarely used, but one accepted by Gordon Pask and Robin McKinnon Wood, who collaborated with CLRU at the time. The cybernetic notion of self-organisation was crucial to the ISU approach to physics, one quite different from the then dominant artificial intelligence paradigm. But AFPR was never a cybernetician at all, for that always involved a process view of the world, using continuous mathematics. AFPR wanted a structural theory, using discrete mathematics, at least where language and physics were concerned.

AFPR's writings and thoughts on language processing and structure were not all of a piece. Some were occasional items, such as his astonishing 'A constructive criticism of NUDE' (1965), a title which requires deconstruction, as some would say. NUDE was a semi-formal language (from 'naked ideas') based on ideas first set out by R.H. Richens, a biologist interested in plant classification. It had about eighty primitive items, Anglo-Saxon monosyllables for the most part (like FOLK, STUFF, MAN, THING), along with brackets and connectives. The idea was to use NUDE formulae to describe the meaning of sentences in a way that was formal, removed from English or any other natural language, but which could be used by a program to, say, translate from one language to another by means of an intermediate language, or interlingua, such as NUDE.

Although eccentric, NUDE came to look strikingly like the languages for semantic coding derived much later by linguists for meaning representation (e.g. Jackendoff, 1990). But AFPR was never very warm towards the interlingual stream in CLRU thought, which was basically MMBs. When accused by her of scepticism, his practice was to sit down at his typewriter and immediately write what he considered a constructive contribution. That typewriter deserves a special mention: AFPR did not believe in advanced machines, even those of the Sixties. He had an old fashioned manual typewriter, with which he was able to create mathematical symbols at will by advancing the carriage half a space and then overtyping another letter, a process that

produced a wholly novel and extensible alphabet that looked a little like Hebrew. In the same spirit, he had a beautiful hand calculator that he used with astonishing versatility long after there were tolerable electronic ones.

Yet the AFPR paper I refer to is extraordinary: he noticed immediately, what still is not obvious to linguists and logicians, that it is decidedly odd to code a natural language in forms that are plainly those of a real language (English in capital letters in the case of NUDE). So he set out to construct an artificial interlingua, with a vocabulary of about 150 primitive items, and a syntax more rigorous than that of NUDE but also speakable, so that its 'sentences' were no longer than what they encoded: for NUDE formulae could easily be twenty times the length of the word they expressed.

Here is a part of his language, which I think never escaped from AFPR's drawer, except for a draft he gave me. It was, perhaps, too clever and too ingenious to have been adopted, even if published. It might conceivably have become a new Esperanto for people who wanted a smaller language, easier to learn; AFPR remained fascinated by artificial languages and that must have been part of his motivation in constructing this language. He also imposed a very odd and original constraint on his language, which he believed would make it transparent, in the sense that two observers would always agree on what an expression meant (which was not always the case with NUDE).

qa topàr qa tòmùr dvir	"both him and her"
qa tomùr e topàr tir	"her or even him" (bot not both)
o topàr nu tomùr tir	"him indeed but not her"
qa tomùr o topàr dvir	"her and certainly him too"

Figure 1: A fragment of AFPR's Language.

He postulated that every string of syllables could be a constituent of the language. On the face of it this is an odd demand, and one not met by any actual language. It would mean, for example, that if English were a language obeying such a constraint, then in 'I want to go to Los Angeles and be in movies' the strings such as (Angeles and) or (go to Los) would all be constituents of the sentence, which they are plainly not, in the sense that (conventionally) 'I' is its subject constituent and 'want to go' its complex main verb and 'Los Angeles' a proper name within it. One could say that the whole purpose of the syntax of a language is to give a formal device to specify which substrings of a sentence are constituents and which are not.

On further examination, however, this conventional wisdom turns out to rest on highly intuitive assumptions about linguistic propriety: Steedman (1982) proposed later that it could make perfect sense to postulate a parser for English in which every string, of any length from the beginning of a sentence, could be considered a constituent: a claim that violates exactly the deep intuitions that AFPR's principle does. However, AFPR was not making a claim about English, but stating a formation rule for a language he was inventing. In practice, and from the fragment of the language he gives, his rule would have led to great difficulties. One could say that it violates the whole notion of a "higher order" item if any lower order selection can be one: that it makes such a language informationally inexpressive, in the way that a language would be if any string from its alphabet was a word. We would then lose redundancy if a character was lost in transmission, since what was left would always still be a word.

AFPR's suggestion may or may not be open to a similar objection, an issue which is closed since the paper was effectively buried and had no successors or discussion, and I mention it only to bring out his ingenuity and playful creativity: a whole new speakable mini-language was created overnight to answer a charge of unhelpfulness about NUDE. A point worth drawing out here in passing is that AFPR, unlike most contemporary linguists, really did know languages, at least as regards reading them. He learned them as a hobby and would sometimes admit to twenty three: he said they became easier after the first half-dozen because you probably had a hook to hang a new one on.

Inferential Semantics

AFPR's major statement on language structure is the book *Inferential Semantics*, which has not been influential, in spite of its visionary quality. It begins with his own special kind of gusto: a brief excursion into the notion of self-reference which is implicitly illustrated by the fact that the book itself is listed in its own bibliography. A footnote warns us that this is not normal practice.

It was not his first theory of language structure, which was probably the method for syntactic parsing he developed from Michael Halliday's grammatical theories (1965) and which I attempted to program on a Hollerith punch card machine in about 1962-3. His method was to map Halliday's syntactic constituents for a sentence, and their inclusion relations, onto some elementary Boolean lattice and then to attempt to determine the correct parsing by lattice operations. In fact, the punch card machine was a clumsy but perfect

engine for the purpose because the computation involved none of the elaborate binary codings common at that time: the physical overlap of punched holes from card to card determined the set intersection relations and the results could have been read by eye.

He always attributed his use of lattices for natural language structure to MMB, although her original insight was quite different from what he came to use those structures for. MMB believed that a thesaurus, like Roget's, which classified words by their meanings, by placing them under about 1000 general head concepts (MOTION, SUBSTANCE, VEHICLES etc.) was a key tool for the computation of meaning, and of more use than dictionaries, because a thesaurus worked from the meaning (the head concept) to the word and not the other way about (as a dictionary could be said to do).

I will assume the general notion of a lattice as a system of partially ordered sets such that every pair of such sets has a unique upper and lower bound, usually taken to be Boolean set intersection and union. If the generators of the lattice (in the graph representation, these are conventionally the line of nodes drawn horizontally across the middle) are identified with sets corresponding to the 1000 head concepts of Roget then, if a thesaurus is a lattice, one will expect each pair of heads (and so the sets of words associated with them) to have a unique intersection and union, and each of those again to have a unique intersection and union with any other set in the system. In that way overlaps of word sets under thesaurus heads can be calculated in a definite way, and this, MMB believed, could form the basis of calculations to, say, resolve semantic ambiguity in texts as part of a machine translation project.

That particular method was not wholly successful when experiments were done, and MMB did not always distinguish between assuming a thesaurus was represented by a lattice and showing that it was. Nonetheless, the idea was well ahead of its time in terms of using an existing language data base for natural language processing, an idea that only became practical in the last ten years. Even interest in the use of Roget has revived and Yarowsky (1991), using a complex refinement of MMB's original method, now claims to resolve over 98% of words in text successfully to a unique sense, where that is taken to mean attaching a word in context to one and only one thesaurus head.

The main motivation for using a lattice was one of multiple classification: the desire to put a word context in a number of sets simultaneously, in a way one cannot do with trees, then the dominant linguistic data structure. AFPR, though, was never very committed to MMB's main idea of the lexical lattice, a lattice of words classified by their senses: in his book he gives clear counter examples to this as a general structure for lexicons, yet, nonetheless, he continues throughout to use it as an ideal lexical structure. This is almost certainly the first work to give a full abstract structure for an ideal lexicon (apart from MMB's own work), an idea that has revived in recent times with inheritance structures like Gazdar and Evans' DATR (1990).

AFPR's goal in the book, as its title suggests, was not primarily lexical but a general theory of inference in natural language and, most ambitiously, the relationship of inference to understanding. The lattice gave, as does no other plausible elementary set theoretic structure, the closure of inference that he wanted. It is an old chestnut in the philosophy of mathematics that proof and understanding are related: that, in mathematics more than ordinary language,

one understands a proposition by proving it, and the proof becomes a context that gives the theorem its interpretation. This notion had been extended to natural language by Wittgenstein and in particular by R. Bosanquet in the 1940s whose work was published after his death (1945) by Richard Braithwaite, MMB's husband, so AFPR could have known of it.

My own thesis work, to which AFPR's book makes reference (1968), constructed a computational model of that idea, that understanding and a very weak notion of inference in text were closely related. But AFPR wanted a more radical relationship: his basic claim in the book is that there is a weak notion of inference $a \rightarrow b$ just when understanding a allows b to be understood. His inference lattices are a model of that relationship, and provided wider textual context by means of binary, applications of lattice operations. I find the fundamental notion fascinating but unbelievable, in that, given any sentence I understand I could not then tell you much about what set of sentences it allows me to understand. In the simple manner of standard logic, one can say that:

I have a red hat

allows me to deduce just

I have a hat

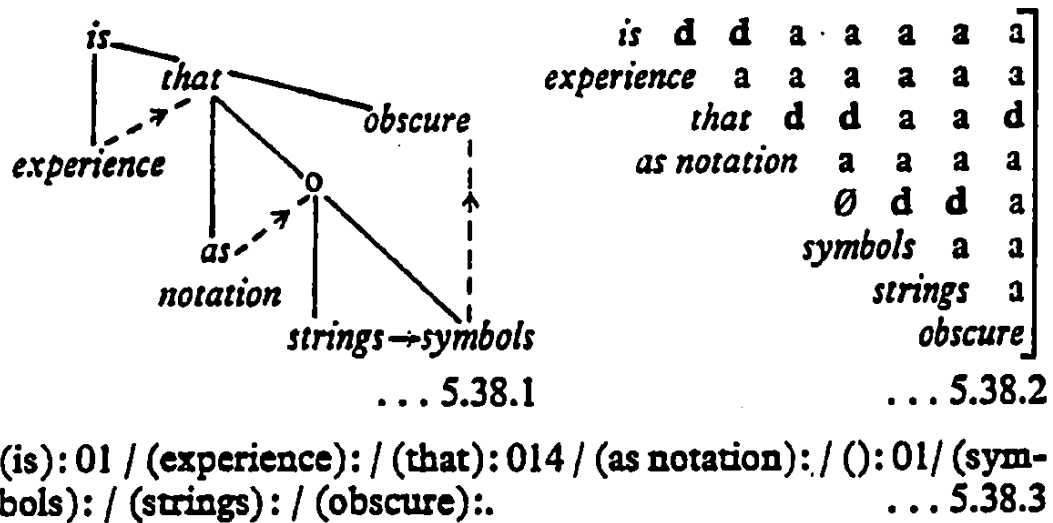
and

I have something

but I cannot be sure that understanding the first allows me to understand the last because I may not happen to know the word 'something'; perhaps I know no words over four letters long.

With this basic relationship defined, however, AFPR's game is afoot: the lattice gives the closure of the structure of inferences of a language (See Figure 2 below) and the sets at the nodes of the lattice are structures representing rhemas, the utterances themselves, whose structures are directed graphs (See Figure 3 below). He says he decided, wisely in my view, against calling the book simply Rhematics. The basic mathematics are then all from either Birkhoff's Lattice Theory (1949) or Harary's Graph Theory (1969) with some original bits to tie the two together via node algebras, so that an associated set of sentences at a lattice node correspond to a set of graphs that themselves form a sublattice of the whole.

The following example will illustrate the three notational methods described: it is based on the utterance "*It is commonly found that symbol-strings are notationally obscure*", which I interpret rhematically as:



In the diagrammatic notation, the designatory arcs are indicated in solid lines, the accessory arcs, where they do not coincide these, as dotted lines (with arrows, since they sometimes have to run upwards, whereas the designatory arcs can always be drawn running downwards).

Figure 2: AFPR's rhemas (p.105).

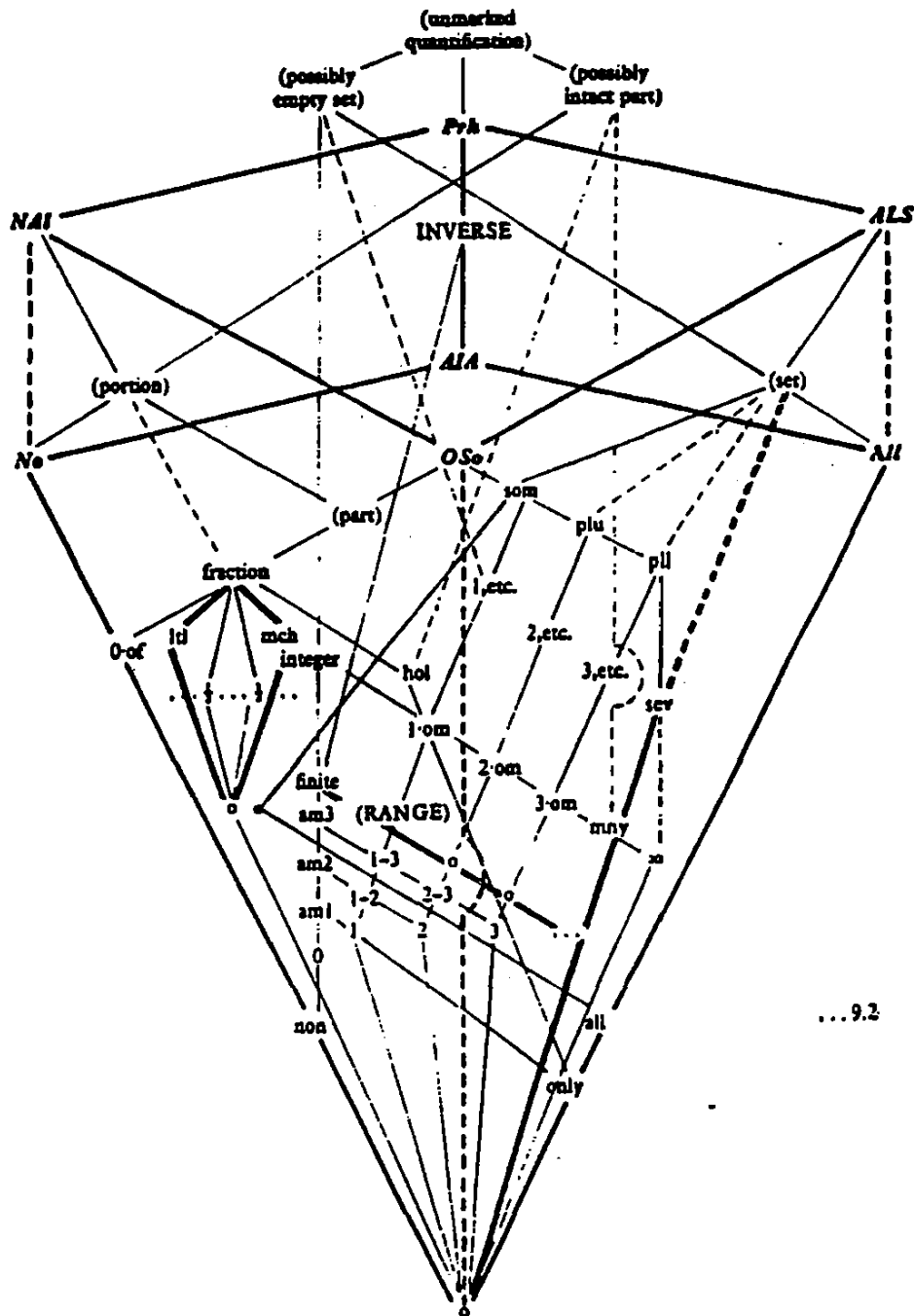


Figure 3: The Quantification Lattice

Into the rhema graphs, AFPR packs all the then fashionable parts of fringe linguistics: such as case grammar and speech acts, for graph theory is infinitely flexible. The originality is the uniform structural treatment of all levels of phenomena by a method utterly distinct from then conventional linguistics which had (and has) few structures beyond trees and a little elementary logic. But AFPR demands more than such a structure can offer; he claims at one point (*ibid.* p. 63) that the structure yields decidability, which means an algorithm to decide whether or not one can understand some *b* given some *a*. I cannot believe that such a fragile relationship can be captured by so powerful a machine.

Notice there is no role for process here - it is all abstract structures - nor is there any role for data, which is now the fashion in linguistics again, and AFPR strongly rejects any form of empiricism. I believe that, strong as his vision was, data could not be provided for it in any straightforward way since, as I asked above, how could one find, in a determinate manner, the set of sentences that an understood sentence allowed one to understand? And, just as AFPR's notion of structure is far more powerful than anything in linguistics then or since, it is not in any sense artificial intelligence either, since it accords no role to the knowledge required for language understanding: for him, understanding is simply a primitive relationship on which everything else rests.

AFPR had no time for Chomsky, normally considered the founder of the formal turn in post-war linguistics, yet they have much in common: the main difference between them being that Chomsky was satisfied with a much simpler mathematical structure for language, that of general logic itself, in Post's production rule form. Chomsky, too, worried about the decidability of his system, transformational grammar: it was not decidable, as became clear. Like AFPR, he had no real interest in computation or processes, though he did, initially at least, believe in structures of simple propositional meanings, which he called the deep structures underlying language utterances, a notion AFPR could never accept.

In the end, I think the most original idea in AFPR's work is the implausible primitive notion of understanding on which everything rests: it becomes somewhat more acceptable when one realises it has nothing at all to do with inference as normally understood, but only with the relationship between what he calls 'repertory' (the set of rhemas inferable from a rhema, in his special sense of infer) and 'rehearsal' (the set of surface rhemas that make up a particular discourse). It is this relationship that the whole magnificent formal architecture is designed to capture and behind it all is his conviction that understanding/inference is a more local matter than normally thought, where by "normal" I mean those aspects of contemporary continental thought, as well as Wittgenstein a generation earlier, that have conditioned us to believe that to understand a sentence fully we have to understand a whole language, and that everything is part of everything else.

But there is a process-related and cognitive strand of thought abroad these days, well supported by evidence, (e.g. Walker and Rambow, 1994) that shows the locality and finiteness of normal conversational inferences, and simple graphical structures have recently been suggested for capturing them. AFPR's architectonic could be seen as an early model for that, and for local

closure, one that kept the inferential repertory very close to the surface rehearsal of language.

Representation and Computation

An obvious feature of AFPR's language work, published and unpublished, was that he freely used strings of binary numbers not just as representational devices for language constituents, in the sense of naming them by binary numbers rather than by conventional forms like NOUN-PHRASE or ANIMATE-NOUN, but also as contentful strings on which direct computation could be done.

This view marked him off totally from contemporary language work in both linguistics and artificial intelligence, where it had been an item of dogma since the mid-Sixties that:

- (1) arbitrary names were absurd and a way of deceiving ourselves about what we knew about language, and that
- (2) although computers, and perhaps brains, worked with binary strings, that level of representation had no significance, in that it was merely a machine code version of the higher structures with which brains and computers really functioned.

That 'higher' level was what AI languages like LISP and prolog manipulated best, or which could even, according to some, be a form of English-like brain language, or indeed the plainly English-like forms of NUDE and contemporary linguistic representations. AFPR could not accept this: as we saw with his critique of NUDE, he actually wanted arbitrary representations, which he thought users should take the trouble to learn, implicitly rejecting the riposte that those arbitrary languages would then, on being learned, have all the ambiguity of actual languages, as Esperanto itself is said to have developed.

Although one could reject his level of representations as proper representations, nevertheless AFPR did remain committed to representations as such, and to simple mathematical structures like lattices and Boolean logic for manipulating them. Had he survived to see today's swing of the pendulum in AI back towards statistical methods in the form of connectionism and neural nets, AFPR might have been expected to welcome a movement that was anti-representational and to have found structures like binary vectors a proper starting structure for a language 'acquisition' device, and to have reused statistical functions which, while not those of his clump theory, were not a million miles away either.

However, I suspect he would not in fact have welcomed that development, partly because he believed low-level representations accessible as such, and interpretable, perhaps even at the brain level, and because he did not seek an overall consistency between the phenomena he treated statistically (such as clumps over data sets) and those he thought of combinatorially (such as language and quantum phenomena). However, his addiction to binary code--which I have argued was more than the conventional attachment of a programmer to much earlier generations of machines-----and his desire to

avoid the self-representation of a language by moving to a wholly arbitrary interlingual scheme, showed that he could not accept the main assumption of those in the so-called Language of Thought movement do accept. That is that language can, in brain or computer, be its own representation and models of it are not like models in science where the model and modelled are, by definition, different types of formal object. His book began, you will recall, with warnings against the perils of self-reference.

I close with this point because, so far as I understand it, that self-modelling is exactly what he did come to accept in the realm of physics where, like von Weizaecker (1971), he was 'against models as such' for quantum phenomena since his combinatorial logic itself became the object of study (as opposed to being modelled by it, as in the conventional view of science). If that is the case, AFPR again did not seek any kind of metaphysical consistency between the study of language and quanta. And why should he, as they are plainly not the same? Of course, since I happen to hold the "self-description" view of language as possible and indeed necessary, I wish AFPR had held it, too, but there it is. Things might well have turned out differently if he had had someone to talk to about his language ideas at CLRU as he did in information retrieval and quantum physics.

Let me leave you with his words, not mine: he once returned to work after a friend's funeral and on being asked, conventionally, how it went, he said, 'Oh, it was quite successful; yes, very successful'.

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THEORETICAL QUANTITIES: AND ENTROPY

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The notion of theoretical quantity, or theoretical concept, has played an important part in the philosophy of physics. Electrons were (and remained) theoretical concepts for Mach; the status of quarks is still not entirely clear. But the notion itself is unclear. Now the viewpoint of combinatorial physics is that all quantities entering the discussion are necessarily physical so that theoretical quantities, properly so-called, cannot enter. It is interesting to see how the hierarchy structure provides for this in a simple example. The particular example arose in the course of a continuing investigation of how to carry out the statistical treatment of the genesis of a discrimination system. Although in the past I have often talked glibly about the building up of a dcs, the algebraic structure does not really deal with this but only with the completed dcs. As an interim report I will confine myself to a very simple example worked out in detail. The example is like the ones found in elementary textbooks on probability.

Consider drawing balls from a bag containing such a large number that drawing one ball does not materially alter the probabilities. Of course, this is not the sort of thing that we are really concerned with, any more than it would be in a text devoted to teaching probability to demographers. The balls look exactly alike but differ slightly in mass. Discrimination is carried out by means of a sensitive balance without weights. The balls are in fact of three masses, light, standard and heavy but we don't know this; this 3 is

the first theoretical quantity and the point of the example is to study how theoretical quantities can develop into empirical ones. In the experiment, the proportion of light balls is 20%, of standard 30% and 50% are heavy. These probabilities are naturally again theoretical quantities, like electrons or quarks. The process of drawing and discrimination will in due course give rise to D_3 (the dcs generated by three elements) the elements of D_3 being, I remind you, not balls but bits of information. For simplicity in working out the arithmetic, I shall ignore the elements 12, 13, 23, 123 and concentrate on 1, 2, 3.

A draw is made and the result is labelled 1 so that, as an artefact of the labelling system, if $p^r(n)$ is the probability of getting n at the r th stage of the process,

$$p^1(1) = 1, \quad p^1(2) = 0, \quad p^1(3) = 0.$$

Another draw is made. What is the chance of 1 again? The chance that 1 means "light" is 0.2 and in that case the chance of another 1 is again 0.2. Similarly for the two other weights so that, since $(0.2)^2 + (0.3)^2 + (0.5)^2 = 0.38$,

$$p^2(1) = 0.38, \quad p^2(2) = 0.62, \quad p^2(3) = 0$$

(because, if the element 1 does not come up, the rule is that the new element is called 2). Before going on to the next stage, I sort out a more general method. At stage 2 there are two possible sequences:

11 which can occur as LL, SS, HH

12 which can occur as LH, HL, LS, SL, SH, HS.

so the previous results can be obtained in the form:

$$p(11) = \sum (0.2)^2 = 0.38 = p^2(1),$$

$$p(12) = 2 \sum (0.2)(0.3) = 0.62 = p^2(2).$$

At stage 3 the possible sequences are 111, 112, 121, 122, 123. Evidently

$$p(111) = (0.2)^3 = 0.16,$$

$$p(123) = 6(0.2)(0.3)(0.5) = 0.18,$$

leaving over

$$p(112) = p(121) = p(122) = \frac{1}{3}[1 - 0.16 - 0.18] = 0.22$$

from which it follows that

$$p^3(1) = 0.16 + 0.22 = 0.38 \text{ (again!)}$$

$$p^3(2) = 0.44, \quad p^3(3) = 0.18.$$

So it looks as if $p^r(1)$ has already reached its steady state value when $r = 2$.

This will turn out to be the case.

In fact we have:

	n			S_r
	1	2	3	
1	1	0	0	0
2	.38	.62	0	0.9581
3	.38	.44	.18	1.4969
4	.38	.38	.24	1.5550
5	.38	.3572	.2628	1.5677

These probabilities are still theoretical quantities though $p^r(1)$ is verging on the empirical since empirical probabilities arise from long-run steady-state values.

The right-hand column contains the corresponding values of another theoretical quantity, the entropy defined in the usual way as

$$S_r = \sum_n p^r(n) \log[1/p^r(n)]$$

with logs to base 2. If you work out this and other numerical examples,

this always increases. I wonder why I cannot prove this? Anyway, the process is exhibited as one tending to a state of maximum entropy and this maximum entropy is determined by the steady-state values of $p^r(n)$. What are these steady-state values?

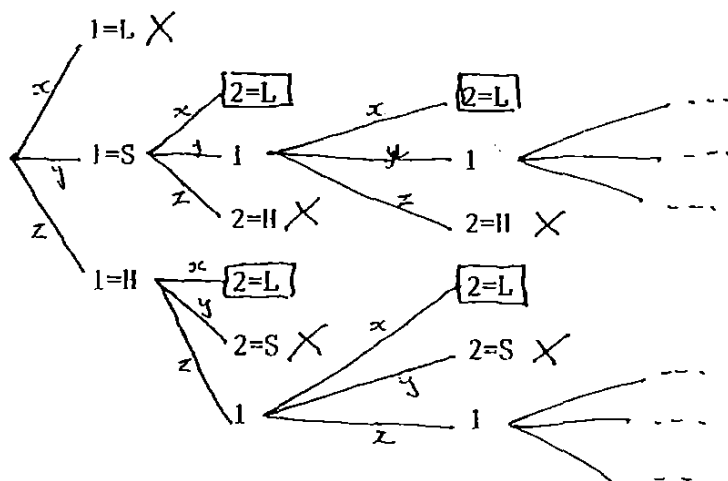
Let us replace 0.2, 0.3, 0.5 in the above by x, y, z , where

$$x + y + z = 1.$$

It is then easy to confirm that, as soon as $r = 2$,

$$p^r(1) = \sum x^r.$$

To find $p(2)$, the steady-state value of $p^r(2)$ consider the tree diagram below and ask for the probability $p(2 = L)$ that 2 means "light"



It is obvious that, at each stage after the first, there are two growing points labelled 1, two blocked off and two where $2 = L$. Continuing along an infinite tree one gets

$$\begin{aligned} p(2=L) &= yx + y^2x + y^3x + \dots + zx + z^2x + z^3x + \dots \\ &= x[y/(1-y) + z/(1-z)]. \end{aligned}$$

From this it follows that the limiting value $p(2)$ is

$$\begin{aligned} p(2) &= xp(2=L) + yp(2=S) + zp(2=H) \\ &= \sum x^r [y/(1-y) + z/(1-z)] \end{aligned}$$

and then $p(3) = 1 - p(1) - p(2)$. The limiting values in the numerical example are then .38, .3393, .2807 with $S = 1.5740$.

Now at last we have empirical, not theoretical probabilities. If Bastin & Kilmister are right, we ought to be able to infer the original theoretical probabilities from them and so reify those original ones into empirical ones. But can we? In this example, this is just an algebraic problem.

We have

$$\sum x = 1, \quad (1)$$

$$\sum x' = p(1) = a \text{ (say)} \quad (2)$$

$$\sum x'[y/(1-y) + z/(1-z)] = p(2) = b \text{ (say)}. \quad (3)$$

Here it is easy to see that

$$1 \geq a \geq \frac{1}{3}, \quad \frac{1}{3} \geq b \geq 0.$$

The problem is to determine x, y, z from a, b .

We begin by squaring (1) and subtracting (2) giving

$$\sum yz = \frac{1}{2}(1 - a). \quad (4)$$

We then reduce (3), using (1), (2) to a simpler form:

$$xyz = \frac{1}{2}(1-a)(1-a-b)/(4-2a-b) = c \text{ (say)} \quad (5)$$

(see appendix). Since $c \geq 0$, we can conclude that $a + b \leq 1$, as well as the other two restrictions. "Restrictions" is a misleading word. The conditions on a and b will be satisfied automatically if the process really is the drawing of balls. They are the conditions that the probabilities in the process can be interpreted in terms of those particular theoretical entities. Thus they are a bit like dispersion relations.

It follows at once from (1), (4), (5) that x, y, z are the roots of the cubic equation

$$f(u) = u^3 - u^2 + \frac{1}{2}(1-a)u - c = 0.$$

Since $f(0) = -c$ and $f(1) = \frac{1}{2}(1-a) - c = \frac{1}{2}(1-a)(3-a)/(4-2a-b)$, there is certainly a root in $(0, 1)$ and so presumably 3. That a, b should indeed provide three

roots in $(0, 1)$ gives rise to another "dispersion relation":

$$-(3a-1)^{3/2} \leq [5-9a-54c]/\sqrt{2} \leq (3a-1)^{3/2}$$

APPENDIX

We have, using (1), that (3) is equivalent to

$$\sum x(y^2 + z^2)/(1-x) = b,$$

or to

$$\sum x[(y+z)^2 - 2yz]/(1-x) = b.$$

But (1) gives $(y+z)/(1-x) = 1$, and since $\sum x(y+z) = 2 \sum yz = 1-a$ from (4), we have

$$2xyz \sum 1/(1-x) = 1 - a - b.$$

But $\sum 1/(1-x) = D^{-1} \sum (1-y-z+yz)$, where $D = 1 - \sum x + \sum xy - xyz$. Hence

$$\sum 1/(1-x) = (1 + \sum yz)/(\sum yz - xyz) = (1 + \frac{1}{2}(1-a))/(\frac{1}{2}(1-a) - xyz),$$

using (4). From this it follows that

$$xyz/[\frac{1}{2}(1-a) - xyz] = (1-a-b)/(3-a),$$

from which (5) follows easily.

HETEROGENEOUS OBJECTS

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Mike Horner drew attention to my oft-repeated talk about the way objects at the limits of extrapolation differ from normal objects in that as we get towards them the number of independent lines of enquiry gets whittled down until ultimately there is only one. The description then has a quite different logical status from anything imagined in conventional physics. We can call them quantum objects if we don't mind including 'the observable universe'. They throw back at us our own logical categories in a most unpolitically-correct way, and our efforts at combinatorial physics have centred on the need to explain why these logical categories and not others. Mike now suggests that it is muddling if we speak of the quantum object as a limiting case of the classical object, and simply accept it as the norm. If we do that then we have to give some account of the classical object. Mike suggests we use the term heterogeneous object and so suggest from the outset that it is the classical object with its strange mixture of quantum properties and macroscopic connexions that is the funny one.

Some remarks about labels. An element is already stored when it is specified as a single element of the algebra. Then the CH construction can be interpreted as storage in that there is a single dcs which is then represented by a matrix -i.e. a single element at the next level. And that's all there is to it. Now the reason why we fought shy of this simplicity was that there is no sense in talking of storage unless there is something you could call 'non-storage'. Storage had to be a two-stage process. If we could separate these stages then we should have the principle for the use of the 'second progression' in CH which, subject to correction from Clive I shall identify with the matrix algebra and which we have always assumed in some way to be for storage of information. He agrees that we must have something but thinks there may be other possibilities. The necessary new idea to separate the stages is to recognize level ambiguity and to prescribe that when we decide to remove the ambiguity we are imposing a constraint. So the element 1 is not stored because it can be $(1, 0)$, $(1, 0, 0)$ and so on, (writing row vectore for columns for convenience). But once the vector space is brought in, and it is written $(1, 0)$ then it is stored.

At the first stage elements arise and are labelled 1, 2, 12, 3,... We now define STORAGE as that which occurs when these labels are recognized as constrained to a particular D_n These forms are called 'exact' in contrast to the systematic ambiguity envisaged by unconstrained labels, where 1 might stand for all the bit-strings with 1 in the

first place and any number of zeroes.

Now back to 'constraints'. We wish to use that term to refer to deviations from the assumption of indifference in the background which conveys empirical information. Now we have a much better articulated definition, but it is different at first sight. Clearly the two are connected. I make an intuitive jump here and assume that the constraints which give particular shapes to the filling up of (and giving useful structure to) the mapping or label space (the linear or matrix space) can be built up from changes in the specification of exact levels. I conjecture that this is right and will assume its truth but can't give a proof at the moment. In the case of physics I have in mind a picture in which the space of the matrices is used as a repository for empirical spatial information -necessarily with a rigid level structure. It is the conflict of this rigidity with the way information accrues within the manifold of the dcss that is completely unrecognized in current physics and is responsible for paradoxes and failure of vision over the relativistic limits.

You will see that the introduction of memory (or storage with the necessary facilities for referring to it) is rendered possible at the cost of sacrificing level ambiguity. You will say however that this is all still oriented to the old scheme and we still have no picture of the use of the labels. The next step gets us a bit further on though because of the idea that when we allot a particular level to a given entity then we are imposing a constraint. We have hitherto thought only about completed dcss, but now we are thinking about the building up process of each by considering the constraints on each element of what would become the dcs if we didn't use the constraints to incorporate some more interesting information.

If we take this notion of a constraint, C say, into the discrimination system and note that there C needs to preserve discrimination, then

$$C(a) \text{ together with } C(b) \rightarrow C(a + b).$$

This simply says that the set of elements x satisfying the constraint, $[x : C(x)]$ is a dcs.

Now a new vision of where this is all leading. The interpretation of the combinatorial hierarchy has so far been essentially solipsistic, and Keith Bowden has criticized us on these grounds. It describes the world from the point of view of one observer. In the special case of relativity, the dilemma resolves itself into that of providing two truly independent observers -particularly in the context of special relativity where the problem has always been recognized in the very limited form of giving them equivalence. (No one saw that there was a problem in having them at all.) Clive has a telling example to illustrate a possibility for resolving an input sequence into two or more separate 'parts' or persons from music. There are the partitas for unaccompanied violin of which I know only the very famous one in -I think- E-minor. He says: "I call what I believe to be the clue in understanding the possibility of two viewpoints the PARTITA ARGUMENT. The reference is to those unaccompanied violin pieces of J.S.Bach, where a single player is heard by a casual listener as producing two interwoven themes. The question there is, how is this not merely possible, but a way of listening strongly urged by the music? The answer seems to me to lie in the notion of a constraint on the potential next note of a theme, so that a note which does not satisfy the constraint is assigned by the listening brain to a second theme." Then we can take this constraint into the discrimination system as discussed above.

In our search in a combinatorial theory for the theoretically identifiable points of physical reality (and if anyone objects to my language, speak now or forever hold your peace, for I think I could find language to suit all tastes) we have to look at the cosmological limit as well as the quantal. (Obviously, since cosmological constants, including c for this purpose, appear in our primary identification of theory with experiment.) The two ends of the

scale of complexity (note that I avoid saying the scale of magnitude at this stage) have to be seen as analogues of each other: even complementary.

Here we meet a puzzle. In the quantum limit conventional theory maps the quantum effects onto a continuum background with consequences which it is a large part of our corporate endeavour to sort out. Whatever the difficulties however, there are well-known devices, integral to the theory, which specify precisely the extent to which the dynamical concepts are to be taken to have their classical meanings. I say this, please note, in spite of my contending that the absence of any consistent language in which the imposition of the proper safeguards enter automatically instead of being unnaturally imposed, amounts to a disaster.

On the cosmological scale the situation is very different, and we may well say very much worse. We find ourselves working with a quantity which we call "the observable universe" -in other words everything which is within the "event horizon". What is more, we attach numerical measure to this quantity and to quantities related to it including the velocity of light. The paradox comes about through the incurable realism of our habits of thought. We overrule the thinking which tells us that the objects we meet as we approach the event horizon undergo some subtle change as a result of which they become less immediate to our perception because the approaches to them become fewer. Overwhelming commonsense says "You leave the objects alone because how we happen to be able to know about them can only be accidentally part of our description.

Thinking with the quantum situation in mind might suggest that we postulate some sort of compensatory device such as an uncertainty in the identification of the cosmical objects. The idea would be that we should apply a sort of second discrimination to the objects in a consistent way through some parameter (involving distance or velocity or some combination of those) which shows that we have interpreted a finite distribution of objects as an infinity of them.

I hope I have said enough to demonstrate that there is a very serious problem here. My suggestion about developing an analogy with the quantum uncertainty principle was mainly to put it to you that dire remedies must be preferred to having no remedies at all. I haven't done the necessary thinking that would be needed before proposing this suggestion but I have the feeling that it wouldn't work. Unlike the quantum objects the cosmological ones are irreducibly complex, and if we tried to override that complexity we should be mistaking something that was only a model for the reality (a bit like the old water-drop model of the nucleus). It occurs to me though that someone here may have heard of some such idea being proposed, in which case I should like to hear about it.

So I see the way forward differently. I put my cards on the table bluntly and crudely, although what I say may at first sight sound atavistic or insisting on looking back to a more primitive past. I want to use the distinction between the constructive manifold and the linear or mapping space in a more literal way than it has been hitherto, so that the conventional space is our linear space. I remind you that dimensional structure and the quadratic form which arises as soon as we contemplate the simplest metrical relationships comes from the constructive manifold and not from conventional space at all, so that there is a new division of function. However the linear space is where the storage of information is done and its elements are the labels which permit retrieval of it.

Two different kinds of investigations now confront us:- one technical and the other more philosophical. The technical one consists in showing how the partial constructions of dcss which I discussed earlier lead to simple forms of mechanics and go at least as far as the accelerated classical particle. We have done almost nothing in this direction, but progress will

certainly proceed from a bridge between these two progressions. I suggest that the red shift gives us a fixed point in understanding this bridge. We look at it as the prototypic metrical quantity instead of finding that on the laboratory scale. My guiding thought is that when we codify information from the very distant objects we are doing two things -not just one. Firstly we are imposing the metrical structure of the linear space in interpreting them as receding, and secondly we are treating them statistically as one piece of information after another in a sequence. Our perplexity arises from seeing everything as happening in one go.

The more philosophical investigation concerns perception and particularly perception of things in space. Pope has thought long and hard about what happens to perception and space when you really take the discrete seriously, and finds himself forced to postulate a "relative continuum" of ones own and other people's ordinary and scientific perceptions. This "blocsace" is entirely different from the absolute continuum of classical physics and relativity. He says "My relative continuum (i.e. blocspace) is a physical discontinuum, or discretum, and is continuous only relationally or cognitively".

One may think that Pope ought to have warned the relativity theorists whom he has tried to evangelize about all this, but anyway my purpose is to observe that his position has more than a passing resemblance to the one I am arguing for with the two progressions. In particular the blocspace is to be compared with my mapping space. In my case too, the mapping space has a cognitive character since it is how we expect the universe to behave -except when it doesn't. Also there is no c in the linear space: all that comes from the first progression, and relativity is the prime example of the confusion you get into by not knowing about the distinction between the two progressions. Pope would vehemently deny any dualism in his position. In our case dualism is ruled out because of the continual interplay between the two progressions. I do not know what Pope's judgment on this position will be, though I think he will find it advantageous in one way since it explains how the blocspace has the degree of physical reality that it evidently does since classical objects behave according to its dictates.

Does what I am suggesting really make any serious impact on the paradox in the unitary (conventional) picture from which I began? Are we now in a position in which the question "are the things which we see scurrying over the event horizon real objects?" ceases to perplex? My answer has to be along the following lines:- All perception requires interaction of the two progressions, though in homely circumstances (like between the Oval and the Andromeda nebula) we can use only the one without getting into trouble. Through this familiarity we learn a language (the classical theory-language) whose identification with physical reality we find it difficult to avoid. This 'commonsense' ceases to work, and we are forced to become aware of the two progressions in each access of knowledge (information if you must) when the supply of elements from the first progression gets progressively restricted (eventually to nothing at the event horizon). Then, in the American phrase, the damndest things happen. The muddle starts because we are still assuming objects with a lot of complexity without realizing that we cannot-as a consistent matter- represent the complexity. It actually is a bit like the way we cannot distinguish two electrons.

I hope this gets us out of the conceptual impasse and lets us get on with the technical bit (which I cannot do any more to now).

As I said I hope to get as far as the accelerated particle starting from my Ur-velocity. To go further we shall need input from a new source because we need electromagnetism. Here I get encouragement from recent work of Noyes arising out of his paper with Kaufman. He has a Zitterbewegung where the velocity change $+c$ to $-c$ discretizes the velocity. These vacuum changes are associated with spin flips at the "corners" to (to continue the

undemythologized talk) to get to the Dirac equation for the free electron in a $1+t$ space. I think this dimensional bit is really equivalent to our saying that dimensionality has not yet been defined. Now we have long supposed that the spin quantum number enters the hierarchy picture as the simplest level change, and an argument exists for deriving the half-integral spin this way. This thinking also agrees with Manthey's treatment. However we have lacked a way of making the obviously necessary development of electromagnetism out of the combinatorial spin concept, and now it looks to me as though Noyes' method might fill the bill. On that extremely sketchy tie-up of ideas, I close.

SOME REMARKS ON DISCRETE PHYSICS AS AN ULTIMATE DYNAMICAL THEORY*

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The standard model of quarks and leptons currently fails to meet 't Hooft's criterion for an "Ultimate Dynamical Theory" in that it contains 19 parameters which must be taken from experiment. Evaluating discrete physics in the same way we find that it requires 17 parameters and has already succeeded in computing 7 of them. While we are confident that the rest can also be computed, the very concept of an ultimate theory is incompatible with our attitude toward physics.

Our preferred descriptive phrase for discrete physics, or bit-string physics, is "a new fundamental theory", harking back to Eddington.^[1,2] Unfortunately, it has become the fashion to call such schemes "Theories of Everything"; I have even used the phrase myself in order to establish a context, although I was careful to put it in quotation marks.^[3] This fashion has recently been criticized by 't Hooft, the leading Dutch theoretical physicist.^[4] He points out that the most that can be accomplished by the theories which physicists have in mind when they use the phrase is to provide dynamical laws for physics and physical cosmology which have no arbitrary parameters and admit of no exceptions. What actually happens and is happening in the world of experience is much richer than any modest descriptive framework of this type could possibly explain, even if it should turn out to be successful from a physicist's point of view. 't Hooft suggests that we should use instead the more accurate term "Ultimate Dynamical Theory", which

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I have employed in the title of this paper. I believe that this corresponds to what Eddington had in mind when he used the title "Fundamental Theory" for his last work, and am confident that it is compatible with Fredrick Parker-Rhodes' vision of "The Inevitable Universe".^[5,6] It is also what Steve Weinberg is talking about in his *Dreams of a Final Theory*. One of my reasons for bringing up this topic here is that these discussions by prominent physicists allow us to claim that, after 17 years in the wilderness, we can now place some of the ANPA work within the spectrum of conventional speculative physics.

't Hooft makes it quite clear in his discussion of this topic that the most clearly articulated candidate for an Ultimate Dynamical Theory, the standard model of quarks and leptons, falls short of the mark. The obvious reason for its failure is that the standard model contains 19 (or 20) parameters which have to be taken from experiment. 't Hooft has to name two numbers because of the ambiguity created by the problem of whether or not to count the parameter(s) needed to make contact with the international system of units (kilogram, meter and second). If Newton's constant of universal gravitation (G) is used to fix the mass scale in terms of the standard kilogram it can be called the 20th parameter, but *some* such convention is necessary. I agree that this step should not be included in the parameter count. Once the mass scale is set, one still needs to define units for length and time. In the case of SI units, the unit of length (meter) is *defined* as the distance light travels in $1/299\,792\,458$ seconds, but the unit of time also needs to be defined. Presumably 't Hooft follows the usual theorists' convention that this can be fixed without ambiguity by measuring the quantum of action ($\hbar$) or of angular momentum ($\hbar = h/2\pi$), but he does not discuss this explicitly. For instance, one can take M_{Planck} to be the unit of mass given by $[\hbar c/G_N]^{\frac{1}{2}}$, the unit of length as $\hbar/M_{\text{Planck}}c$ and the unit of time to be $\hbar/M_{\text{Planck}}c^2$.

What are the 19 empirical constants that the standard model of quarks and leptons requires? 't Hooft specifies these as follows: 3 gauge coupling constants; 3 lepton masses m_e, m_μ, m_τ ; 6 quark masses $m_u, m_d, m_s, m_c, m_b, m_t$; 4 quark mixing angles; 1 topological angle θ_s ; 2 self-interaction parameters for the Higgs field (one

gives the mass of the Higgs boson m_H and the other the strength of the Higgs to vacuum transitions). Even to determine these parameters from experiment, except for the lepton masses, is a difficult and theory-laden task. The Higgs boson has yet to be observed, and the detailed interconnection between the various parameters referring to the strong (quark) interactions keep many theoretical and experimental particle physicists employed in “testing the standard model”. Many such detailed tests have been carried through to quantitatively successful conclusions, and self-consistent sets of *empirical* parameters. This large body of work is the solid basis from which dreams of a final theory take off. Nevertheless, the fact that the theory does not allow these 19 parameters to be calculated remains untouched.

In this respect bit-string physics is clearly superior to the conventional standard model of quarks and leptons. Thanks to the stability of the proton in our theory, we can use it to fix the mass scale and *compute* Newton’s constant G_N in agreement with experiment (Ref. 3):

$$G_N^{-1} \frac{\hbar c}{m_p^2} = [2^{127} + 136] \times \left[1 - \frac{1}{3 \cdot 7 \cdot 10}\right] = 1.693\,31 \dots \times 10^{38}$$

$$experiment = 1.693\,58(21) \times 10^{38}$$

Once this is done, we can fix the length and time scales using an experimental value for $\hbar$ and the conventional value for c . Our model does not require the Higgs boson in order to generate weak-electromagnetic unification and the particle masses, which eliminates two of the 19 remaining parameters at the start. What about the remaining 17?

Bit-string physics does much more than eliminate two parameters needed in the standard model. We share with the standard model a common description of the quantum numbers of quarks and leptons as analyzed using the weak interactions. The conventional theory has no systematic way of computing any of the 19 empirical parameters they need other than by putting self-consistent *experimental* bounds on them. But we have already computed 7 independent, basic parameters

which they can only take from experiment. Further, the remaining 10 we need should be within our grasp. We endeavor to explain why in what follows.

The success of the Parker-Rhodes construction⁽⁷⁻⁹⁾ in yielding $\hbar c/G_N m_p^2 = 2^{127} + 136 \approx 1.7 \times 10^{38}$, allows us to fix our mass scale by picking the proton mass as our parametric contact with experiment, and then *computing* G_N in agreement with experiment, as indicated above. The McGoveran correction is given in bold face (see below). Thus we replace 't Hooft's step of fixing the mass scale by using G_N with a number more easily, and more accurately, related to elementary particle phenomena. This $20 \rightarrow 19$ reduction in parameter count is quantitatively the same as 't Hooft's, but qualitatively superior in that we can use a parameter (the proton mass m_p) that is better known experimentally than G_N .

Parker-Rhodes' construction also gave $\hbar c/e^2 = (2^2 - 1) + (2^3 - 1) + (2^7 - 1) = 137$ as the inverse fine structure constant. But at that stage in our history, it was all too easy to dismiss these two numerical results as coincidences, with no basic reason for identifying them with the results of physical measurements. One of my reasons for founding ANPA was to underpin these basic results with a fundamental theory which could be understood by physicists. If one accepts subsequent work in discrete physics as a theoretical justification for accepting 137 as a first approximation to the measured value of α^{-1} , this gives us the equivalent of one of the three gauge coupling constants needed in the standard theory. We then have 16 additional numbers to compute, compared to the 19 needed in a conventional approach.

In our theory, the three gauge coupling constants in the conventional theory are replaced by $\alpha^{-1} = \hbar c/e^2$ at the mass of the electron, the Fermi constant for the weak interactions (G_F) in the dimensionless form $\hbar c/G_F m_p^2$ and the weak angle (θ_W), usually given in terms of $\sin^2 \theta_W$. Bastin suspected long ago (Ref. 8) that the 256^2 in the second Parker-Rhodes sequence was related to the inverse strength of the weak interactions. When Noyes got involved, he noted that the Fermi constant relates to experiment in a different way than the Yukawa-type coupling used to

understand α . Also, it requires the specification of a mass scale. Putting these two facts together with the necessity of using m_p to set the hierarchy mass scale, one concludes that $\hbar c/G_F m_p^2 = 256^2 \sqrt{2}$, which agrees with experiment to about 7%. The equivalent of a third gauge coupling constant ($\sin^2 \theta_W$) was identified by Noyes as 1/4 from the structure of our theory of weak-electromagnetic unification. At this point we had 14 constants to go, but the lack of precision in comparison to experiment allowed our critics to feel, with some justice, that the whole scheme was still a bizarre, numerological coincidence.

The situation changed, for me, when McGoveran invented a systematic way to improve on these results.^[10] Combinatorial corrections to all 3 weak-electromagnetic constants, and to our prediction for G_N were reported^[11] at the same time that the Sommerfeld formula for the fine structure spectrum of hydrogen was derived in our context. These values, with the McGoveran corrections in bold face, are

$$\alpha^{-1}(m_e) = 137 \times [1 - \frac{1}{30 \times 127}]^{-1} = 137.0359 \text{ } \mathbf{674}....$$

$$experiment = 137.0359 \text{ } \mathbf{895(61)}$$

$$G_F m_p^2 / \hbar c = [256^2 \sqrt{2}]^{-1} \times [1 - \frac{1}{3 \cdot \mathbf{7}}] = 1.02 \text{ } \mathbf{758}... \times 10^{-5}$$

$$experiment = 1.02 \text{ } \mathbf{682(2)} \times 10^{-5}$$

$$\sin^2 \theta_{Weak} = 0.25 [1 - \frac{1}{3 \cdot \mathbf{7}}]^2 = 0.2267...$$

$$experiment = 0.2259 \mathbf{(46)}$$

These results are not as yet in complete agreement with experiment, but so far the weak parameters have not received appropriate electromagnetic corrections; symmetrically, the value for α^{-1} still requires weak interaction corrections. Another problem is to make rigorous the qualitative (unpublished) argument which shows

that the value of 137 at low energy will “run” down to about 128 at the mass of the Z_0 in much the same way that the conventional theory requires. Nevertheless, we are far ahead of the conventional theory, which doesn’t have a clue as to how to compute numbers of the quality already achieved in 1962 and 1966!

As to mass values, Parker-Rhodes^[12] calculated the electron mass shortly prior to the foundation of ANPA, and two attempts have been made to justify this calculation in the combinatorial framework.^[13,14] The combinatorial value for the muon mass was published without discussion by Noyes, together with the correction.^[15] The pion masses were noted by Noyes in his first paper on the combinatorial hierarchy^[16] to be a consequence of the Dyson argument^[17] establishing the breakdown of conventional QED when it attempts to describe more than 137 charged particle pairs at short distance. The corrections are again due to McGoveran (Refs. 10,11, 13). The relativistic Bohr formula, which McGoveran derived combinatorially without being aware of Bohr’s 1915 result, was the starting point for McGoveran’s calculation of the fine structure splitting. It can be generalized to what I have called the “handy-dandy formula” connecting masses to coupling constants *non-perturbatively*, as is also explained in Ref. 11. Applied to the pion modeled as a nucleon-antinucleon pair, it gives the correct value for one strong interaction coupling constant, as quoted below. We now have three coupling constants, two of the three lepton masses and two of the strong interaction parameters ($m_\pi, G_{\pi NN}^2$), making seven of the 17 parameters we are looking for, Comparison with experiment is remarkably good, considering all the corrections which are still needed in a self-consistent theory:

$$m_p/m_e = \frac{137\pi}{\frac{3}{14} \left(1 + \frac{2}{7} + \frac{4}{49}\right) \frac{4}{5}} = 1836.15 \ 1497 \dots$$

$$experiment = 1836.15 \ 2701(37)$$

$$m_\pi^\pm/m_e = 275 \left[1 - \frac{2}{2 \cdot 3 \cdot 7 \cdot 7}\right] = 273.12 \ 92 \dots$$

$$\text{experiment} = 273.12\ 67(4)$$

$$m_{\pi^0}/m_e = 274[1 - \frac{3}{2 \cdot 3 \cdot 7 \cdot 2}] = 264.2\ 143 \dots$$

$$\text{experiment} = 264.1\ 373(6)]$$

$$m_\mu/m_e = 3 \cdot 7 \cdot 10[1 - \frac{3}{3 \cdot 7 \cdot 10}] = 207$$

$$\text{experiment} = 206.768\ 26(13)$$

$$G_{\pi N \bar{N}}^2 = [(\frac{2M_N}{m_\pi})^2 - 1]^{\frac{1}{2}} = [195]^{\frac{1}{2}} = 13.96..$$

$$\text{experiment} = 13.3(3), \text{ or greater than } 13.9$$

In order for us to calculate the remaining 10 parameters, or their equivalent in our theory, we will have to develop a detailed finite particle number scattering theory using the clue given by the “handy dandy formula” mentioned above, and the coupling between the three generations of quarks and leptons implied by the bit-string representation of the standard model quantum numbers already outlined in Ref. 3. The fact that we get the correct pion-nucleon coupling constant from a nucleon to pion mass ratio of seven to one gives us confidence that the (necessarily non-perturbative) strong interaction theory can be worked out, while the fact that we get the correct muon-electron mass ratio indicates that the coupling between the three generations of quarks and leptons (and hence the equivalent of the Kobayashi-Maskawa mixing parameters) can also be calculated combinatorially. What is needed is a lot of hard work, which would go faster if I could interest another elementary particle physicist our program.

Assume that this program succeeds in computing all elementary particle cross sections to appropriate accuracy. It would then have met the first challenge of an Ultimate Dynamical Theory. Assume further that the gravitational theory works, that our tentative model for dark matter fits in with the observational facts, and that a fully articulated Program Universe accounts in detail for the fluctuation spectrum in the background radiation — the current major challenge that any cosmological theory must meet. Then, at that point, I think it would be fair to say that the “final theory” conventional theorists currently envisage would have come into existence. But I gather that Ted Bastin and Clive Kilmister have a somewhat different vision of where we should be going.^[18]

Consulting Clive’s new book (Ref. 1), we find that Eddington was much impressed by the way in which the two separate fields of electricity and magnetism were connected by Maxwell. One of the facts used was that the connection between electrostatic and electromagnetic units had to be a velocity which was, empirically, close to the velocity of light. This equality between the numerical results of two very different types of measurement became an *a priori* necessity once Maxwell’s theory was accepted. Further, Eddington saw clearly that in the hands of Einstein, this clue and the equivalence principle led on to a new understanding of gravitation, unifying all of classical physics using tensor calculus. His enthusiastic adoption of Einstein’s theory led to a highly successful career. Somewhere along the way, he began to think that the unification between quantum mechanics and gravitation might be brought about by understanding the combinatorics of the tensor calculus as a necessary part of the construction of physical theory. Clive does not make clear just when this happened. But he does indicate that Eddington was shocked to learn that relativistic quantum mechanics as constructed by Dirac did *not* fit into the tensor calculus scheme he had been taking as his starting point for a fundamental theory. The result was, according to Clive, the two difficult books *The Relativistic Theory of Electrons and Positrons* (RTPE) and *Fundamental Theory* (FT) which did much to erode (some might say destroy) Eddington’s standing in the physics community.

Clive works out this tale in a very interesting way (Ref. 1), but I ended up feeling puzzled as to what actually happened in Eddington's mind. In the course of preparing this paper, I hit upon an historical conjecture that might be worth pursuing. My idea is this. In 1915, Sommerfeld applied the Bohr-Sommerfeld quantization rules to the relativistic Bohr atom. He found that this removed the degeneracy between the circular and elliptical orbits, and explained the fine structure spectrum of the hydrogen spectrum in terms of a single, dimensionless empirical parameter — which has been called the “fine structure constant” ever since. My conjecture is that Eddington saw early on that *if* he could derive this number from the tensor calculus (his original value of 136 for the inverse of the fine structure constant came out of such an exploration), then he might have the tool which would unify relativity and quantum mechanics in a manner analogous to Maxwell's earlier triumph. In fact, the Sommerfeld calculation applied to gravitation would predict a precession of the perihelion of Mercury, but of only 1/6 the observed magnitude.^[19] Thus, Eddington might have thought that by explaining this factor of 6 combinatorially, he might understand how the quantum mechanics of Bohr and Sommerfeld could be unified with *general* relativity. That Dirac's relativistic derivation of “spin” provided an alternative explanation for the fine structure of hydrogen that was radically different in its conceptual foundations, as well as in its mathematical transformation properties, could then well have had a devastating effect on Eddington's program.

Clive responds:^[20]

“I can't see any reason against your hypothesis. Personally I think it arose simply from Dirac's demonstration that the tensor calculus had its limitations and Eddington had built his reputation on GR with tensors as one support. But I can see you view as possible.”

Unfortunately, we will probably never know more. Eddington left very papers, and little correspondence survives. According to Clive, his interactions were mainly with colleagues at Cambridge, and were verbal, so the relevant historical material

does not exist.

Eddington's failed to entice the professional community to follow after him in looking for the unification of quantum mechanics and relativity from a chain of thought which is ontologically prior to experiment. I suspect that Maxwell would also have failed to acquire many early adherents if he has presented his theory in that way. After all, it required the experimental production of the Hertzian waves that were predicted by Maxwell's theory to start the process of empirically minded physicists accepting the theory. I believe that it will take something comparable to put ANPA on the map. I argued somewhat along those lines last year (Ref. 18) in asking for more help in the reconstruction of relativistic quantum mechanics (RRQM), and have gotten more specific this year in indicating what still needs to be done with the quark part of the problem.

My final caution applies even if some spectacular triumph should come out of our studies. Fredrick (Parker-Rhodes) expressed the belief (in Ref. 5) that it was just possible that in his theory of indistinguishables, the search for basic structure might indeed have "bottomed out". Even granted that we succeed, I am dubious. We may well learn that, carried out and analyzed in a certain way, we can always expect that the result of many specific experiments could have been calculated in advance. But I suspect that when our confidence gets too great (or more likely, even before that), experience will thrust new puzzles under our noses that collapse larger or smaller pieces of the edifice we have so painfully erected. In short, I will always remain an empiricist. I agree with David Hume that there is no way we can bind the future.

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On the Relationship Between Physics and Philosophy²

I

Recently, within the topical frame of ANPA discussions, the philosophical embedding of the hierarchy model has become more and more important.³ The success however of such an undertaking has been comparatively modest. In my opinion, one reason for this is probably that it is necessary in fact, to start from a sound philosophical basis rather than from philosophical ideas based on more or less intuitive (and thus random) associations of a mixed collection of notions which are not rigorously derived but merely presented off-hand. Hence, the basic question when looking at the relationship between physics and philosophy is nothing but what philosophy can actually mean today, given the results of science (and indeed, there have been quite many people who have thought about that before, so that it is not really necessary to have all these ideas oncemore). And answering this question is in fact a decisive difference as compared to the objective of ancient philosophy which was mainly one of providing the foundation for the sciences such that the results of the latter would be a straight forward derivation starting from this very foundation (this being the explicit Aristotelian objective for metaphysics e.g. – a conception dominating philosophy up to the 19th century).

Nowadays however, it is very much the other way round: the sciences giving the results and thus the empirical framework for viewing the world, and philosophy following up then by critically taking in sight these results and orienting man accordingly. Philosophy is generalizing therefore the results of science in a double sense (as displayed schematically on table 1): On the one hand, it is generalizing in terms of „gaining distance“ to local problems within a specific field of research, gaining a more general perspective at the same time, being able therefore to discover completely new solutions and achieving a completely new insight into the structure of given problems. On this level, philosophy is able to offer new Ansätze for further research. On the other hand, philosophy is generalizing in terms of a systematic taking

in sight of such structures which are locally similar or common to different fields of science, so that it might be possible to recognize certain analogies between apparently distant fields of research which normally are not connected with or associated to each other at all.

Hence, this taking in sight is basically an heuristic approach which by its very nature opens up interdisciplinary and overlapping lines of thought which are encroaching on various different domains. And this taking in sight is a critical one, because it does not suffice to simply transfer one notion from one domain to another, but it is necessary to also reflect on the systematical and methodological genesis of notions to be transferred. Not simply single notions are transferred then, but contexts of notions rather. Hence, taking in sight is also calling in question (or challenging) at the same time so that reflexion is always based on a sceptical attitude. For the first time, this philosophical perspective has been put forward by Hegel (concerning the historical analysis of notions) and Marx (concerning the orientation with respect to the sciences), and has been developed further within this tradition, most recently by Theunissen.⁴ It serves mainly the enlargement of the interpretational space of free play made available by this heuristic method which we call „metaphorization“.⁵ It is important never to forget the heuristic character of this undertaking which can be clarified in some more detail when looking at the explicit techniques involved.

Table 2 e.g. shows two cases of weak and strong metaphorization, the examples being taken from physics: In the first case it helps to interpret the abstract similarities of mathematical equations (describing the oscillator) in physical terms when applying a weak form of metaphorization by transferring the known meaning of mechanical entities to the electromagnetic domain. We know of course that inductivity e.g. is not really mass, but the structural similarity helps to shed a new light on the character of oscillations and to decide on what is common to the observed phenomena in one domain and another and what is not.

In the second case this can be carried further when discussing the law of energy conservation in terms of throwing an object into the air (in the sense of classical mechanics), and transferring this picture then to the expansion of the universe altogether, noting that the basic equation involved has a structural similarity to Friedman's equation. Now, these are examples well-known from physics itself. But this method can be

easily extended to other fields of science (e.g. when discussing similarities between physics and psycho-analysis – we will come back to that⁶). In fact, there is a complete hierarchy of levels on which metaphorization of this kind can work successfully.

The result of such a metaphorization is a new offer to a single field or to different fields of science in order to continue its/their research in one or the other direction. Beside its more anthropological task of orienting, this is what philosophy can practically achieve for science nowadays. A recent example is given by modern developments within the field of computer science: In fact, the project of AI has been shown to be inconsistent with results in psycho-analysis and the theory of consciousness, because it is based on the assumption that everything that can be thought can also be said (that is: everything in the world is expressible in terms of some suitable language). In order to write a program for a computer (e.g. for one eventually performing AI) it is necessary to be able to codify this program by a set of propositions in first place. Hence, AI is mainly based on the propositionality of the world. In fact, Analytical Philosophy would partially confirm this picture. But the other main line of modern philosophical thought (Hermeneutical Philosophy) rejects this conception and assumes that there is always some irreducible, singular structure within consciousness which is pre-reflective and cannot be codified by propositions therefore – an idea being originally introduced by Fichte following the line of Spinoza, being continued then through the philosophies of Hölderlin, Schelling, Bloch, and Sartre (Freud in fact, receiving this basic idea particularly by his reading of Schelling). Hence, according to this perspective, pre-reflective consciousness cannot be programmed and is necessarily absent from AI which is incomplete therefore and in contradiction with the objective defined by itself. The heuristic offer by Hermeneutical Philosophy given to the AI people is this result about inconsistency (criticizing by the way Analytical Philosophy at the same time – note the conceptual difference of these two levels of reflexion, because the latter is actually one of reflecting on reflexion itself, indicating a meta-level of research). And one reaction initiating further research on the level of computer science is e.g. the argument of Roger Penrose: If the problem of AI cannot be solved on a logical level, it could be solved on a physical level. Hence, one should try to change the underlying physics (of computers) in order to secure consistency of the problem.⁷

Note by the way that this irreducibility of pre-reflective consciousness expresses nothing but a genuine self-reference problem. Because reflexion cannot actually refer back to its proper origin, there is always the need for doing hermeneutic work (and this is the reason why Hermeneutical Philosophy is actually called so). There is an unbridgeable gap within all contexts of meaning which cannot be comprehended exhaustively. So the anthropologically characteristic tendency to go to the bottom (or to the roots) of observed phenomena is permanently limited by such gaps and asks for refined techniques of interpretation in order to decide what is mere appearance and what is the essence of real being, a point stressed for the first time by Hegel when discussing fundamental aspects of alienation. Because there is never any complete knowledge obtainable, and a lot has to remain assumption, philosophy is basically of a speculative character (the word „speculative“ used here in a technical, not in a derogatory sense). It is the main merit of Kant's theory (and probably the only one) to deny the possibility of scientific knowledge about objects which do not belong to the kind of nature to which we ourselves are bound by our experience. For us nowadays, this sounds somewhat trivial, for Kant's time this was a real achievement in delimitating philosophy from classical and inexact metaphysics.

There is one more point to this modern picture of philosophy which is its self-reflexion: Although philosophy is no more capable of laying the grounds for the sciences, it is nevertheless obliged to found its own line of arguments in order to secure its own consistency. Table 3 illustrates this basic scheme as a philosophical motion (of developing concepts). In fact, this motion is a double motion in two opposite directions. First of all, there is a causational component basing arguments on a chain of other arguments which are actually founding them, hence establishing a motion which finally regresses onto the foundation of everything (of the world that is). Necessarily, this will end up with a foundation which is (logically) situated outside the world, because according to Schelling (PP 440, II/2, 42) „foundation is against that to which it is foundation, non-being.“ Hence, it is what has not yet become, in particular, not become worldly. Note that this foundation is non-being and not nothing. It is a prime conception of the Tübingen axiomatic (being originally introduced by Hölderlin and lying at the roots of Schelling's philosophy) that the creation of the world out of nothing is not conceivable at all.⁸ While at the beginning of the 19th century this

led to a substitution of a transcendental, infinite God by an immanent infinity, basically of a spinozistic character⁹, today, we would like to replace this idealistic conception by a materialistic equivalent (though on the same spinozistic line of thought, in fact).

Hence, by the regressive component of philosophical reflexion, it is possible to shed some light on the foundation of the worldly. This can be used then (by the second component) to progressively derive the worldly (taken in sight so far) out of this foundation. This regression onto the foundation displays what has become, the subsequent progression out of the foundation displays what could have become. The reason for this is the fact that the foundation (being visualized as non-being) is potential of becoming and drafts out a field of possibilities such that the realized possibilities out of this field constitute what reality actually is. By this discrepancy (between what has become and what could have become) it is possible to gain some insight into the quality of the processes discussed. At the same time it provides decisive hints towards the consistency of the reflective motion so undertaken. (In fact, on the level of science, the application of this progressive-regressive method has been very successful in both physics and psycho-analysis¹⁰.)

In this sense, worldly evolution can be visualized as the actual outcome of an immanent tension between the That-foundation (Daß-Grund) and the What-essence (Was-Wesen) of worldly existence. In fact, the permanently self-expressing mode of such a relationship which tries to establish an explicit reference of the That (Daß) to the What (Was) is the categorizing of the worldly as performed by man. This line of argument which has been introduced by Ernst Bloch (EM 78) is actually a modern rephrasing of the Aristotelian conception on the relationship between „quodditas“ and „quidditas“ in the form as discussed in detail by Thomas of Aquino.

In case of the Blochian view, this conception is also based on a modern notion of matter: For Bloch, matter is itself process and theory-praxis at the same time, on the one hand as „being-in-possibility“ (κατὰ τὸ δυνατόν) as that which conditionally determines the historically capable of appearing, on the other as „being-for-possibility“ (δυνάμει ὄν) which is the correlation of the objectively real possible. (TE 233, MP 143, EM 139) In fact, that which is really acting within the worldly is, according to this terminology, „ἐνέργεια“ (energy). But underneath this worldly existence, there is

always the problem of the origin at its foundation. (We have mentioned this already.) The „theorem of foundation“ (Satz vom Grund) points to a That (Daß) „which we all basically are (was wir im Grunde sind) without being explicitly subjected to an objective predicate (yet).“ (EM 75, cf. 74) So the real (worldly) evolution is motion and *logicon* at the same time: „If there is a focus of production in nature (as is the case in Schelling), then the structure of this origin cannot be exhaustively comprehended by means of sub-atomic models or by a universal field law. Especially, if this origin permanently refers to a new beginning (or innovative start) within the worldly process, in a tendency to manifest itself, instead of being limited to a mere beginning alone.“ (PH 805)

This reflects in fact, the explicitly *onto-epistemic* constitution of the world, due to the *spinozistic* ingredients in this line of thinking, because it is Spinoza who introduces the foundation (substance) as *onto-epistemic* entity for the first time: „Per substantiam intelligo id, quod in se est, et per se concipitur: hoc est id, cujus conceptus non indiget conceptu alterius rei, a quo formari debeat.“ (Eth. p. I, def. 3) „Substance I call that which is in-itself and can be comprehended by itself: it is that the concept of which is not in need of the concept of anything else in order to be formed.“ This directly relates to the famous proposition VII of part II: „Ordo, et connexio idearum idem est, ac ordo, et connexio rerum.“ (The order and concurrence of the ideas (= imaginations) is the same as the order and concurrence of the things (objects).)

Moreover: Worldly evolution gains an explicit *project* structure, because the immanent tension between foundation and essence (which is questionable in itself) determines existence as a kind of asymptotic approximation of essence, the actual tendency of which is defined by the outcome of interacting single projects of the various worldly structures – some of which are thinking people in fact.

II

Now, what is actually happening in detail within this dynamical relationship between (or rather: interaction of) physics and philosophy? Table 4 displays the actual cycle of

attaining knowledge: We start from a phenomenological region (using this term in a non-philosophical meaning) within the inner-physical context about which a standard knowledge (the „state of the art“) is given at a time. The description of this region in terms of such a theory (which is actually mapping the region in terms of a model achieved so far) we call „explicatio“ according to a conception originally introduced in Renaissance philosophy as „a perhaps loose, but clear, vivid, and expressive sum of characteristics and properties which are collected one after the other and brought into an order more or less.“¹¹ In any case, the „explicatio“ rests on an already given inventory of knowledge accompanied by Feynman’s intuition¹² which triggers further research in first place, according to Feynman’s picture of his „babylonian“ perspective taken with respect to physics. By an associative process of looking for analogies (and by explicit metaphorization), the given inventory is eventually enriched by new notions and concepts such that the inner-physical context is actually left. The „explicatio“ is raised into a new space of meaning which in turn represents all that which is chosen background for a future philosophical embedding. This background is sketched out by the minimal consensus with respect to which the research is actually undertaken. For us, the totality of the world and its unity, as well as the onto-epistemic basis could be aspects of such a minimal consensus. One result of this is to choose the principle of „compossibilitas“ (a general compatibility of being¹³) as a suitable criterium, and analogy and metaphors are the adequate instruments for entering this process of tentatively drafting out a picture of the „analogon mundi“.

The next step is to bind this somewhat distinguished „explicatio“ on a rigorous context of the philosophy of nature. With respect to what is the minimum requirement it is necessary now to clarify the associated notions and concepts within the process of philosophical explication within the inner-philosophical context. While in the case of the inner-physical region the criteria for consistency are determined by the theory-immanent „backbone“ which basically consists of a mathematical component and another component of intertheoretic (physical) connections of references (which e.g. secure that a new theory contains older theories of the same line of thinking as a special case) – this region in fact intersects the more general region of logical reasoning – in the case of the inner-philosophical region these criteria are determined by a similar backbone which basically consists of an axiomatic component (containing the „theorems of

thinking“), an economical principle (Ockham's razor), and a historic component which secures the continuity of philosophical thinking (similar to the intertheoretic compatibility condition of physics).

As we have seen before, the „theorem of foundation“ points to the immanent worldly tension between the That-foundation and the What-essence. Hence, in principle, it formulates the transmission of causality into finality. And if the real is described in terms of suitably chosen categories (for which the theorem is the initial impetus), then, by its very project structure, it „contains in its being the possibility of being which is utopian ...“ (EM 238) Bloch calls this the „Patmos-contents“ of real being.¹⁴

The second theorem („of identity“) formulates the trials of identification in the categorial gestalts referring to the real-utopian. (EM 243) Finally, the „theorem of contradiction“ formulates the impossibility of permanent co-existence of objective contradictions. Note that taken in terms of the onto-epistemic perspective, these theorems reflect the four-fold nature of being itself (corresponding to the four roots of the theorem of foundation which is the most basic of them): Being in this sense can be visualized as identity (expressed by the symbolical formula $A = A$), as existence (expressed by the logical existence operator $\exists A$), as „copula“ (of judgements) of the form $S = P$ (in Blochian terms: $S = \text{not yet } P$), and as „being true“ (in the logical sense).

Ockham's razor basically secures that there are no assumptions which are unnecessary (or arbitrary). It is mainly an instrument for reducing the redundancy of arguments.

The historic backbone criterium (which can be called the „consistency of thought lines“ (Denklinienkonsistenz)) secures that one is not stepping back behind conceptions which have been already cleared up (and that one is not permanently developing ideas which are already available). Once, it is decided to follow a particular line of thought, consistency is required in the sense that new concepts have to be compatible with the older concepts belonging to the same line. They also can contain the older ones as special cases.

Today, we have internationally (within the domain of philosophy in the Western tradition) two main lines of thinking, Analytical and Hermeneutical Philosophy, as already mentioned. They dominate innovative theories. But in principle there is a mul-

titude of other approaches, ranging from a basically unhealthy, apologetic Neo-Kantianism in German philosophy, or post-modern tendencies in France (encouraging a large amount of arbitrariness rather than traditional French rationality and „clarté“), up to modern forms of Italian humanism, various versions of Marxist theories, diverse pragmatic (and sufficiently eclectic) theories, and singular and isolated groups dealing with Schopenhauer or similar philosophers. But whatever one chooses, the fundamental theorems of thinking remain the decisive criteria for judging any utilization of a theory as being appropriate and interesting or not. They are a strict and powerful instrument against any kind of esoteric approach or anything worse. By its quality, this strictness is not less precise than that of physics.

Basically, the motion of reflecting concepts can be visualized as a succession of reconstructions of the given phenomenological region in question. But the cycle is not yet complete (because a cycle it is), due to the fact that the possible philosophical embedding to be achieved implies a space of free play which is still too arbitrary for being able to be practically utilized within a scientific context.

From the beginning on, research as such consists of a large amount of adequate „ars inveniendi“ as an art of inventing concepts which establishes a first heuristic ansatz mainly by intuition and metaphorization. This procedure is heuristic and encroaching in a double sense, namely within the inner (endo-theoretic) region of a chosen subject (physics say) as well as within the exo-theoretic region among various different subjects. Remember e.g. that a strictly mathematically founded geometry has been constructed due to spatial intuition. The dynamics however, which actually determines the real physics, is obtained from the experience of measurements (this bringing Feynman's babylonian aspect into play). The gap between the two shall be bridged by theoretical research. This is in fact a point which Kant has not seen correctly, because his reasoning leaves speculation out: Not experience alone can be sufficient for eventually being able to reconstruct space and time. But insofar as experience is concerned Kant has confirmed what was immanent, but nothing else (or more fundamental) could be concluded from Kant.¹⁵ In this sense, „construction of an inner-worldly real connection“ means a method of uncovering internal mediations and notional structures of phenomenological regions in question (in order to eventually reconstruct them). But there is still enough of that incongruence between the „necessary

conditions of form for thinking“ and the „factual character of the existing“ left.¹⁶ Nevertheless, the cross-section of effectivity of such a research is comparatively large, because the backbone of mathematical formalization and logical foundation leads to a strong restriction of possible spaces of free play (for the interpretation) and a large reduction of redundancy and arbitrariness. This pragmatic advantage however, is given away again by the conceptual association of concepts undertaken on a given philosophical background when extending the implicational context of a given „explicatio“. Hence, the inner-physical backbone secures a consistent utilization of a „technical explicatio“ and a certain amount of communicative objectivity, and so does the inner-philosophical backbone. But the prime task of modern philosophy – namely to critically explicate that which is taken in sight in order to thematize what is the Other of the worldly – cannot be appropriately grasped (or only sufficiently prepared) by science (because the latter does not point to real inner-worldly connections, but only to their perspective restriction), and thus it counteracts the stabilizing tendency in destabilizing the gained concepts anew. So, the still missing criterium is therefore the logical explication of concepts (going back to an idea of Carnap) which illustrates that a theory of science is not simply formal logic: On the contrary, it is necessary to adapt the actual research activity undertaken to the implicit philosophical questions raised by the cycle discussed so far.¹⁷ This can be basically achieved by reconstructing inter-theoretic networks rather than single theories.¹⁸ This is very much in the sense of the appeal put forward by René Thom with respect to a modern philosophy of nature.¹⁹

III

Now, let us choose (exo-theoretically) a philosophical line of thinking as discussed above (in our case that of Hermeneutical Philosophy) and – endo-theoretically – a paradigmatic picture of modern physics (in our case the pre-geometrical ansatz according to Penrose, that of Newman being thought of as roughly equivalent). Then a number of results on the actual world picture can be extracted which I will simply state in the following as an illustration. For further details I refer to my earlier contributions to ANPA.²⁰

List of Ten Points

- (1) Out of an original, pre-worldly unity develops a becoming multitude of the worldly. (Prigogine/Penrose/Spinoza/Schelling)
- (2) Nature is thus explicitly historical. It is constituted in a self-processing manner. (Prigogine/Penrose)
- (3) For this worldly process, fluctuations are constitutive. Discontinuity and deterministic chaos dominate continuity and determination. (Prigogine)
- (4) Hence, the notion of „law of nature“ has to be substituted by a new concept, e.g. by an approximate concept of tendency. (Prigogine/Bloch)
- (5) The investigating of space-time-matter becomes its progressive-regressive reconstruction as product of a historical process and as its producer at the same time. This establishes an immanent notion of self-reference. (Mandelbrot/Sartre/Bloch)
- (6) This self-reference of nature points to its derivative status with respect to an initial singularity on the one hand, and to a primordial pre-geometry as its foundation on the other. (Penrose)
- (7) Space-time-matter as being self-processing gains a proper project structure and can be visualized as a permanent self-differentiation of its foundation, expressed by the immanent tension between That-foundation and What-essence. (Spinoza/Schelling/Bloch/Sartre)
- (8) Primordial unity emanates „prima materia“ as deterministic chaos, expressing itself then within space-time-matter and tending toward a final unity as „ultima materia“ and stochastic chaos. (Bloch/Penrose)
- (9) The conditional framework for the fundamental project of space-time-matter, spanning a field of possibilities, is reflected in the set of anthropic invariants. Hence, the unfolding of the universe gains a transcendental character. (Carter/Bloch/Sartre)
- (10) Nature is not a container of processes therefore, which can be controlled by man as external observer. On the contrary, there is a reciprocal immanence and transcendence of observed/signified and observing/signifying. (Schelling/Lacan/Sartre/Bloch)

I have added the names of those whose contributions (be they of physical or philosophical kind) have actually supported one or the other aspect of this picture of the world. This does not mean that any single person indicated above would have had a conception in mind similar or identical to the one put forward here. In this sense, the list of „ten points“ represents a developed synthesis and a continuation of the line of thinking chosen here. It can be visualized therefore as a new basis from where to start philosophizing anew.

IV

In fact, the modern function of philosophy as outlined here can be illustrated easily when looking closely at Shakespeare's „The Tempest“ utilizing this play for a metaphorization of its own sake, and by the way shedding some light on the actual modernity of its topic:

As is well-known, „The Tempest“ is concerned with the opposition of nature and art (and insofar it is a drama of the pastoral kind): Basically, Caliban represents nature without the benefit of its being culturally unfolded. Nature is opposition to an art which is man's power over the created world, but also over himself (very much in the Stoic tradition – not in the neoplatonic one as is often indicated incorrectly²¹). Hence, it is the antithesis (as represented by evil magic) as compared to Prospero's benevolent art. This latter is nothing but a perfected version of the philosophy of nature, including the arts made available by Prospero's books.²² The idea is to develop regularity („*melior natura*“) out of chaos and confusion in refining the productivity of „*natura naturans*“. (II i, 155-160) This is chiefly the project of Prospero (V i, 1), and his books represent the lore providing the appropriate techniques for liberating the soul from the passions (i.e. from nature – taken the explicit psycho-analytic view according to which „we are such stuff / As dreams are made on; and our little life / Is rounded with a sleep.“ – IV i, 156-158). He practically applies a discipline of which the primary requirements are learning and temperance, and of which the mode is contemplation (which are all stoic aspects, in fact). Prospero has to achieve the necessary control over himself first, before he is able to govern nature (what he is doing in a refined form,

pointing to a symbolic function of his techniques which stands for the world of the better, for the utopian quality²³).

Although this project parallels somewhat the alchemistic process, it is nevertheless based primarily on rational philosophy and purified reflexion („exact fantasy“), not on mystic insight (hence the books). With his books Prospero is conducting an experiment which depends on the purity of all concerned, and manages to refine nature: this is the experiment of the world (Bloch's „experimentum mundi“), and its main precondition is the achievement of „exact fantasy“ (of φαντασία καταληπτική – a concept Bloch has taken from the Stoa, cf. PH 141). Hence, Prospero's books (or one book in several volumes) represent nothing but the philosophical project: the volumes standing for science, art and religion, respectively, their philosophical reading representing the actual execution of the „experimentum mundi“ (implying a unity of being and thinking pointing to onto-epistemic aspects) (I ii, 109-110, 166-169, III i, 94-96). Philosophy is the totalizing attitude acting as a catalyst for letting this process emerge. Prospero controls the three different levels (or world-views rather) with the help of Ariel. He practically harmonizes these different, apparently isolated views and matches them with each other, until in the end, they are completely brought together again and truly unified. (I ii²⁴) Is the utopian quality finally achieved, Prospero can drown his books, because as compared to this final, ideal state they represent but a „rough magic“ which he can abjure (V i, 50 sqq.): „Now I want / Spirits to enforce, Art to enchant“, he sketches in the epilogue the Empire of Freedom.

V

Referring to the „hierarchy“ now (in the sense of ANPA), it suffices to shortly look at the criteria we have listed above in order to recognize what has still to be done for achieving a philosophical founding of the underlying, physical ideas. We discuss only the points displayed on table 5: the onto-epistemic aspect, the progressive-regressive method, the theorem of foundation, Ockham's razor, the consistency of thought lines, and the intertheoretic network problem.

(1) The onto-epistemic aspect.

The traditional division of ANPA reflects a basic difference of approach when viewing the „hierarchy“: On the one hand, there is the British group visualizing the hierarchy as a model for a process obtaining knowledge about the world. On the other hand, there is the American ANPA-West thinking of the hierarchy in terms of a classical model for real (physical) processes.

In the first case, the idea is to see the universe as being divided into an unknown and a known part²⁵: the latter representing a system „of which we are a part“, the former indicating a region beyond.²⁶ Hence, „(the) intention is to capture in a discrete model the continually developing quality of a scientific investigation by a process theory.“²⁷

The problem with this approach is that the unknown region in question cannot be an actual part of the universe in the strict sense: That is, not the universe is divided into an unknown and known part (because the idea is not really to discover more galaxies or so). In fact, it is the abstract domain onto which knowledge could be mapped what is enlarged. You could write down e.g. many entries for an encyclopaedia (which as a collection of books is in fact a physical part of the universe), but what these entries actually mean can obviously not be treated in terms of space-time points: What I mean is that knowledge is located somehow in the brain of persons, and these brains are pieces of matter, and as such they are a physical part of the universe. But knowing that would not help you at all to learn anything new about epistemology. You will simply learn that thinking (as a process initiated by human brains) is a comparatively late product of cosmic evolution. This will indeed demonstrate that the world is constituted in an onto-epistemic way. For modelling a process of attaining knowledge however, this is a far too general result. In fact, all details which are already known about this process are absent (the results of the theory of science, the theory of consciousness, modern psycho-analysis, anthropology, semiotics etc.).

The problem with the second case is more of an inner-physical nature: If the hierarchy is interpreted in terms of being a new „fundamental theory“ mainly based on combinatorial aspects, then it has to meet the requirements any physical theory has to meet. In the last ANPA newsletter this question has been discussed in more detail. I will not elaborate on this further. On the other hand, one basic question is whether it is

really „counting“ alone which is lying at the roots of doing science. I doubt that, and I remind you of my earlier remarks concerning the mathematical formulation of geometry. In the case of Penrose e.g., you can see clearly that he indeed started with a purely combinatorial structure. But in the meantime, twistor theory is far from pure combinatorics. On the contrary, it has gained a very complex structure by now.²⁸

Another point is the following: The fundamental idea of Program Universe (and a very challenging one) is (I think) the aspect of practically simulating emergence and evolution. Mike Manthey has called PU „a bit-based Big Bang“.²⁹ This is very close in fact, to the project of Peter Eisenhardt and Dan Kurth on the description of emergence³⁰, because it sets in motion the explicit self-unfolding of a worldly structure (for which we are all looking forward). Nevertheless, it is a simulation, and insofar it belongs entirely to the epistemic side. Even, if everything could be reproduced which is known so far, this would still be nothing but an alternative way of describing physics, and it would still be necessary to introduce a correspondence „to bring us back to laboratory practice“.³¹ But then, the question is why not staying there in first place (which is a problem of the theorem of foundation and of Ockham's razor in fact). And any ontological claim can only be substantiated when eventually producing entirely new results (which – as far as I know – has not been done up to now).

(2) The progressive-regressive method.

Both of the ANPA groups have not yet performed any systematic evaluation of a progressive-regressive approach to the foundation of the hierarchy concept. In the case of ANPA-West, this has only inner-physical consequences. It leads e.g. to a somewhat over-abundant emphasis of quantum theory which could be disputed in terms of other theories which favour gravitation as a fundamental aspect, or a pre-worldly structure, respectively (as is the case in the theories of Penrose, Newman, and the Wheeler school, which are actually favoured by myself). Of course, there is a physical start from the line of Parker-Rhodes and Eddington (and perhaps Dirac), but there is no (at least no obvious) linkage to any of the earlier main lines of tackling the basic problem to have one physics, but two disjoint domains of describing it.

In the case of the British group, there is virtually no relationship recognizable which would furnish any linkage to one of the main lines of epistemology. Here we

have even the double-faced difficulty that beside linking the chosen ansatz to any of the main lines, it is also necessary to give an explicit development of the co-evolution of the ontological and the epistemic perspective as an actual part of theory itself.

(3) The theorem of foundation / Ockham's razor.

The same is also true with respect to and in fact closely connected with the theorem of foundation: Apparently, there has been no attempt to actually discuss a proper foundation for the co-evolution mentioned above. Again, this is mainly due to the fact that the concept of „world“ itself is not completely defined. In the case of ANPA-West it is e.g. a very unsatisfactory feature to have to start from one or two particles before there is any concept of space-time-matter. This is an old problem, but there are alternatives which appear to be more satisfactory (I remind you on the Zel'dovich school and also on Leonard Parker³²). This „substratum problem“ is similar to the earlier one discussed in the ANPA when the division of the two groups was not yet so clearly cut.³³

On the other hand, it is very much the question whether a sufficient foundation for the meaning of the hierarchy could be to reproduce numbers by a given algorithm due to a „curious algebraic construction“ that are „interpreted immediately as the inverse of ... coupling constants“ (as was the case in the very first ANPA paper³⁴).

The argument should be the other way round: indicating the start from (not the return to) a main line, be it from physics or from philosophy. That is, not a singular beginning should be fitted to an available main line, but a plausible main line should be selected in first place and then generalized by fitting a new idea.

Kuhn has characterized this process of innovation: If there is a critical scenario in which a governing paradigm is challenged, in the sense that anomalies can be observed which contradict the expectations prescribed by this paradigm, then intuition, invention, or simply coincidence can lead to the creation of truly innovative structures implying a generalization of the paradigm. The inherent stability of standard inventories (within the given paradigmatic context) will tend to suppress generalizing alternatives of this kind. But if the explicative power of the new paradigm is strong enough, the old paradigm has to be abandoned sooner or later (mostly later).

But note that in any case, this process starts with anomalies in the context of a governing paradigm (that is: a main line of thinking). And not all theories are paradig-

matic in fact: Only, if experiment and heuristic approach as well as speculative context are congruent with each other, can such an innovative structure eventually emerge, and is the subsequently developed new theory paradigmatic.³⁵ So, indeed, research takes place in many different worlds³⁶, but communicative objectivity asks for a minimal consensus from where to start, and the orientation towards such a consensus is based on a main line of thinking in first place, any time and once and for all. And without communicative objectivity of this sort, even a brilliant new idea does not help: We have not to think of Leonardo at once, but a prominent example for this is the controversy between Priestley and Lavoisier concerning the nature of oxygene.³⁷

(4) The consistency of thought lines.

This leads immediately up to the point of consistency of thought lines, in particular with respect to the philosophical attempts undertaken so far. In fact, to put it briefly, I cannot recognize any line within these attempts at all, and sometimes the problems discussed appear somewhat strange to me. I would just like to mention one example: The basic ideas leading to the hierarchy have originally emerged out of a context which is related to problems of quantum theory – which in turn has been developed within a special line of European physics dominating the first three decades of our century. So, why should philosophy in the Western tradition be criticized then, including one of its aspects usually referred to as „dualism“, if this background of thought is actually that which founded this very European physics? It is unlikely e.g. that an Asian conception of philosophy (say one of Taoism) could achieve any better results for the problems we are dealing with here.

In particular, I can see no problems with the concept of dualism: Seen in terms of the modern materialistic perspective which I have shortly sketched out in the beginning, there is no substantial dualism as to the worldly substratum itself (and the process of evolving reality): because everything worldly is space-time-matter, and its foundation is a unique one. But, nevertheless, due to the properties of human perception and cognition, there is indeed an epistemic dualism which cannot be overcome, because, despite knowing that thinking is nothing but a sort of being and thus a special aspect of the same underlying worldly substratum, we think in a digital way, in contrary to the processing of nature which works in an analogous way, namely in

terms of temporality. Probably, thinking is a kind of inhibiting the „stream of reality“, similar to what on a mechanical level the escape-wheel of clocks is doing. This is obviously a specific property of the process structure determining the kind of matter which we call mind. When talking of onto-epistemic conceptions (as we have done here), we have to take this into account. In principle, these aspects do reflect nothing but the already mentioned, very basic duality between the worldly and its foundation (remember my remarks on the problem of origin). Hence, the difficulty „to reconcile discrete aspects of the world with an objective spacetime continuum“ (which is also seen by Penrose) is indeed a Zeno-like difficulty³⁸, but this simply means (and this is what Zeno actually intended to show by his examples): that it is not possible to reflect about real processes in the same way as they actually work. Hence, the distance between subject and object, hence, the already mentioned „incongruence“ discussed by Holz. In fact, a careful and consistent foundation of philosophical reflexion on one of the available main lines of thought would probably help to shed some more light on these fundamental aspects of the world which have to be understood before any construction on the scientific level can be performed in a suitably reasonable way.

(5) The intertheoretic network problem.

Another problem related to the questions discussed here is that of reconstructing a suitable network of theories into which the hierarchy model is logically fitting. The method put forward by Balzer, Moulines, and Sneed should be helpful for that.³⁹ This relates somewhat to the inner-physical aspects already discussed with respect to the ANPA-West approach (and refers mainly to the problem of linking the model to anything else which is already available).

*

A careful inspection of at least these five main points could probably make a revisional discussion of the hierarchy model possible, but now with the advantage that the basis from where to start this new discussion would be a comparatively sound (though conventional) one. Probably, this would also help to enlarge the communicational basis for

getting in contact with other people from physics and philosophy who up to now are sceptical when being confronted with the ANPA project.

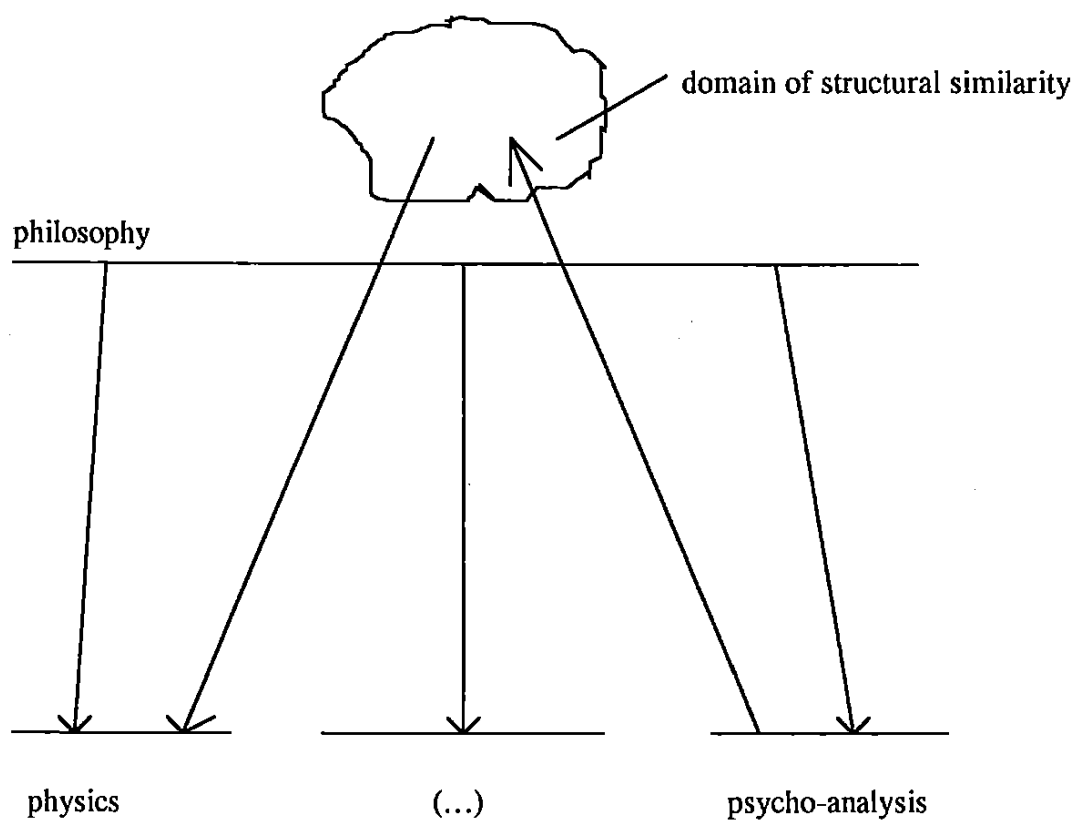


Table 1. heuristic function of philosophy: A: distancing function, generalizing of perspectives, B: encroaching on different disciplines

Weak metaphORIZATION

$$\left. \begin{aligned} x + 2\kappa x + \omega_0^2 x &= \Omega \\ 2\kappa &= \frac{\beta}{m}; \omega_0^2 = \frac{k}{m} \end{aligned} \right\} \quad \text{mechanical oscillations}$$

$$\left. \begin{aligned} j' + 2\kappa' j + \omega_0'^2 x &= \Omega' \\ 2\kappa' &= \frac{R}{L}; \omega_0'^2 = \frac{I}{LC} \end{aligned} \right\} \quad \text{electromagnetic oscillations}$$

strong metaphORIZATION

$$\begin{aligned} \frac{1}{2} \dot{v}^2 - \frac{GM}{R} &= -\frac{1}{2} k \\ v &= \dot{R}, M = \frac{4}{3} \pi R^3 \rho \\ \Rightarrow \dot{R}^2 - \frac{8}{3} \pi G R^2 \rho &= -k \\ 4\pi G &= \frac{1}{2} \Rightarrow \text{Friedman eqn.} \\ (4\pi G &= 1) \end{aligned}$$

Table 2. metaphORIZATION

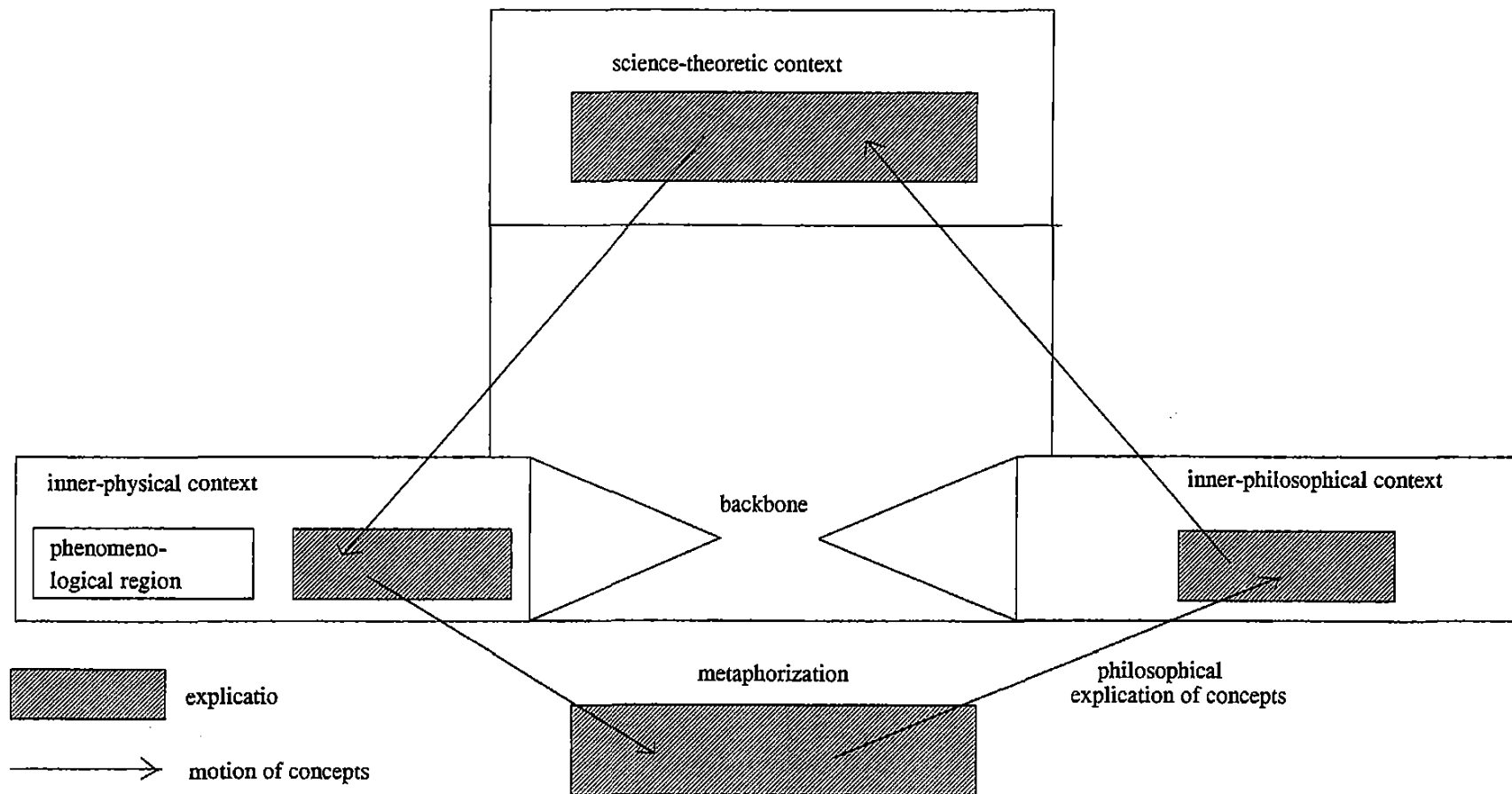


Table 4. Cycle of heuristic and encroaching method of attaining knowledge

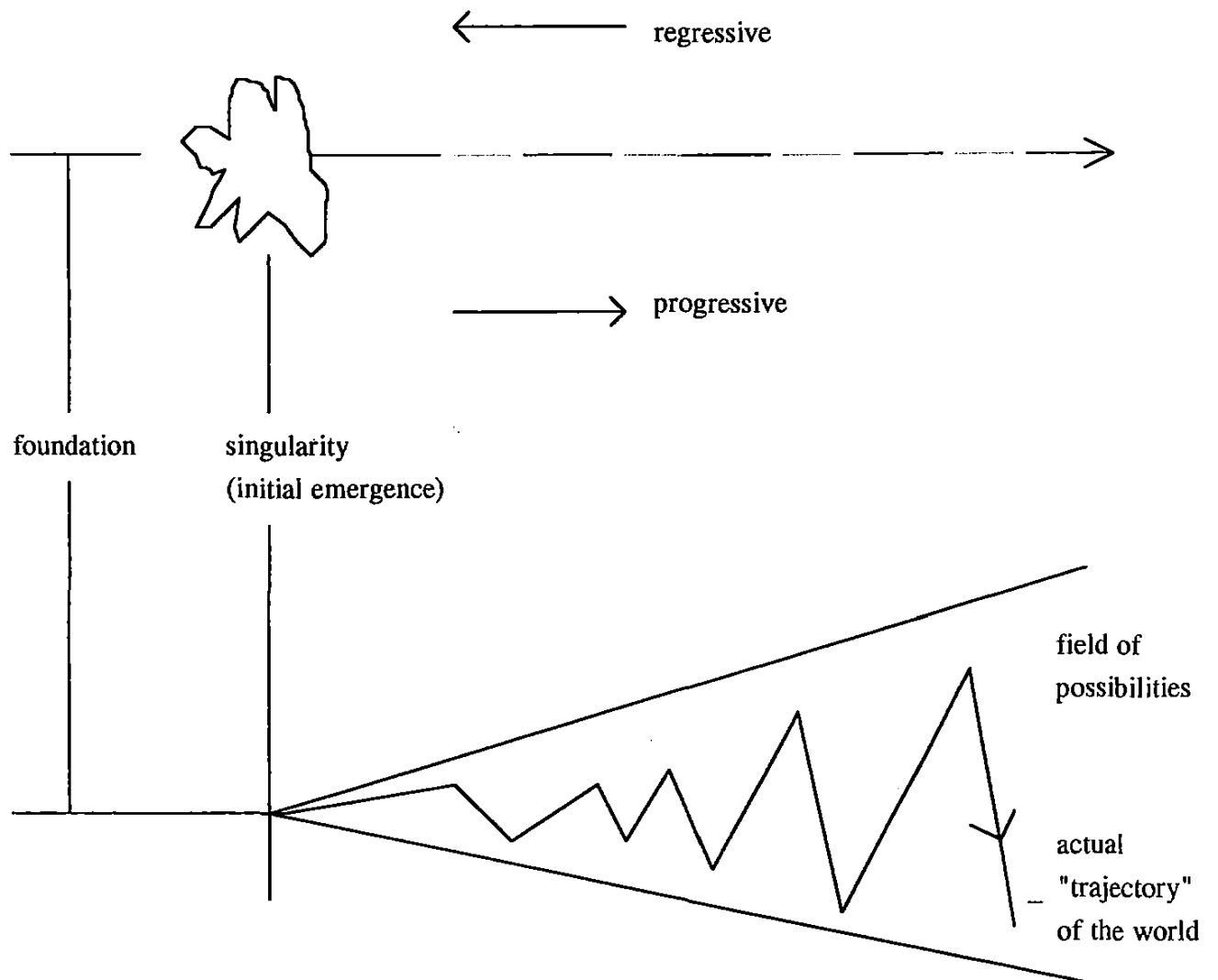


Table 3. progressive-regressive method

ANPA revisited

1. onto-epistemic aspect	known / unknown (counting, PU: simulation)
2. progressive-regressive method	(absent)
3. theorem of foundation / Ockham's razor	one / two particles „curious construction“
4. consistency of thought lines	(absent)
5. intertheoretic network problem	(absent) $\Rightarrow$ BMS

Table 5. open problems of foundation

Notes.

- ¹ Fachbereich Allgemeinwissenschaften, Fachhochschule München, Lothstr. 34, D- 80335 München.
- ² Paper presented to ANPA 17, UK – Cambridge, 1995.
- ³ C. W. Kilmister: Everybody's Doing It Now. Proc. ANPA 15, 31-38, P. Marcer, C. W. Kilmister: Talking About Creation – A Dialogue, *ibid.*, 57-66.
- ⁴ M. Theunissen, Möglichkeiten des Philosophierens heute, in: *id.*: Negative Theologie der Zeit, Suhrkamp, Frankfurt a.M., 1991, 13-36.
- ⁵ R. E. Zimmermann: Selbstreferenz und poetische Praxis, Junghans, Cuxhaven, 1991, 83-84.
- ⁶ J.-P. Sartre: Questions de méthode, Gallimard, Paris, 1960.
- ⁷ R. Penrose: Shadows of the Mind, Oxford University Press, 1994. Cf. the excellent review by Colin McGinn: Past computation, TLS 6th January 1995, 12-13.
- ⁸ D. Henrich: Philosophisch-theologische Problemlagen im Tübinger Stift zur Studienzeit Hegels, Hölderlins und Schellings, Hölderlin-Jahrbuch 1986-1987, Mohr, Tübingen, 1987, 60-92, here: 85.
- ⁹ Jacobi's discussion of Lessing's view on Spinoza was the initial onset for a violent controversy triggering the soalled „Pantheism dispute“ in Germany.
- ¹⁰ While the biographic method of Sartre based on his social philosophy has been the first explicit instrument of applying this conception (actually invented by Sartre, cf. note 6), fundamental aspects of it have been independently introduced into physics when analyzing the evolution of given processes in terms of the Prigogine mechanism of reaction-diffusion systems introducing a true historical element into otherwise static physics. Cf. e.g. I. Prigogine, I. Stengers: Das Paradox der Zeit, Piper, München, Zürich, 1993.
- ¹¹ P. Kondylis: Die neuzeitliche Metaphysikkritik, Klett-Cotta, Stuttgart, 1990, 102.
- ¹² R. P. Feynman: Vom Wesen physikalischer Gesetze, Piper, München, Zürich, 1990, 55 sqq., 195 sqq. (German version of: The Character of Physical Law, MIT Press, 1965) See especially: 69 sq., 74.
- ¹³ R. Toretti: Die Frage nach der Einheit der Welt bei Kant, Kant-Studien 62 (1) 1971, 77-97, here: 78 and 87.
- ¹⁴ He refers to the „Hymn to Patmos“ by Hölderlin of which verses 4-5 are: „Wo Gefahr ist, wächst / Das Rettende auch.“
- ¹⁵ C. W. Kilmister: Space, Time, Discreteness, Philosophica 50 (2), 1992, 55-71, here: 57.
- ¹⁶ H. H. Holz: Artikel Dialektik, in: H. J. Sandkühler (ed.), Europäische Enzyklopädie zu Philosophie und Wissenschaften, Meiner, Hamburg, 1990, I 556, 549.
- ¹⁷ O. Neurath, R. Carnap, W. Morris: Foundations of the Unity of Science, Chicago University Press, 1970. – Cf. M. Heidelberger: Einleitung zu: W. Balzer, M. Heidelberger (eds.), Zur Logik empirischer Theorien, de Gruyter, Berlin, 1983, 1-23, here: 11.
- ¹⁸ W. Balzer, C. U. Moulines, J. D. Sneed: An Architectonic for Science. The Structuralist Program. Reidel, Dordrecht etc., 1987.
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- ²⁰ Cf. my papers in the proceedings of ANPA 12, 15 and 16. See also in: System & Struktur II/1, 1994, 11-14.
- ²¹ So Walter Kluge in the Shakespeare handbook, Kröner, Stuttgart, 1978, 539.
- ²² The Tempest, edition F. Kermode, Routledge, London, New York, 1954, 1989, xiv sq., xxv, xli. The work itself is also quoted according to this edition.
- ²³ *Ibid.* xlv sq. – referring to the prefache of „Theatrum Poetarum“ by Edward Phillips, 1675.
- ²⁴ Handbook, *op. cit.*, (note 21), 538.
- ²⁵ C. W. Kilmister, *op. cit.* (note 3), 32. – Cf. *id.*: *op. cit.* (note 15), 63-65.
- ²⁶ T. Bastin: A Systems-Theoretic Critique of Process-Hierarchy Models, Appendix II to: H. P. Noyes: On the Construction of Relativistic Quantum Theory. A Progress Report. SLAC-PUB-4008, 1986, 64.
- ²⁷ C. W. Kilmister, T. Bastin: book manuscript, August 1988 version, ch. 6.
- ²⁸ The development of the twistor concept after 1970 is discussed in some detail in my paper in: System & Struktur I/1, 45-46. In this paper there is also listed a more detailed version of the „ten points“ of philosophical consequences mentioned earlier, cf. 47-50.

- ²⁹ M. Manthey: The Combinatorial Hierarchy Recapitulated, Proc. ANPA 15, 1993 (1994), 39-50, here: 42.
- ³⁰ P. Eisenhardt, D. Kurth: *Emergenz und Dynamik*, Junghans, Cuxhaven, 1993.
- ³¹ H. P. Noyes, D. O. McGoveran: An Essay on Discrete Foundations for Physics. *Physics Essays* 2 (1) 1989, 76-100, here: 95.
- ³² J. B. Zel'dovich, I. D. Novikov: *The Structure and Evolution of the Universe* (= *Relativistic Astrophysics II*), The University of Chicago Press, 1983, part IV. – See in particular: V. A. Belinskii, I. M. Khalatnikov: Influence of Viscosity on the Character of Cosmological Evolution, *Sov. Phys. JETP*, 42 (2), 1976, 205-210. – For another approach see L. Parker: The Production of Elementary Particles by Strong Gravitational Fields, in: E. P. Esposito, L. Witten (eds.), *Asymptotic Structure of Space-Time*, Plenum, New York, London, 1977, 107-226.
- ³³ T. Bastin, op. cit. (note 26), 64.
- ³⁴ T. Bastin, H. P. Noyes, J. Amson, C. W. Kilmister: On the Physical Interpretation and the Mathematical Structure of the Combinatorial Hierarchy, *Int. J. Theor. Phys.* 18 (7) 1979, 445-448. Here: 460. – Cf. C. W. Kilmister, op. cit. (note 15), 55.
- ³⁵ T. S. Kuhn: *Die Struktur wissenschaftlicher Revolutionen*, Suhrkamp, Frankfurt a.M., 1981 (1967), 74. (German version of: *The structure of scientific revolutions*, University of Chicago Press, 1962.
- ³⁶ Ibid. 132.
- ³⁷ See my yet unpublished paper: *Die undefinierte Konzeption. Joseph Priestley und die pneumatische Chemie in England*. In: *Seminar*, TU Berlin, 1983.
- ³⁸ C. W. Kilmister, op. cit. (note 15), 56.
- ³⁹ Refer to note 18.

The grammalogues used throughout the text refer to the standard editions of the works of Spinoza, Schelling, and Bloch, respectively. In detail, the following grammalogues are used:

- PP = II/2 – Schelling: *Grundlegung der positiven Philosophie*, München 1832-33, ed. H. Fuhrmans, Bottega d'Erasmus, Torino, 1972 (II/2 referring to the standard edition, part II volume 2).
- Eth. Spinoza: *Ethica etc.*, in: *Opera* (Latin/German), Wissenschaftliche Buchgesellschaft Darmstadt, 1980, ed. K. Blumenstock, 2 vols.
- EM Bloch: *Experimentum Mundi*. 1975.
- TE Bloch: *Tübinger Einleitung in die Philosophie*. 1963.
- MP Bloch: *Das Materialismusproblem etc.* 1972.
- PH Bloch: *Das Prinzip Hoffnung*. 1949/1959.
- This edition has been published by Suhrkamp, Frankfurt a.M.

WHAT IS TO BE DONE Draft 2.2

KEITH BOWDEN

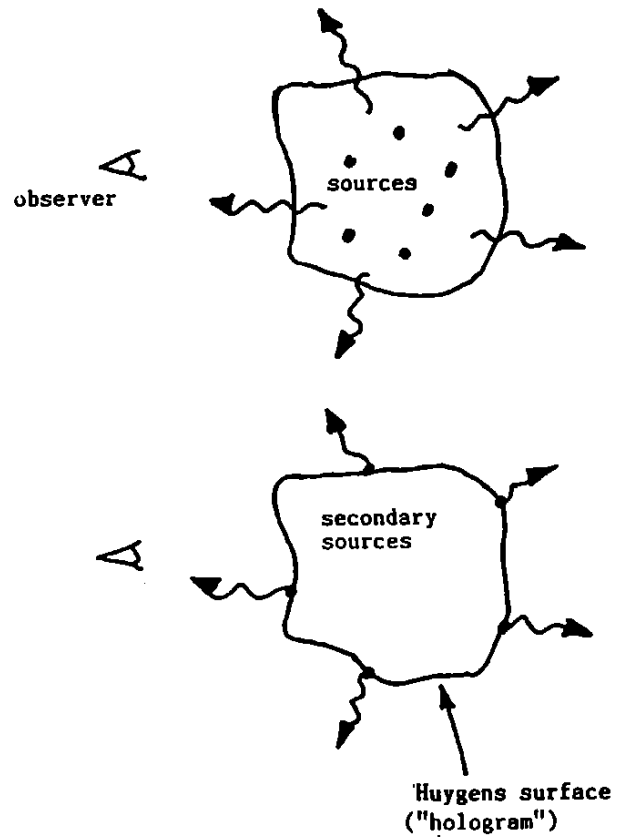
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Dagenham, Essex RM8 2AS, UK.*

For some time I have been looking at the relevance of the equivalence between Kron and Jessel's physical paradigms to ANPA's Discrete Physics, and looking for an appropriate philosophical framework for this relationship. A number of people have recently started to make connections with my original work on Kron and Jessel. This has resulted in my starting to look at it again and highlighted a number of issues that need sorting out, tidying up or fleshing out, as the case may be, before these connections can properly be made. In order to make the best use of various offers of help with these issues I am going to take this opportunity to rapidly redraw a picture I've drawn for you in different ways a couple of times before, and in the process to identify and explicate the issues in question.

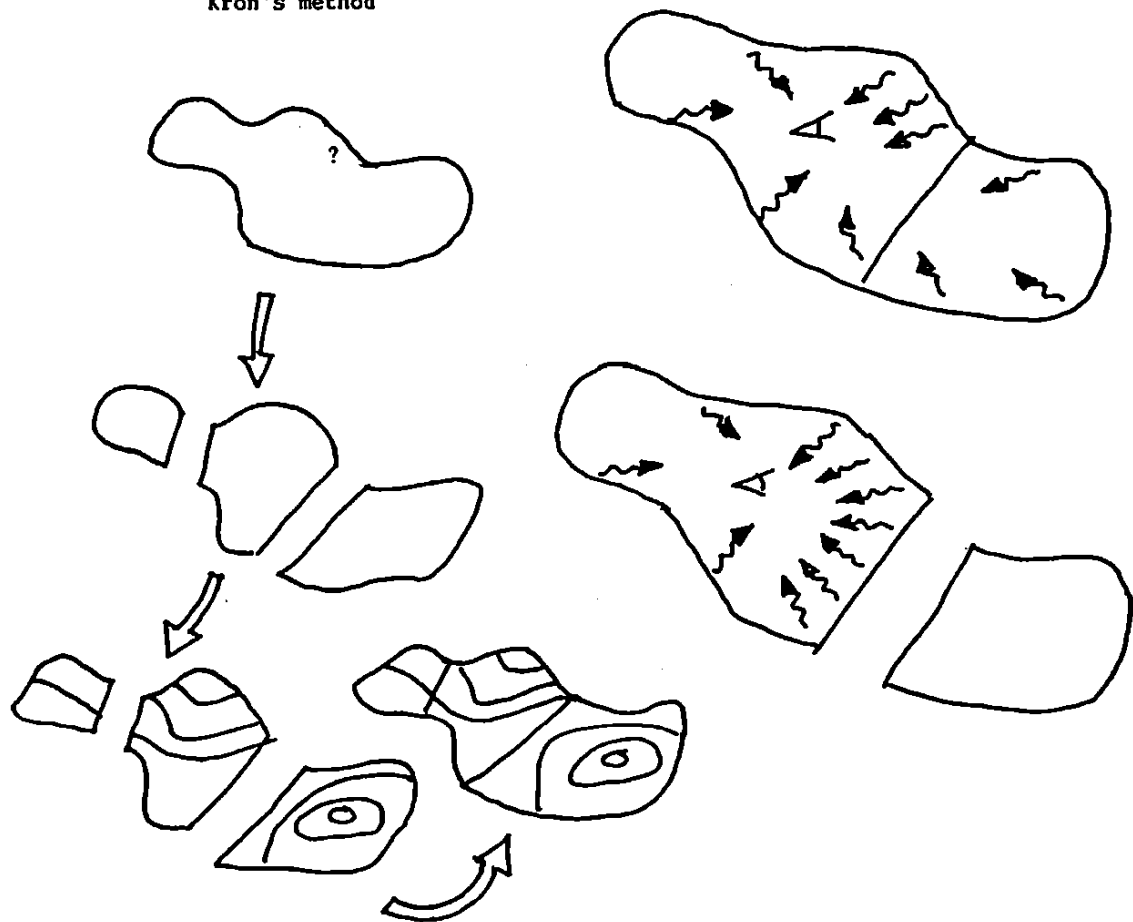
I am going to start with the finite element type models of field equations such as $\nabla^2\phi=0$, or indeed $\nabla^2\phi=\rho$, where there are sources. These are traditionally modelled by computer or numerical people, effectively by a network of electrical resistances. The resistances exist over the region of discourse of the original problem, boundary conditions are put in around the edge of the voltage mesh and values are obtained at the mesh points for ϕ , in a rather discrete model of a continuous problem. Now the methods of finite differences or finite elements assume a hidden isomorphism between electric circuit theory and electric field theory or between mechanical stress theory and discrete models of mechanical stress theory etc. This was the starting point for the work of Gabriel Kron in the earlier part of this century. Kron invented the technique of solving large problems by topologically "tearing" them up into little bits, solving the field problem or whatever in the subregions, and then putting the whole thing back together again to get an exact overall solution. He developed a formalism for this that seemed to work in general.

Fig (1.1) is a picture of a field problem and Fig (1.2) gives a rough picture of the process of tearing the field problem up into bits. The reason I draw this diagram is to emphasise that the important part of this process is that which goes on at the boundaries between the different sub-problems. There are no obvious restrictions on the method in general. Controversy raged about the validity of this in general until the mathematician J. Paul Roth (1952?) showed how

Fig(1.1)
Huygens Principle



Fig(1.2)
Kron's method



Fig(1.3)
Kron's Method from
Huygens' Principle

Kron's formalism could be written in terms of Homology Theory, the most general part of mathematics applicable to this sort of problem, and then, showed how the Method of Tearing, could in a very general sense, proved to be correct within homology theory. Later Sun Ichi Amari, who is still around in the computer industry in Japan I believe, generalised this further.

Homology theory is hard, and for some time in the eighties I tried to think of other ways of approaching a problem like this. My attention was brought to some work of Maurice Jessel, which has been described here many times before. Essentially, the idea is that the waves due to a number of sources within a boundary can be replaced by a new set of "secondary" wave sources on the boundary, such that from outside the boundary the resulting new set of waves is indistinguishable from the original. I realised that Jessel's "Principle of Secondary Sources", which can be considered as a fairly general paradigm for continuous field theory, is equivalent to Kron's Method (see Fig 1.3). The latter involves a hidden assumption that the subproblem solutions can be calculated exactly from a set of boundary conditions on the subregion. This is exactly equivalent to Jessel's Principle of Secondary sources. Kron's discrete world of electrical circuits is modelled mathematically by a particular construction called Singular Homology. Similarly the world of continuous fields of Maurice Jessel is described by de Rham Cohomology, and there is a very general theorem, due to de Rham, which says that these discrete and continuous worlds are, in some very wide sense, isomorphic. The Kron-Jessel equivalence however is far more accessible than the de Rham theorem, particularly because there is a wealth of written material and physical meaning behind this work.

Now, there are some problems with this. The first one is that my interpretation of the de Rham theorem, the isomorphism between the discrete and the continuous, is not Kron's interpretation. So we now have two different interpretations. Kron's interpretation was that the isomorphism gave an equivalence between the field surrounding an electrical circuit and the electrical circuit itself. Now these two interpretations are actually slightly different, and I think that is the first thing in my mind which needs ironing out. The second is the Kron- Jessel relationship which has always resisted my attempts at a rigorous general formulation, in that there are two different approaches to it which are contradictory. I have not been able to solve this yet.

The next thing that comes up in my mind, which is part of what I am beginning to think is quite a bag of worms that needs addressing again is the question of what we mean by discrete. I quite happily say that the de Rham theorem gives a relationship between the discrete world of ANPA and the continuous world of more classical physics, and that is reasonably OK, except that the discrete world I am talking about here, has a discrete topology, but the variables in that world are not discrete, they are continuous voltages and continuous currents, and so in actual fact I am only half way to discrete physics. [At this point Lou Kauffman commented that 1. there are many theorems relating the discrete to the continuous and that we should be considering the implications of these as a collection and 2. that it is "great to have a have a situation that is half continuous and half discrete".]

Even ANPA's position is not clear. Recently one of the things I have been doing is writing a paper which Clive Kilmister has been looking at (about philosophical frameworks for the combinatorial hierarchy) in which I made some comment about a finite amount of space holding a finite amount of information. "Well, OK", says Clive "but what happens if, in this

finite bit of space, we put an object, for example an apple? Just by doing that I am giving you an infinite amount of information because I am telling you about all the things I am not putting in there." I worried about this for a short time and then I realised that Ted Bastin and Clive had made an assumption in their work, that Pierre Noyes definitely does not make in his, and although I have heard it said before, it had not really sunk in how different the two approaches are. Pierre has a largest integer therefore, if Pierre has an arbitrary integer in a finite space he is not getting an infinite amount of information. Clive and Ted are. [Pierre commented here that the necessity to be both discrete and finite was David McGoveran's innovation.]

Singular Homology Theory is algebraic topology. It talks about points and edges and faces of polyhedra in space and about how the boundary of a surface is a collection of edges, and how the boundary of a boundary disappears because if you take the boundary of a disc you get a circle and the boundary of a circle does not exist because it has not got any ends. These sequences of boundaries are known as chain complexes. So in these structures the boundary of a boundary is always zero, and in fact that is really the crucial point about homology. The dual de Rham Cohomology theory is the algebra of the topology of continuous fields.

As well as the duality defined by de Rham theorem there is also a duality between voltage like things, potentials, which have values at a point in space and current like things, flows, which in the various algebras transform in a different kind of way. However, if you think of these things as Kron described them as "through variables" and "across variables", variables which are measured across two points like voltage differences and variables which are measured through something like currents. Homology theory traditionally only ever deals with one of these things at once, so mathematicians would deal with voltages or with the currents but they would not deal with both of them together. Roth's innovation in creating what has become known as Roth's diagram, was to look at the natural isomorphism between chain (potential) complex and its dual cochain (current) complex. Boundaries take dimensionality down in the chain complex sequence and coboundaries take dimensionality up in the cochain complex.

What Roth did was to put the two things together to get the two directional arrow (or ladder) diagram (Fig 2), which Mike Manthey talked about in an earlier paper, and then he noted that the isomorphism between the one dimensional currents and the one-dimensional voltages contained this curious twist in it. Some 15 years ago in my PhD thesis I noted various other things about it and in particular I re-drew the diagram in a rather more explicit form to show the twist. I also talked quite a bit about how the twisted bit relates to impedance or resistance or whatever the analogous property is in the field of endeavour you are looking at, and how the product of the thing at the top and the thing at the bottom of the diagram talks about energy or money or whatever it is you are minimising or maximising, and made some tentative attempt to formalise the connection between this process of optimisation and the topological structure that Roth describes. The achievement of Roth in formulating this approach to Kron's work is that Kron's electric circuit equations, essentially containing Kirchhoff's laws, or Maxwell's equations, come straight out of the commutativity of the loops in the diagram. However there are ambiguities and contradictions in the various descriptions of Roth's diagram that badly need clarifying. In particular Branin, apparently with the consent of Roth, and I in various places, treat the chain complex as if it were an "exact sequence" (by "modding" the chain groups by cycles). This seems to be a valid approach but is very

nonstandard and needs putting on a rigorous basis.

A comment Clive Kilminster made led me on to think about what would happen if the boundary operators did not quite square to zero, or what would happen if the diagram did not quite commute. I was speculating in 1981, when I was thinking about the optimisation properties of this structure, that the commutativity and the optimisation properties of Roth's diagram were connected, so that if it does not quite commute you may lose the optimality. Another sense in which it may be possible to interpret this is, what happens when the boundary operators do not quite square to zero i.e. the boundary of a boundary does not quite become zero. On this Lou Kauffman says, "Well, think about a circle, and if the boundary of the circle is a lot of little short, straight arrows and they do not quite map all the way around then perhaps there is a bit of granularity that we can get in here." Getting slightly more speculative now, if Roth's diagram does not quite commute in this sense then we get a nonzero value for a Lie bracket-like structure which begins to look a little quantum mechanical. Certainly there is scope here for thought.

Another connection between Roth's diagram and quantum-mechanics, which has come to my attention just recently, is that the diagram Tom Etter was drawing on the board earlier today, of splitting up spaces of quantum probabilities, is pretty exactly Kron's theory. Indeed, Tom wanted his picture to be hierarchical, which I found pretty satisfactory, because something I worked on for some time was a hierarchical version of Kron's method.

I was complaining that although Kron's topology is discrete, his paradigm is not completely discrete because the variables are continuous. Consider Fig II. For a long time I wanted to put another column down here where everything was discrete (ie Boolean). There are approaches which might be useful here. One is that there is a thing in homology theory called the 'Universal Coefficient Theorem' which basically says that if homology works for the integer coefficients then it should work for anything, so if it works for the integers it should work for the Booleans. On the other hand, looking for some more nitty-gritty connection to a world like this, I spent some time talking to Jessel about what happens if you make networks of computers and the things which are flying around the 0's and 1's, and we talked about distributed computing and the main reason it did not get anywhere was that it was all very complicated. Later on I tried to simplify it by thinking about cellular automata which are much simpler kinds of computers which still have 0's and 1's floating around, but I did not really get very far until talking to Daniel Dubois, who has spent a lot of time looking at cellular automata and has got a version of Jessel's paradigm which is now beginning to look as though it may well work quite well in a purely Boolean world. [At this point Tom Etter pointed out that the Boolean formulation was a special case of probability so that Tom's paradigm may be of use here.]

About a year ago I went to a lecture at the IEE by a gentlemen called Alain Bossavit who spent some time trying to persuade the Electrical Engineers that, rather than thinking about vector algebra or tensor algebra, that there would be a lot of advantage in them moving up the descriptive power of mathematics and using Differential Forms instead. Differential forms have a number of advantages over tensors. In particular they are coordinate free, that is when doing the algebra you do not have to worry about the co-ordinate system at any particular point in time. In doing this, Bossavit put some diagrams on the OHP screen that looked

remarkably like Roth's diagram, but they were different in rather constructive and clever ways. In some ways they did not have the richness of Roth, but they did have other things as well. He called these Tonti diagrams. And indeed there is a gentlemen in Italy called Enzo Tonti who has independently rediscovered much of Kron's work over the last 15-20 years and has published this in Italian (which is why we never read it). I am now in touch with Tonti and have found that he and a number of other people are working like mad on this stuff at the moment. They are particularly interested in the Finite Element method, which is where we started from, and which is why they are asking questions like, "What are the conditions for diagrams like Tonti's (or Roth's), of the continuous world, to be discretisable". They are asking these questions because they want to know when they can compute Finite Elements. They are not interested in natural philosophy, but they have a number of conditions, which are completely different to anything I have seen before, which may be relevant to Discrete Physics.

It might be worth me commenting here that Kron and Jessel both saw what they were doing not just as the development of computational procedures, but also as natural philosophy. In fact I am now of the opinion that what Kron was actually trying to do was build an ontology of Engineering. It was quite a different kind of ontology to anything anyone had attempted before. It was an attempt to explicate the richness of the things he saw about his Engineering view of the world. I think that that is a reasonable definition of an ontology. [Clive Kilmister commented here that he would see Kron as an intellectual defendant of Heaviside, an opinion also expressed by others.]

There is another potential connection between electric circuits and discrete physics which Lou Kauffman has been bringing to my attention over the last several years. He has been looking at a newly discovered natural isomorphism between electric circuit theory and knot theory. I think that this is a fascinating connection because Lou has a mechanism for things he can do with the world of knots, and Kron and Jessel and the rest of this structure (points to diagram) is a big mechanism for the things you can do with the world of electric circuits, and they indeed appear to say rather different things, and if we could bring those together then the richness of the whole structure together would be immense. I am quickly going to try and show you this, or a very simple version of it, because I think what Lou has done is lovely. Consider Fig 3. The things on the right are bits of string and the things on the left are electrical resistances. A piece of wire in an electric circuit is replaced by two bits of string. A positive resistance is replaced by a twist to the right, a negative resistance is replaced by a twist to the left. (A negative resistance in practice might be a transistor, or any circuit, which was set up to give you a current of +1 in one direction when you put a voltage of -1 in the opposite direction, which you would have to do with an active kind of circuit, but you could do it).

I should make a restriction here, Lou's electric circuits, at least in the initial description of what he is doing, have integer (and hence rational) resistances, and indeed that is equivalent to a network of resistances which have value +1 or -1. (Clearly it can have a resistance of +2 by putting two resistances in series and indeed can have smaller resistances by putting two resistances in parallel. Of course this would give rational resistances, so it is fairly general.)

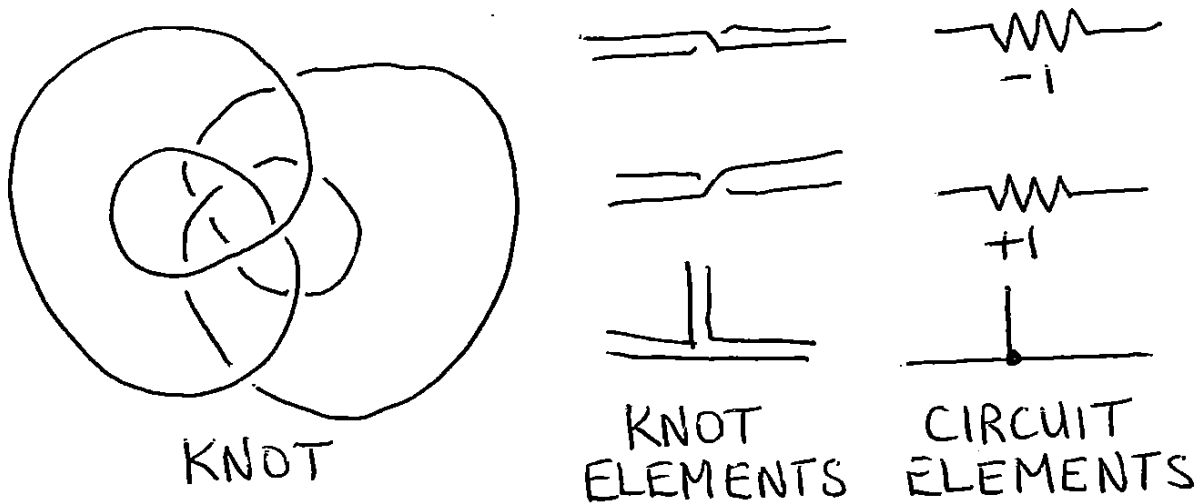
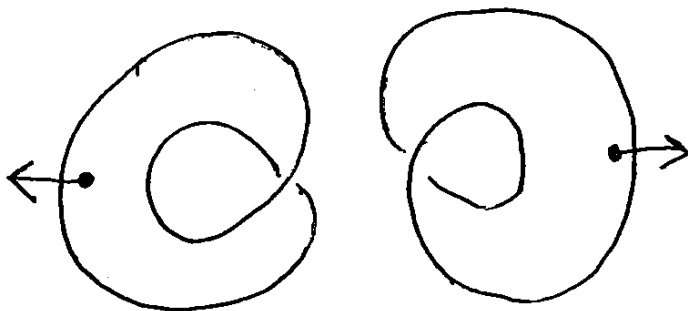
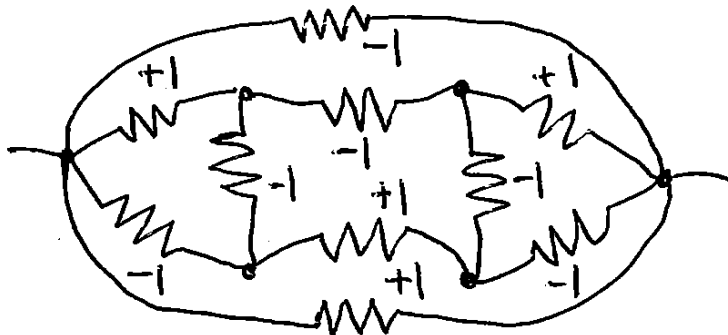


Fig 3 Knot elements and their equivalent electric circuit elements

These are the elements of electric circuit theory and the elements of Kauffman's knot theory and there, indeed (points to screen), is a knot and to go one way we replace a twist that way by positive resistances and a twist the other way by negative resistances, and you have got to start off with some reference point but it all works out quite nicely. Now what use is this? For it to of be any use you have to have something that is invariant across the circuit-knot transformation. The invariant which Lou discovered holds across the transformation is, in the case of knots, whether or not if, in the knot, you grab a point with your left hand and a second with your right, the whole thing comes apart thus:



and in the case of the equivalent electric circuit, whether or not the conductance between the two corresponding points is zero.



So the interesting thing to investigate now is how tearing can be applied to knots.

[At this point Kauffman pointed out that the reverse implication is not true. If the conductance

vanishes, that does not necessarily mean that the knot will fall apart. Anything you calculate for knots only shows you a little bit about them. It never tells you the whole story. So that whenever we calculate topological invariants of any kind, homology, knot invariants, or whatever, it is always much weaker than what can actually happen. But you can calculate the conductance of the electrical network by calculating something from the knot theory. What you calculate from the knot theory is the ratio of two scattering amplitudes where the knots are considered to be small Feynman diagrams, so that you're actually calculating something quantum mechanical informally.]

So to summarise the areas which we have identified need further investigation are

1. Interpretations of the de Rham theorem
2. Rigorous general formulation of Kron-Jessel equivalence
3. What do we mean by "discrete"?
4. Problems with Roth's diagram and "exact cochain complexes" **
5. What happens when Roth's diagram becomes approximate?
6. Boolean homology and Dubois' cellular secondary sources
7. Relationship between Mike Manthey's Clifford algebra representation and the ladder diagram
8. Relationship between Tom Etter's quantum separability and Tearing ***
9. Relationship to Tonti and Bossavit's work
10. Relationship between Tearing and Knot Theory

In particular 4. needs clarifying before we can proceed further with a number of the other connections.

Notes added in proof.

** With the prompting of Mike Manthey, Clive Kilmister, Mike and myself met in Lewes earlier this year and sorted out the situation with respect to the detail of Roth's diagram fairly clearly. The diagram, it turns out, does not quite commute in the strict sense which can be seen clearly by following the elements in my pictorial version of the diagram in the figure attached. This work will be written up in detail at a later date.

*** This is something I have been working on and a new paper will appear in the forthcoming Special Issue of IJGS, along with Tom's original paper on Link Theory. I will present some of this material at ANPA in 1996.

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FROM THE POCKET CALCULATOR TO DE RHAM'S THEOREM¹

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Abstract This paper is about discrete (= combinatorial) foundations of analysis. Before, I had published two papers on infinite loop spaces in Springer Lecture Notes in Mathematics, particularly in volume 99, 1972. This work had been criticised by J. P. May (Geometry of iterated loop spaces, SLNM, vol. 271(1972)). Being challenged to defend it, I responded by writing this paper, which introduced new themes into the discussion. But the end result was too speculative (and the bibliography too disconnected from the paper to be more than suggestive). Subsequently I published two more papers on simplicial sets and foundations of analysis: SLNM volume 753(1979), and Journal of Pure and Applied Algebra, 19(1980). After that, I gave up publishing in this vein.

1 The Real Numbers

In spite of its mathematical successes, the classical conception of reality

$$\mathbb{N} \longrightarrow \mathbb{Z} \longrightarrow \mathbb{Q} \xrightarrow[\text{completion}]{\text{Cantor-Dedekind}} \mathbb{R}$$

leaves some mathematical desires unsatisfied. Alternative continua are sug-

¹This paper appeared originally in 1976 in the technical report series of the Department of Mathematics, Århus University, Århus, Denmark. We reprint it here, with the author's kind permission, for its potential interest to ANPA members.

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gested by non-standard analysis, constructivism, intuitionism - topos theory, physics, computational mathematics - pedagogy. I link the last two because a computational approach to the real line is easily grasped by students. Although mathematicians are not used to thinking about the real numbers from the point of view of specific numerical representations, we all know that the properties of the real line can be explained perfectly well in terms of the familiar concept of infinite decimal expansions (see [5, p.58], for example).

In purely mathematical terms, if theory is to be guided by practice, such an approach requires the use of constructivist ideas, and seeing how far constructivist ideas can be pressed. This positive point of view is essentially that of E. Bishop's book [1].

Such concepts as "finite calculation" and "constructive object" then have to be interpreted in a concrete way. For the first I propose to employ a heuristic concept of a *calculator*, by which I mean a human/machine combination, probably graphically interacting. I present calculations in a fixed decimal-point pocket calculator mode for clarity of the number representations. But in fact much of the numerical experimentation on which the sketchily-described results that follow are based was actually carried out in the Lie algebra:

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Mathematics in a calculator is subject to roundoff truncation error. To smooth out such errors, I call *constructive* certain concepts defined by limiting processes over an infinite network of progressively finer calculators. To the study of this network I apply methods of algebraic topology. Unfortunately, in this preliminary version I have not been able to supply many details on how this is actually carried out.

It is not surprising that algebraic topology may contribute to the understanding of computational mathematics. Much of the subject has its origin, after all, in Poincaré's idea of simulating a continuous manifold by a discrete model divided into computational cells.

This connection between the two fields has governed my choice of topics. I shall give a “constructive” proof of the theorem of de Rham relating differential forms and cochain calculations, and raise some natural questions about constructive manifolds (= PL manifolds?). The tenor of the argument is that the surgery obstructions, rather than being part of a remote superstructure, actually lie like wormholes in the infinitesimal structure of the line [12].

No effort has been made to achieve computational verisimilitude. But all of the geometric objects described are to be thought of as complexes of simpler calculators, and the geometric dimensions of these complexex attain as a measure of computational complexity.

This material was described in the topology seminar at the Matematisk Institut, Århus, in October 1976, and I wish to express my thanks to the Institute for making it possible for me to do this work under its auspices.

2 Calculators

Let C_k be a calculator capable of performing various function evaluations and fixed-point “decimal” arithmetic to an accuracy of b^{-k} where b is the base of the number representation ($b = 10$ in common use, $b = 2, 8$ for switching, $b =$ an odd prime p for topology, and the choice of the base will make a difference). C_k displays in one of the forms

$$\boxed{\pm} \quad \boxed{a_k, a_{k-1} \dots a_0 \star a_{-1} a_{-2} \dots a_{-k}}$$

$$\square \quad \boxed{\text{error}}$$

where $0 \leq a_k, \dots, a_{-k} \leq b - 1$. Letting x be the variable in C_k , put $\Delta x = b^{-k}$. The largest “real number” in C_k is $b^k - \Delta x$, and $1/\Delta x = \text{error}$. We have to limit the numerical range of C_k anyway; it seems harmless to place the decimal point (preferred to the ambiguous comma) in the center of the display. (Floating point representation will not be used, but probably enters in a natural way as indicated³.)

³in the missing diagram. In truth it is possible to model the additive of C_k with carrying and roundoff slots. See my forthcoming paper “Numerical Computation and Combinatorial Topology” for these details. *[this note added in proof]*

Among the function evaluations assumed in C_k are the usual algebraic operations. C_k is an abelian group under $x \pm y$ except for overflow. Multiplication in C_k is defined by

$$xy = (x \times y \text{ in the usual sense})_{\text{truncated into } C_k}$$

The product of two numbers in C_k naturally lies in C_{2k} , but the last k digits cannot be displayed. This leads to underflow. As a result, the multiplication is non-associative, non-distributive, and has zero-divisors; in particular, $(\Delta x)^2 = 0$. On the other hand, $(\Delta x)^2 = 0$ is good for differentiation, and the multiplication is commutative. Underflow does not affect integer calculations, so if we let $\mathbb{Z}(C_k)$ denote the elements displayed in the form $\boxed{\pm} \boxed{a_k \dots a_0.00 \dots 0}$ then $\mathbb{Z}(C_k)$ is a commutative ring up to overflow and C_k is a $\mathbb{Z}(C_k)$ -module.

C_k is a partially ordered set under $\leq$. The partially ordered structure is used to give C_k a simplicial structure; for example, the basic intervals $[x, x + \Delta x]$ are 1-simplices. These simplices are needed for the constructive validity of some classical theorems. For example, the "intermediate value theorem" in C_k states that an intersection number of two 1-simplices is $\neq 0$:

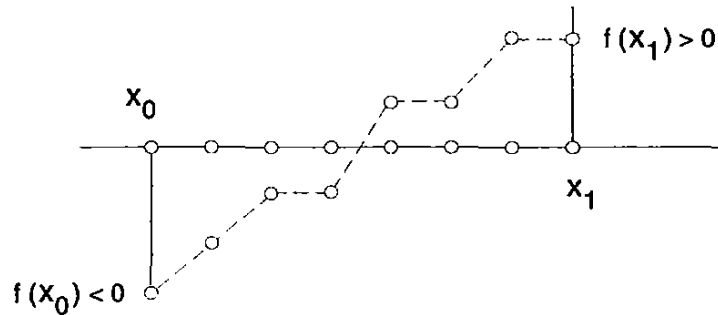


Figure 1:

The software must accept such simplicial information, and in fact we may as well assume that C_k can calculate the homology of a finite complex, in view of its good additive properties. (But there will be some trouble in the cohomology ring.)

The geometrical structure of C_k allows us to interpret the errors introduced by truncation as homotopies. In effect, we regard C_k as a homotopy-associative, homotopy-distributive ... object after the manner of [3]. This involves a type of forward error analysis [15] which can be formulated in various ways (compatible with the simplicial structures).

Elementary calculus in C_k presents some curious aspects. The integral

$$\int_{x_0}^{x_1} f(x)dx$$

is probably best defined by accumulating its value in C_{2k} and truncating into C_k . Its non-bilinearity must be dealt with by homotopies. The only possible definition of the derivative of a function $f(x)$ in C_k is

$$f'(x) = \frac{\Delta f}{\Delta x}$$

which is always an integer. "Stokes' theorem" is therefore valid:

$$\int_a^b f'(x)dx = f(b) - f(a)$$

but the "Poincarè lemma" $\frac{d}{dx} \int_{x_0}^x f(t)dt = f(x)$ can only be true for functions with integral values. This means that de Rham cohomology in C_k cannot be homotopy invariant.

3 Infinite networks of calculators

Infinity enters through the index set $0 \rightarrow 1 \rightarrow 2 \rightarrow \dots$ over which we form some limits:

$$\begin{aligned}
\mathbb{R}_{\text{constr}} &= \bigcup_{k \rightarrow \infty} C_k \quad \begin{array}{l} \text{the } \underline{\text{constructive real numbers}} \\ = \text{all finite decimals} \end{array} \\
\downarrow & \\
\mathbb{R}_{\text{LPO}} &= \varprojlim C_k \quad (\text{all infinite decimals}) \\
\downarrow & \\
\mathbb{R} &= (\varprojlim C_k) \quad \begin{array}{l} \text{modulo the relation } x = y \\ \text{if the two infinite decimals} \\ \text{represent the same real number} \\ \text{eg. } x = 0.9999\dots, y = 1.0000\dots \end{array}
\end{aligned}$$

More precisely, the direct limit $\mathbb{R}_{\text{constr}}$ is taken over the inclusions $C_k \rightarrow C_{k+1}$ which append zeroes, and includes the higher dimensions and simplicial structures mentioned before.

The inverse limit $\mathbb{R}_{\text{LPO}}$ is taken over the system of truncation maps $C_k \leftarrow C_{k+1}$ which discard digits (and are particularly prone to over/underflow with our calculator conventions). The suffix “LPO” stands for Bishop’s “limited principle of omniscience”, on the basis of which he believes that most of classical analysis can be carried out [1]. Here this principle will be interpreted to mean that all of the digits of infinite decimals are known. Thus

$$3 \leftarrow 3.1 \leftarrow 3.14 \leftarrow 3.141 \leftarrow 3.1415 \leftarrow \dots$$

is a typical element of the machine $\mathbb{R}_{\text{LPO}}$; its 0^{th} component is an element of C_0 , which has not really been defined. $\mathbb{R}_{\text{LPO}}$ is in fact a sort of non-standard model of the reals; for example, it contains many infinitesimal higher-dimensional simplices, such as the 1-simplex $[0.9999\dots, 1.0000\dots]$. These become degenerate under the identification $\mathbb{R}_{\text{LPO}} \rightarrow \mathbb{R}_{\text{classical}}$. Both of the arrows $\mathbb{R}_{\text{LPO}} \rightarrow \mathbb{R}_{\text{classical}} \rightarrow \mathbb{R}$ are homomorphisms with respect to appropriate descriptions of the algebraic structures. If all of the calculators C_k in the network are working in the same number base b , then these maps are

$$\mathbb{Z}[1/b] \rightarrow \mathbb{R}_{\text{LPO}} \rightarrow \mathbb{R}.$$

The network $0 \rightarrow 1 \rightarrow 2 \rightarrow \dots$ could be complicated by including other bases. But in any case, $\mathbb{R}_{constr}$ will be a subring of the rationals, possibly $\mathbb{Q}$ itself.

In view of the intrusion of infinities, the constructive imperative has to be re-examined. It is obviously no longer sufficient to say that we shall work in a finite calculator. Instead, we shall consider certain geometrical theories $\mathbb{X}$ (involving the real numbers) for which it turns out to be possible to construct reasonable models $\mathbb{X}_{C_k}$ in the finite calculators, as well as maps $\mathbb{X}_{C_k} \rightarrow \mathbb{X}_{C_{k+1}}$ induced by the inclusions. The *constructive model* will then be defined as the direct limit $\mathbb{X}_{constr} = \lim_{\rightarrow} \mathbb{X}_{C_k}$ as $k \rightarrow \infty$. There will also be a natural map from the constructive model into the model based on the classical real numbers:

$$\mathbb{X}_{constr} \longrightarrow \mathbb{X}_{classical}$$

This I shall call the *Bishop map*. Its failure to be an equivalence measures non-constructivity of $\mathbb{X}$. The Bishop map always factors through a non-standard model $\mathbb{X}_{LPO}$.

In the following sections we shall examine the Bishop map in the cases

$\mathbb{X}_0$ = de Rham cohomology theory

$\mathbb{X}_1$ = theory of topological manifolds

We shall find that $\mathbb{X}_0$ is constructible, but in a tentative fashion that $\mathbb{X}_1$ is not.

4 De Rham cohomology

De Rham theory is usually set up first for open subsets or simplices in euclidean space, then globalized by gluing these elements of space/cohomology together. Constructive modelling of the gluing process presents difficulties, however (see the next section for some further remarks on this). Thus we start with the Whitney-Sullivan [13] - Thom [14] theory of differential forms on simplicial sets. In that case the transitions between the local models

are combinatorially given. The natural meeting point of the two approaches seems to be the theory of simplicial manifolds [7].

As far as possible we follow the classical model, so let us review it.

Let K be a simplicial set and let $A^*(K) = (A^p(K), d)_{p \geq 0}$ be the theory of global real differential forms on K . (That is, $\omega \in A^p(K)$ is a family of locally defined p -forms ω_σ , one on each simplex σ of K , which fit together under the simplicial operators.) Let $C^*(K)$ be the real-valued cochains on K . Integration of forms over simplices defines a map

$$A^*(K) \xrightarrow{\langle \int \omega, \sigma \rangle = \int_\sigma \omega} C^*(K)$$

By Stokes' theorem, f is compatible with exterior derivative and coboundary operators, hence induces a cohomology map

$$H_{deRham}^*(K) \longrightarrow H^*(K).$$

The simplicial de Rham theorem states that this map is an algebra isomorphism. Locally the two theories coincide, since if ω is a closed form on a simplex, the equation $\omega = d\eta$ can always be solved by Poincaré's lemma. Globally, the two theories glue together in the same way.

To imitate this scheme constructively, we start with an isolated calculator C_k and model K by gluing together C_k -models of its simplices:

$$(\Delta_n)_{C_k} = \{f(t_0, \dots, t_n) \mid \sum t_i = 1, t_i \geq 0\} \subset (C_k)^{n+1}.$$

Then a p -form on K is locally given by an expression

$$\omega = \sum(t_0, \dots, t_n) dt_{i_1} \wedge \dots \wedge dt_{i_p}$$

where $f : (\Delta_n)_{C_k} \rightarrow C_k$. Let $A^p(K)_{C_k}$ be the $\mathbb{Z}(C_k)$ -module of global p -forms; in its full structure it is actually a higher-dimensional complex containing Boardman-Vogt homotopies.

$$A^p(K)_{C_k} \xrightarrow{\int} C^p(K; \mathbb{R}_{C_k})$$

is defined by numerical integration. Since Stokes' theorem holds in a calculator, f induces a cohomology map

$$H_{deRham}^p(K)_{C_k} \xrightarrow{\int} H^p(K; \mathbb{R}_{C_k}).$$

This map is an isomorphism when $p = 0$, but when $p \neq 0$ the fact that only forms with integer coefficients can be integrated (failure of Poincaré's lemma) implies that there is a large kernel generated by torsion of type $(\mathbb{R}/\mathbb{Z})_{C_k} = \mathbb{Z}[1/b^k]/\mathbb{Z} = \mathbb{Z}/b^k\mathbb{Z}$. This happens, for example, if $K = \Delta_n$. The de Rham theory also assigns differing values to Δ_n and Δ_0 . Thus the Whitney cochain operator only supplies a one-sided inverse for the integration operator, and $H_{deRham}^p(K)_{C_k}$ is not a cohomology theory. Of course, $H^0(K; \mathbb{R}_{C_k})$ is just ordinary cohomology.

The C_k -de Rham theory of an open subset U of euclidean space can be developed in the same way. The simplicial structure on which $C^*(U; \mathbb{R}_{C_k})$ is defined is that of the intrinsic "quantization" of the C_k -models of U . This is readily comparable with the C_k -singular complex.

Even though de Rham's theorem isn't true in an isolated calculator, it is just this point about quantization which indicates why the theorem is true constructively, and in the "real" world. Since computational space is naturally divided into cells, smooth processes must reduce to combinatorial ones, and differential forms to cochains. The translation is effected by the formula $\frac{d\sigma}{dx} = (\Delta x)^{-1}$ where $d\sigma$ is the usual cochain measure assigning mass 1 to every simplex and Δx is the step size of the calculator. Of course, the translation is ruined by the finite calculator's bad algebra, but it can be fixed by passage to the infinite network described in §3.

We return to the case of a simplicial set K . Without details, a *constructive* form on K will be defined on a C_k -model of K and take values in some C_ℓ , $\ell \geq k$. This ensures the validity of the Poincaré lemma. The solution η of $d\eta = \omega$ has double precision values. In fact, we define

$$A^p(K)_{constr} = \lim_{k \rightarrow \infty} A^p(K_{C_k}; \mathbb{R}_{constr})$$

the direct limit process being a piecewise linear extension of $\mathbb{R}_{constr}$ -valued forms to higher precision models of K . Clearly, the constructive forms give "correct" cohomology.

To make contact with the classical theory, consider the commutative diagram of Bishop's maps (the horizontal arrows in the diagram):

$$A^*(K)_{constr} \longrightarrow A^*(K)_{LPO} \longrightarrow A^*(K)_{classical}$$

$$\int \downarrow \qquad \qquad \int \downarrow \qquad \qquad \int \downarrow$$

$$C^*(K; \mathbb{R}_{constr}) \longrightarrow C^*(K; \mathbb{R}_{LPO}) \longrightarrow C^*(K; \mathbb{R})$$

In the induced cohomology diagram the integration arrows are isomorphisms, the left one by the reasoning above:

$$H_{deRham\ constr}^* \longrightarrow H_{deRham\ LPO}^*(K) \longrightarrow H_{deRham}^*(K)$$

$$\int \downarrow \simeq \qquad \qquad \int \downarrow \simeq \qquad \qquad \int \downarrow \simeq$$

$$H^*(K; \mathbb{R}_{constr}) \longrightarrow H^*(K; \mathbb{R}_{LPO}) \longrightarrow H^*(K; \mathbb{R})$$

$$\int \downarrow \simeq$$

$$H^*(K; subring(\mathbb{Q}))$$

The middle $\int$ arrow is proved to be an isomorphism by extending numerical integration in C_k to a “non-standard” method of integration in the PLO model. Applying this integral to C_k -forms, by pulling back to K_{LPO} along the geometrical map $K_{LPO} \longrightarrow K_{classical}$, one obtains the standard Poincaré lemma. Thus the right-hand $\int$ arrow is also an isomorphism. The standard Bishop map

$$H_{deRham\ constr}^*(K) \longrightarrow H_{deRham}^*(K)$$

therefore reduces to a coefficient homomorphism.

We conclude that (0-dimensional simplicial) de Rham cohomology is constructible. Since constructive forms on K are defined as a direct limit, this means that some finite calculator is capable of simulating all of the real cohomology behavior of K . (With more information about K , one could estimate how powerful the finite calculator would have to be.)

The $\int$ arrows on cohomology are compatible with $\wedge$ - and $\cup$ -products. The standard computer science construction of the minimal calculator (ifetch loop?) with the same behavior should reduce to Sullivan's minimal DGA model for real homotopy types [1,6].

5 Constructive manifolds

Let M be a topological manifold covered by a finite number of euclidean coordinate charts U_i . To get a constructive model of M , we start by modelling the open sets U_i and the transition homeomorphisms $U_i \cap U_j \rightarrow U_j \cap U_i$ in a calculator C_k (this is only possible up to homotopy, at best). Define

$$M_{constr} = \lim_{k \rightarrow \infty} C_k\text{-model}(M)$$

If such a construction succeeded, then the Bishop map $M_{constr} \rightarrow M$ would, intuitively, yield a PL -structure on M . Indeed, there should exist constructive analogues for the classical infinite loop spaces of the theory of manifolds, and arrow '1' in the following diagram should be a weak equivalence:

$$\begin{array}{ccc} PL_{constr} & \xrightarrow{1} & PL \\ 2 \downarrow & & \downarrow \\ TOP_{constr} & \xrightarrow{3} & TOP \end{array}$$

'2' should also be an equivalence, since the concepts of continuous and PL functions in a calculator coincide. Thus the Bishop arrow '3' may be indentifiable with the standard inclusion $PL \rightarrow TOP$. Constructivity of the theory of topological manifolds thus seems to be obstructed by the Kirby-Siebenmann element in $\pi_3(TOP/PL) = \mathbb{Z}/2\mathbb{Z}$.

Whether constructive mathematics can *prove* the Kirby-Siebenmann theorem, or prove generally useful in studying other types of manifold structures, such as C^∞ , is another question. There are suggestions of connections with surgery obstructions, and of 2-torsion resulting from an interaction of the gluing process and the non-cohomological finite de Rham theory $H_{deRham}^*()_{C_k}$.

We are far from being able to define the "convenient category" of manifolds, whose objects are machines with extra structure, that one might hope for. But although computational space is poor in algebra, the relation between differentiation and space is actually clearer than in the classical case. For example, the computational line has the exceedingly strong linearity property called "line type" in [10] (at least up to homotopy). Furthermore, the tangent vector representor D of categorical dynamics [11] appears to *exist* in the computational case, in contrast to the classical theory. This may allow the elegant formalism of categorical differential geometry to be applied to the more difficult forms of manifold cohomology (for example [7]).

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THE PHASE WEB PARADIGM

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We describe an information-oriented, computational physical theory based on expressing knowledge of the world via patterns of synchronization. The systems this theory describes are self-organizing, display emergent phenomena, and can be expressed in terms both of Clifford algebras and algebraic topology, and practical running programs.

KEYWORDS information, self-organizing systems, distributed systems, quantum mechanics, planning, learning, robotics, Clifford algebra, twisted isomorphism, wave-particle duality, emergence, cycle hierarchy, combinatorial hierarchy

1 Introduction

The research program which generated the results described in this article began about twenty years ago with the author's recognition that the hierarchical structures used in computer science are inadequate to express the properties of systems consisting of many interacting processes. A new hierarchy was proposed [Manthey and Moret, 1983] which constitutes a conceptual generalization of the hyper-cyclic hierarchy of Eigen & Schuster [1979], based on the computational concept of resource invariants, which in turn are defined in terms of the synchronization relationships that obtain between otherwise distinct processes. These invariants are the computational equivalent of physics' conservation laws [Manthey, 1992]. Besides its fundamental generality, this "cycle hierarchy" has the very pleasant ability of being able to express emergent phenomena.

The mid-1980's saw the application of the cycle hierarchy to AI as the basis of an attempt to solve, in tandem, two classic AI problems: planning and learning. In this context, learning would consist of an entity's constructing a cycle hierarchy as its representation of what it had learned. This same hierarchical structure would then be used to guide the achievement of goals, i.e., the carrying out of 'plans'. The basic idea was that the cycle hierarchy, by virtue of its conceptual basis in the fundamental concept of synchronization, would be able to represent arbitrary knowledge independent of domain,

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and this same basis - in the form of its resource invariants - would together with the logarithmic reduction in complexity arising from the hierarchical structure, attack the combinatorial growth of computational complexity that accompanies goal achievement. This complexity is of order n^m , where n is the number of distinct actions an entity can carry out (eg. grasp, release, move hand, etc.) and m is the number of steps in the plan.

Since both m and n can have values in the hundreds, and easily much larger, the result is an *immense* space of possible plans, of which only a tiny fraction will be correct. It is this observation that lies behind the classical planning-paradigmatic view of 'planning as search'. Unfortunately, despite much investment throughout the 1970's and 1980's, this paradigm saw only limited practical success. One reason is that putting in constraints on the search, even in high-level or abstract form, is still 'putting in by hand' and, ultimately the more telling consideration, has not borne the desired fruit. Another reason is that the search paradigm implicitly assumes that the planning *per se* can be done prior to the execution of the plan, which breaks down miserably in the face of the numberless unforeseen peculiarities of the real world.

Brooks' school of robust, simple-minded *sensor-based* robots [Brooks, *et alia*] represents a reaction to the above, and has seen reasonable success. This approach is quite compatible with the content of this article. Indeed, one could say that we present an underlying *theory* for how to build such robots so that they can learn (ie. construct) their internal structure from their real-world experience. This obtains because currently both the static and dynamic structure of these robots is ultimately *ad hoc* and entirely 'put in by hand'.

Howsoever, the problem with the cycle hierarchy in the 1980's was that, although it was possible to imagine how to use it to guide goal-oriented behavior using the invisible hand of synchronization, it was unobvious how to construct it in the first place.

The crucial breakthroughs occurred in the period 1990-1992, when it was realized that the critical, *and sole*, distinction the system - by now called 'Topsy' - needed to make was whether two or more sensory inputs could occur simultaneously or (appear to) exclude each other. This led quickly to the realization that a Clifford algebra provided the appropriate mathematical framework. Since Clifford algebras represent a consolidation of vector spaces, linear algebra, tensors and the like, for which reason they are promoted as a unifying mathematical framework for physical mathematics [Hest89], the intuition of very general applicability which had driven the research program for so many years was thus shown to have been justified. It also resulted in the implementation of a proof-of-principle distributed computer program which successfully demonstrated that Topsy could learn from experience and exhibit novel behavior on the basis thereof [Irskov and Jensen, 1993]; we are currently re-implementing this program to make it 'usable' [WWW]. The entire approach is now called the *phase web paradigm*, to distinguish it from the particular realization in the form of Topsy.

The connection between the above AI-flavored research program and physical theory rests on the fact that the scientific endeavor - to understand Nature and produce theories of how it works - correspond respectively to *learning* from Nature, and condensing

what is learned into causal relationships which can, if desired, be used to manipulate Nature, ie. *planning*. It should perhaps be mentioned that planning necessarily implies what is called a 'goal-oriented' program, that is, the program possesses an internal representation of the state(s) it is trying to reach. When the paradigm is interpreted as a physical model, this teleological component may be understood to express 'potential' in the physical sense, though without directly tying this word prematurely to its otherwise well-defined counterparts in physical theory.

The goal of this paper is to impart the overall flavor of the phase web paradigm, and to sketch the form, character, and possibilities of the structures it implies. The remainder of this article therefore describes (1) the computational concepts of synchronization and mutual exclusion; (2) the derivation of the basic inductive principle - called *co-exclusion*; (3) the interpretation this induces in a Clifford algebra; (4) how the cycle hierarchy appears; (5) the relationship of the resulting conceptual framework to physics; and (6) why - very briefly, and necessarily speculatively - the phase web overall can also be advanced as a serious candidate for a model of complex living systems, including human consciousness. Before doing so, we present a brief - and still tentative - mini-essay on the topic of emergence.

2 What is 'Emergence'

In our view, emergence is, at bottom, an intuitive concept. Those who would immediately conclude that all is already lost with this admission should recall the formally undefined 'intuitive' concepts *point* and *line* in geometry, or 'effective computation' in theoretical computer science, or biologists' inability to define 'living system' [Rosen].

The commonest and pithiest definition of emergence is "The whole is greater than the sum of its parts". The problems come when one attempts to apply this definition to particular cases: what constitutes a "whole" is an old philosophical question, and the interpretation of "sum" is open. In our attempt to negotiate the conceptual swamp, emergent phenomena will in the following be classified as either static *properties* or dynamic *effects*.

In general, what in mathematics is termed a 'global property' expresses one aspect of emergence: a property of a collection of things which *in principle* cannot reside in any subset of these same things. Examples abound: the 'closedness' of a curve does not inhere in any subset of its constituent points; whether a rope is knotted or not does not inhere in the "sub-lengths" constituting the rope; etc. Hence our definition of emergence for this aspect is

An emergent property is a global property of a fixed set of things, which property does not inhere in any proper subset of these same things.

This view of emergence applies to Minsky's example [Min] of building a mouse trap by assembling six planar walls to make a box. Minsky says - with only superficial

analysis - that to claim 'emergence' here is clearly rubbish. We disagree, cf. the example of the global property of closedness, and conjecture that our differing conclusions spring from another aspect of the example: the mouse trap can obviously be assembled by a *sequence* of actions, and it is hard to see anything amazingly emergent in this, with which we agree.

Thus, while entirely respectable, the mathematical 'global property' view does not seem to exhaust the concept of emergence. The mathematical world-view tends strongly to presume a *static* or situational view, cf. the specification of a 'fixed set of things' in the definition above. Put another way, mathematics tends to express its insights using the concept of equality, '=', in the sense of "always was and always will be". Truth is not dependent on time! In contrast, computation deals inherently with *change*, using the concept of value replacement, ':=' , as in $a := b$, that is the current value of a is replaced by the current value of b . This is to say that an *algorithm* and its corresponding 'rolling out' in time are rather different from a statement of static 'truth'.

Our definition of emergence in this dynamic or process-oriented context is therefore

An emergent effect displayed by a whole is an effect produced by co-operating partial effects of that whole that cannot be expressed as a sequential combination of said partial effects.

On this view, there is truly nothing emergent about the assembly of the mouse trap. On the other hand, examples of effect emergence also abound, but for some mysterious reason (to which we will return in a moment) seem not to be recognized as such by many critics. To wit: spin, charge, gravity, 'gridlock' or deadlock, non-determinism in concurrent systems, and wave interference are all examples of effect emergence. The disagreeing reader need not accept the first three examples, since their origin is still not fully understood, although hopefully s/he will see later why they have been included here.

Concerning deadlock: it is simply nonsense to single out one of the four cars 'gridlocked' in an intersection and say "It's *your* fault". The effect - utter standstill - is clearly founded on the presence of all four cars together.

Concerning non-determinism: Consider two processes sharing a memory cell. Each process running alone will produce the same deterministic result every time it is executed, but execute them together and you get an unpredictable result. If one cannot call this effect 'emergent', what then is it?

Concerning wave interference: The situation is similar, and just as clear, as non-determinism. The famed double-slit experiment in quantum mechanics is also an example.

A final example is illuminating². Consider an object weighing 5 units, and two workers who can lift 1 and 4 units respectively. Singly they cannot lift the object, but together they can. We ask: is this an example of emergence or not?

²I heard this example at a lecture ca. 1990, but can unfortunately not remember the speaker's name. The analysis is mine, however.

The definition of property-emergence is satisfied, in a most pedestrian fashion, in that $1+4=5$. However, if the object is decomposable into pieces with unit weight, then either worker can do the job alone, ie. with a sequence of actions. So the definition of effect-emergence is not satisfied. Hence, if the object can meaningfully be viewed as being so decomposable, there is no emergence.

But what about non-decomposable objects? At this point we believe we have arrived at the heart of the matter, since objecthood in itself inherently and implicitly attributes some emergent property or effect to a collection of parts. So: what is an object? This is of course an old philosophical question, but we will not tread that particular path. Instead, we will follow that of physical science.

While the Newtonian and classical chemical/atomistic world view were clear on the issue of objecthood, quantum phenomena - in particular, wave-particle duality - have taught us otherwise. A quantum object is - viewing it from a wave perspective - an interference pattern. As noted earlier, we view interference as a prime example of an emergent phenomenon, and as such cannot be meaningfully decomposed without destroying it.

Moreover, even macroscopic objects can be viewed as interference patterns. We seem thus to have arrived at an impasse, for clearly there do exist macro-scopic objects which cannot be meaningfully viewed as being decomposable.

The only exit is the word 'meaningfully', with which we have modified 'decomposable'. Thus enter elements of context and semantics into the fray, and the scope of the discussion widens accordingly. We will correspondingly close our discussion at this point, and merely note that the phase web paradigm, by virtue of its basis in the act of observation, implicitly includes a contextual element; and similarly, since every object the paradigm constructs simultaneously 'is' and 'does', the paradigm implicitly contains a semantics or 'behavior' for same.

To round out this discussion of emergence, it is worth noting³ that many commentators view such things as chaotic systems, neural nets, and cellular automata as exhibiting emergent phenomena. The intuition seems to be that one is 'surprised' at what happens, in that the closed-form of the computation does not seem to promise it in any obvious way. Concerning chaotic systems, their non-determinism qualifies them - in terms of our preceding discussion - to claim effect-emergence. Furthermore, their intimate relationship with the complex plane would seem to indicate that the mathematics implicitly implies the sort of mechanism that is the main subject of this paper.

Similarly, cellular automata rely on the simultaneous presence of neighbors, which brings them as well in under the paradigmatic umbrella we will present. On the other hand, the transductional $y = f(x)$ character of neural nets would seem to speak against admitting emergence, since transduction is a very sequential affair. However, this conclusion is ameliorated to the extent that the presence of *simultaneous* inputs from multiple sources plays a crucial role.

³I am indebted to Peter Eisenhardt for pointing this out at the ANPA 94 conference.

Summarizing then, on the basis of our analysis and examples, we find no reason to dismiss the concept of emergence as the product of fuzzy thinking or scientific ignorance. Indeed, it would appear that when one peers closely, emergence is the rule, not the exception! On the other hand, it cannot be denied that the concept of emergence is frequently abused. We see three aspects to the controversy

1. The concept of emergence, with its focus on the part-whole relationship, implicitly invokes the concept of *hierarchy*: 'emergence' is a property of the change in viewpoint as one crosses a hierarchical level. One must however be clear on exactly what *kind* of hierarchy one is talking about. As we discuss later, the typically assumed hierarchy relation - function composition - is *incapable* of expressing emergent phenomena, and we propose an alternative, which *can* express them.
2. It is also important to distinguish between strong and weak reductionism. A *strongly* reductionistic view categorically denies any possibility of emergent phenomena: any phenomenon exhibited at a given level can/must be isolated to individual effects from the level below, composed sequentially. A *weakly* reductionistic view allows emergent phenomena, but denies those which are not grounded in effects at the level below. Thus we *advance a weakly reductionistic position* in this paper. *Fuzzy minded* emergence preaches that "anything can happen" - grounding in lower level effects is not necessary; we find this view entirely indefensible.
3. The above two considerations ought to suffice, but unfortunately they do not explain the emotional intensity and purblindness of critics of the concept of emergence. Those whose opposition derives from not seeing a scientifically defensible concept of emergence will hopefully find an acceptable alternative in the cycle hierarchical model we develop in this paper. And perhaps over-familiarity with such long-standing examples of emergence as non-linearity has obscured the utter respectability of the concept. Howsoever, although the argument falls outside the scope of this paper, our ordinary everyday form of consciousness - ego-consciousness - is fundamentally based on function composition, and the emotional intensity of the denials of emergence emanate, in our view, from the incredibly strong grip this consciousness form holds on some individuals' view of what the world contains.

We will see later that, in our model, emergent *properties* appear, and are captured, with the concept of co-occurring indistinguishables. Emergent *effects* appear and are captured via a vector product that produces a new dimension in a space of mutually exclusive distinctions.

In *all* cases of emergence, we have found reason to believe that at bottom, they derive universally from *multiplicity*, that is, from the *simultaneous* presence of multiple entities. To convince the reader of the reasonability of this belief, we therefore begin with a discussion of the computational mechanism - synchronization - that allows the expression of simultaneity, and its opposite, mutual exclusion.

3 Synchronization and Mutual Exclusion

Most people are by now familiar with the basic idea of a computer program as a specification of a sequence of discrete steps ('instructions') to be carried out by an underlying computational engine. We will refer to these steps as *events* e , and the execution sequence of these steps as $e_1, e_2, \dots$ [For our purposes, e_i must precede, but need not necessarily entail, e_{i+1}]. Such a sequence is called a *process*, and the program which specifies it a *sequential program*. Thus a common everyday computer program such as one might write in Pascal, C, Lisp, Prolog, etc. - however complex - is in its execution an example of a *sequential process* [in which, since these languages express deterministic processes, e_i entails e_{i+1}].

In this connection, it is important to distinguish between the 'program' and the execution sequence (process) it produces. A 'program' is a *specification* of a behavior, ie. it specifies *a priori* what is to happen (ie. the 'process'). The two terms 'program' and 'process' are often used interchangeably, but the intent here is almost always 'process'.

The idea of a *computational* physical theory inheres in the extension of the computational (version of the) concepts 'event' and 'process' to physical systems. This should however not be understood to mean that the universe is to be viewed as some sort of "giant computer", but rather that all the necessary elements of such a theory must be expressed in terms of information and its emergence, transformation, and destruction.

The notion of a 'process' is inherently sequential, cf. the above definition. Ordinary programs are also *deterministic*, in that the event sequence that will occur can be predicted *a priori*. However, the definition of 'process' does not insist on this, and hence it is perfectly okay to speak of *non-deterministic* processes [wherein e_i does not necessarily entail e_{i+1}].⁴

Modern computing systems contain multiple interacting processes, and it is impossible to discuss them without introducing the notion of synchronization. Most multi-process systems are inherently non-deterministic. A further distinction exists as to whether a multi-process system is 'centralized' or 'distributed', which refers to the extent to which the 'knowledge' or 'control' that determines the system's overall behavior is centralized or local.⁵ The construction of distributed systems is a central and compelling topic in contemporary computer science research and technology; the phase web paradigm proposes a new and explicit model for an utterly and completely distributed execution regime.

Notice that there is an implicit hierarchical moment in the above definitions, in that a process as a whole can be distinguished from the events which comprise it. We will

⁴The confounding of the distinction between determinism and 'causality' is what caused many physicists in the grip of the quantum revolution to conclude wrongly that, since determinism had gone up in a cloud of smoke, so had causality as well. Rather, to abandon the position that things happen as a result of other things' happening is to abandon science.

⁵The cycle hierarchy, described in this article, resolves the tension between 'locality' and 'hierarchy', and thus allows them both to co-exist and obtain simultaneously.

see later that without due care, this hierarchical moment can quickly become strongly reductionistic, a restriction which is not only unnecessary, but which also constitutes a deadly conceptual albatross⁶ The cycle hierarchy provides the necessary alternative, one which naturally encompasses non-determinism, distribution, and emergence (cf. last footnote).

In a multi-process system, the ordering of events which seemed so natural in the sequential case evaporates: one can in general no longer tacitly assume *anything* about the order in which events in one process occur relative to those of another process. 'Time', viewed at bottom as a 1-1 mapping of the events in the sequence to the integers, thus becomes a very dodgy entity, and a 'relativistic' moment appears immediately at a fundamental level. This relativism spreads immediately to any hierarchical consideration, such that this too becomes context dependent. Synchronization is the *mechanism* via which 'order' can be introduced without violating all the other considerations. Thus the phase web paradigm proposes a mechanistic model, but in the computational - that is, informational - domain, for which reason we prefer the term *neo-mechanistic*. Surprisingly, wave phenomena are nevertheless naturally included in a fundamental way.

The subtlest trap in the above definitions is the way one is seduced into thinking of the computation carried out by a process *solely* as the sequence of states engendered by the events constituting it. We can write this explicitly as $e_1, s_1, e_2, s_2, \dots$, where s_i denotes the state which obtains after event e_i , often referred to as the process' 'state vector'. One then defines the 'result' of a process as the state s_Ω in which the process 'halts', and indeed, the notion of a process' halting with a result lies at the core of Turing's definition of 'computability'.

Notice however that this means that systems which do not halt hereby are classified 'not computable', and hence such things as computer operating systems, real-time control systems, communication networks, biological cells, etc. whose *raison d'être* is never to halt are paradoxically placed in a computation-theoretic limbo. The way out of this demise is to view the s_i not so much as states, but rather as *synchronization points* whose momentary existence permits other events to occur. This is however a very subtle distinction, whose power it takes time to appreciate.

All in all, it follows from the above that the concept and mechanism called 'synchronization' is where we must begin. [NB: the computational concept of synchronization we are discussing here and throughout must *not* be confused with the kind of synchronization (of observer frames) common in discussions of relativity theory. In particular, we so to speak *elicit* the photons the latter presumes.]

The computational concept of *synchronization* is embodied in a generic mechanism called a *synchronizer* (see Figure 1). This figure illustrates three successive stages in a generic synchronization act. Notice first that the left and right synchronizers are 'closed', as indicated by the little 'c' inside the circle. The middle one is similarly 'open'. The

⁶An allusion to Coleridge's 19th century poem *The Rime of the Ancient Mariner*, wherein a sailor pays a fateful price for unthinkingly killing an albatross, the seaman's symbol of good fortune.

little 's' and 'w' outside stand for 'Signal', as in "Signal the other process that it may proceed now"; and 'Wait', as in "Wait until some process Signals", respectively.

Focus now on the leftmost synchronizer: since it is 'closed', any process Waiting (stippled line) to proceed is delayed until a Signalling process (heavy line) eventually arrives and Signals. Assuming this has occurred brings us to the middle state of the figure. Notice that, as a result of the Signal which has just occurred, the synchronizer is now 'open'. When the Waiting process proceeds, it will set the synchronizer's state back to 'closed', as shown in the rightmost part of the figure.

In general, the *act* of synchronization expresses that the system consisting of the two processes (Waiter and Signaller) has evolved to a particular state, a state which permits the Waiter to now proceed to do whatever it now wishes to do (which it probably in turn would Signal some other process about after it has done it). Viewing the world as consisting of discrete processes implicitly introduces the concept of synchronization; synchronization is thus the handmaiden of discreteness.

As a concept, synchronization is what implicitly occurs between successive events, marking the end of one event and the permission for a logically entailable one to now occur.⁷

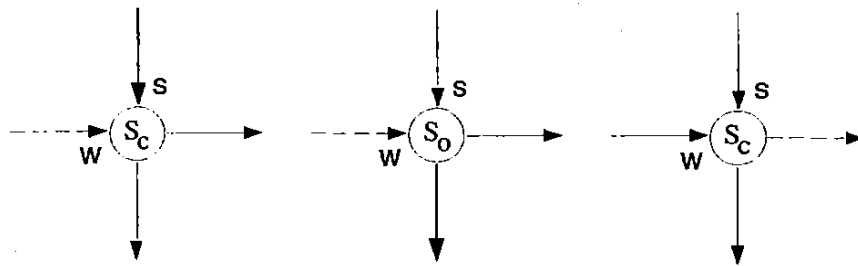


Figure 1: Synchronization ensures that one event occurs before another.

As a mechanism, a synchronizer establishes a before-after relationship between events belonging to two distinct processes. By definition, synchronization affects only the ordering of events, and not functionality. This means that no information is passed between synchronizing processes, and it is this property which allows a concept of

⁷This is clear enough until one realizes that the phase web is built *solely* out of synchronizations, which is to say that that which 'causes' a given state change (or, as it were, occurs 'between' two events) is just more synchronizations: this is the recursiveness of the structure, the hierarchical nature of the phase web. It is the locally inferred, 'subjective' invariance (see below) which defines the hierarchy for each observer. The discussion here, and the definitions of events (etc.) earlier, are thus relative to some given fixed level in some observer's hierarchy. The mind-numbing self-describing manifold herewith induced - whose multiform symmetries recede endlessly in all directions like Russian dolls made out of yin-yang symbols made out of Russian dolls - is the principle aesthetic charm of the phase web!

emergence which does not violate the usual billiard-ball notion of causality. It also means that if we very strictly ask what 'time' is, it is the ordered progression of synchronizations between a process and its surround - 'time' in a process is thus always and naturally relative.

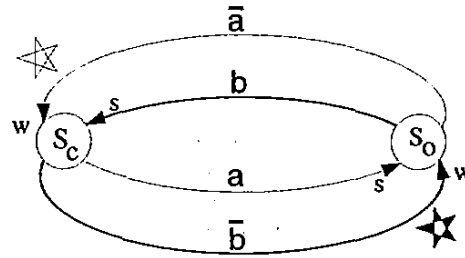


Figure 2: Two synchronizers used to create symmetric mutual exclusion. The thin process (A) has as possible states a and $\bar{a}$, and analogously for B.

Figure 2 illustrates the use of two synchronizers to express the fact that two processes (thick and thin ellipses, current state $\{\bar{a}, \bar{b}\}$ denoted by the two stars) mutually exclude each other's presence on the two innermost arcs, ie. states a and b will not co-occur, as follows. The thin process (unfilled star) cannot proceed because the synchronizer is closed. The thick process (filled star) can proceed and does so, implicitly closing the synchronizer behind it and placing itself on its interior arc. From this state, the only thing that can happen is the Signalling of the leftmost synchronizer, causing it to be open.

The system is now in a state complementary to that in which it began: now it is the thin process (unfilled star) that proceeds and closes the left synchronizer behind it, occupies its interior arc, and eventually Signals the right synchronizer. The system is now in its initial state again, and the alternation of the two processes in their occupancy of the interior arcs will repeat itself indefinitely. We will see later that this pattern of synchronization is necessarily exhibited by all 'actions' in the phase web.

It will always be the case in Figure 2 that at most *one* of the synchronizers will be 'open', and if this is not the case, then both are closed. That is, at most *one* of the processes will be on an inner arc. A convenient way to think about this is the image of the stick that is passed from runner to runner in a relay race. In the figure, the stick is currently 'in' the right synchronizer, indicated by the fact that that synchronizer is 'open'. Similarly, there is no stick in the left synchronizer, so the thin process is currently delayed - it Waits there. The thick process proceeds through the right synchronizer, implicitly taking the stick with it by setting the synchronizer to 'closed'. When it is finished with its private business, it implicitly places the stick in the left synchronizer by setting it to 'open', and proceeds around to Wait at the right synchronizer again. The thin process can now take the stick, do its private business, and place the stick back in the right synchronizer. The

system has now returned to its initial state, and the whole sequence will repeat.

Sticks are not bits of information, and their presence can only be detected indirectly by the fact that the system changes state.

A *resource invariant* is the statement of the fact that a system like Figure 2 conserves sticks - which one may view as naked instants of time - a fact which is captured by the equation

$$n = n_0 - \sum (\text{sticks taken}) + \sum (\text{sticks returned})$$

always holds for such a system. In this equation, $n_0 = 1$ expresses the fact that the system of Figure 2 initially possesses a single stick. The two Σ 's bookkeep the successive takings (Waits) and returnings (Signals) of the stick(s), and n - whose value alternates between 0 and 1 - expresses consequent the fact that, at most, one of the synchronizers will be 'open' ($n = 1$), and sometimes both of them will be 'closed' ($n = 0$).

Depending on how we initialize it, Figure 2 can illustrate three different invariants. $n_0 = 0$ corresponds to both synchronizers' being 'closed' and both processes Waiting: the system will never change state (n is and remains zero), which state is called *deadlock*. We have just seen how $n_0 = 1$ yields the alternation characteristic of mutual exclusion, that is, neither process will occupy its interior arc when the other is doing this. $n_0 = 2$ corresponds to "non-exclusion", that is, *co-occurrence* of the processes in the interior arcs. We note however that the invariant in this case is not truly invariant, in that (reader exercise!) one of the processes can Signal twice in a row, which causes a stick to be lost (the synchronizers cannot count beyond 1). Hence co-occurrence can be seen to be an unstable, coincidental, or fleeting state.

Notice finally that resource invariants will always be reflected by a cyclic structure, this being the collection of 'orbits' on which conserved sticks move. It is these cycles which are understood when we refer to the *cycle hierarchy*, which is the hierarchical arrangement of (invariance) cycles by which the phase web structures the world. We emphasize that these cycles should **not** be thought of as iterative cycles - 'oscillation' is much more appropriate.

A resource invariant is produced by the particular way a set of processes happens to (always) interact. At the same time, it is something which is not possessed by any single process in the set. Hence a resource invariant is collective phenomenon, a property of the whole but not of any single part thereof, i.e., the existence of a resource invariant is an emergent phenomenon.

The author's original point of departure in the 1970's was "What can be known?", where it was understood that the question was asked and was to be answered within a multi-process computational framework. More precisely, assuming that "reality" is to be viewed as a potentially unbounded set of discrete and interacting processes, the question "What can be known?" was translated to "What is the nature of an object?".

Viewing the world as a set of interacting processes means that the question must be answered with the understanding that everything is in flux. One must therefore ask what does not change, even when everything is changing; such a thing is called an *invariant*. The concept of change (or flux) implicitly introduces the concept of time, and time is, in the last analysis, an ordering of events. Hence we sought an event-based invariance in the time domain.

The relevant invariance concept is thus the *resource invariant* we derived above. The computational concept of a resource invariant corresponds to what has traditionally been called a conservation law: that through the course of a flux, there is something which is preserved, which is conserved, which neither appears nor disappears.

Hence an entity using the phase web paradigm comes to 'know' in terms of resource invariants experientially determined to exist in the world: these form the objects of the entity's world, and are the things which it reasons with and about, and manipulates, to achieve ends.

The phase web works with three kinds of invariants:

- mutual exclusion - two (or more) events mutually exclude each other ('mutex')
- deadlock - two (or more) events cannot occur⁸
- co-occurrence - two (or more) events occur together

It follows from the preceding discussion that these invariants define and create the objects that then constitute an entity's world model. [Note that this definition of 'object' differs from and is stricter than the current use of the term in programming languages: phase web objects necessarily consist of *multiple* concurrent processes linked by a resource invariant.]

As an example of the use of invariants to form objects, consider a library. Barring theft and new acquisitions, books are borrowed, read, and returned for re-use. A Martian and a Venusian viewing the situation would both conclude (1) that "books" are objects by virtue of their co-occurring features (or molecules, if you will); (2) books are used on a mutually exclusive basis, since if one Earthling takes a given book, then another cannot and does not; and (3) the number of "books" is conserved, and the Earthlings always bring a given book back to a certain place before another takes it again. These considerations establish for both (actually, all three) beings that a "library" is a persistent thing, an object, in this case of the mutual exclusion variety (due to its being the component common to all the individual book mutexes).

Even theft and new acquisitions can be incorporated, in that both are non-conservative processes, and hence must belong in some larger conservative context which is to be discovered. Thus lack of conservation provides both a basis for and a framework in which curiosity, and theory-formation and testing, can be understood and realized. Furthermore, conservation laws supply the hitherto missing 'invisible hand' of control via which we

⁸We henceforth view deadlock as a degenerate form of mutex.

can realize the goal of truly distributed systems: globally coherent behavior based solely on local decisions.

Notice as well that the concept of "library" that is formed implicitly includes the activity surrounding it, in particular, the borrowers. They are a natural and integral part of the concept - independent and distributed though they be; this seems entirely correct, since a library without borrowers is meaningless. In general, the method of using resource invariants as the definition of objecthood implicitly includes a surrounding environment which gives the object 'meaning'.

Summarizing this section, the computational mechanism lying behind the basic constituents of the phase web - co-occurrence and mutual exclusion - is synchronization. The concept of *resource invariant*, which emerges from mutual synchronization relationships, provides a means by which the abstract concepts of 'object' and 'action' can be defined. Synchronization is thus the mechanism by which the otherwise quite ethereal phase web is 'grounded'. As Shakespeare wrote,

And as the imagination bodies forth
The form of things unknown, the poet's pen
Turns them to shapes, and gives to airy nothing
A local habitation and a name.

4 Co-Occurrence and Co-Exclusion

In this section we apply the concepts of synchronization, co-occurrence (local simultaneity), and mutual exclusion in an observational context, and conclude with the insight that mutual exclusion is the computational way to express 'action'.

The coin demonstration - Act I. A man stands in front of you with both hands behind his back, whilst you have one hand extended in front of you, palm up. You see the man move one hand from behind his back and place a coin on your palm. He then removes the coin with his hand and moves it back behind his back. After a brief pause, he again moves his hand from behind his back, places what appears to be an identical coin in your palm, and removes it again in the same way. He then asks you, "How many coins do I have?"

It is important at the outset to understand that the coins are *formally* identical: indistinguishable in every respect. If you are not happy with this, replace the coins with electrons or geometric points (or synchronization sticks!).

The indistinguishability of the coins now agreed, the most inclusive answer to the question is "One or more than one", an answer which exhausts the universe of possibilities given what you have seen, namely *at least* one coin. There being exactly two possibilities, the outcome can be encoded in one bit of information. Put slightly differently, when you learn the answer to the question, you will per force have received one bit of information.

The coin demonstration - Act II. The man now extends his hand again and you see that there are two coins in it. [The coins are of course identical.]

You now know that there are two coins, that is, *you have received one bit of information*. We have now arrived at the final act in our little drama.

The coin demonstration - Act III. The man now asks, "Where did that bit of information come from??"

Indeed, where *did* it come from?! Since the coins are indistinguishable, seeing them one at a time will never yield an answer to the question. Rather, *the bit originates in the simultaneous presence of the two coins*. We call such a confluence a *co-occurrence*. The emergent nature of the information conveyed by a co-occurrence should be apparent.

At this juncture, we hasten to mention that we are dealing here with *local* simultaneity, so there is no collision with relativity theory. Indeed, Feynman [Feyn65, p.63] argues from the basic principle of relativity of motion, and thence 'Einstein locality', that if *anything* is conserved, it must be conserved *locally*.

As an aside, an implicit claim of the Turing model of computation is that a single sequence of computational events can capture all essential aspects of a computation. In contrast, we see from the coin demonstration that there is information, *computational information*, available in the universe *which in principle cannot be obtained sequentially*. Thus we have in the coin demonstration a compelling argument that, at the very least, the Turing model of computation fails to capture all relevant aspects of computation: it is semantically incomplete, and the thing it ultimately lacks is *space-time* - space: co-occurrence, time: mutual exclusion. Synchronization operators represent precisely the way computations can express space-time relationships and give them semantic content.

If we replace the coins by synchronization sticks, which are surely indistinguishable ('inscrutable', perhaps, according to [Parker-Rhodes, 1981]'s classification), we can then say that the information received is indicative of the fact that two co-occurring states (represented by their sticks) do not mutually exclude each other. Furthermore, since (1) by the definition of simultaneity, the two states occur neither before nor after each other, and (2) sticks represent what might be called 'naked instants' of time, (3) we see that in the computational frame of the co-occurrence itself, *there is no time*. Physicists call such an entity a (Wheeler-Feynman) boson; we present additional evidence for the correctness of the identification of co-occurrences with bosons later. It is well known that photons (which are bosons) are used to construct space-time.

We will now sketch how to build a "theory structure" (for lack of a better term) from the information obtained from co-occurrences. Consider the following 'blocks world' scenario.

The blocks demonstration. Imagine two 'places', p and q , each of which can contain a single 'block'. Each of the places is equipped with a sensor, s_p respectively s_q , which can indicate the presence or absence of a block. The sensors are the only source of information about the state of the places.⁹ The two states of a given sensor s are mutually

⁹Cf. the discussion in §3, here the value of a sensor s is captured by the presence of a stick reflecting the current holding of the given state-value, rather than an actual 'bit'. On reflection the reader will see that this is an example of the recursiveness of the cycle hierarchy mentioned earlier. We also discuss this later.

exclusive, so a place is always either 'full', denoted (arbitrarily) by s , or 'empty', denoted by $\tilde{s}$. Suppose there is a block on p and none on q . This will allow us to observe the co-occurrence $\{s_p, \tilde{s}_q\}$. From this we learn that having a block on p does not exclude not having a block on q . Suppose at some other instant (either before or after the preceding) we observe the opposite, namely $\{\tilde{s}_p, s_q\}$. We now learn that not having a block on p does not exclude having a block on q . What can we conclude?

First, it is important to realize that although the story is built around the co-occurrences $\{s_p, \tilde{s}_q\}$ and $\{\tilde{s}_p, s_q\}$, everything we say below applies equally to the 'dual' pair of co-occurrences $\{s_p, s_q\}$ and $\{\tilde{s}_p, \tilde{s}_q\}$. After all, the designation of one of a sensor's two values as ' $\sim$ ' is entirely arbitrary. It is also important to realize that the 'places' and 'blocks' are story 'props': all we really have is two two-valued sensors reflecting otherwise unknown goings-on in the surrounding environment. These sensors constitute the *boundary* between us (or some other entity) and this environment.

Returning to the question posed, we know that s_p excludes $\tilde{s}_p$ and similarly s_q excludes $\tilde{s}_q$. Furthermore, we have observed the co-occurrence of s_p and $\tilde{s}_q$ and vice versa. Since the respective parts of one co-occurrence exclude their counterparts in the other co-occurrence (cf. first sentence), we can conclude that the co-occurrences *as wholes* exclude each other.

Take this now a step further. The transition $s_p \rightarrow \tilde{s}_p$ is indicative of some *action* in the environment, as is the reverse, $\tilde{s}_p \rightarrow s_p$. The same applies to s_q . Perceive the transitions $s_p \leftrightarrow \tilde{s}_p$ and $s_q \leftrightarrow \tilde{s}_q$ as two sequential computations, each of whose states consists of a single value-alternating bit of information. By assumption these two [sensors viewed as] computations are completely independent of each other. At the same time, the logic of the preceding paragraph allows us to infer the existence of a third computation, a third *compound* action, with the state transition $\{s_p, \tilde{s}_q\} \leftrightarrow \{\tilde{s}_p, s_q\}$. In effect, in combining in this way two single-bit computations to yield one two-bit computation, we have 'lifted' our conception of the actions performable by the environment to a new, higher, level of abstraction. This inference we call *co-exclusion*, and can be applied to co-occurrence pairs of any arity > 1 where at least two corresponding components have changed.

The next section describes a mathematical model for the development to this point. The section following then returns to the hierarchical issues broached in the preceding paragraph.

5 Mathematical Model

In this section we present briefly the results of our mathematical analysis of co-occurrence and co-exclusion. Most of the detailed underpinning is left out; the reader is referred to [Manthey, 1994] for this, and to [Hestenes, 1986] for a very readable introduction to the vector algebra we use. One foundational issue ought however to be mentioned: we assume that our use of a discrete rather than traditional continuous space, due (if only) to the modest demands placed on it, will not introduce any 'problems'.

5.1 States and Sensors

The main conceptual points in the mathematical transcription of our preceding universe of discourse - events and states - are

- Previously, a state s_i included a process' program counter plus its entire memory state including files etc. Here we are interested in processes with absolutely minimal state, in fact, a single bit. Larger processes/states are built up out of the states of constituent sub-processes. It follows that application of this formalism to practical problems will require computer support.
- The traditional concept of a process' state is God-like, and presumes that an entity's internal state can be known from the 'outside', as it were. Wishing to avoid the assumptions involved in allowing such a view, we look to express the concept of state as seen from *within* an entity, ie. the *local* view demanded by a distributed paradigm. This means that an entity's local state comes to be represented by *sensors* expressing both the state of the entity's otherwise unknown surround and its internal state. Whereas s_i before represented the literal state of some process after event e_i , now s_i represents the presence of a synchronization stick [ie. a Signal] advertising to that process' surround that literal state s_i currently obtains. [This is the way out of the 'seduction' problem mentioned earlier.] Stated a little more explicitly, given process a in state s_i^a in process b 's surround, this is viewed by process b as surround-sensor s_j^b being in state 1, and when process a is no longer in state s_i^a , process b 's sensor s_j^b will have the value $\tilde{1}$. From now on, the symbol s will carry this local, sensorial interpretation.
- The interpretation of $\tilde{1}$ is '-1'. To ease the interpretation of the symbolism, we write s when $s = 1$ and $\tilde{s}$ when the $s = -1$. Thus, when we write $s \rightarrow \tilde{s}$, we mean the value of s changed from '1' to '-1'; and $-\tilde{s} = s$. This is only a notational convention, however, and the algebra itself is independent of it. We note that the restriction to binary states implies no loss of generality.
- Finally, the values of sensors are usually treated as being scalar, but we wish to view them as vectors. This can be interpreted to mean that from the internal point of view, the surround is 'oriented', *vis a vis* the particular sensor, in the direction (1 or $\tilde{1}$) indicated by the value of that sensor. Put a little differently, a sensor s is a minimal two-state process that reflects the state of an otherwise unknown process in the surround; s 's state can only take on the two values, 1 and $\tilde{1}$, and the orientation of s indicates which 'way' the process it represents is 'heading'.

The entity experiencing the surround is thus, in the first approximation, represented by a vector space S with basis $\{s_1, s_2, \dots, s_n\}$. The sensors are all mutually perpendicular because, in principle, one sensor says nothing about another (and it is the job of the entity, via co-exclusion, to construct their correlations).

5.2 Co-Occurrence and Co-Exclusion

As mentioned earlier, the events constituting a co-occurrence are unordered by virtue of their occurring simultaneously. Shortening a long story, we interpret the operation '+' to mean 'co-occur', so that $s_1 + s_2$ means that s_1 and s_2 co-occur. The commutativity of '+' means that $s_1 + s_2 = s_2 + s_1$, that is, the states s_1, s_2 are unordered.

The expression $2s$ is interpreted via 'stone age binary' to mean $s + s$, that is, the co-occurrence of two distinct but otherwise indistinguishable s 's.

We interpret zero to mean "can/does not occur". Thus $s + \tilde{s} = 0$ means that s cannot simultaneously be in two different states, that is, the states s and $\tilde{s}$ exclude each other.

This is all that need be said about the algebraic representation of co-occurrence.

To talk about co-exclusion, we must first introduce the concept of a Clifford algebra, which is an algebra over a vector space with (a) $(s_i)^2 = \pm 1$ [for present purposes, we take $s_i^2 = +1$], and (b) the *product* of two basis vectors is *anti-commutative*, that is, $s_i s_j = -s_j s_i$ if $i \neq j$. This product is defined

$$s_i s_j = s_i \cdot s_j + s_i \wedge s_j$$

where the first term on the right is the 'dot' or 'scalar' or 'inner' product of the two basis vectors, and the second term is the so-called 'outer' product of same. The inner product can be described as the scalar magnitude of the projection ('shadow cast') on the one by the other, and is commutative. The magnitude of the outer product is the area of the surface spanned by the two vectors, and the *orientation* of this area is given by the direction in which one 'walks' its perimeter. Thus the outer product expresses the idea of an *oriented surface*, and this orientation can be understood as a directional axis perpendicular to this surface, oriented via the "right hand rule".

The import of this will become clearer as we proceed. What is important to understand right now is that given two dimensions s_1, s_2 , their product $s_1 s_2$ yields a *new* dimension with two possible orientations or directions, 'up' corresponding to $s_1 s_2$, and 'down' corresponding to $s_2 s_1$. We are now equipped to express co-exclusion.

The information from co-occurrence - that two sensory values namely do *not* exclude each other - is parlayed via the co-exclusion inference to mean: given

$$(s_1 + s_2) + (\tilde{s}_1 + \tilde{s}_2) = 0$$

or equivalently,

$$(s_1 + \tilde{s}_2) + (\tilde{s}_1 + s_2) = 0$$

it is entirely correct to draw the conclusion

$$s_1 s_2 + s_2 s_1 = 0$$

The first two equations express the two possible sensory configurations that co-exclude. The third equation expresses the object/action that can be inferred given one of the first

two.¹⁰

Notice that the two bits of information contained in the two co-occurrences (say) $s_1 + \tilde{s}_2$ and $\tilde{s}_1 + s_2$ reappear in encoded form in the orientation of $s_1 s_2$.¹¹

The co-exclusion inference can be derived via the boundary operator ' ∂ ' of differential topology. Taking ' $\hat{s}$ ' to mean 'left out', it is defined

$$\partial(s_{i_1}, s_{i_2}, \dots, s_{i_k}) = \sum_{r=1}^k (-1)^{r+1} s_{i_1} \dots s_{i_{r-1}} \hat{s}_{i_r} s_{i_{r+1}} \dots s_{i_k}, k > 1$$

and for $k = 1$, $\partial(s_i) = 1$ (rather than 0 - arbitrarily chosen - as in the usual theory). Then

$$\partial(a + b) = \partial a + \partial b$$

$$\partial(\partial a) = 0$$

and

$$\partial(ab) = (\partial a)b + (-1)^o a(\partial b)$$

where o is the order of a (ie. if a is a p -simplex, $o = p + 1$).

As a simple example of the boundary operator, consider an ordinary triangle ABC , where we specify it in terms of its vertices A, B, C and its edges are thus AB, BC, CA . Then

$$\partial(ABC) = BC - AC + AB$$

Since specifying the triangle's edges in terms of the vertices means that edge AC is oriented oppositely to edge CA , we can rewrite the above as $AB + BC + CA$, which is indeed the boundary of the triangle ABC .

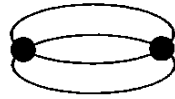
To make the connection between topological boundaries and our present discourse universe of processes and synchronization clearer, we can draw a directed graph of the synchronization relationships between symmetrically mutually excluding processes, of which Figure 2 is the simplest. The next case, for three processes all mutually excluding each other, is the octahedron shown in Figure 3. The boundary operator corresponds to removing one of the octahedron's 'great circles' together with its nodes, yielding as expected the top 2-ary 'polyhedron'. Similarly, removing any one of the great circles of the bottom polyhedron yields a tetrahedron.

We now ask the question, "What is the boundary of $s_1 s_2$ ", or rather, for reasons which will become quickly apparent, "What is $\partial(s_1 s_2 = -s_2 s_1)$?" We know that $s_1 s_2 + s_2 s_1 = 0$, and working out ∂ of this we find

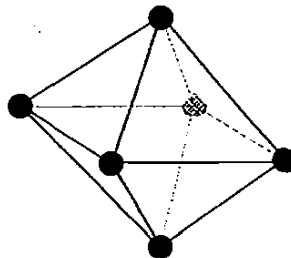
$$s_2 - s_1 + s_1 - s_2 = 0$$

¹⁰The algebra does not distinguish the two cases, which are duals, although the surround may only allow the one and not the other, hence the importance of actually observing, ie. sensory experience.

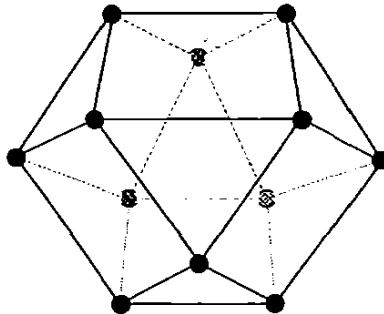
¹¹If we were in an exclusive-or algebra (which we are most definitely *not*), co-exclusion looks like the transition from two the bit-strings 01 and 10 to 01|10 = 11. Cf. Bastin and Kilmister's article in this issue.



arity 2



arity 3



arity 4

Figure 3: Actions also have a topological structure - a family of 4-vertexed polyhedra that approach a 3-D sphere as the arity increases. The vertices represent Wait/Signal interactions between two processes in the order Wait, Wait, ... Signal, Signal, ... around a given 'great circle'; the corresponding orientation of the arcs is suppressed. One is reminded of the Greeks' and Kepler's intuitions.

Re-writing this in terms of our notational convention, this becomes

$$s_2 + \tilde{s}_1 + s_1 + \tilde{s}_2 = 0$$

and, given that $+$ is commutative, we can rearrange this in two different but equivalent ways:

$$(s_1 + \tilde{s}_2) + (\tilde{s}_1 + s_2) = 0$$

and

$$(s_1 + s_2) + (\tilde{s}_1 + \tilde{s}_2) = 0$$

which is to say that $(s_1 + \tilde{s}_2)$ may not co-occur with $(\tilde{s}_1 + s_2)$, nor may $(s_1 + s_2)$ co-occur with $(\tilde{s}_1 + \tilde{s}_2)$. Notice that these are the initial premises (both forms!) of the co-exclusion inference.

Now it is obvious that if $\partial b = 0$ then there is an element a such that $b = \partial a$. For, if not, this would give b as a 'cycle', corresponding to a 'hole' in the space (non-trivial homology) whereas the Clifford algebra has trivial homology. So, running the derivation in the opposite direction¹², we can conclude that *the fact that a co-occurrence and its complement cannot co-occur means that the product of the component vectors is a Clifford product. In other words, the co-exclusion principle is really a statement about 'integrating' change to derive a whole, and the 'action' performed by such a whole is to change the orientation of its boundary!* All this generalizes to co-exclusion inferences of arbitrary arity, and thus gives the original intuition a firm formal basis.

We can interpret this result further. Think back to the fact that the computational foundation of co-exclusion is mutual exclusion, and that when two processes mutually exclude each other there exists a resource invariant. The resource invariant expresses the fact that the synchronization 'stick' must move on a closed orbit, which orbit corresponds to the boundary expressed by the operator ∂ . The polyhedra of Figure 3 show the form of this boundary. The fact that the resource invariant is indeed *invariant* is captured by the fact that $\partial(\partial(a)) = 0$, or as Wheeler put it, "The boundary of the boundary is zero".

5.3 Modelling 'Action'

The last mathematical aspect we wish to present here is how to actually capture the change of state that is associated with an 'action'. From the above, we saw that actions, via the co-exclusion inference, take the form of a product, eg. $s_1 s_2$. We seek a linear transformation which preserves these products, which means we seek an automorphism. If the space contains an even number of elements, then the required automorphism is *inner*, that is, of the form

$$q \rightarrow q' = a q a^{-1}$$

¹²The inverse ∂^{-1} is not unique, which means that additional inferences are possible: $\pm k_a s_1 s_2 + k_b (\sum \text{other } s_i \text{'s, such that } \partial \Sigma = 0) + k_c$. The $k_a \neq \pm 1$ term is uninteresting, and the k_c and k_b terms merely state the possibility that a larger context is necessary to actually complete the definition of the action. As it is, we know the basic inverse, and this is enough.

[In general, Clifford algebras exhibit various quirks when the basis contains an odd number of elements. Regarding the above automorphism, for *odd* algebras an automorphism is either inner, or it is the composition of an inner automorphism with $i \rightarrow -i$ (complex conjugation).]

Applying this automorphism to 2-actions, and recalling that $(s_1 s_2)^{-1} = s_2 s_1$, we find that

$$s_1 s_2 (s_1 + s_2) s_2 s_1 = \tilde{s}_1 + \tilde{s}_2$$

which is exactly what an action does. We can as well interpret the parenthesized quantity on the left as the current state which is operated upon by the action $s_1 s_2$. Similarly, for 3- and 4-ary actions, we get

$$-s_1 s_2 s_3 (s_1 + s_2 + s_3) s_3 s_2 s_1 = \tilde{s}_1 + \tilde{s}_2 + \tilde{s}_3$$

where the minus sign reflects the effect of the complex conjugation mentioned above, and

$$s_1 s_2 s_3 s_4 (s_1 + s_2 + s_3 + s_4) s_4 s_3 s_2 s_1 = \tilde{s}_1 + \tilde{s}_2 + \tilde{s}_3 + \tilde{s}_4$$

Finally, this apparatus leads to a composition rule for actions, where by ‘composition’ we mean that one action acting on the result of another can be viewed as a single combined action.

Suppose we have the actions $s_1 + s_2 \rightsquigarrow \tilde{s}_1 + \tilde{s}_2$ and $\tilde{s}_2 + s_3 \rightsquigarrow s_2 + \tilde{s}_3$, where it is the $\tilde{s}_2$ in the second action that expresses that it builds on the result of the first, and which therefore allows it to be composed with it. From above we know

$$s_1 s_3 (s_1 + s_3) s_3 s_1 = \tilde{s}_1 + \tilde{s}_3$$

We can rewrite the righthand side as

$$s_1 s_3 (s_1 + s_3) s_3 s_1 = \tilde{s}_1 + (\tilde{s}_2 + s_2) + \tilde{s}_3$$

since $\tilde{s}_i + s_i = 0$. In terms of our interpretation, this substitution means that, since the two valuations of s_2 cannot co-occur, they must occur in some order or other. We choose the order

$$s_1 s_3 (s_1 + s_3) s_3 s_1 = (\tilde{s}_1 + \tilde{s}_2) + (s_2 + \tilde{s}_3)$$

in that we substitute with the rightmost two terms’ inner automorphisms, yielding

$$s_1 s_3 (s_1 + s_3) s_3 s_1 = s_1 s_2 (s_1 + s_2) s_2 s_1 + s_2 s_3 (\tilde{s}_2 + s_3) s_3 s_2$$

which is the desired composition rule. This generalizes easily to actions of greater arity.

We end our mathematical exposition here. As stated at the beginning of this section, our presentation has not been at all rigorous, but is rather intended to give the overall flavor of the mathematical model we have found. We are still in the process of putting it together, and several important aspects of the phase web remain to be captured. Nevertheless, we are confident we are on the right track.

6 The Cycle Hierarchy

We now examine the hierarchical structure induced by the co-exclusion inference. The usual, and generally *tacitly* assumed, hierarchy relation in computing and much mathematics, is function composition, $f \circ g = f(g)$, where the idea is that the 'result' of g is to be 'composed' with, or 'put into', f . Function composition captures for example the idea of a 'sub-routine' very neatly, and an entire sequential computation E can be viewed as $e_{stop}(e_{n-1}(\dots(e_{start}))\dots)$. Notice however that $\circ$ implicitly imposes an *order* on the computations represented by f and g : first do g , then do f . This basic property of $\circ$ means that any consideration of co-occurrence is immediately impossible.

Furthermore, the values we are receiving from the sensors qua *values* are entirely predictable and unsurprising, in that 1 is always followed by $\bar{1}$ and vice versa. What is unpredictable and surprising is *when* they occur, and this relative to each other. Hence, in essence what we are doing is interpreting a sensor's value as a synchronization stick indicating when the sensor is in a particular state. If we were to program a computer to observe co-occurrences, apply co-exclusion to them, and model phase web execution therewith (which we have done), the code will consist entirely of *wait* and *signal* operations. Neither of these operations can be meaningfully viewed as 'functions' in the domain of the states they order.

The conclusion we draw from the preceding two paragraphs is that the hierarchical transition effected by co-exclusion cannot be described in terms of a standard function composition hierarchy. The latter is implicitly and hopelessly sequential in its conception; the arguments to co-exclusion are co-occurrences, which cannot be captured sequentially at all; and the computational mechanism describing what's going on (synchronization) is not even functional in character. Rather, instead of the function - sub-function relationship, co-exclusion captures the part-whole relationship, and this in a pure and distributed process-oriented context.

This part-whole hierarchy can - via interpreting resource invariants as stick movement on closed paths - be viewed as a hyper-cyclic hierarchy. A function composition hierarchy has difficulty answering questions like, "But what is a quark made out of?". In our view, the problem here is that such things as quarks are being viewed in terms of what they 'do', their 'doing-ness'. In contrast, the cycle hierarchy is founded on co-occurrence, whose timeless 'is-ness' effectively grounds such questions by referring to a clearly defined sensory (ie. informational) boundary. In this way, the door is kept open to considering, as Leibniz intuited, that everything is ultimately defined by the presence of everything else.

In terms of the mathematical description in the last section, every action $s_1 s_2 \dots s_k$ represents a resource invariant. Hence a rough indicator of hierarchical structure is the arity k of such actions together with the observation that the action $s_1 s_2 \dots s_k$ could be viewed as the composition of (say) $s_1 s_2$ and $s_3 \dots s_k$. However, this view can only produce a finite number of combinations. Hence the cycle hierarchy will 'close' after only a single level transition.

To see how to break out of this limitation, recall that the every action represents an oriented k -dimensional surface, which orientation can be represented in terms of an axis perpendicular to all the s_i which constitute it. This orientation, which we view as an emergent property of the given s -ensemble, can be mapped to $\{1, \tilde{1}\}$ and represented by a *new* sensor vector $s_{k+1} = s_{s_1 s_2 \dots s_k}$. Given two or more such vectors, we can co-exclude them to form *meta-actions*! Since co-exclusion represents a mapping to a *new* space - that of ∂^{-1} - we gain an entirely new level, that of abstractions representing the relationships between actions (and the objects they reflect) *as wholes*.

As an example, suppose we have inferred two 2-actions, $s_a s_b$ and $s_c s_d$, which move two blocks, respectively between places a and b and c and d . If there were only a single hand available to move the blocks, then the *meta-action* $s_{ab s_{cd}}$ would represent the fact that the move between a and b excludes the move between c and d - the one must occur before or after the other. This fact is only implicitly present at the level of the original two actions, and the meta-action makes it explicit. Hence the cycle hierarchy can be viewed as a hierarchy of *distinctions* and meta-distinctions based throughout on co-occurrence vs. mutual exclusion.

The table below shows the successive distinctions that can be expressed in the first few levels of this *Combinatorial Hierarchy*, where ' $\mapsto$ ' means 'maps to'.

level 0		level 1		level 2		level 3
0	$\mapsto$	0	$\mapsto$	0	$\mapsto$	0
1	$\mapsto$	1	$\mapsto$	1	$\mapsto$	1
$\tilde{1}$	$\mapsto$	$\tilde{1}$	$\mapsto$	$\tilde{1}$	$\mapsto$	$\tilde{1}$
		$\{1, \tilde{1}, 1 \tilde{1}\}$	$\mapsto$	s	$\mapsto$	s_1
				$\{1, s, 1 s\}$	$\mapsto$	t
				$\{\tilde{1}, s, \tilde{1} s\}$	$\mapsto$	u
				$\{1, \tilde{1}, 1 \tilde{1}\}$	$\mapsto$	s_2
				$\{1, \tilde{1}, s, \{1, s, 1 s\}, \{\tilde{1}, s, \tilde{1} s\}, \{1, \tilde{1}, 1 \tilde{1}\}\}$	$\mapsto$	$s_1 s_2$
				$\{1, s, 1 s\} \{\tilde{1}, s, \tilde{1} s\} \{1, \tilde{1}, 1 \tilde{1}\}$		
						...

This is the same hierarchy as that discussed by [Bastin and Kilmister, 1995], though the above is in terms of products in a Clifford algebras rather than their exclusive-or algebra. Or rather, we believe the two versions to be isomorphic but as yet have no proof of this. Certainly they have some remarkable differences: the above lacks the 'cut-off' after four levels but expresses the necessary anti-commutativity very early, whereas the opposite is the case of the exclusive-or version. *Much* can be said about this hierarchy and its conceptual underpinning. The only points we wish to make here are:

- As is apparent from the sensory basis $\{s_1, s_2, \dots, s_n\}$ accorded a given entity, the hierarchy constructed by it - that is, the understanding of the world it develops in the course of its experience - will in general be unique. [The issue of whether it is ultimately subjective will be discussed later.] However, since the hierarchy

is constructed on the basis of genuine *facts* about the world - what can co-occur, we believe that the hierarchies generated by two or more entities experiencing the same world will be *consistent* with each other. Indeed, modulo the limitations of relativity, we believe that they would ultimately converge.

- One is tempted to conclude that the height of the hierarchy, and hence the amount of understanding that an entity can gain of its world, is ultimately limited by the finiteness of the basis sensory set $\{s_1, s_2, \dots, s_n\}$. The argument is basically that n vectors can only generate $\sum \binom{n}{i} = O(2^n)$ distinct actions. However, as demonstrated by the character of ∂^{-1} and shown in the table above, the opposite is the case: emergent distinctions can be meaningfully combined with those from which they are derived to generate new distinctions, which themselves can be similarly combined, etc. Hence the cycle hierarchy is unbounded.

7 Interpretation for Physics

In this section we will sketch the parallels between physics and the computational structures that appear in the phase web. The language will be that of the mathematics we presented earlier, but the reader should also bring the understanding emanating from the underlying computational mechanism to bear, as these complement the mathematical picture.

The parallels we will present are necessarily speculative, given the current level of development of our mathematical description, but we nevertheless believe them to be well founded. A principal reason for our optimism is that, as Hestenes has argued for some time, Clifford algebras are the natural mathematical language for modern physics, encompassing linear algebra, differential forms, tensors, etc. For example, the quaternion, Pauli, Dirac algebras and the Lorentz group all find natural expression in its terms.

Spin, fermions, and bosons. We identify the fact that $(s_1 s_2)^2 = (s_1 s_2 s_3)^2 = -1$ with the quantum mechanical attribute called 'spin one-half'. The way this anti-commutativity is connected with the orientation of $s_1 s_2$, together with the fact that the transition $s_1 + s_2 \rightarrow s_1 + \tilde{s}_2$ both changes the orientation of $s_1 s_2$ and the state of the system, yields the PT part of CPT invariance automatically. We identify therefore co-exclusion-based actions generally as fermions, even though (eg.) 4- and 5-actions have square +1.

Similarly, we identify the fact that $\frac{1}{n}(s_1 + s_2 + \dots + s_n)^2 = 1$ with spin one, that is, co-occurrences are bosons. It follows that co-occurrence implicitly expresses wave-like properties (although we will not argue the point here). We conjecture that the simplest of these, 2-co-occurrences, models photons and vector-bosons, and that 3-co-occurrences model gluons. We speculate that a 'graviton' is a meta-co-occurrence over ≥ 4 -actions.

Particles & anti-particles. [Man92] describes how the computational concept of a resource invariant can be interpreted as a criterion for objecthood. In particular, it implicitly advances the idea that a fermion should be viewed as a mutual exclusion

structure, such as is represented by co-exclusion-based actions. It would seem to follow that the particle - anti-particle relation is that of s_1s_2 to s_2s_1 , in that both the orientation and direction of change are reversed. In general, the present model implies that what are called 'particle' and 'anti-particle' are two states of the 'same' object that are 180° out of phase. It follows that a primitive 2-action, being the simplest action, should be identified with a neutrino. Modelling the neutrino - anti-neutrino interaction (perhaps naively) as the co-occurrence $s_1s_2(s_1 + s_2)s_2s_1 + s_2s_1(\tilde{s}_1 + \tilde{s}_2)s_1s_2$ yields $(s_1 + s_2) + (\tilde{s}_1 + \tilde{s}_2) = \gamma^{LL} + \gamma^{RR}$, which can be interpreted as the two neutral vector bosons.

We speculate that electrons are meta-2-actions, in that their additional properties require corresponding additional information-carrying capacity. A general property of the model is that 'generations' of particle 'families' arise via identical arities, but composed from different co- and/or meta-co-occurrences. Greater mass/energy is presumably indicative of greater information-carrying capacity.

Quarks. See [Manthey, 1994].

Quaternions. Quaternions are a group of three operators that express rotation in three dimensions. Their defining properties are: $ab = -ba$, $bc = -cb$, $ca = -ac$; $ab = c$, $bc = a$, $ca = b$; $(ab)^2 = (bc)^2 = (ca)^2 = -1$. They appear as the boundary of the 3-action abc , ie. $\partial(abc) = bc - ac + bc$. Hence the meta-3-action created by co-excluding $a = s_1s_2$, $b = s_2s_3$, $c = s_3s_1$ implicitly contains, as it were, the ability for the corresponding 3-object to 'understand' 3-dimensional space from its own point of view. Full-blown 3+1 dimensional space time will presumably require four tetrahedrally-related such 3-objects to appear.

Causality. The action-composition rule given earlier can be viewed as providing a model of causality, ie. the allowable sequence(s) in which actions can be performed.

Formal Structure. Although we have only hinted at it, it appears quite certain that the basic overall mathematical structure of the theory we have presented is that described by homology theory. Roth's 'twisted isomorphism' [Roth, 1955] [Bowden, 1982] between homology and co-homology seems particularly worth further investigation, and we comment further on this in the next section.

Closer to what we have presented is the matter of the *commutator* product: $A \times B = [A, B] = \frac{1}{2}(AB - BA)$, where A, B are arbitrary multi-vectors. One of the hallmarks of quantum-mechanical behavior is $[A, B] \neq 0$, which is true of both 2- and 3-actions. More interestingly, Dyson [1990] presents a derivation due to Feynman (1948) of Maxwell's laws from Newton's laws plus the commutation relations between position and velocity of a single non-relativistic particle. Tanimura [1992] generalizes this derivation to encompass both special and general relativistic contexts. According to Tanimura, besides Newton's laws as appropriate, all the derivations appeal only to the *form* of the commutator product, and are completely independent of the interpretation placed on its components. In this connection, it is interesting that Kauffman [1987] shows that one needs only parenthesis-operations and component-ordering to arrive at both spinors and the Lorentz group.

It appears that the main thing separating us from a ∂ -based expression of Maxwell's

laws is that we must show that Newton's laws apply. This depends on properties which we expect to first emerge with the appearance of ($\geq$)4-meta-actions, namely 'ordinary' space-time.

Space and Time. The availability of these latter concepts rests on one of the more radical implications of the model being presented here, namely the fact that the usually primitive concepts of 'ordinary time' and 3-dimensional space are neither given nor assumed a priori, but rather *emerge* as the hierarchical structure expands. The reason why this is so, simply stated, is that to give these concepts semantic content requires a context with sufficient information-carrying capacity to express the required distinctions. Such capacity accrues solely via (cycle-)hierarchical aggregation. The same reasoning applies to charge and mass: they are emergent attributes and hence will appear in due course as the information-carrying capacity of the constantly growing structure evolves.

Summarizing this section, we believe we have a case for the position that the computational structure we have described captures the essential aspects of quantum mechanical systems. Indeed, we believe it represents the (neo-)mechanistic model of QM that has hitherto been lacking, and therewith that this structure in execution will be able to provide a blow-by-blow account of Feynman's sum-over-paths formulation. It also seems reasonable to believe that relativistic considerations are accommodated.

8 Up the Hierarchy

In this section we discuss how the phase web, with its cycle-hierarchical structure, can reasonably be seen to be a candidate for a 'theory of everything'. Indeed, there is no choice here: such a candidacy is a necessary consequence (and burden) of a hierarchical theory. Were the cycle hierarchy strongly reductionistic in character, we could be very brief, since we could then argue that having captured the particle level (cf. preceding section), larger scale structures are simply combinations thereof. Since it is not reductionistic in this way, there is rather more that can be said. Much of the following is necessarily conjectural, but this seems permissible under the circumstances.

Not at all conjectural are the following observations of the differences between function-composition and cycle hierarchies. Focusing first on the former, it can be construed in two ways: (i) nodes of lower levels are composed to create a node at a higher level, and (ii) vice versa. These correspond respectively to top-down control, viewing the environment as a (passive) state to be changed, and bottom-up control exercised by the environment, forcing the entity to change *its* state. Alternatively, one can say that a leaf-node (ie. where actual change occurs) in (i) lies in the environment, whereas the leaf-node lies in the entity in (ii). 'Control' is the only thing that can be expressed, since functions can only 'act', and not simply 'be'. As pointed out earlier, such functional control is inherently conceptually sequential (although this does not rule out ordinary non-interacting parallelism). Furthermore, the two regimes oppose their co-existence (try for example expressing this in a function-based language). By its nature,

the 'top'-nodes so created express a centralized 'single-cause' view of change.

In addition, as one proceeds 'up' a function-composition hierarchy, information is discarded, in that only 'critical' parameters are abstracted and promoted upwards. In the last analysis, it is precisely the co-occurrence-derived information that is lost (cf. Coin Demo). Thus, the top-most node (in the entity-controlling-the-environment case, (i)) is informationally remote from entity's sensory fringe.

In contrast, as one goes 'up' the cycle hierarchy from the sensory fringe, the scope of co-occurrence *expands*: the higher up, the *more* a given node expresses, the *further out* into the environment the co-occurrence relationships it encodes reach. The process of going up the cycle hierarchy can be compared with Huygen's principle: that around a given primary radiating source can be placed an arbitrary boundary containing secondary radiators such that from the outside, one cannot distinguish between the two situations. The succession of upward-going levels corresponds to successive such boundaries, with the original primary radiator corresponding to the 'top'-node representing the current environmental situation as a whole.

This concludes our comments on the differences between the two hierarchical regimes, and we turn now to the larger picture.

Some hints of the structure produced by the cycle-hierarchical build-up have been broached earlier: roughly, photons and neutrinos, then leptons and quarks, then hadrons, with a concomitant production of sufficient information-carrying structure to express chirality, charge, parity, electromagnetism, mass and 3+1 space-time, and gravity. This build-up falls over circa four levels. Conjectural as this all is, we will not embroider it further.

Building again emergently on this, the next step is nuclear and atomic structure, followed by the corresponding objects' mutual exclusion in space, simple and co-valent bonding, leading to molecules. This probably requires another $2 \times$ four or so levels.

The next, molecular, level gives rise to polar and chiral interactions and, concomitantly, catalytic structures. We are now at the levels described by [Eigen and Schuster, 1979], which include the capacity for reproduction with heritable characters, and hence natural selection and evolutionary capacity. The most important emergent property here is clearly 'life', and we believe that the phase web satisfies the demands of [Rosen, 1991] for its description.

The next expansion of complexity produces simple cellular structures. This is followed, echoing the cooperative nature of catalytic structures, by eukaryotic cells, and thence simple multi-cellular organisms. By now, if not before, it becomes increasingly difficult to predict how many levels this development requires, but presumably fewer than ten. In addition, the levels are by now very broad, permitting numerous variations on the available themes.

The pattern should by now be clear: next are simple societies of multi-cellular organisms leading eventually to control (eg. hormonal and nervous) systems and thence organ-based entities. Then societies of these. By this stage the complexity, both of structure and interaction, is simply enormous. The logarithmic nature of hierarchy is the

only brake on the accompanying conceptual explosion.

At some point in the preceding, the entity-controlling nervous systems formed the basis for a new twist in the hierarchical expansion, producing increasingly sophisticated cognitive functions. Consciousness - which we will not attempt to define - is the most interesting development here. At the current top of this expansion lie *homo sapiens* and perhaps the other great apes, dolphins, and whales. We will return to this in a moment.

Temporarily ignoring this orthogonal, nervous twist, we find the complexity progression: planets, solar systems, galaxies, super-galactic structures, and the physical universe as a whole. One can now - given that we have a paradigm which by its nature encompasses all structure, activity, and distinction - debate in rather more precise terms than have heretofore been available, whether and/or to what extent the hierarchy at the level of the physical universe encompasses its other prong, human cognitive and spiritual development. In a reductionistic model, the answer is clear, whereas in an emergent one such as ours, the answer is less so. For example, the expansion of the physical universe - viewed as an information structure - may contain as one of its aspects the expansion of consciousness of itself.

While the above 'story' is open to any number of modifications and detailed criticisms, its abstract structure turns out to be well-known, and lends, in its own way, credibility to the overall progression. This abstract structure is that of what are called chain-complexes (cf. [Bowden, 1996]), which in our case would be

$$\dots \xrightarrow{\partial} \text{meta}^2\text{-actions} \xrightarrow{\partial} \text{meta}^1\text{-actions} \xrightarrow{\partial} \text{actions} \xrightarrow{\partial} \text{co-occurrences}$$

which is called an homology, *or equivalently*

$$\text{co-occurrences} \xrightarrow{\delta} \text{actions} \xrightarrow{\delta} \text{meta}^1\text{-actions} \xrightarrow{\delta} \text{meta}^2\text{-actions} \xrightarrow{\delta} \dots$$

which is called a cohomology. As illustrated in Figure 4, the two chains run parallel to each other in opposing directions, and, surprisingly and profoundly, the mathematical entities at opposite ends of any given rung of the ladder are isomorphic to each other, ie. have *exactly the same underlying structure*, their vastly differing semantics notwithstanding. In our particular case, the homology is that of the trickling down of goals, and the cohomology is that of the bubbling up of sensory impressions. Which is to say that, in a deep sense, change and co-occurrence are two sides of the same coin.

However, this coin has a truly unique structure, in that the isomorphism that connects the two sides is 'twisted': the 'wholes' in the domain inhabiting one end of a ladder rung correspond to the 'zero' of the domain inhabiting the other end. Intuitively, this can be understood from our earlier statement about thinking of co-occurrence as constructive interference (ie. non-zero) and mutual exclusion (= action) as destructive interference (ie. zero); and oppositely, an action has an effect (ie. non-zero) whereas a co-occurrence has none (ie. zero). These identifications will be made explicit in a forthcoming paper.

Based on these observations, we conjecture the following:

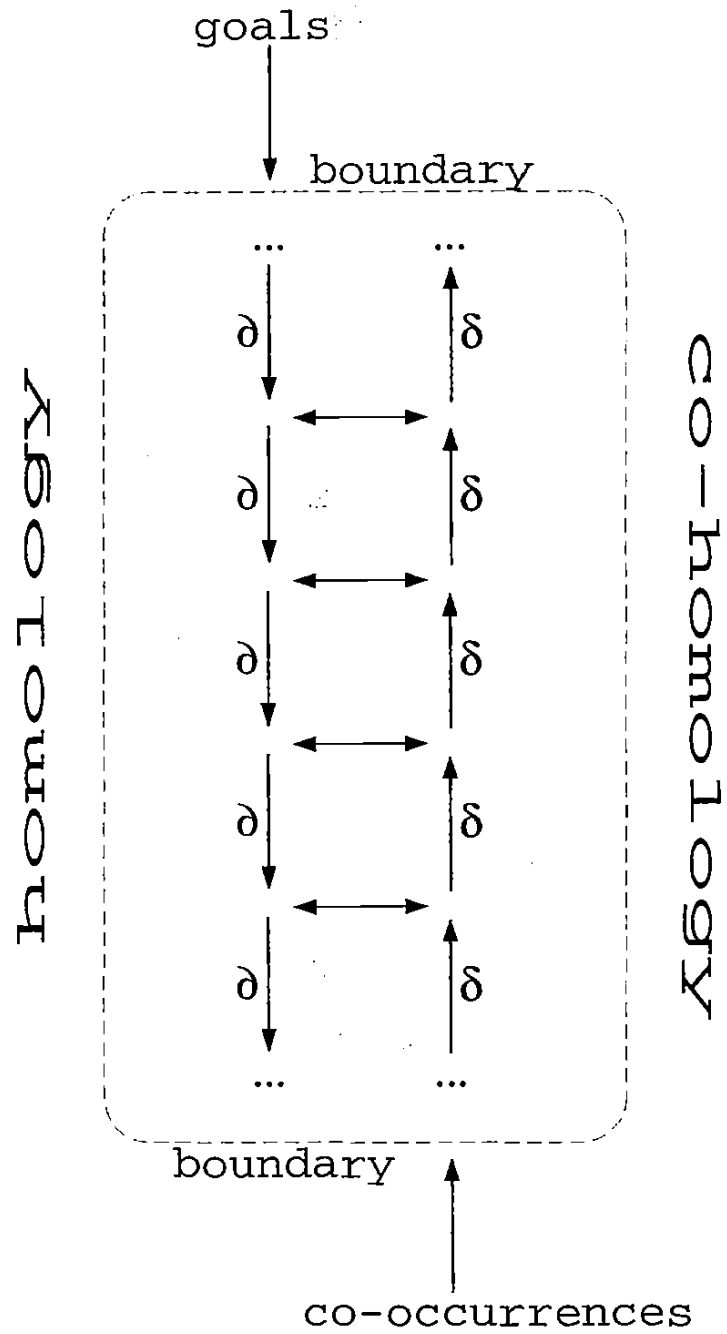


Figure 4: The Homology - Cohomology Ladder Embedded in Its Surround.

- The origin of wave-particle duality, and hence quantum mechanics' uncertainty principle and Planck's constant, is to be found in the twisted isomorphism;
- In a concrete realization of the paradigm, both sides of the ladder exist concurrently as computational activities. Furthermore, there exist explicit computational entities that have the same "meta" relationship to these goings on as synchronization sticks have to the states underlying the ∂/δ streams. Co-excluding the meta-entities, which corresponds to inferring actions that connect the two corresponding sides of the ladder and thus express the twisted isomorphism itself, constitutes yet another orthogonal, conceptual 'breaking out', this time into an *intentional* space. This is the 'twist' mentioned earlier in this section (which may indeed have occurred 'earlier'). This new space will, since its co-occurrences and actions are themselves expressible in a Clifford algebra, itself have the same ladder structure, and hence its own twisted isomorphism. Etc. Thus the paradigm apparently includes, along with everything else we have described, an unbounded, emergent hierarchy of intentional choice. We speculate that the origins of consciousness and free will are to be found here.

Finally, one might ask if there is anything that is *in principle* beyond the reach of this hyper-exponentially expanding, multitudinously branching, multi-dimensionally unbounded hierarchical structure. The answer is, apparently, yes: Leibnitz' intuition that everything is defined by the presence of everything else applies to everything within what we have called the boundary, which we have seen is roomy indeed. But this begs the question of what is on the other side of the ultimate boundary. The origin of the entire combinatorial hierarchy invokes the existence of true indistinguishables, whose only property is existence, and compared to which synchronization sticks are vastly concrete. These indistinguishables inhabit the other side of the boundary, a place Parker-Rhodes called 'the inchoate plane', a plane which is forever, and forever beyond our reach. Interpret this as you will.

Tell all the Truth but tell it slant –
 Success in Circuit lies
 Too bright for our infirm Delight
 The Truth's superb surprise

As lightning to the Children eased
 With explanation kind
 The Truth must dazzle gradually
 Or every man be blind –

Emily Dickinson, ca. 1868
 Johnson[1960]

9 Summary and Conclusions

We have outlined how to construct a physical theory based on the computational concept of synchronization. Synchronization is more than a concept however: it is a *mechanism*, an informational mechanism. As such, we are proposing a mechanistic theory, though since neither this mechanism nor what which it elicits (namely information) are material, it is more correct to term the proposal *neo-mechanistic*. Since the point of view taken by our proposal is novel, we have given it a name: the phase web paradigm.

With synchronization we can define both simultaneity (co-occurrence) and exclusion. The Coin Demonstration showed us that there is information to be gained from co-occurrence that is in principle not available from sequential computations. This means that we leave both Newton's and Turing's worlds, both of which are founded on total event orderings, behind us. In terms of an 'information budget', it is the information from co-occurrence that is used - by co-exclusion - to erect the hierarchical structures characteristic of the paradigm. On the other hand, the emergent nature of the information from co-occurrence implies that this budget is unbounded.

From a philosophical point of view, it is worth noting that co-occurrence deals with indistinguishables, whereas exclusion - the negation of co-occurrence - deals with opposites. Co-exclusion juxtaposes the two - a conceptual co-occurrence of opposites - producing unification from paradox.

The mathematical framework provided by Clifford algebras was shown to fit co-occurrence and co-exclusion nicely indeed. Given that the goal is to demonstrate a *physical* theory, it is therefore very comforting to know that these algebras represent a most cogent way to express the very mathematics that is used by contemporary physical theories, theories deriving from entirely different premises.

Co-exclusion itself represents a demonstration of the neatness of the fit: it is expressed as the simplest Clifford product, $s_1 s_2$, and simultaneously contains both the complex plane (since $(s_1 s_2)^2 = -1$) and chirality. These in turn lead to the arch-quantum-mechanical phenomenon of spin $\frac{1}{2}$. Other properties of the algebraic formulation of the various computational entities inhabiting the paradigm lead to similarly reasonable physical interpretations. This encourages us both that we are not merely transforming airy nothing to airey nothinge (to quote the Bard once again), and to continue development of the paradigm.

The hierarchical nature of the theory is something of a novelty. First, it points out that one must be quite explicit about exactly which hierarchy relation one is using, the invisibility of emergent phenomena in the (usually tacitly assumed) function composition hierarchy being a case in point. Second, since the same hierarchy-constructing tools - here co-occurrence and co-exclusion - are available by definition at every level, the phenomena these tools capture are necessarily level-independent. Thus a hierarchical theory must also assume the burden of being a theory of everything within its given purview. Since the cycle hierarchy is based on the primitive notion of 'same or different' in the time domain - that is, co-occurring or excluding - the phase web casts a very broad (and dizzying) net.

We have therefore, in the interests of a full exposition, attempted to sketch how we *think* this IOU might be redeemed. We hope that the reader will accept the speculations of the last section in this spirit.

Acknowledgements.

The complex of ideas presented here derives from many sources. Leibniz' monadology seems to be the Western origin of the idea of indistinguishables and of deriving the concept of space from their co-occurrence. Similarly, Hamilton's discovery of quaternions, not to mention Grassman's and Clifford's related contributions, mark the emergence of the ideas surrounding and delineating co-exclusion. Finally, as Bohr noted, the yin-yang symbol expresses the same basic notions in a most compact form.

Most of these ideas I came to know via my contact with the Alternative Natural Philosophy Association and its members, in particular that of indistinguishability. In this connection, I am grateful to H. Pierre Noyes for sharing over many years his knowledge of physics; to Clive Kilmister for similar tutoring in the mathematics displayed here; and Keith Bowden, for telling me about the twisted isomorphism. But also to the entire ANPA membership for their critical ear and broad expertise. It was an article by Eddington [1956], a posthumous honorary member of ANPA, that put me onto Clifford algebras. I also owe an early debt to C.A. Petri for encouragement in the notion that computation *can* be used to build a physical theory.

Related ideas, which I for the most part became aware of after the fact, but nevertheless have found stimulating, can be found in the work of Kauffman [1987], Shoup [1994], Oshins et al [1992], Pope and Osbourne [1992], Rosen [1991], Stern [1988], and Spencer-Brown [1979].

A great debt is also owed to the students who over the years have helped me, via their energy and hard work, to develop the ideas presented here. In particular, Carsten Boe Jensen and Peter Frandsen, who were there 'all the way' when co-occurrence and co-exclusion suddenly appeared and issued their challenge; Morton Irskov and Peter Kroghsgaard Jensen, who with insight and professionalism implemented the first version of Topsy; and similarly, Thomas Kjær Nielsen, Jan Simonsen, and Tom Bisgård Sørensen for the first 'production' version.

Finally, thanks to IEEE for allowing me to use portions of [Manthey, 1994] in §4, 5, and 7, to which the interested reader is referred for further details.

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WWW. Theses and related publications are available via <http://www.iesd.auc.dk/general/DS/index.html> or <ftp://iesd.auc.dk/pub/reports/papers/manthey:X> where X = PhysComp94.ps.Z for [Manthey, 1994], and PhaseWeb_Software.README for software status. I would greatly appreciate, if/when someone writes a paper referencing this one or the ideas contained herein, their sending me a (paper) copy. Software implementing the Phase Web paradigm will hopefully be available in summer, 1996; inquire via the README. NB: the use of the cycle hierarchy and co-exclusion is governed by US patents 4,829,450 and 5,367,449 and other patents pending; see README.

THE HIERARCHY - A POSSIBLE DEVELOPMENT FROM COMPLEMENTARITYD. Bohm and E.W. Bastin.

3rd March 1967

Bohr has drawn attention to the results of the exhaustion - even bankruptcy - of the concept of mechanical movement and to the consequent need for a new concept of undivided wholeness of action which appears as the quantum character of physical processes. However, Bohr has also assumed that we could only use the existing mechanical language for describing the real nature of matter. He considered that this limitation of language forced us to use the concepts of position and momentum in a complementary (as opposed to a simultaneous) way. However, this notion leaves his concept of wholeness very obscure and seems to make far too much depend upon a merely linguistic difficulty.

We are hoping to come to a better understanding of this new situation in physics by developing the possibility of discrete ordering, in an explicit way, as the foundation of physical theory, and as a fundamental pre-condition for the expression of the laws of physics to replace continuous measure relations.

We emphasise the importance of the explicit formulation of principles of ordering, even though it may be said that these principles are already implicit in current theory. For the ordering concept as it is now used, in a generally tacit way, limits us in just such a manner as to prevent our having a clear insight into the nature of the quantum theoretical restrictions on observation. The current tacitly assumed type of order is really a co-ordination - a reciprocal relation to entities and their movements all on one level. A simple example of co-ordination at one level is provided by the traffic signal on a road junction, which is designed to co-ordinate two streams of

traffic so that there will be no collisions. Any simple machine works by a similar co-ordination of its parts, and we get the same idea in the use of the word "co-ordinates" which are devices to express the relation of (or co-ordinate) events to reference frames.

In its essentials quantum mechanics evidently transcends this mode of description by co-ordination because a vital point of quantum mechanics is that particle movement cannot in this way be co-ordinated to a frame. Of course the current theory includes algorithms which enable us to compute the outcomes of such movements statistically. Nevertheless, we maintain that Bohr was right in his contention that within the language of co-ordination, there is no scope for going beyond such a statistical description.

We differ from Bohr, however, in that we question his assumption that no language except that based on co-ordination is possible. Indeed, we go further and make tentative proposals for language to go beyond the point at which Bohr's assumed limitations come into force. We get our clue by observing that nature has a structure which is a hierarchy of orders - higher levels consisting of abstractions from the structure at lower levels. In the mechanical point of view this hierarchy is only a convenience of our description and the only real structure is taken to be the order defined among particles. We propose by contrast that the hierarchy of orders is necessary and fundamental to the laws of nature. To make the hierarchical structure essentially not reducible to a single level of co-ordination, one must see it as dynamic: changes at each level being dictated from other levels and themselves dictating changes at other levels. (Indeed, the expression "dynamic" means this, and no more than this, for we are not postulating a time external to the whole system).

If one observes this type of ordering either in nature or in human society, one sees it depend on two "streams" of movement. There is an upward, abstracting, flow of information across levels and a converse downward flow which specifies actions in accordance with information carried by the upward flow. Just as there are laws ordering the "horizontal" movements in mechanics, so such hierarchal systems display "vertical" laws. The vertical laws are indeed the main factors determining even the horizontal movement at each level, and therefore constitute a basically new feature not previously taken advantage of in physics.

We are suggesting that all nature has this vertical character. And indeed it can easily be seen that nature does abstract; consider our eyes and all our measuring instruments, as well as the light rays that abstract the whole structure of the environment and carry it to each point in space.

Observation inherently involves action, because the upward information flow always requires some downward action in a non-static hierarchy. This feature of a hierarchical law is exemplified in the special case of quantum theory, where no ultimate separation of observer and observed is possible. In our view, this follows because observer and observed are both aspects of the one total hierarchically ordered process.

Under certain conditions the law of the hierarchy would repeat the same structure at all levels. This situation would correspond to the classical limit and one single "horizontal" section would be adequate to represent the whole. Ordering would then reduce to mechanical co-ordination.

In general however, a vertical section of the hierarchy would show variation and we shall identify this variation as a generalised concept of momentum. Hence, our difficulties in representing momentum as a classical concept arise from the fact that it really is a function of the vertical movement which can only be statistically related to the horizontal.

These foregoing remarks suggest a broad programme. We must start by considering special forms of hierarchy which can be as simplified as we like provided only that they preserve the vertical feature and therefore differ essentially from current mechanics. We shall propose some such special forms which have permitted calculations of a physically significant sort (notably of coupling constants) as a basis for discussion of these ideas of hierarchy and as a method of exhibiting the resulting type of thinking in contrast to types at present current. In addition, we shall indicate the general lines for development of a new kind of mathematics that can describe the process of hierarchical ordering. This is very different from the infinitesimal calculus, but it has certain interesting similarities to the matrix formulation of quantum mechanics, as well as some even more interesting differences.

EMERGENCE AS ANTIMORPHIC ACTION

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Abstract. The paper deals with the fundamentals of a mathematical theory of emergence in the framework of category theory. Our main point consists in the concept of 'antimorphism' introduced independently by EISENHARDT / KURTH and CORNISH. We use an extension of and generalizing deviation from CORNISH to show that an emergent transition can be represented as a 'non-reversible' and (complex) structure-producing specific anti-identity-operation in the sense of *strong antimorphic action* having some resemblance to the intuitionistic negation (in a Heyting algebra of a topological space). So we consider emergence as antimorphic variation over the genus of topological spaces (the latter is a measure of complexity), and propose that it underlies a toposic structure.

KEYWORDS: Antimorphic action, antimorphism, anti-homeomorphism, category theory, complexity, emergence, supervenience, topos theory.

1 ANTIMORPHIC ASPECTS

1.1 Antimorphism as Not Structure Preserving Mapping

In 1985 two of us presented a specific version of a theory of emergence which identified emergent transitions with not-structure-preserving transformations¹. We considered that every transition can be reduced mathematically to an iteration of mappings implying the conservation of structures - these mappings or transformations are called *morphisms*. But when an emergent event takes place essential structures of a system change radically, so one has to look at a type of mapping which is not structure-preserving. This sort of mapping means a change of topology of the respective system. We called it *antimorphism*². Changes of such a kind appear in topology when you observe a non-homeomorphism, i.e. a non-bi-unique mapping being also non-continuous regarding the converse mapping. The antimorphism does not conserve essentially topological invariants, for

example the genus³. To give a very simple example of an antimorphism: the mapping of a sphere (genus 0) into a torus (genus 1) - these figures are thought of as bent surfaces. Another example is known as *mathematical phase transition*⁴: such a catastrophic phase transition appears going over a path out of the interior of the MANDELBROT set to the boundary which is surrounded by particular Julia sets. At this point there is a dramatic change because the structure of connectedness of the topological manifold is altering, namely from a (globally) connected structure to a totally disconnected structure. The altering of the sphere to the torus mentioned above is from „simply connected“ to „triple connected“.

We worked out that antimorphism in detail and followed another trail which lead to the concept of *emergence as structural enrichment* (see fig. 2). It comes up to a general meta-level containing *invariants of any complexification*. In a sense it is the most general description of the transition of structures becoming more special.

One problem which then had been not yet satisfactory solved was that of giving a convincingly general mathematical equivalent. Of course there was the analogy between the antimorphic concept of emergence and the embedding of topological spaces with more (special) invariants in such with fewer of them. This leads to an explanation of emergence as structural enrichment. But two things were missing a) a way of deriving both of the elements of this analogy from one common mathematical root, and b) the very action which is associated with the actual occurrence of emergence.

1.2 Categorical Antimorphism

1.2.1 The categorical aspects

In this paper we want to claim that such a common mathematical root as well as a sufficiently general and flexible tool for dealing with emergence in such a 'structuralist way' could be

achieved on the firm ground of category theory. Category theory (like set theory) provides a general framework for dealing with structures.

... it achieves this by transcending the *particularity* of structures. But set theory and category theory go about doing this in entirely different ways. Set theory strips away structure from the ontology of mathematics leaving pluralities of structureless individuals open to the imposition of new structure. Category theory, on the other hand, transcends particular structure, not by doing away with it, but by *generalizing* it ... (BELL [1981], p. 356).

1.2.2 Some basic concepts of categories

A category may be thought of as an universe for a particular kind of 'structural' discourse. A category $\underline{X}$ consists of *morphisms* ($f, g, h, \dots$) and *objects* ($A, B, C, \dots$). The respective morphisms link the domain of objects ($A, B, C, \dots$) to a codomain of objects ($A', B', C', \dots$). A morphism of a category $\underline{X}$, going from an object A to an object B is expressed as f_{AB} . If A and B are the same objects, indicated by writing f_{AA} or f_A , this is called an *automorphism*. When the domain of a morphism g is the codomain of another morphism f , there is a *composition* h , indicated by writing $h_{AC} = f_{AB} \cdot g_{BC}$. A special important kind of automorphism is the *identity* as a neutral structure, so that for example, the condition $g_{BC} = 1_B \cdot g_{BC}$ holds. A *functor* F is a kind of metamorphism, that links different categories as meta-objects. The *components* of a functor associate object with object and morphism with morphism. An *autofunctor* links a category with itself (like automorphisms do with objects). The *functor identity* Id of a category $\underline{X}$ is often noted as $Id_{\underline{X}}$. Functors of (the same) categories are linked by (*natural*) *transformations* (written as $\alpha, \beta \dots$)⁵.

1.2.3 Categorical antimorphic action

CORNISH [1986] introduced a specific form of antimorphic action to deal with subcategories of categories $M\text{-}\underline{X}$, so that M is a monoid which acts on the objects of the category $\underline{X}$. In this case we have submonoids M^+ and M^- with endomorphisms and anti-endomorphisms respectively. Remember that an *anti-morphism* of a structure S into another structure S' in the precise mathematical sense means a morphism of S into the structure S'' , the *conjugate* of S . So an anti-homomorphism of a ring R into a ring R'' is a homomorphism of R into the ring R'' (conjugated relative to R') which differs from R by multiplication: $a \times b = b \cdot a$ (with $a \times b$ in R'' and $b \cdot a$ in

R'). Only in category theory one can generalize anti-homomorphisms, anti-endomorphisms, anti-homeomorphisms, etc., so that one gets the general concept of an anti-morphism.

Now CORNISH paves the way for a general concept of an antimorphism: he extends the notion of an antimorphism to an arbitrary category⁶. His essentials run as follows:

The category $\underline{X}$ is a subcategory of $\underline{X}^\pm$ in the way that they consist of the same objects but differ in their respective morphisms: „ $\text{Ob}(\underline{X}) = \text{Ob}(\underline{X}^\pm)$, and $\text{Mor}(\underline{X}^\pm) = \underline{X}^+ \cup \underline{X}^-$, where $\underline{X}^+ = \text{Mor}(\underline{X})$ “⁷.

Afterwards a functor T is defined which transforms the objects and the morphisms of a category into their conjugate; applying T twice gives the initial state ($T^2(A) = A$). Furthermore a „metafunctor“ δ is introduced acting on T as a natural transformation and yielding $\delta : T^2 \rightarrow T$, where $\delta^{-1} = \delta T$, more precisely it is a natural isomorphism. CORNISH calls the morphisms in $\underline{X}^-$ the antimorphisms of $\underline{X}$ and the quadruplet $(\underline{X}, \underline{X}^\pm, T, \delta)$ an expansion (by antimorphisms) of $\underline{X}$; when $\underline{X}^+$ and $\underline{X}^-$ are disjoint, the expansion has *separate* antimorphisms.

1.2.4 Beyond CORNISH's antimorphism: Emergentic antimorphic action

For our purpose of explaining emergence as antimorphic action, we use a modification of this antimorphic structure, described by CORNISH (see above). In a category $\underline{X}$ with an antimorphic action based on a meta-functor, the anti-identity δ , there is an autofunctor T such, that for a morphism f , there is still an associated antimorphism $g = T(f)$. Differing from CORNISH's definitions, there is an anti-antimorphism $T^2(f)$, an anti-anti-antimorphism $T^3(f)$ and so on, in our approach, such that we have: $\forall n \neq m \in \mathbb{N}: T^n \neq T^m$. This means, applying T twice, never gives the initial state. We call this generalized version of CORNISH's concept of antimorphism a *strong antimorphism*. The condition $T^n(f) \neq T^{n+1}(f)$ is a generalized version of CORNISH's concept of *separate antimorphism*. Hence, for the established *anti-identity* δ , the new deviating condition $\delta^{-1} \neq \delta T$ holds. For reasons of terminological clarity, we rename this new version anti-identity as *strong anti-identity* χ . Given n and f , the *strong anti-identity* χ leads from $T^n(f)$ to $T^{n+1}(f)$. This paves the way to our concept of emergence. For an emergent transition, there at least one of the mentioned strong separate antimorphisms is required. For the sake of continuity of the chain of

being, there always has to be at least a *non-separating* transformation too. According to our terminological conventions we call this generalized version of CORNISH's concept of anti-identity a *strong antimorphic action*.

2 EMERGENCE AS ENRICHMENT OF STRUCTURES

In [1974] P. MEDAWAR pointed to a intrinsic mathematical model for the explanation of emergence in natural systems. The ideas for this development, he had first presented in [1969]. His choice was the group theoretical classification of geometries known as the Erlanger Program of FELIX KLEIN. In an emergent hierarchy, MEDAWAR gets the classification into *topological space*, *projective geometry*, *affine geometry*, *plane geometry* and *metric geometry*.

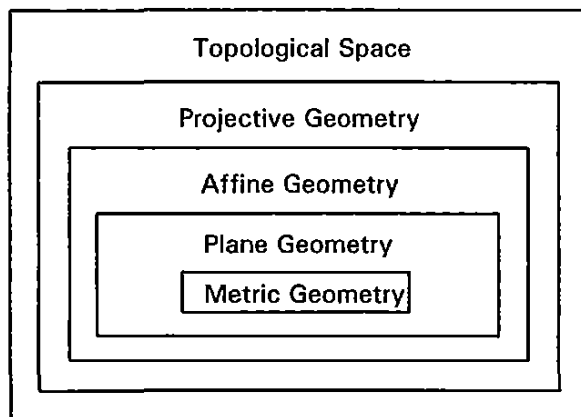


Fig. 1: 'Geometrical' enrichment.

Another kind of hierarchy, describing the emergence or a metric continuum may be done by varying the properties in the neighborhood and coverings of objects in a topological space.

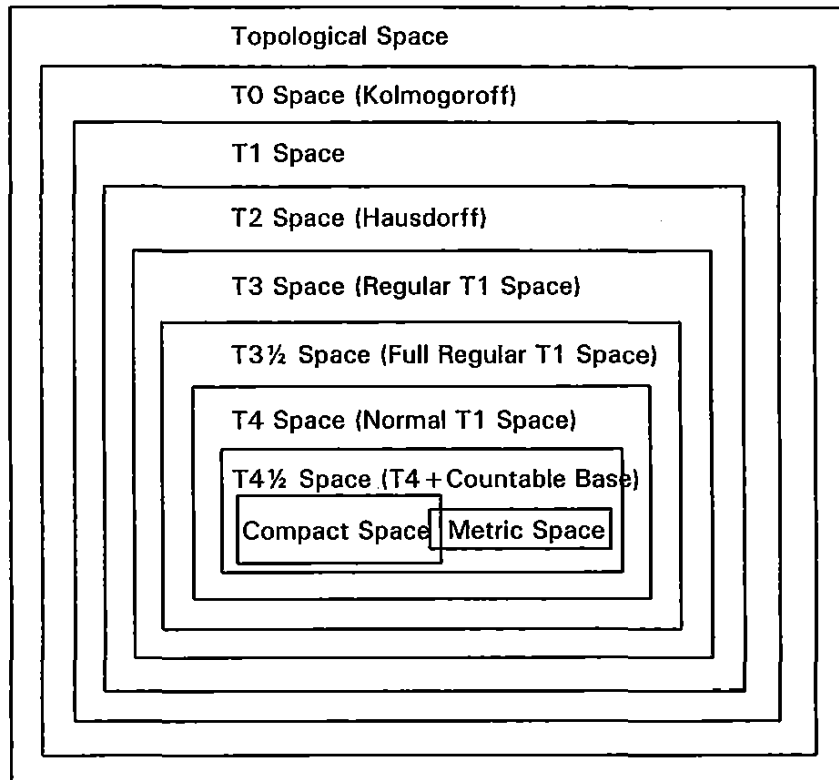


Fig. 2: 'Topological' Enrichment.

But obviously structural enrichment is simply not emergent enough for modeling the dynamics of emergence, but merely a ordered sequence of static snapshots of emergent levels, just missing the dynamic links between them.

3 EMERGENCE AS ANTIMORPHIC ACTION

3.1 Categorical Generalization of Antihomomorphism

We think that the Erlanger Program cannot give more than a kind of support for the intuition that there should be a way of explaining emergence mathematically. There is also something common in the Erlanger Program way of MEDAWAR and our approach of explaining emergence in term of antimorphic action. In both cases the central feature is invariance and the natural embedding of structures with invariants of lesser degrees in structures with invariants of higher degrees. So far this is nothing special, and it obviously resembles the symmetry theoretical approaches, but we think the antimorphic action proposal of ours differs in not only giving a post festum model of

emergence but grasping the actual way it works in (antimorphic) action. The reason for this believe is founded in a) the universality of the category theoretic approach, and b) due to this the flexibility and hence naturality it allows in uniformly explaining emergence on all different levels of complexity.

With category theory we now will use a more general and advanced approach than that of structural enrichment. Cornish's approach is useful for a starting position, let us paraphrase his main points.

As already mentioned above CORNISH⁸ describes a categorical structure, he called *antimorphism*.

A category $\underline{X}$ with *antimorphisms* consists of two subcategories $\underline{X}^+$ and $\underline{X}^-$ such, that the following conditions hold:

- (1) $\underline{X}$, $\underline{X}^+$ and $\underline{X}^-$ consists of the same objects: $\text{Ob}(\underline{X}) = \text{Ob}(\underline{X}^-) = \text{Ob}(\underline{X}^+)$.
- (2) $\underline{X}$ consists of the *morphisms* of $\underline{X}^+$ and *antimorphisms* of $\underline{X}^-$: $\text{Mor}(\underline{X}) = \text{Mor}(\underline{X}^+) \cup \text{Mor}(\underline{X}^-)$. Hence CORNISH introduces a novel morphism $\text{Mor}(\underline{X}^-)$, i.e. the *antimorphism*, not contained in the initial category $\underline{X}$.
- (3) The *conjugate* T of each morphism f^+ is an antimorphism $g^- = T(f^+)$ and vice versa, the *conjugate* T of each antimorphism g^- is a morphism $f^+ = T(g^-)$.
- (4) The *conjugate* T of each object A^+ is an antiobject $B^- = T(A^+)$ and vice versa, the *conjugate* T of each antiobject B^- is an object $A^+ = T(B^-)$.
- (5) T is an autofunctor with $T \underline{X} \cdot T \underline{X} = 1_{\underline{X}}$. This means, applying T twice gives the initial state $T^2(A) = A$.
- (6) There is a natural transformation, the *anti-identity* δ going from f^+ , A^+ to $g^- = T(f^+)$ and $B^- = T(A^+)$ and vice versa an inverse δ^- from f^- , A^- to $f^+ = T(g^-)$ and $A^+ = T(A^-)$.
- (7) δ is an natural isomorphic autotransformation from $1_{\underline{X}}$ to $T \underline{X}$ with $\delta^{-1} = \delta \cdot T = T \cdot \delta$ and $\delta = \delta^{-1} \cdot T = T \cdot \delta^{-1}$.
- (8) An object A and a morphism f in $\underline{X}$ is called *self-conjugate*, if $T(A) = A$, $\delta(A) = 1_{\underline{X}}(A)$, $T(f) = f$, $\delta(f) = 1_{\underline{X}}(f)$.

(9) A morphism f is *separated* from an associated antimorphism $T(f)$ if they are not self-conjugate, which means, $T(f) \neq f$, $\delta(f) \neq 1_{\underline{X}}(f)$.

Points 3, 4, and 6 are loose translations of CORNISH's presuppositions. We have to alter his points in a specific way to represent emergence inherently; the following conditions must *not* hold: $T^2(A) = A$, and $\delta^{-1} = \delta T$, but $T^2(A) \neq A$, and $\chi^{-1} \neq \chi T$ (see above 1.2.4), because emergence is no „zero-sum game“ - *the emergent gains some structure*. To perform the same (emergent) transformation twice does not yield the initial state again. For an explication we follow an illustration of Mac Lane which elucidates the correspondence of the intuitionistic negation with our strong antimorphic action χ . There he gives an geometric example for a Heyting algebra.

The algebra of all open subsets U of a topological space X is a Heyting algebra. Here the intersection or the union of two open sets is again open, by the axioms of topology, so intersection and union are defined, with the usual properties. However, the complement $\neg U$ of an open set must be defined to be an open set; it is taken to be the largest open set in X disjoint from U . As a result, $\neg\neg U$ may be larger than U . For example, if X is the real line and U the open subset of all positive reals except 1, then $\neg U$ is the set of negative reals and so $\neg\neg U$, the set of *all* positive reals, is larger than U . (MACLANE [1986] p. 404).

If we interpret the real line X as a topological space and U as the subspace $X \setminus (CU \cup \{1\})$ of all positive reals, i. e. all positive reals except 1, then one can say, MACLANE's iterated use of the intuitionistic negation 'eliminates' a hole in X . This transformation is 'irreversible' ($\neg\neg U \neq U$) but structure-forgetting because of the very definition of the complement („the largest open set in X disjoint from U “). The *classic* complement is defined as structure-producing but reversible: the structure you get is the old one, so nothing is gained. The wanted emergent transformation has to be structure-producing and irreversible! Let us have a look at CORNISH's δ and T ; his main natural transformation goes as follows:

$$\begin{array}{ccccc}
\text{Spo} & & T^2\text{Spo} & \xrightarrow{\delta_{\text{Spo}}} & T\text{Spo} \\
\text{ho}\downarrow & \rightarrow & T^2\text{ho}\downarrow & & T\text{ho}\downarrow \\
& \delta & & & \\
\text{Spo}' & & T^2\text{Spo}' & \xrightarrow{\delta_{\text{Spo}'}} & T\text{Spo}'
\end{array}$$

(Spo =_{df} open topological space; ho =_{df} homeomorphism; TSpo = Spc =_{df} closed topological space)

As it is easy to grasp, neither CORNISH nor the $\neg\neg$ operation of the intuitionistic logic resp. the Heyting algebra results in *gaining* new structure. But by not selecting the different substructures in the first step of the transformation they inevitably fall back to the original structure with the next step, e.g. TTSpo yields Spo.

In contrast to MAC LANE we prefer the 'production' of holes for our emergent purpose. But this goal is easy to achieve now. Starting with $\neg\neg U$ [as an application of the intuitionistic negation] this simultaneously results a) in U [according to its definition i.e. all positive reals *except* 1] or equivalently in b) $\neg\neg U$ itself [i.e. of course all positive reals *included* 1].

This strikingly resembles CORNISH's concept of structural expansion by antimorphisms (see above 1.2.3). But the intuitionistic structure refers to more than that, it points to the chimerical nature of the entities in an emergent transition. There necessarily must be a phase of an ambiguous double-nature for which a distinctive classification of these entities does not hold.

But this 'ambiguous double-nature' is itself the nature of emergence. In an emergent transition an 'ontological equivalent' to the intuitionistic negation actually occurs. I.e. there always must be a minimal timelet for which the emergent does no more belong to the class of the predecessor entities and also not yet to that of the successor entities, but is just - alike the result of the intuitionistic negation - undecideable. This timelet marks the contingent nature of the emergent transition.

The basic problem of emergence can now be characterized as follows: On the one hand an emergent has to be identified as a new one, distinct from its predecessor, so it is discrete. On the other hand the dynamics of the new as such requires a sort of continuity. The reason is, that a discrete chain of emergents only represents a static map of a basic processuality as inexhaustibility of the potential. Therefore, in the emergent transition the seemingly conflicting properties of „not-being-false“ of „being discrete“ and „not-being-false“ of „being continuous“ ‘simultaneously’ apply altogether with „true“ of „being new“. In our category theoretical approach, this will be represented by the *strong antimorphic action* χ such, that the following conditions hold: a) the truth value „true“ (of „being new“), b) the *characteristics*⁹ of „not-being-false“ (of „being discrete“ or of „being continuous“). Further, the associations are described by *strong antimorphisms* (mentioned above). The *strong antimorphic action* may also be described as the partially separating *anti-identity* χ . This means, some of the antimorphisms (at least one) are separated (for example $f \neq T(f)$) for the property of being discrete. Some of them (at least one) are self-conjugate (for example $g = T(g)$) for the sake of being continuous. These considerations characterize the conjugate T and the anti-identity χ as *emergentic* for the separations by antimorphic actions. We call $T(f)$ an *emergent* and $\chi(f)$ an *emergence* for separating antimorphisms. For the reason of the self-conjugate g , $T(g)$ is called a *resultant* and $\chi(f)$ a *result*. For intercategory antimorphic actions – this means for example that the categories of f and $T(f)$ differ – the conjugate T (now a transformation) and the anti-identity χ (now a natural transformation between transformations) are called *supervenient*. $T(f)$ is a *supervenient* and χ a *supervenience*. These additional considerations will be necessary, if we want to describe incidents, like the emergence of the mental from the physical. So, supervenience may be regarded as a special kind of emergence, a level violating emergence. This - by the way - implies something as ticklish as a reduction (!) of the concept of supervenience to that of emergence.

The n of $T^n(f)$, i.e. the ‘steps’ of emergence, may be thought as a measure for complexity. As will be shown next, n induces a natural kind of *complexity* classes.

3.2 Emergence as Antimorphic Variation Over the Genus of Topological Spaces

Consider Top to be the category of topological spaces as objects and the homeomorphisms as morphisms. If S_G is a space with a certain genus G , the conjugate $T(S_G) = S'_{G+1}$ will be a space 'emerged' from S_G . Thus the antimorphic action of the (possibly iterated) anti-identity χ in this 'special case' is to increase the genus of the topological space at least by one and therefore to describe the process of emergence. Here the strong antimorphic meta-functor χ again causes a violation of the bijectivity and continuity of the mapping, now representing a change of the local discrete structure of S' compared to that of S . Vulgar: the number of holes in the respective representation space increases (see Fig. 3).

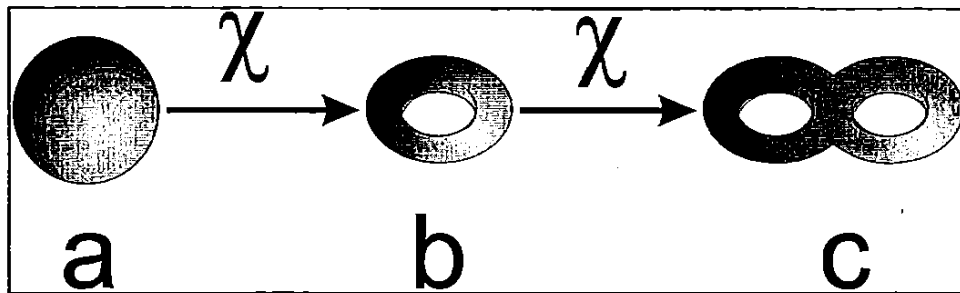


Fig. 3: Increase of the Topological Genus.

Other components save their structure, representing the required continuity. As described in EISENHARDT / KURTH [1993], this special kind of antimorphic action bears the potential of strong affinity to processes in self organized systems and nonlinear dynamics. So it seems most appropriate for natural systems, as it represents somewhat like a broken symmetry of the emergent process.

Therefore of special importance is the fact that by the bridge of loop-complexity¹⁰ there is a direct analogy between the *topological complexity*¹¹, represented by the respective genus ('the respective number of holes') and the well established *algorithmic complexity* (Solomonoff-Kolmogorov-Chaitin complexity)¹². Then the emergent process stepwise 'produced' by antimorphic actions

results in a natural hierarchy of emergent levels, with every level characterized by a certain complexity class. It now seems appropriate to have a closer look to this emergent process itself.

Even without further details about category theory than the basic definitions given above it is easy to grasp that this emergent process has itself a categorical structure. In fact due to the partial correspondence between the anti-identity χ and the intuitionistic negation it turns out to be a category with a Heyting algebra as its internal logic, i.e. a topos. We already gave it the name AMOR, as it seems fit for the topos of AntiMORphic actions¹³.

Now, with the category theoretical definitions in mind it is simple to reveal AMORs toposic character:

- ◆ As usual the topos in question is characterized by its (special class of) morphisms, here of course the *antimorphisms*.

- ◆ And again, as always in topos- and category theory the respective *objects* are the elements of the *domains* linked by the morphisms (here the elements of the complexity classes linked by an emergent transition).

- ◆ As the characteristic class of morphisms for AMOR (the strong antimorphisms) occur just within the scope of the strong antimorphic action χ , which is comparable with the intuitionistic negation, AMORs internal logic is intuitionistic, i.e. a Heyting algebra.

- ◆ Then the *initial object*¹⁴ would be a completely *locally disconnected state* obviously the equivalent of a (global) state *with no physical objects of any complexity*. (This in fact would not prevent this state itself from having *globally* any arbitrary degree of complexity.)

- ◆ The *terminal objects* relatively to the respective emergent levels (or complexity classes) are in our emergent context simply the objects of this classes themselves. The *terminal object* of AMOR (or the emergent process as a whole) would then be an object with a maximal degree of local connectedness, i.e. an object with maximal complexity.

4 THE NATURAL HIERARCHY OF COMPLEXITY CLASSES IN AMOR

With the proposed category theoretical approach, we hope to have found the missing 'common mathematical root of antimorphic mapping and structural enrichment' (see above), and we are confident that antimorphic action actually is 'the very action ... associated with the actual occurrence of emergence', we sought for as well. Furthermore we think that this category- and especially topos theoretical approach will provide the means for a comprehensive explanation of the inevitably intertwined concepts of emergence and complexity. We believe that by means of antimorphic actions, MEDAWAR's idea of a natural hierarchy of emergent levels, which of course was just a more specified version of similar ideas by LLOYD MORGAN, ALEXANDER and others has been reformulated on a new formal concept. Our antimorphic approach to emergence bears some evident familiarity with that of the 'combinatorial hierarchy' as developed by T. BASTIN and C. KILMISTER. By such a concept for the first time the intended 'naturalness' of this hierarchy is produced by a kind of operation, the antimorphic actions in AMOR. So it is not just suggested by intuitions about - well perceived - analogies between empirical sciences and geometries¹⁵, or between the complexity of natural forms and such derived from the algorithmic complexity of recursive functions¹⁶.

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¹ EISENHARDT / KURTH [1990] (1985).

² *ibid.*, p. 31.

³ cf. EISENHARDT / KURTH [1993].

⁴ PEITGEN / RICHTER [1986], p.12.

⁵ cf. BELL [1981] *passim* for further details of category theory.

⁶ see CORNISH [1986], p. XV.

⁷ see CORNISH [1986], p.1.

⁸ see CORNISH [1986], p. 1–3.

⁹ see MARQUIS [1995].

¹⁰ cf. MEYER, A. R.; RITCHIE, D. M. [1967]

¹¹ cf. KURTH, D. [1996]

¹² cf. SOLOMONOFF, R.J [1964a] [1964b]; KOLMOGOROV, A.N [1965]

¹³ cf. EISENHARDT/KURTH [1993], p. 77

¹⁴ For the definitions of *initial* and *terminal objects* cf. MCLARTY [1992], p.16.

¹⁵ cf. MEDAWAR [1974], p. 61.

¹⁶ cf. as an example BENNETT [1994].

Geometric Theory of Machine Awareness for Legal Information Retrieval and Reasoning

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1 Abstract

This report¹ considers that links in hypertext are representable as links in thought by covariant arrows between categories. Taken in dynamic context, the right-exactness of the Heyting implication $A \Rightarrow B$ corresponds to inference and the next document in a non-linear trail through hypermedia. Awareness is provided by the dual contravariant arrows with the important special case of the intension-extension relationship. The corresponding left-exactness is the closure limit that invokes consciousness.

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Suggested Keywords

hypertext, legal reasoning, information retrieval, category theory, adjoints, Heyting algebra.

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2 Introduction

The wealth of information available today online cries out for systems to have some awareness and alerting capability to identify automatically relevant documents. The need for an alerting function was recognized early in law [Bing & Harvold 1977] where it has always been necessary to handle large quantities of data. This is now a problem which faces an average user of electronic mail.

Current tools to explore and handle information overload on the InterNet (like Gopher, WAIS, USENET, MOSAIC, TELNET and ARCHIE) depend on preset buttons, predetermined indexes and algorithms, statistical clustering or human intervention. These methods are proving inadequate. Systems now need to exhibit some characteristic of self-consciousness. They need to be aware of their own contents to provide the user with dynamic links to be made in context at run-time. The current state of the art [Nielsen 1990, Manager 1995] like the use of dynamic frames has more to do with formatting displays than connecting and identifying relevant content. The hypertext markup language HTML of World-Wide Web provides the facility of tags for connecting to related material elsewhere on the Web but the user has to provide the means for identifying the material. Enhancements of SGML and proprietary software like HEADS from Hitachi, DotBook from Novell, HotJava from Sun, and Cairo in the new version of Windows NT [Rymer, Guttman & Matthews 1994], are approaching the concept of mark-up in a semantic context. New environments and operating systems like Windows95 come with built-in facilities for network surfing.

However, users need to be assured of the quality of their legal information system. The quality controller like in any industrial process has to be at a separate supervisory level. This is a trigger mechanism in the system to identify relevant information in context and is also a self awareness where the information checks itself for completeness and its own limitations. Intelligent hypertext is an initial step at this level but intelligence is insufficient without a layer of consciousness. The quality assurance level is a closure over all participating sub-systems, whether local to the end-user or global and belonging to the information providers.

The information in the subsystems may come in any form or format. Of great importance for law is the image data found in multimedia for the large quantities of documents that are being input by scanners in legal cases [Chepalis 1994]. A hypertext system that cannot search, identify and retrieve the contents of documents held as image bits has little value for lawyers. A move in this direction to provide features for heterogeneous data can be seen in systems like HyperNet [Marovac & Osburn 1992].

Without proper procedures, the *less paper office* can result in a loss of integrity. To ensure consistency, some formal model is needed to underpin the interoperable subsystems [Heather & Rossiter 1993]. As information systems are real-world and open, the models need to be drawn from constructive mathematics [Heather & Rossiter

1994] where intuitionistic logic seems to be able to give a high formalism to common sense reasoning and experience.

As legal information systems become larger and more complicated it is very easy for the user to get lost in them. Even intelligent hypertext becomes inadequate. It can provide the user with a non-linear connectivity but to be of value the system needs to know where the user is, where the user has come from and where the user is going, all relative to the contents of the information at any point.

3 Types of Awareness

But the system also needs to provide a variety of awareness features. There is the nature of the information relative to the informational needs of the user and incidentals like the appropriate methods of displaying the data relative to the users individual preferences. More important is the self-awareness of the information relative to information elsewhere, for example to a source of continuous updates. Otherwise, hypertext can misinform.

Whether computers will in the future exhibit the characteristics of human consciousness is an interesting subject of speculation. Of more current importance is the need for information systems to have a current awareness of the contextivity of the information that is available. This encompasses both local disks, CD ROM, etc under the user's physical control as well as the interaction of these with any central on-line facilities over which the user has electronic control at least as far as access and availability.

Even with the old printed medium there were and still are various levels of information providers and information users. At one end, there is the consciousness that a book seller has of the availability of information extending little beyond a stock of items currently in market demand or potential demand together with a list of titles and authors that can be ordered. Unless specialized, the bookseller's awareness will not normally extend beyond books in print. Librarians have more regard for the content of the information but this information may not go far beyond books currently in print and the use of bibliographies.

A reference librarian on the other hand has more interaction with the contents of the information. That is the librarian actually opens the books in question. At the extreme is the lawyer whose function is to dispense the information in the form of advice involving the construction and application of legal source documents to clients who may never actually see the books themselves or even know that they exist. The lawyer has to have an appreciation of knowing what exists and how to find it. Because of the volume and complexity of the information available, the awareness carried around in the lawyer's head is only how to go about finding some relevant piece of information and not any particular source. It is this sophisticated aim to make conscious reasoned connections that legal hypertext must address.

3.1 Conscious Reasoning as Connections

Legal reasoning lies at the heart of the practice of Law implicit in routine procedures and explicit in activities like legal research, legal information retrieval and legal decision making.

Each age has its own view of logic and the application of logic in the wider concept

of reasoning. Reasoning today is making connections: making connections between different categories of objects which may be mixed abstract and concrete. To reason is to impose a structure on the objects. This order may be one of many possible. There may be a unique path but in the law there is usually not just one way to reach the same conclusion and sometimes there may be equally valid paths to other conclusions. In terms of discrete mathematics, the ordering is to identify relationships between objects.

Whether the objects and relationships are considered separately or wrapped-up together is a matter of viewpoint. The Anglo-Saxon word *thoughts* expresses this dichotomy perhaps even better than the word *reasoning*. Comparable perceptions are to be found in other modern relevant topics such as the object-oriented paradigm and connectionist models like parallelism and neural nets.

The older concept of the continuum has been displaced by discrete objects partly because of strong digital influence from computer science and partly arising from a better understanding of physical processes. A critical feature in this reasoning is the connections that pass through real-world events. This provides a source of variety and normally determines that no particular path is fixed for all time.

3.2 Hypertext Connections need to reflect Real-world Links

Inferences in making the right connections involve identifying the objects to connect. These are heavily dependent on pragmatic considerations which may be very subject to the peculiarities of the situation or there may be a more generalizable context sensitivity. Take a question involving law and economics.

Who pays a higher rent: tenants of controlled or uncontrolled tenancies?

Controlled tenancies are connected to security of tenure so that tenants subject to a controlled tenancy have security of tenure whereas the uncontrolled do not have the same security. As security is more valuable, the first impression might be that controlled tenants will pay more for the tenancy if secured. In practice, secure tenants on average pay much less than unsecured tenants. It is a matter of making the right connections. For controlled tenants also have their rents regulated so that they cannot be increased. The real-world situations that need to be connected are: because of the demand for housing, those who cannot obtain controlled tenancies have to take tenancies where there is no control over the rent charged.

This is a kind of context sensitivity that shapes the result that uncontrolled tenants pay more. This could be determined by reference to statistical data of mean rents for the different types of regulated property. However, a few moments reflection will show that no empirical knowledge is needed. The correct conclusions can be made by making the right connections. Again, these connections are the only ones that could be made, namely that controlled tenancies only exist in times of shortage for

otherwise market forces will keep the rents low.

What form of mapping do the relationships take? Essentially it would seem that they depend on some kind of pattern matching. At the simpler level, it is the identification as in the sequence of characters in connections made through vocabulary. Higher order connections can be made at a very abstract level as in a pattern of legal objects and events or facts which by induction make a new legal object and which can by a connection of transference amount to a reasoning by analogy. It is the basis of hyperspace navigation that the low-level connections and the high-level reasoning processes are essentially equivalent classes. The question of the practical technology is how to handle these objects and their identities. This is a class of problem relevant to any implementable information system, including knowledge bases, deductive databases, advanced expert systems, as well as hypertext.

The connections are made in a coordinate space which is not cartesian and where the points are objects of some complexity. A human being is a good example of a complex object consisting of a bundle of characteristics with a unique identification. External positions change with time. Internal characteristics depend on time and also on external positions.

4 Law as Structure of Identifiable Objects and Relationships

4.1 Objects and Subobjects

What are the legal objects? They appear to be recursive structures that can be analyzed in various ways as, in common law jurisdictions, principles, norms, concepts, authority, precedent, *ratio decidendi*, *obiter dicta*, primary and secondary rules, duties, obligations, responsibilities, rights. There are finer distinctions, for instance between the fiduciary duty of a trustee and that of an agent. Common law cases are very full instances of legal objects forming nodes in a network approximating to a Boolean lattice of citations or some more complicated partially-ordered set of a non-linear chain of inference. While the document is an object, it is also composed of many interesting concepts that may be legal objects in their own right. The document is an aggregation of these subobjects where it has to be noted that the position of a document in the network normally depends on the connections between the subobjects. A single document may be a member of quite different networks because of quite disparate subobjects.

The primary sources of the common law of England at present consist of highly-structured documents, cases and statutes inter-locked together [Rossiter, Sillitoe & Heather 1990] by the subobjects they contain. These are the principles, concepts and arguments that have to be connected to arrive at the conclusion to resolve a

legal problem. In order to represent readily the complex relations, both statutes and case reports employ an elaborate presentation of text with diverse formats and multiple character sets and sizes. To capture adequately the detailed structure of the relationships embedded in this text, sophisticated modelling techniques are required to retain the information and transform it to a machine representation.

Database techniques can provide in electronic publishing the typing corresponding to typography. The printed version presents a fixed view of the structure which has to be accepted in a common form by all readers although different views will be in their minds as they read the text. The organization of the text data as appropriate for different users, however, is now available by exploiting the power of computers for logical re-organization. By holding the complete structure of each statute and case in database records, for example, the display format can be readily changed to meet new circumstances: presentation of output to the human eye requires quite different forms from operation in a machine-machine mode, where the output is directly into a word-processor or mark-up language [Heather 1989], or where one machine interrogates another for information [Connolly 1985].

4.2 Connections in the Law

Hyperspace is equivalent to a multimedia database composed of the complex objects described above in contrast to traditional types of data. Databases for simple data developed out of advanced file handling and it was soon realised that it was necessary to identify the kind of relationship which existed between data. The traditional structures are the network, hierarchical and the relational model of Codd. With networks based on the theory of directed graphs, the hierarchical or nested trees and the relational model relying on mathematical sets, these are still only lean representations of relationships in the real world.

Semantic models have developed to meet the need to specify information about the kind of relationships like the way that the relational model was extended in Codd's Tasmanian model RM/T [Codd 1979] to capture more meaning through the introduction of rules of relationships and integrity. The most popular of the semantic models is probably the E-R model of Chen but this has been extended in different ways to suit different users [Spaccapietra 1987]. For instance the type and attribute form had to be added to Chen's original style of representation and Sakai added generalization. The important deficiency of manipulation and the specification of behavioural characteristics has been satisfied in later models by Taxis, Event and SHM+. The complex nature of the objects on the other hand has been satisfied by the development of object-oriented models such as the Semantic Association Model SAM* for use in statistical databases.

They show the way and some of these models have already been applied to document structures for example E-R [Heather & Rossiter 1987], [Rossiter & Heather 1988],

extended relational and Taxis [Rossiter & Heather 1989], and object-oriented and SQL relational [Rossiter & Heather 1990]. None of the models have all the necessary features. The kinds of relationships needed are:

- abstractions such as aggregation, generalization, specialization, inheritance, classification, definition, designation, associative;
- structural such as models, nets, tables, hierarchies, entity classes, E-relations, P-relations;
- statistical such as summation, averages, probabilistic, fuzzy;
- ordering such as sequence, Markov chain, probabilistic, temporal, stochastic;
- reductionist such as projection, parallax, derivation, view;
- behavioural such as dynamic, functional, transaction, operational;
- synthetic such as composition, join, union, cross-product, combined, concatenation, insertion, injection, embedded, tributary;
- analytic such as selection, intersection, adjacency, parametric, attribution;
- parallel such as synchronization, collaborative, collateral, adjacency, adjoin, redundant, orthogonal, anti-parallel, contributory.

Database technology has made progress with some but not all of the above categories. In some relations, the user is not concerned with detailed procedures and these may be the beginnings of automated reasoning. For instance, in aggregation the user is unaware of the way in which the subobjects are put together. An example of this is the way that the current version of a section of an English Act of Parliament may be derived from a number of textual amendments in later Acts. Automatic identification of objects and their characteristics is needed to make the selection with the right interconnections for the aggregation. From a database point of view, the identification is provided by the keys in the system [Rossiter & Heather 1988]. Some universally recognized form is necessary to recognize the keys. This enables documents to be addressed and cross-referenced in a natural manner with a standard identification mechanism. Data typing can be used to characterize components of the keys so that documents can be composed from their underlying subsections (subobjects) in a transparent way. The use of natural keys and relations avoids the unnecessarily reductionist methods of early legal retrieval systems.

Elaborate data management systems are needed to provide the high functionality required for structuring, manipulating and maintaining the data with the necessary integrity to provide professional information systems so that end-users may have access transparently to goal-information in a highly organized state. To do this the

management system has therefore to recognize inherent relationships in the data to make the necessary hypertext links.

There are often very many, if not an infinite number of, natural connections that can be made. The author or information provider may predefine certain of these based on some expectation of the user's requirements. Alternatively it may be possible to provide some automatic assistance based on predetermined criteria. It is a simple matter to have a dynamic button to pick up references for a glossary or thesaurus where there is a direct connection usually because the item in the text is itself a simple key to the citation. Where there is a partial or a composite key, the system has to have some awareness functionality of what is needed.

Both these methods are limited. The system needs to be able to follow any potential connection under the control of the user. This requires the system to be conscious of where the user is within the document. A very simple example might be given of anaphora when reading from a Hansard CD-ROM, to deal with a sentence beginning with

"As I said in my speech on 28th October to this House ..."

the system needs to be aware of the name of the speaker, of whether the current speech is being made in the House of Commons or the House of Lords and of the date of the speech to identify the appropriate year for the 28th October. This necessary awareness required is therefore beyond intelligent hypertext. It also illustrates the practical point that this awareness needs to be a runtime facility. For identifying all possible cross references in advance when only very few of them will ever be required is very inefficient in preprocessing and storage and almost impossible manually.

The law is typical of a number of areas of information where there is a large store of static data which can be kept locally cheaply on video-disk or CD-ROM but where it is essential to be able to check the present state against some master version. Take the example the legal question of defining an infringing copy of a piece of software. The current law is to be found in the Copyright, Designs, and Patents Act 1988. When it came into force it confirmed software as having the protection as that of a literary work. Section 27 defined the meaning of 'infringing copy' without any special reference to software.

However under the Copyright (Computer Programs) Regulations 1992 (Statutory Instrument 1992 No 3233) which came into force on 1st January 1993 the 1988 Act was amended to vary the definition of 'infringing copy' from the meaning for other literary works to bring English law in line with the European Directive No.91/250/EEC (O.J. No L122, 17.5.91, page 42). This situation shows that some awareness and alerting function is necessary to know that some later version is needed. This is only one way in which amendments are prepared in English law. All of which need to be recognizable by the system.

This awareness is now essential in very many areas of business which need continual

access to information on changing standards and regulations. This awareness function can be achieved by overlaying another layer of metadata on top of the basic hypertext system. This is a necessary part of intelligence in information retrieval systems [Heather 1985]. We have to provide this additional layer to simulate a human metamemory for any type of document [Rossiter 1987] and to be reliable as comprehensive as one that like consciousness provides a closure to an open system [Heather 1988].

5 Formal Modelling under Geometric Logic

The logical reasoning obtained with axiomatic methods are subject to the uncertainties of the applicability of the axioms. Constructive mathematics on the other hand attempts to develop logically what works in practice and can perhaps provide the necessary universality for interoperability of heterogeneous data systems with consistency and quality assurance in the real-world. Geometric logic is particularly appropriate for modelling relationships in hyperspace [Heather & Rossiter 1994] for it is essentially concerned with links between objects.

From the simple concept of the arrow, formal categories can be constructed of objects with arrow links between them. These provide a natural model for a document. Geometric logic is the formulation between the categories and can therefore represent manipulation of documents in this model of hyperspace. In this model it turns out that linking documents and reasoning are equivalent. This confirms our earlier informal discussion.

We are concerned with general categories which may be used to represent any system or a class, object, entity, set, etc. that satisfy the four categorical axiomatic constructs for arrows namely composition, compatibility, associativity and compositional identity [Barr & Wells 1990; Heather & Rossiter 1994]. These required constructs do not cause many problems in applying category theory to real-world models which deal with things that actually exist. However, it may be necessary to check carefully that the components of a virtual reality system satisfy the definitions of a category. Because categories are general it is often only a matter of convenience for a particular model how objects and arrows are to be identified. With hypertext a document forms a natural category. Other categories are always available to provide the necessary typing. For example a particular Act of Parliament can be considered as a category **Act**. **Act** can be typed by an arrow from the category **Enact** of statutes consisting of all parliamentary enactments including both civil law codes (from the subcategory **Code**) as well as statutes (of the subcategory **Stat**) found in common law jurisdictions.

Figure 1 shows functor arrows K, L between categories **A** and **C** containing objects $A, B, C, \dots$ interrelated by arrows $f, g, \dots$. In Figure 1, K assigns from the source object A the target object $K(A)$ to **C** and from a source arrow f the target arrow

$K(f)$ to g . These are covariant arrows. The direction of K and L may be reversed to give the dual contravariant arrow.

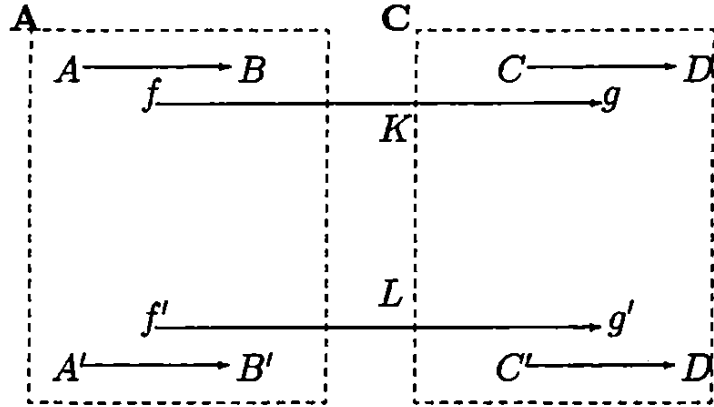


Figure 1: Functors compare Categories

Documents and the concepts they contain may be represented by categories $A, B, C, \dots$. The functor arrow generally represents a hypertext link between documents. The functors can also represent inferences. In geometric logic a deductive system is based on the arrow as a proof:

$$\frac{F : A \longrightarrow B \quad G : B \longrightarrow C}{GF : A \longrightarrow C}$$

where $F : A \longrightarrow B$ is more than a proof theory entailment. The arrow is the *reason* why A entails B [Lambek & Scott 1986]. The *reasons* F and G may be a mixture of propositional and predicate logic or even deontic. The inference in the composition GF is then a graph in geometric logic of legal reasoning. At the same time it is calculable through the algebraic form.

Any model needs a philosophical view to be taken of the universe. Hypertext too cannot exist in a philosophical vacuum. Therefore it is perhaps worth mentioning in passing the ontology of this formal model. Reality in the everyday world is made up of rational links as discussed in the first part of this paper. What exists are limits in the sense of geometric logic. A hypermedia is a model of these limits in some cyberspace. These can be represented in this way because of the universal abstract character of category theory.

In general a finite limit in the category C means a limit of a functor $J \longrightarrow C$ where J is a finite category. An object in the functor category C^J is a geometric diagram in C of type J which can be represented in general by the cone (together with dual cocone) [Rhydeheard & Burstall 1988].

The nature of proof in category theory should be emphasised. The diagram is a formal diagram. It is a geometric representation equivalent to an expression in algebra. We are in constructive mathematics and the one proof needed is the proof of existence. Therefore so long as it can be shown that the entities belong to formal categories [Freyd 1964], proof up to natural isomorphism is by composition. A formal diagram is in effect a *quod erat demonstrandum*. Freyd & Scedrov [1990 p.29–36] give a formalization of the diagrammatic language.

5.1 Adjointness

Adjointness between two categories

$$F \dashv U : \mathbf{A} \longrightarrow \mathbf{B}$$

has left and right components which specify how an arrow in category $\mathbf{A}$ is related to an arrow in category $\mathbf{B}$. This is the fundamental concept of implication to be found in geometric logic. The left component is the free functor $F : \mathbf{A} \longrightarrow \mathbf{B}$ and the right component the underlying functor $U : \mathbf{B} \longrightarrow \mathbf{A}$. F is left adjoint to U and U is right adjoint to F . This is a natural bijection between arrows which holds subject to the condition for all objects $A \in \mathbf{A}$ and all $B \in \mathbf{B}$ such that:

$$F(A) \longrightarrow B \text{ implies and is implied by } A \longrightarrow U(B)$$

Written as a geometric logic inference where the double line indicates the biconditional (iff):

$$\frac{1_{\mathbf{A}} \leq UF}{FU \leq 1_{\mathbf{B}}}$$

With this condition there are two natural transformations or unit of adjunction:

$$\eta : 1_{\mathbf{A}} \longrightarrow UF, \quad \epsilon : FU \longrightarrow 1_{\mathbf{B}}$$

Adjointness is particularly relevant to hypertext for it represents the concept of relative ordering which is the basis of the connections between documents. $A \leq B$ means B is a later document than A in a hypertext trail. The unit of adjunction is a natural transformation that amounts to an abstraction of the components of the adjointness representing the concepts, objects, message passing, etc which connect the documents.

By virtue of the adjoint functor theorem [Freyd & Scedrov 1990], left adjoints preserve colimits (right-exactness) and right adjoints preserve limits (left-exactness). Colimits are the dual of limits. Both limits and colimits will be examined in more detail.

A basic form of awareness can be provided by means of indexing. The simplest index is an inverted file (concordance) which is an example of adjointness, $F \dashv U : \mathbf{A} \longrightarrow \mathbf{B}$. If category $\mathbf{A}$ is a collection of documents where the objects are words, the arrows would be the relationships of natural language with higher-order arrows relating documents. The free functor F is the (arbitrary) addressing for the position of each word in the full text in the documents as adopted by early systems in law like LEXIS. The objects of category $\mathbf{B}$ could also be words and addresses (logical or physical disk addresses); the arrows of $\mathbf{B}$ would be to provide some ordering, usually lexical. The underlying functor U finds for any given word its position in the text of a document as shown in Figure 2:

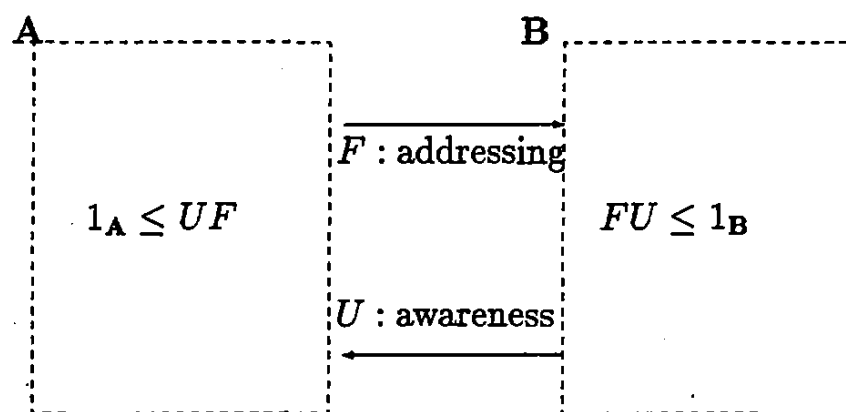


Figure 2: Adjointness in Indexing

Adjointness is satisfied because a given text may be completely decomposed into its vocabulary (right-exactness under functor F) and then reconstituted again in natural language sequence (left-exactness under functor U); that is $1_{\mathbf{A}} \leq UF$. Alternatively starting from the index, the full text can be constituted and then decomposed again; that is $FU \leq 1_{\mathbf{B}}$. Notice that $1_{\mathbf{A}} \leq UF$ consists of all the orderings in the text and $FU \leq 1_{\mathbf{B}}$ all the orderings in the index.

Therefore the contravariant functor $U : \mathbf{B} \longrightarrow \mathbf{A}$ provides the overall awareness of the contents of the documents in category $\mathbf{A}$. LEXIS like most full-text systems has a stop-list of common words to save index storage and therefore operates on a subcategory of $\mathbf{A}$ and also neglects arrows representing other relationships between and within documents. The awareness of these can be retained with a more elaborate database management model [Heather & Rossiter 1987]. Consciousness goes a stage further to identify relevant contents.

5.1.1 Adjointness between Text and Image Data

Imaging is rapidly becoming a major industry and the manipulation of image data based on content and meaning is a burning research topic. Geometric logic shows well the adjointness between textual and graphical information. Both are mapped into the electronic medium as a bit stream.

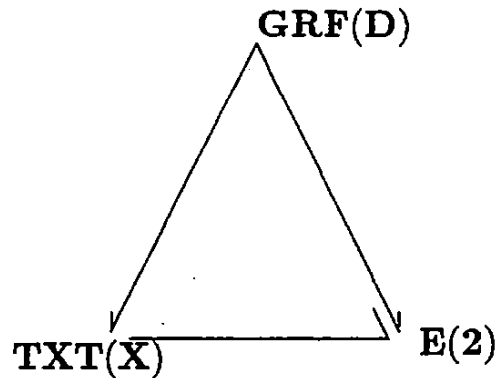


Figure 3: Adjointness of Electronic Forms

Multimedia are logical rather than physical based. They are therefore an abstract category of a document which may be represented as a textual file or as an image file resulting from input by means of a scanner. Clearly the two forms contain equivalent information although they would appear in quite different electronic forms. This is an important example of adjointness as demonstrated in Figure 3. $\text{TXT}(X)$, $\text{GRF}(D)$ and $E(2)$ are categories corresponding respectively to text, graphics and electronic form. Each of these categories is a free functor. $\text{TXT}(X)$ is a map from the alphabet X on to finite strings so a character, x , goes to a string, $x \mapsto \langle x \rangle$. $E(2)$ is correspondingly composed of strings of zeros and ones. $\text{GRF}(D)$ is the much more interesting graphical version which contains all the semiotic significance of the text beyond the mere characters (i.e. punctuation, capitalization, italics). There may be a loss of information from the category $\text{GRF}(D)$ to $\text{TXT}(X)$.

5.2 Intension-Extension Mapping

The links in hypertext may be at different levels. The mappings representing the links would therefore need to be typed in geometric logic. There is the simple linking between documents like a citation of a label or name (the intension). A more powerful level of connection is within the semantics (the extension). There is also the intension-extension relationship which has been shown by Lawvere [1969] to be composed of adjoint contravariant functors.

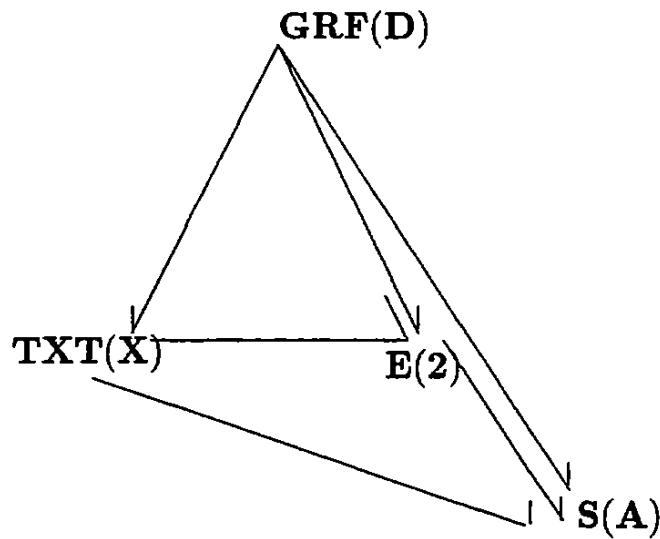


Figure 4: Adjointness in Real-world Semantics

The extension level of the abstract document is therefore the same for the three categories of text, graphics and electronic bits. Equality in geometric logic is provided for by composition. The possible relationships between the three categories of documents at the two levels can therefore all be summed up in a simple geometric formal diagram.

A real-world semantics $S(A)$ can be represented in any of the three forms of graphical, textual and electronic. There will therefore be intension, and extension consisting of contravariant functors between each of the three and $S(A)$ as in the diagram in Figure 4.

5.3 Geometric Database Models

Database modelling reduces to a small family of concepts in geometric logic. The various types of database relations described above may be summed up in Table 1. For fuller details that have been worked through for a product model based on limits, see [Rossiter, Nelson & Heather 1994; Nelson & Rossiter 1995].

database operation	categorical construct
abstractions	exactness
structural	adjointness
statistical	subobject classifier
ordering	adjoint functors
reductionist	co-exactness
behavioural	comma category
synthetic	exactness
analytic	co-exactness
parallel	adjointness

Table 1: Database Concepts in Categorical Terms

6 Formal Contextual Sensitivity

6.1 Limits, Colimits and Context

A very fundamental concept that has only been appreciated in the last thirty years is that of *limits* and *colimits* [Mac Lane 1971]. In arithmetic a limit is constructed by multiplication and colimits by addition. Within set theory, intersection is an example of a limit and disjoint union a colimit. With more general categories, limits and colimits become very powerful. A colimit is a deconstruction and provides no new information. The colimit of A and B is given by the fullest possible combination of taking them together and written $A + B$. A partial colimit would be obtained by taking together only certain parts of A and B . The parts that are significant when taken together may be provided by the context of a different category C . The pushout $A +_C B$ as shown in Figure 5 then expresses this colimit in context.

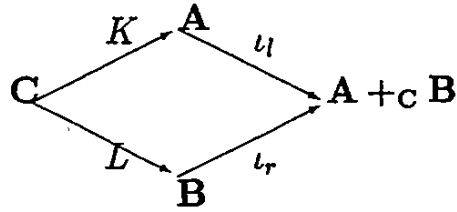


Figure 5: Diagram of Pushout of A and B over C

This is the geometric logic representation of the hypertext link which brings together the documents A and B through the context C . Note that this does not give any new information, but only identifies those parts of A and B which are relevant together in the context of C .

An example of pushouts can be seen in the geometric logic representation of the remark referred to earlier: "As I said in my speech on 28th October to this House ...". The diagram in Figure 6 shows a pasting together of pushouts in which the result of one pushout *House* is included in turn in another pushout forming I .

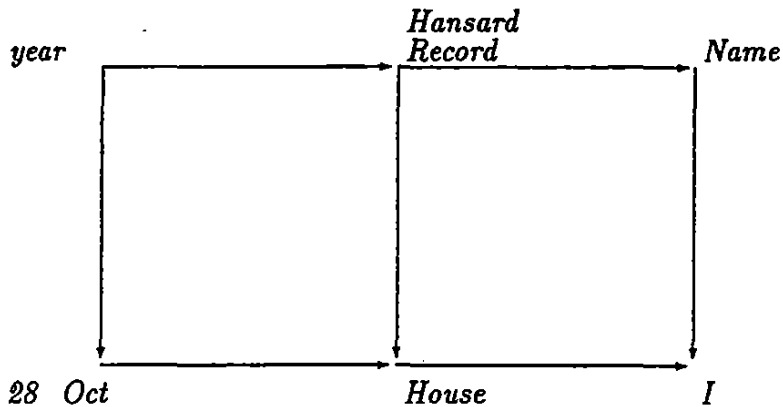
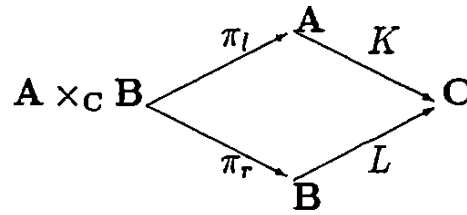


Figure 6: Composition of Pushouts for
"As I said in my speech on 28th October to this House ..."

New information attained by linking A and B is given by the product limit $A \times B$. This for a context C is the pullback $A \times_C B$ shown in Figure 7. In general the difference between a limit and colimit may be summed up in that a limit produces some creative outcome of a link whereas the colimit is a link between standard information.

Figure 7: Diagram of Pullback of **A** and **B** over **C**

Hypertext is a family of trails and it is important to recognize whether two trails are distinct or whether they merge. Thus there may be two parallel links between the same two documents. The question arises for the hypertext system whether two separate trails arriving at the first document are then merged. For example a legal case may cite a second case more than once during the report but it may be on two quite distinct points of law or even branches of law. Two cases may be connected on a substantive point of law and also quite separately on a point of legal procedure, adjectival law. A document often cites another more than once. Links between two documents in this situation become a limit point in the two trails if they merge there. However, geometric logic shows that there is a duality of limit also in this instance.

A coequalizer is the situation where there are distinct connections between the same two documents so that separate trails can pass across without merger. With the equalizer any separate trails arriving at the first document leave the second document by the same path. An equalizer is a context limit **C** represented in the diagram of Figure 8. All trails through **A** and **B** are merged through context **C** which will be shared by both **A** and **B**.

$$C \rightarrow A \rightrightarrows B$$

Figure 8: The Equalizer **C** as a context limit on arrows from **A** to **B**

The corresponding coequalizer is given by Figure 9. The context of **C** is null, that is the limits are independent in thought but from the document perspective there is a context of documents where the two trails coexist with local independence. In other words links between documents may be equalizers or coequalizers.

$$A \rightrightarrows B \rightarrow C$$

Figure 9: The Coequalizer **C** where two distinct trails coexist independently

Two other special limits are the terminal object and (its dual) the initial object. An object in a category **C** where there is one and only one arrow from every other object

to it is known as the final or terminal object of C . This may be denoted by T which is the last object in the trail. Dually (or oppositely) to the final object there may exist a corresponding initial object where there is an arrow from it to every other object in the category. This is $\perp$, the starting point in the trail and the arrows from it are every potential trail. This has significance for the reasoning and logical content that resides in the hypertext links.

In hypertext the initial and terminal objects may have only a local context. There may not be one single starting point, there may be a number of origins for any given trail. Likewise a trail may diverge to more than one finite point. Also natural language is a more general category than that of sets and the trails need not be disjoint. The same words could be used but with two distinct links in thought.

7 The Hypertext Lattice as a Heyting Algebra

As pointed out by Ted Nelson in his early idea of hypertext as "non-sequential writing with reader-controlled links" [Bolter 1990], links in hypertext are rarely linear but branch and form a distributive lattice. The internal logic of a lattice is geometric logic which is more general than Boolean logic. The logic of a lattice is well-established. It is equivalent to a Heyting algebra. Any Heyting algebra has a fundamental binary operation of implication $\Rightarrow: A \longrightarrow B$. This arrow is commonly written in the form $A \Rightarrow B$ and this shorthand version will be used here. This implication arrow is defined by the adjunction

$$\frac{(C \times A) \leq B}{C \leq (A \Rightarrow B)}$$

$A \Rightarrow B$ is the largest category connected with A which is contained in B . In hypertext terms if the current document (A) in its context (C) precedes document B , then B is the next document after A in that context. In terms of concepts rather than documents, the concept may not be represented by a document in existence and from the point of view of a writer would be the next document to write.

By the application of this implication we can obtain the more generalized type of negation found in natural systems. Indeed in natural language it is often possible to represent negative concepts in a positive way. This is also true in hypertext where falsity and truth are not simple atomic entities. These are geometric concepts. Truth is given by $B \Rightarrow T$ and falsity by $A \Rightarrow \perp$, sometimes written $\neg A$.

Truth and falsity are relative to context. In hypertext, $A \Rightarrow \perp$ is (usually back) in the direction of the initial document, a state of ignorance, whereas $B \Rightarrow T$ is forward in the direction towards the last document to be viewed in the lattice, the state of enlightenment. Knowledge and ignorance in hypertext are the counterparts of true and false.

The nature of the pseudocomplement then $A \Rightarrow \perp$, that is not A , may be further understood by substituting the special instance $\perp$ for B in the definition of the adjunction above. We then get

$$\frac{(C \times A) \leq \perp}{C \leq (A \Rightarrow \perp)}$$

In the real world two negatives do not always make a positive. This is familiar in natural language which opposes the principle of *tertium non datur*. The pseudocomplement is so important that natural languages often make it a separate word. For example the concept *relevant* has the pseudocomplement *irrelevant* which results in the further concept of *not irrelevant*. So *not irrelevant* is not equivalent to *relevant*. In fact there is a Heyting ordering:

$$\begin{array}{lcl} B \Rightarrow \perp & \leq & (B \Rightarrow \perp) \Rightarrow \perp \leq B \Rightarrow \top \\ \text{i.e. irrelevant} & \leq & \text{not irrelevant} \leq \text{relevant} \end{array}$$

In hypertext terms, this gives a ranking of the relevancy of the documents in general terms for B the next possible document. It is an irrelevant document, if it is in the direction of the first document. It is the required next relevant document, if it is in the direction of the final document in the trail. Note when it is not irrelevant. That is, if it is not in the direction of the first document, whether or not it is in the direction of the final document. It is this three-level ordering which is the basis of much fuzzy thought and a generalization of fuzzy sets.

In terms of the Heyting algebra, $C \Rightarrow \perp$ is another special case of $A \Rightarrow B$. As noted above $A \Rightarrow B$ is itself a concept/document and $C \Rightarrow \perp$ is an *irrelevant* context concept/document. A fundamental feature is that the pseudocomplement $A \Rightarrow \perp$ is the largest category disjoint from A .

8 Geometric Consciousness

8.1 Contextual Awareness in Hypertext

The earlier discussion on context with pullbacks and pushouts deals with the simpler straight-forward type of static and objective contextuality but it is perhaps worth looking at the example previously raised:

"As I said in my speech on 28th October to this House ..."

A simple form of contextual awareness can be attained in this example by state of the art database techniques using fields, relations or keys. Thus the information identifying *I*, *House*, and *year* can be anaphorically resolved by reference to meta-records in the database system. Fuller details on how this works using keys in a Hansard database is given in [Rossiter & Heather 1988] and [Hudson 1985] where partial or composite keys are examples of colimits.

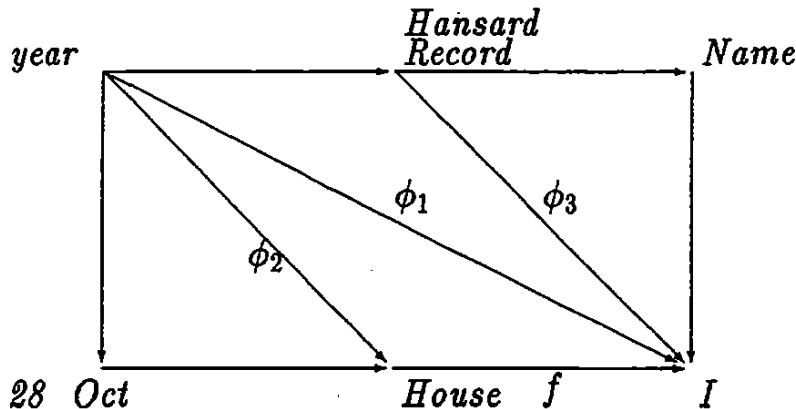


Figure 10: Awareness to identify *I* and *House* in
 “As I said in my speech on 28th October to this House ...”

The hypertext system of awareness is one that identifies for the user the next document to see. This is available from the implication $A \Rightarrow B$. Thus awareness is the contravariant natural transformation $\phi : B \rightarrow A$. Awareness in hypertext is therefore the self identification of the document *B* in $A \Rightarrow B$. In Figure 6, it is the document records. The awareness to identify *I* is the *Hansard Record*, the *House* and the *Year* as given in Figure 10. This figure shows how the awareness works. The identity of the speaker *I* is given by ϕ_3 , the identification of which house (Lords or Commons) by ϕ_2 and the awareness of the date of the speech from ϕ_1 . These can be obtained algebraically. For example $f \circ \phi_2 = \phi_1$. *f* is the meta-record giving the house where the speech is given. In a database implementation *f* consists of those parts of the composite key which uniquely identify the *House*.

8.2 Computational Model of Consciousness

While the basic purpose of hypertext awareness is for that next document *B* in $A \Rightarrow B$ to identify itself to the user, the position is complicated by the fact that the user is operating at two levels the intension (represented by the document) and the extension (represented by the meaning). This identification is a precompositional contravariant arrow $\alpha^* : B \rightarrow A$ which is a backward selection from the relevant documents of the

document to which they are related.

Hypertext links are really connections between the semantic objects in the current document with related semantic objects in the documents to be retrieved. The connection is between objects $A_1, A_2, \dots$ in the category $S(A)$ (that is the meaning of the contents of the document under examination which is A) with objects $B_1, B_2, \dots$ in the category $S(B)$ which are the meanings in the documents to be retrieved B . The identification of the relevant documents depends upon the purpose and intentions of the user. This is a natural transformation $\eta_A : A \rightarrow S(B)$ as shown in Figure 11. The conscious awareness is given by the inverse natural transformation α^* .

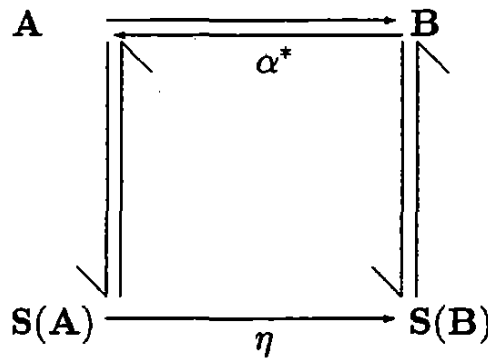


Figure 11: Commuting Target Square for Awareness as a Natural Transformation

This is equivalent to analogous reasoning because the inverse natural transformation α^* preserves limits and colimits as well as implications.

$$\alpha^*(B \Rightarrow B') \cong \alpha^*(B) \Rightarrow \alpha^*(B'),$$

$$\alpha^*(\top) \cong \top,$$

$$\alpha^*(B \times B') \cong \alpha^*(B) \times \alpha^*(B'),$$

$$\alpha^*(\perp) \cong \perp,$$

$$\alpha^*(B + B') \cong \alpha^*(B) + \alpha^*(B')$$

The signs for products and sums are again used to represent generally limits and colimits respectively. Table 1 shows that none of the database relationships and structures in paragraph 4.2 require any operations beyond these. Therefore $\alpha^*(B)$ can claim to be a general awareness relationship.

An example of the reasoning counterpart to hypertext can be seen in the question discussed earlier: *Who pays a higher rent: tenants of controlled or uncontrolled tenancies?* The inference that controlled tenancies pay lower rent (despite the apparent more valuable security of tenure) comes by Heyting logic. The geometric-logic representation is:

$$\frac{C \leq (A \Rightarrow \perp)}{(C \times A) \leq \perp}$$

If A represents a tenant paying rent, $A \Rightarrow \perp$ represents the concept of a tenancy at low rent, from the simple two-object category $\text{Rent}(\top, \perp)$. If C is a controlled tenancy, the proposition $C \leq (A \Rightarrow \perp)$ means that a controlled tenancy is equivalent to paying low rent. The effect of the control is that such rents are low. The inference $(C \times A) \leq \perp$ is that (the product) those who pay rent and are controlled tenants pay low rents.

8.3 Relative and Dynamic Contexts

Simple categories may be built-up to represent the greater complexity found in hypertext systems. For instance a concept that emerges from a structure of related documents itself is a diagram as previously indicated and may be used to replace a single object A . These can be employed to give hypertext the facility to deal with dynamic, subjective content. In geometric logic this amounts to manipulating more sophisticated structures for diagrams are a more general form of objects and simple categories.

For example the comma category has attracted considerable attention in computing science [Barr & Wells 1990] and can provide general contextuality. The comma category can add structure to an ordinary category by considering the arrows from the point of view of a particular object. Given a category A with a variable object A which may be represented by A' (when we want to distinguish different instances), the arrows $f : A \longrightarrow A'$ relative to C are objects in the comma category A/C (sometimes written $A \downarrow C$) as shown in Figure 12. It should be emphasised that the objects in the comma category are arrows; the comma category arrows are triangles. For a map of the domain A and codomain A' together onto C specifies $f : A \longrightarrow A'$.

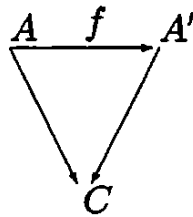


Figure 12: Diagram of Comma Category

In practice hypertext does not just relate two documents but two documents in their respective categories. Therefore the hypertext link between document *A* (the one being viewed) and the next document *B* is given in Figure 13. The functor *K* is the hypertext link between the categories where the objects in each category are triangles composed of lower-level arrows. This shows up the dynamical aspects of context.

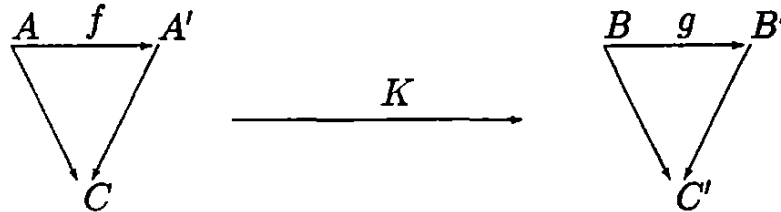


Figure 13: Covariant Functor *K* between Comma Categories

Figure 14 shows the corresponding contravariant functor α^* between comma categories.

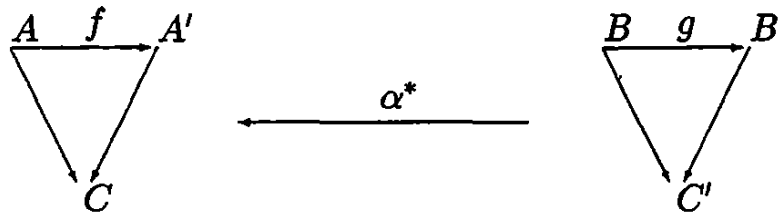


Figure 14: Contravariant Functor α^* between Comma Categories

Consciousness, with relative and dynamic context, is obtained by generalizing from the following relationships shown in Figures 13, 14:

$$K : A/C \longrightarrow B/C'$$

$$\alpha : f \longrightarrow g$$

$$\alpha^* : B/C' \longrightarrow A/C$$

The whole collection can be viewed as analogous reasoning thus confirming the equivalence of reasoning and hypertext.

9 Conclusions

The hypermedium has developed into a very sophisticated information system, one that is populated by a variety of distributed hypermedia source material. These heterogeneous materials are pulled together to create a contemporaneous document of the instant based upon the view of the user applied to the inherent structure in the source material. This view of the user is shaped by the real-world perceptions of the user interacting with the various documents encountered on the way. Because of the power to backtrack and because of inherent branches, it is more than just being shaped by a linear sequence of ideas.

If the system is to aid the user in handling this complexity, it is not only to show awareness of its own contents but must also be able to make the required inferences and connections. This is a form of consciousness. We have concentrated on a theoretical description in terms of geometric logic of the attributes of this machine consciousness as needed in information systems. This theory shows an equivalence of reasoning between documents and their contents. It seems therefore that this example from constructive mathematics may give us more insight into the concept of human consciousness. The general nature of the categories used in this study suggest that systems like the brain may operate in an analagous α^* natural transformation.

One certain result is that human consciousness is no less than machine consciousness and any theory of human consciousness must require at least the same capability to cope with concepts of geometric logic and to be able to operate at and across different levels. It is now recognized that a scientific representation of consciousness is needed for a *Theory of Everything* [Taylor 1993]. Constructive mathematics can perhaps provide an alternative [Barrow 1992] description of consciousness through geometric logic.

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HOW QUANTUM MECHANICS MAPS ONTO REALITY

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ABSTRACT

Arthur Schopenhauer said, "The world is my idea - to have brought the proposition to clear consciousness, and in it the problem of the relation of the ideal and the real, i.e. of the world in the head to the world outside the head is the distinctive feature of modern philosophy."

1. INTRODUCTION

The premise is that unless the phenomenal, i.e. the world in the head as perceived, captures the properties of the noumenal or world-in-itself with increasing accuracy during the course of Evolution, then higher organisms and especially ourselves would be most unlikely to survive. But this condition in image and signal processing where object images coincide with the objects themselves capturing their properties is well defined and known as phase conjugation[Pepper David 1985]. And thus in a model of perception and cognition employing such image and signal processing[Schempp 1992][Marcer 1995a], it can be hypothesized that the emergence of consciousness as brain/mind interaction is the culmination of this process of approximation. That is, the brains of higher organisms like ourselves interface with the noumenal through the condition of phase conjugation, so that the brain's cognitive mappings do indeed correspond to the categories of the noumenal or world as it is. Thus the morphology and dynamics of brains and of the brain/mind responsible for the processes of perception and cognition, should be the object of study, and like the distinctive feature of Schopenhauer's philosophy, will be the means to understand the noumenal or world as it is, i.e. reality, and where the condition of phase conjugation which is self-reflexive, provides the basis of the mappings to and from the ideal or world in the head, to the real or world outside the head.

This approach to understanding reality is in my opinion vastly superior to other approaches such as elementary particle physics or cosmology, because in these disciplines the check between the noumenal or world as it is and the phenomenal or world inside the head, i.e. physical theories is confined to the results of the very limited domain of experimental validation, which is still very much an art (i.e. the design of experiments) rather than a science. For in the case of the brain/mind, Evolution is this artist that has perfected the design[Marcer 1995b].

Similarly in computational models, for in computation as distinct from information processing/engineering, the implicit premise is that the 'objects' of the information processing

taking place such as data sets are canonically labelled. That is the whole purpose of computer science is to provide sound strategies for the canonical labelling of these 'objects' so that errors in the mathematical specification of the model and subsequently in the programming of that specification are avoided. Thus for example phase conjugation constitutes such a strategy in physical image and signal processing using for example quantum holography. For here, phase conjugation ensures that there is a one to one correspondence between object images and the objects of reality. That is, those object images are implicitly labelled iconically by the objects themselves. Thus in the physical encoding and decoding processes of quantum holography[Schempp 1992], the condition of phase conjugation not only ensures that such image and signal processing constitutes computation in the formal sense of that word but the encoding of such object images constitutes experiential knowledge of the real world capturing the properties and the geometry and morphology of the objects themselves. Furthermore the physical processes of holographic encoding and decoding eliminate the need for human intervention to provide a mathematical specification or program. That is holography by electromagnetic means captures the electromagnetic properties of objects, by acoustic means, the acoustic properties etc. Thus the epistemology coincides with the ontology, and the syntax with the semantics. And this is quite distinct from say digital computation where a canonical labelling can at best provide a logical consistency or syntax without a guaranteed mapping onto the real world so that models are at best a consistent fiction possibly outside the realm of fact where the world as it is described, maps onto the world as it is. The implicit phase conjugate labelling of object images by the objects themselves also provides a common basis for communication, for if such a community of communicators share a common arbitrary canonical labelling which makes this implicit phase conjugate labelling explicit, then this constitutes a language by means of which the members of the community can communicate.

2. A NEW PARADIGM OF INFORMATION PROCESSING

Thus quantum computation employing quantum holography differs fundamentally from digital computation in the following respects.

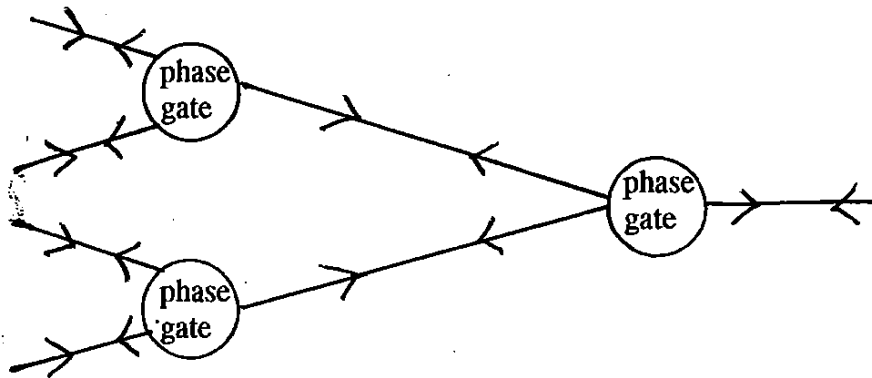
- i) It concerns geometric encoding and decoding not logic encoding and decoding.
- ii) It concerns phase gating not logic or 'bit' gating so that the information concerns the phase between bits, which in quantum holography are always normalised, i.e. have magnitude equal to 1 or is held internal to the bit as for example may be the case for a photon with a polarization, or an electron with a spin. And as Deutsch[1985] has shown, the quantum state space of a single such bit may perfectly simulate any physical process or algorithm.
- iii) Quantum holography is intrinsically non-local in character so constituting processes beyond the realm of both classical physics and classical computation, while digital computation is essentially local in character. This is confirmed by the fact that phase conjugation specifies an adaptive resonance and so is non-algorithmic, and as seems very likely from the above non-Godelian. For if language as postulated above is the implicit canonical iconic labelling of object images by objects made

explicit, then each new fact⁺ when added to the language will formally extend the language logically in the Godelian sense.

- iv) And whereas in digital decision making as a many to one process there is usually the narrowing of choices to a single choice and a loss of information therefore as a result of 'bit' gating, phase gates are reversible and the result of combining many outputs is the sum of those outputs, see Figure 1.

FIGURE 1

Phase gating or lossless information processing where $\rightarrow$ indicates information is combined and $\leftarrow$ that it is filtered into its component parts



Furthermore it is known that phase conjugation occurs spontaneously when holography is performed by excitation of stationary modes inside a wave-propagating medium with reflexing boundaries such that both real and virtual images of recorded information are elicited by wave diffraction[Noboli 1985][Noboli 1987]. Thus quantum holography as non-local image and signal processing defines a model of adaptive self-organized computation where it may be hypothesized Evolution as phase conjugate quantum holographic resonance may be phase conjugately mirrored by brain/minds computing by quantum holography as the basis of their image and signal processing in order to survive. Such a model would explain directly how the quantum holographic interference patterns constructed at say the neural level, can directly result in cognitive images and labelling of the real world. That is phase gating performed phase conjugately allows brain/minds to model, i.e. perfectly simulate in the Deutsch[1985] sense, systems as a whole and to filter them into their component subsystems, so avoiding the difficulty encountered in digital computation that the properties of a system cannot be known fully because it is more than the sum of its parts.

⁺ Wittgenstein's principle that the sense of a proposition can only be given once, i.e. there is necessarily only one proposition for each fact that answers to it.

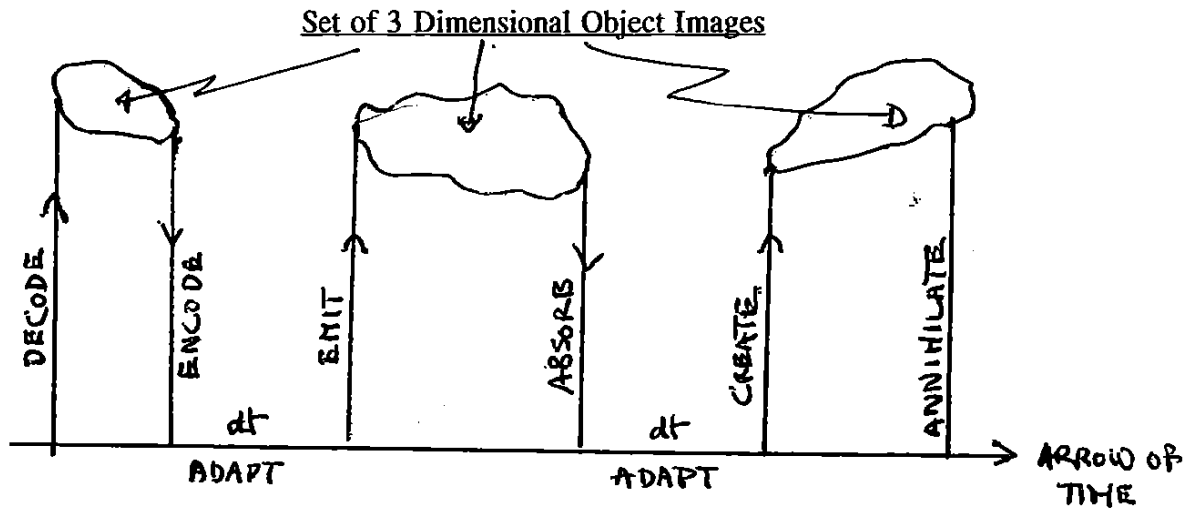
3. THIRD ORDER CYBERNETICS

The first order concerns those cybernetic models where the observer stands independently outside the system modelled as for example in classical physics. The second order is where as in traditional quantum physics, the observer functions as a participator inside the system and so perturbs it, however in the third order not only is the observer a participator inside the system but the observer is aware of this. That is these models concern what above is called phase conjugation, or what in connection with neural Darwinism, Edelman[Edelman 1992] has called the re-entrant process, or what Dubois and Resconi[Dubois and Resconi 1992] have called incursion; and which do, it is postulated, concern organisms with their awareness of their environment. Thus in quantum holography formulated in terms of the emitter/absorber model of quantum mechanics, see Figure 2, the brain/mind is a non-classical machine, working by phase conjugate adaptive resonance to provide the cognitive holographic images, where the mind is a purely quantum mechanism in what is called the quantum vacuum and hence is totally non-local in character self-reflexively connected to the brain in space-time. The mind thus forms part of the control loop for the adaptive resonance appropriate to generate consciousness as the principal mode of such a resonance. In this loop the sensory apparatus of the brain/mind as quantum holographic transducers may through neural events and processes induce mental events and processes in the mind as modelled by the Berry phase of the brain/mind. Or conversely as in thought the mind may induce neural events and processes in the brain, as the result of mental events and processes. The Berry phase[Berry 1988][Anandan 1992], models the mind because in quantum holography it is the quantity of physical significance with respect to the brain/mind as a partially coherent quantum mechanic system or whole, and it is known the Berry phase contains a complete record of where the system has been in space, of how long it was away from any state, and what other quantum states it passed through on its path. That is, the mind maintains in the case of the emitter/absorber model of holographic decoding and encoding, a complete historical record of the brain/mind's adaptive resonant behaviour, and where the action of the mind in such a control loop can therefore be modelled through the fundamental spectral theorem of Hilbert and Von Neumann, i.e.

$$\begin{array}{ccccc}
 & f & \xrightarrow{\quad R_t \quad} & R_t(f) & \\
 F \downarrow & & & & \downarrow W \\
 & F(f) & \xrightarrow{\quad R_t \quad} & R_t(F(f)) = W(R_t(f)) &
 \end{array}$$

i.e. for any vector f in the Hilbert space $H = L^2(\mathbb{R})$ on the real line $\mathbb{R}$, there is a self-adjoint operator F specifying the adaptive resonance to the now geometric weighting operator W which concerns the weight of experience within the brain/mind system's historical record relative to its present experience so determining the rotation R_t in the Hilbert space that will generate the current increment of consciousness, or more precisely the current spectra of adaptive resonance of which the principal mode is what we refer to as consciousness.

FIGURE 2



Emission/Absorption Model of Quantum Holographic Decoding and Encoding

Characteristics:-

- Has an arrow of time
- Consists of a present advancing with the arrow of time
- Has a fixed unalterable past
- Has a present filled with three dimensional objects in relative motion to one another as a consequence of the adaption taking place and where the relativistic invariance of the model says that space-time is a universal property of those three dimensional objects in motion
- Defines an information processing paradigm where object images coincide with the object themselves so that encoded information can be inferred to contain geometric information about the objects perceived including their morphology

And thus if this defines the basis for information processing in the brain/mind, it will account for the stream of conscious perception of which we are all aware.

4. APPENDIX

The Nature of Mapping onto Reality, the Heisenberg Group

This mapping concerns the non-commutative group of symmetries of the Heisenberg Lie Group G which in terms of the unitary of G define the convolution structure of the generation of wavelets from the mother wavelet under Fourier transform action F . These symmetries define the quantum coherence necessary for the quantum holography to take place, and where in particular in regard to the emitter/absorber model G implements in an appropriate gauge the symmetries of phase conjugate based adaptive resonance from the quantum level appropriate to any geometric scale [Schempp 1986] [Schempp 1992].

The multiplicative group G of all unipotent real matrices

$$\begin{pmatrix} 1 & x & z \\ 0 & 1 & y \\ 0 & 0 & 1 \end{pmatrix} : = (x, y, z)$$

with unit element $(0,0,0)$ is a simply connected two step nilpotent Lie group diffeomorphic to the differential manifold $(\mathbb{R}^3) \times \mathbb{R}$ with one dimensional centre $C_G = \{(0,0,z) | z \in \mathbb{R}\}$

G is a realization of the three dimensional Heisenberg group.

The Haar measure of G is the Lesbesque measure $dx \otimes dy \otimes dz$ of the underlying differential manifold $\mathbb{R}^3$ and the Lie algebra $\mathfrak{g}$ of G is formed by the upper triangular matrices

$$\{(x, y, z) - (0, 0, 0) | x, y, z \in \mathbb{R}\}$$

In terms of the canonical basis $\{P, Q, Z\}$ of $\mathfrak{g}$ given by the matrices

$$P := \begin{pmatrix} 0 & 1 & 0 \\ 0 & 0 & 0 \\ 0 & 0 & 0 \end{pmatrix} \quad Q := \begin{pmatrix} 0 & 0 & 0 \\ 0 & 0 & 1 \\ 0 & 0 & 0 \end{pmatrix} \quad Z := \begin{pmatrix} 0 & 0 & 1 \\ 0 & 0 & 0 \\ 0 & 0 & 0 \end{pmatrix}$$

the Heisenberg commutation relations read as follows,

$$[P, Q] = PQ - QP = Z ; [P, Z] = 0 ; [Q, Z] = 0$$

and thus the centre $\mathfrak{c} = \mathbb{R} \cdot Z$ of the Heisenberg Lie algebra $\mathfrak{g}$ is one dimensional and satisfies $\exp(\mathfrak{c}) = C_G$ under the natural Lie diffeomorphism.

The classic uncertainty principle takes the form of the Robertson relation

$$\Delta U_v(P) \cdot \Delta U_v(Q) \geq 1/2 |U_v(Z)| \quad (v \in \mathbb{R}, v \neq 0)$$

where U_v is up to a unitary isomorphism a unique infinite-dimensional irreducible unitary linear representation of the Schrodinger type of G in the standard Hilbert space $H = L^2(\mathbb{R})$;

Δ is the standard root mean square deviation and ν is frequency.

That is, the standard quantum mechanical commutators of the group of symmetries of G are the structure relations of $\mathfrak{g}$ its Lie algebra as was known to Weyl in 1928.

Introduction of the usual complex mode co-ordinates

$$R = 1/2(P + iQ) \quad \text{and} \quad R^+ = 1/2(P - iQ)$$

permits the different alternatives at the photon level that can co-exist in quantum complex linear superposition to be expressed in terms of the annihilation and creation operators of the emitter/absorber model

$$a = U_t(R) \quad ; \quad a^+ = U_t(R^+)$$

through the linear Schrodinger representation U_t of G where in terms of the number states $|n_k\rangle$ which are quantum states with n_k quanta occupying the mode k , these number states are the eigenstates of the number operator

$$N_k = aa^+ \quad \text{and} \quad [a, a^+] = \pi$$

is the bosonic commutation relation.

Example: Quantum Aerodynamics defining the Flight of Birds

Quantum holography says that the controlled stability of flight across an entire aerodynamic domain can be assured if the neurally geometrically encoded aerodynamic object image of the bird coincides with that of the bird itself and where the feathers produce the spectral signature or aerodynamic coherence down to and at the air molecular level necessary for the aerodynamic quantum holography to take place. Thus in the adult bird the evolved wing morphology provides sufficient aerodynamic lift or resonance for the bird to fly, and the evolved neural control morphology will function adaptively to allow the bird to learn, or automatically perform any manoeuvre in the aerodynamic domain that the bird desires. This principle has already been successfully applied to the design of aircraft control by Meirovich and Silverberg [1984] and where the commutative diagram defining the necessary control is set out in [Fatmi et al, 1990].

How the dichotomy between quantum mechanics and general relativity in this model is resolved, owing to the fact the Heisenberg group is a Lie group G where the standard quantum mechanical commutators of G are the structure relations of its Lie algebra $\mathfrak{g}$ (as was known to Weyl in 1928).

Thus let L_x define the action of the Lie derivative where with respect to the Schrodinger equation the symmetries of G hold and $X = (X_1(x,y,z,t), X_2(x,y,z,t), X_3(x,y,z,t), X_4(x,y,z,t))$ then

$$L_x = X_1 \frac{\partial}{\partial x} + X_2 \frac{\partial}{\partial y} + X_3 \frac{\partial}{\partial z} + X_4 \frac{\partial}{\partial t}$$

That is, the invariant pathcurves are those of the associated Lie transformational group and

may be found by solving the corresponding Pfaffian system

$$\frac{dx}{X_1} = \frac{dy}{X_2} = \frac{dz}{X_3} = \frac{dt}{X_4}$$

which are the straight lines in space-time or geodesics of general relativity.

Another way of looking at this [Amari 1990] concerns the phase conjugate adaptive resonance taking place so that the gauge would be defined by the matrix of the metric tensor

$$e_a \cdot e_b = g_{ab}$$

and the non commutivity of G implies that there exists a corresponding metric tensor

$$e^a \cdot e^b = g^{ab}$$

where $e_a \cdot e^b = \delta_a^b$ the Kronecker delta

i.e. one is looking at a system with two coupled Riemannian metrics where one is the inverse of the other, so that it can be shown that if e_a are the tangent vectors along the co-ordinate curves θ_a at θ so that $g_{ab} = g_{ab}(\theta)$

then similarly

$$g^{ab} = g^{ab}(z) \text{ concerns the co-ordinate curves } z^a \text{ at } z.$$

Thus there are two convex potential functions ψ and ϕ by which the two co-ordinate systems are connected by the Legendre transformation

$$\theta^a = \frac{\partial \phi}{\partial z_a} \text{ and } z_a = \frac{\partial \psi}{\partial \theta^a}$$

so that

$$\psi(\theta) + \phi(z) - \sum \theta^a \cdot z_a = 0$$

and there exists Pythagorean relationships between the dual orthogonal geodesics at a point Q such that

$$D(P,Q) + D(Q,R) = D(P,R)$$

demonstrates that such behaviours take place in the Hilbert space with metric $L^2(R)$ with respect to the real line R .

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THE DOGMA OF THE PHOTON

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ABSTRACT

The customary conception of the light-quantum as a travelling particle called the 'photon' has never been more than an *hypothesis*. Since Einstein put it forward, in 1905, this hypothesis has never been seriously questioned. However, any hypothesis which remains unquestioned, for whatever reason, is, by definition, a dogma. The following is the transcript of a talk which challenges this 'photon' dogma on the basis that the relativistic proper-time of a quantum light-contact between an emitter and absorber, over any distance whatsoever, is absolutely and invariantly zero. It is argued that such an intrinsically instantaneous *quantum jump* cannot validly be conceived as a 'travelling particle'.

1. INTRODUCTION

How can a philosopher help physicists? Not by 'doing physics' certainly, nor by offering-up new theories of the sort physicists themselves construct. How, then? One answer is, to act as what Locke called a logical 'under-labourer' to science, clearing up the conceptual clutter which, if allowed to proliferate unchecked, must sooner or later bring the business of trying to understand nature to a halt.

Let's look at some of that clutter. From the point of view of analytical, commonsense philosophy, some of the claims made in science are, quite frankly, ridiculous. One hears, for instance, of 'black holes' (singularities into which information may go but never come out), 'the expanding universe', (Into what is it expanding?) 'tachyons' (particles which endure backwards), 'the creation of time' (At what time was time created?) ... and so on and so on.

The case which concerns us in this paper is a particular kind of nonsense which has been called 'superluminality'. This arises from a demonstrable misunderstanding of the Theory of Relativity which, strange to say, originated with Einstein himself. As he saw it, his theory sets a finite limit c to the speed at which any physical influence can travel. This created what was subsequently called 'Einstein separation', which makes a mystery of the fact that the overall balance of things at a distance is conserved in accordance with Newton's second law of immediate and reciprocal interaction. As a result, it became necessary to postulate physical influences travelling 'faster than light' in order to conserve that balance, so that much time and effort is now devoted in physics to devising and carrying out expensive, time-consuming experiments to prove that these 'superluminal' influences exist.

However, Einstein had strangely neglected to consider that in his own theory, the relativistic proper-time of a light-signal between A and B is uniformly and invariantly zero, which makes the search for 'superluminality' unnecessary. Moreover, the fact that a light-signal *in itself* takes virtually no time at all to get from here to, say, the moon and back, whilst taking one-and-a-quarter seconds relative to a terrestrial observer can seem

paradoxical only in the context of a classical view of time as an absolute, universally advancing GMT. In the context of *Relativity* it presents no paradox at all.

It is surprising, therefore, to say the least, that since light is intrinsically instantaneous, we more and more often hear claims to the effect that these so-called 'superluminal' influences have been *discovered*! These claims are nonsense, because at speeds theoretically 'faster than c ' the proper duration of a physical influence becomes negative, which means that it has to act *backwards* in time. But if any such causal influence could act backwards it could cancel itself – like killing one's own grandfather and thus cancelling both oneself and the act that did the killing. Is *this*, then, what these claimants have 'discovered'? Hardly, because this well-known 'Grandfather' paradox constitutes the *reductio ad absurdum* of backwards causality and, in consequence, squashes any notion of 'superluminality' – as if that notion were necessary, anyway!

Another surprisingly strange notion of Einstein's, allied to the above, is that this relativistically instantaneous quantum-jump is a space-travelling material particle, which he called the 'photon'. I was in a conference where people argued about whether the rest mass of this 'photon' is zero or non-zero. The dilemma this presents is that if the rest-mass of a photon is assumed to be zero then that means, in ordinary commonsense language, that it doesn't exist – far less as a 'particle'. On the other hand, if the photon's rest-mass is non-zero, then at the speed c its mass is relativistically infinite. So the dilemma is that on both counts, the notion that the 'photon' is a travelling particle ends in absurdity.

At the conference, however, theoreticians unable to accept this *reductio ad absurdum* of the 'photon' concept hedged like crazy. Some tried to get out of it by claiming that the photon's speed was *not quite* c , and that its rest-mass was *not quite* zero. By this means they sought to fiddle things so as to get exactly the empirical mass of the photon, which is, in the order of 10^{-38} kilogram for visible light. When it was pointed out that since it is axiomatic in *Relativity* that in *no reference-frame whatsoever* is the 'speed of light' anything *but* c , so that there is no physical frame in which the speed of the photon can possibly be zero, confusion reigned. Moreover, when the question was raised that if the photon doesn't travel *right at* the speed c , then what is it that *does* travel at the speed c , the reply was that nothing travels *at* that speed because it is an asymptotic speed – an unreachable speed. In that case, it was asked, what is it that is *measured* that is called c ? 'A light-signal', was the reply, to which the rejoinder was, 'Is a light-signal, then, logically, nothing?' ... and so on.

In this way, the discussion collapsed into complete absurdity. Every conceivable logical alternative was considered – with one very conspicuous exception. This was the alternative of *abandoning* the hypothesis that photons are space-travelling particles in favour of seeing them for what they really are, proper-time-instantaneous quantum jumps of the sort which satisfy (albeit statistically rather than continuously) the at-a-distance interconnection demanded by Newton's third law.

The following is the transcript of a short prepared talk on the subject of the above-mentioned conceptual alternative to the 'space-travelling photon'. This took about twenty minutes and was followed by thirty minutes of dialogue.

Talk given at ANPA 17, Cambridge, September 1995

'My experience of conferences such as this is that few people can concentrate long enough to follow a long and highly structured argument, especially on a subject which might not immediately invoke their sympathy. The result is that people who present such arguments seem to end up talking mostly to themselves. In this talk, therefore, I shall say what I have to say, as plainly as I can, leaving the logical details to be read in previous Proceedings of ANPA and other published literature, by anyone who may be interested in following it up.

Now we are all, whether we like it or not, *philosophers* of one kind or another. The only difference in that respect is a matter of whether our philosophies are good or bad. The very worst sort of philosophy is the sort which refuses to recognise itself as such, where people say something like, "I'm an ordinary commonsense thinker – an engineer, builder, physicist or whatever – with no interest whatsoever in philosophy." But that's just daft, because if we did not subscribe to *any* philosophical system our society would scarcely be superior to that of the chimpanzees. So, what such *philosophobia* amounts to is simply leaving the necessary philosophising to others, as the Germans left it to Nietzsche and Hitler; the Russians to Lenin and Karl Marx, the Iranians to their Ayatollahs, the Chinese to their Confucius and Mao Tse Tung, and we to people like Descartes, Galileo, Newton, Einstein and Margaret Thatcher. It is the uncritical, off-the-peg acceptance of such philosophies on the part of so many "good commonsense folks" that constitutes social dogma. And since those we leave to do our philosophising for us are never gods but always finite beings like ourselves, then unless the philosophical systems they devise are very, *very* good, they will conflict with one another and, eventually, with our own and perhaps even their best interests. Ideally, these philosophical conflicts should be resolved by pure unbiased reason. However, the brute momentum of dogmatism which usually sustains such systems leaves us little recourse but to resolve these conflicts in wars or other forms of social strife, the outcome of which is scarcely ever rational but, more often than not, purely anthropological and circumstantial.

'All this, of course, we have to accept as a condition of our social existence. However, it is a supreme irony when these same irrational conflicts occur in an ostensibly *rational* subject like science. In physics, for instance, there are two logically distinct or antithetical possible approaches to the subject. One is to think of the light, by which we see things, as travelling in space. The other is to think of those things and the space which separates them as *in the light*. That is to say, according to this latter view, light is simply what we see – the opposite of dark – and the dimensions of space are 'video-projected', so to speak, out of patterns and sequences of brightness and darkness, similarly to the way in which a television viewer projects a whole three-dimensional 'world' out of patterns and sequences of lighted screen-pixels.

'This latter way of thinking is called phenomenalism – or 'Positivism', as physicists (mistakenly) persist in calling it. The other view, conceived by reaction, physicists call 'Realism'. Now, to decide which, if either, of these antitheses is true and which is false has been the job of philosophers since science began. (To assume that they can *both* be true is sheer sophistry). And now the burden of that decision has devolved upon science itself in

the internecine conflict between the two philosophically opposed groups of physicists known, on the one hand, as the Copenhagen group, after Niels Bohr – the so-called 'Positivists' – and, on the other hand the reactionary, anti-Positivist followers of Einstein, the self-styled 'Realists', from whose symposium at Toronto I have only recently returned.

Now each of these groups, of course, has had to cope in its own way with the undeniable fact that the light-energy in which we perceive the world is *quantised*. For the Realist those light-quanta are travelling particles called 'photons'. These, it is supposed, are emitted by atoms of high optical energy (in the sun, for instance) and absorbed by atoms of lower optical energy – in the earth, the eye of the observer or in some observing instrument. For the phenomenalist the beginnings and ends of these quantum events cannot be separated in that way. Each quantum of illumination is the ultimate, irreducible optical sense-datum, or '*photum*', and it is out of informational patterns and sequences of these whole events that the optical information-processor which relativists call 'the observer', projects physical space and all that it contains. In order to offset the usual, philosophically naïve notion that this implies subjective idealism or 'solipsism', let it be understood that since the information-projected space of each and every one of these observers contains other observers and observing instruments, relatively to whom or which *he* appears in the same way as they do relatively to him, that is, as just one among many observational stations, or 'observicles' – the resulting, interprojected space is as observer-decentralised or objective as needs be for the purposes of both physics and commonsense.

Phenomenalism is nevertheless *relativistic*, insofar as in that way of thinking, everything exists, as in Einsteinian relativity, *relatively to the observer* – which, *a propos* my previous remark, is *not* to say relative solely to *oneself* nor, indeed, necessarily any other human being or living thing. In phenomenism as properly understood, time and space, the dimensions of all matter and material process, are projected by each observer or observicle (animal, vegetable or mineral) *individually* out of the quantum information presented, and physical objectivity is nothing but a function of informational *communication*, however complex or rudimentary, between those observers or observicles. (This emphasis on *communication* is, of course, the reason why *language* configures so prominently in modern philosophy, in the way which so bemuses and irritates their 'Realist' opponents.) "Realism", by complete contrast, is essentially absolutistic, or *non-relativistic*. That is to say, in the view of the Realist, space and its contents exist absolutely – that is, perceived as it were "by God", independently of our finite human and instrumental perceptions. That "universe" is conceived, accordingly, as a pristine spatial *continuum*, with a universal present moment advancing at the same rate (i.e., absolutely simultaneously or synchronously) all over. All physical processes in that "universe" are, accordingly, completely determinate.

Nothing, then could be more clearly *antithetical* than these two "Realist" and "Phenomenalist" approaches to nature which have been adopted by physicists. Yet confusion continues to proliferate due to a stubborn refusal of physicists to recognise that what they are dealing with is a *philosophical* issue and one which, if only they knew it, has *already been solved in Philosophy proper*. For instance, as this Association has been informed on more than one occasion, this dichotomy between "Positivism" and "Realism", which physicists unwittingly subscribe to in one way or another, *no longer holds*. This follows from a famous paper called 'The Refutation of Idealism', written at the beginning of this

century by the Cambridge philosopher G.E. Moore.¹ Since then the mainstream modern philosophers, such as Austin, Ryle, Ayer and Wittgenstein have recognised that Berkeley's version of phenomenalism, encapsulated in his notorious subjective-idealist dictum "To be is to be perceived", is fallacious. So there is no longer any justification for thinking of things and our perceptions of them as belonging to what Descartes thought of as the two "separate worlds" of "Mind and Matter". *Information*, which is the basis of modern physics, is as much in the nature of mind as of matter; and so is *observation* in the context of relativity. So the old dualist distinction between mind and matter disappears on both counts, of modern philosophy and of modern physics, and is replaced by a neutralist sort of realism called Normal Realism, which incorporates both. Meanwhile, *physicists*, knowing next to nothing about that, and caring even less, continue to believe that Berkeley's subjective idealism, which predates these developments by nigh on *two and a half centuries*, remains the last word in phenomenalism. What on earth do those physicists think that philosophers have been doing in the meantime? As I say, I have just returned from giving a talk to a group of physicists of the "Realist" persuasion at Toronto. This was a symposium in honour of Jean-Pierre Vigi r, and was called "The Present Status of the Quantum Theory of Light". At the end of my talk, one eminent member, unable to digest my careful philosophical arguments to the contrary, could only conclude that in presenting my modern *phenomenalist* viewpoint I was simply reiterating, as he said, the old 18th Century view of George Berkeley, that "It's all in the mind". And, he added, the *a priori* methods of the philosophers can have no relevance whatever for experimental physics – to which I replied that *that* was as *a priori* a statement as I had ever heard!

'So I have to say that in my considered opinion, the central problem in science, of the continuing conflict between relativity and quantum theory, is that physicists in general are oblivious to the *philosophy* underlying their own subject. For instance, as I began my talk at the Toronto Symposium, a few people left the room, and it was reported to me that when one of these was asked why he had done so, he replied "When he said he was a philosopher I got up and left!" A somewhat kinder, but no less revealing comment was that I had unnecessarily put myself at a grave disadvantage with those physicists by announcing that I was a philosopher, and I was kindly and avuncularly advised to avoid doing that on future occasions. Another questioner asked me whether there was anything in what I had said that was of relevance to experimental physics. When I replied that there was and that this could be seen in the copies I had distributed of the galley-proofs of my latest, *Physics Essays* paper² on the effects of spin on gravitational orbits, he told me, pontifically, "then *that* is what you should have talked about, here, not about philosophy."

'The tragedy, therefore, is that in thinking of philosophy is irrelevant, and even antagonistic to their subject, physicists shut themselves off from what modern Philosophy may have to offer regarding some central problems they are currently wrestling with. When I was at CERN, John Bell said to me: "We have real conceptual problems here. Why aren't you philosophers here in droves helping us?" Looking back, with my present experience of having tried to do just that, I might have replied that it is simply because physicists in

¹ Moore, G.E., 'The Refutation of Idealism', *Philosophical Studies*, Routledge, London, 1922. pp. 1- 30.

² N. V. Pope and A.D. Osborne, 'Instantaneous Gravitational and Inertial Action-at-a-Distance', *Physics Essays*, 8, 3, September, 1995.

general seem to resent being advised by philosophers. Those physicists therefore try to solve their essentially *philosophical* problems by completely uninformed and inappropriate "do-it-yourself" means. They hedge the antitheses, think-up all sorts of metaphysical theories supported by convoluted mathematics and so-called "Gedanken" experiments which, as someone said to me at the Vigier Symposium, is

"... like watching different people throwing different balls in the air and catching them in all sorts of intricate and amusing ways. Everyone politely claps each particular performance but no-one is impressed, because they know that since the balls are purely theoretical and invisible they knew all along that the juggler was bound to catch them."

'So, as someone else remarked, in the same conversation, "Anyone with a grain of commonsense can see that those experimentalist strategies don't stand a cat's chance of solving the *philosophical* problems that physics has raised". Experimental evidence may, of course, be the deciding factor in choosing between clear-cut and well-developed antitheses – as, indeed, I would say, it has already done in favour of phenomenalism (I speak, of course, of the experimental proofs of the relativistic staggering of proper-time and of Lewisian quantum immediacy by Hafele, Alan Aspect, Carol Alley, *et al.*, which militate decisively against classical Realism.) However, it is plain that mere *ad lib* experimentation, in itself, can achieve nothing. Unless the results of an experiment can be shown to decide in favour of or against some *clearly formulated thesis*, they can create nothing but the sort of confusion which, in modern physics, we now witness on all hands. That is to say, without the sort of logical sorting-process which philosophical analysis provides, a mere collection of unconnected theories and experimental information merely "blinds us with science".

'Something which became plain at the Toronto Symposium, was that, as a colleague remarked, "Einstein is very much the 'patron saint' of these scientists." And that is despite the fact that Einstein claimed to be a *philosopher* rather than a scientist. (I'll bet that no-one walked out on *him* for making that admission!) Yet in the scathing estimation of his philosophical mentor, Mach, as a philosopher, Einstein, as the saying goes, would have made a "good butcher". That is to say, while he, Einstein, ostensibly espoused phenomenalism in his observationalist attitude towards space and time, he nevertheless remained a Realist in his adherence to the classical idea of an underlying and determinate *continuum*. Mach would have clearly perceived the contradiction there, that this underlying continuity of Einstein's was fundamentally and irreconcilably at odds with his philosophical relativism – as has transpired in the now well-known incompatibility between Relativity and Quantum theory. Mach's stated opinion, therefore, was that Einstein's physicist's version of relativity would never amount to more than a "passing phase" in our search for physical and philosophical understanding. That predicted "passing phase", however, has been extremely long in passing. This is because Einstein's Theory remains a curious and contradictory mixture of relativism and realism which, due to the saintly and infallible reputation of its creator, resists rational revision. And this is despite the fact that it throws up paradoxes and perplexities like EPR and the insoluble problem of discreteness and indeterminacy.

'All this has been put to this Association on more than one occasion, with ample demonstration of how very much easier it is to deduce special relativity on the basis of

Bondi's purely phenomenalist "radar" interpretation of the constant c as an optical distance-time conversion factor instead of Einstein's "velocity of light in a vacuum". Moreover, no valid objection to this demonstration has yet been sustained. But at the end of the day, the usual talk of "the velocity of light *in vacuo*" continues unabated, on the clear assumption that since it was Einstein's view that light travels in that way it must somehow be true, even though it is complete nonsense. Moreover, this nonsensical and unnecessary conception of light having a constant and continuous velocity relative to a continuous *vacuum* (which has only to be stated to see how nonsensical it is) remains in conflict with the discretist emphasis of quantum physics and, needless to say, of ANPA. All this reminds one of the old truism: "I see and approve the better course but follow the worse."

'On that challenging note I now finish reading these prepared remarks and open the field for general discussion.'

Dialogue:

Marcer: 'You distinguish between realism and phenomenism. But I can conceive a larger domain which encompasses both. In that new domain you might find an extension – a new set of laws which will extend your knowledge beyond that of both the realists and the phenomenists. It seems to me that that is what ANPA, to a large extent, does.'

Pope: 'Well, then, I have to say, so much the worse for ANPA! Because, you see, if you try to reconcile in that way, two such clear-cut antitheses you're in the real of sophistry. This was plain in the other conference I was at. Everyone was coming up with some new idea, in response to which people clapped and went on their way, and it didn't matter a pinch of spit at the end of the day. It just can't happen like that! Geoffrey (Constable) and I had a bit of a ding-dong about this, didn't we ...'

Constable: 'In courteous correspondence.'

Pope: 'Too courteous. *Too* courteous! [*laughter*] There is no place for courtesy where philosophical truth is concerned. As a philosopher I'm not in the business of creating beatific visions of Reality and theories about Truth. Me, I'm an intellectual "bull-fighter". I'm after the guys that are creating "bull" and there's a hell of a lot of that about. So I feel, you see, that even at the risk of offending people (this is the Socratic ideal and ever was), you have to be able to make a *decision*. You can't have guys coming here presenting theories, and twice as many next year and say "We've got twice as many theories this year – we've got *growth* here, right!". And next year we have more and more theories ... Hell, there's got to be some *selection* process.³ I see philosophy, you see, Geoffrey, as having to *select*. I don't know what is Right or True. But I sure as hell know what's *false*.'

³ As Prof. Paul Marmet wrote to me following our meeting at Toronto: 'Most physicists believe that the time of classical physics is over and that it will never be possible to describe modern physics using rational principles.' The characteristic of modern physics, he says, 'is that a phenomenon can be described as the sum of concepts even when these are not compatible. This,' he adds, 'allows researchers to deal with irrationality without having to be bothered by it.'

Constable: 'Can I respond to that, seeing as I'm in the dark here? You responded to the splendid piece I sent you that reported that the whole of Mozart's Fortieth Symphony had been sent at four-point-two times the speed of light. It was no April Fool's joke, but an observation. Your response was to say, "What shocking things are published these days". Have you any further response to that?'

Pope: Only to repeat, *ad nauseam*, perhaps: What the hell is the "speed of light"? The *speed of light*! For God's sake! Who measures the "speed of light"? If the observer measures the speed of light by his laboratory clock he gets a different speed from what he would get if he read the distance that the signal travels *in its own time*, which is an infinite speed⁴.

Deakin: 'May I ask you a question on that! The practice of radar engineers is that they send out a pulse of radiation which may be some hundreds of packets of vibrations of the carrier frequency, so it contains a large number of photons because the frequencies are low compared with light, very low – the energies are low, the energies of the photons. They send this off and a pulse may last for, say, a tenth of a microsecond ...'

Pope: 'Let me stop you there because I know what you're going to say. I'm not denying ...'

Deakin: 'Well I want to put it first and then you can comment on it. The practice is to send it off with a clock, right? A clock carried in the radar. It (the pulse) bounces off something and then comes back again and it's recognisable, and the clock notes the distance – the time – again. Now we *conventionally* turn that into distance. And that's only a *convention*.⁴ But we check our convention by other methods of measurement. In other words we actually do geometrical surveys – which are not dynamical things – to say, yes, the tower the signal bounced off, which we can clearly see on the display, *is* 3.4 miles away – or isn't as the case may be. Now isn't that a *reasonable practice*? And to state that light has a speed is merely a convention, but it's a conversion-factor.'

Pope: 'Well, it was a *convention* to talk about "phlogiston" and "caloric". But because it's a *convention* doesn't mean that it's right. A conversion factor? Sure it is! And Bondi based his idea of light on that, precisely. Because he was working with radar. And he said "Look, we don't need to have the 'speed of light' here." We have a polar system of reference – which you do in your radar. Tony. And you can say that where you have umpteen radar stations you can convert that into Cartesian (rectangular) dimensions by the ordinary mathematical means. In this way we can picture ourselves, as observers, as all little "radar-stations" – I send a pulse to you and you bounce it back. That's alright ...'

Deakin: 'But you talk about the speed of sound ...'

⁴ As I remarked to someone later, note the 'lipservice' by which students are taught the principles of physics, where the teacher says 'Of course, this is only a *convention*,' and that it is not absolutely *necessary* to interpret things that way or choose precisely those premises' What goes unsaid is that it is also *conventionally necessary* to interpret things that way and to choose those particular premises, otherwise you don't pass your *conventional* exams and become employed as a *conventional* physicist.

Pope: 'Yes, you can talk about the speed of sound, of waves of sound. It makes sense to talk about a wave of sound because you have molecules of air waving. But what the *hell* waves in a vacuum?'

Deakin: 'Okay. now let me try one more on you, then. Between the radar and the big target, which it always bounces off, as it were, you can put a lot of subsidiary targets at quite close intervals and you get a bounce from each one of those and so the received thing is a series of received pulses, and you will find that the spacing of the peaks in the pulses corresponds to the distance, and the *same conversion factor*. And because you can do this subdivision you *have* to say that *something* is going from A to B ...'

Pope: 'Not philosophically, Tony, not *philosophically*.'

Deakin: ... because you've got a time [*pointing in the air*] there, there, there, there and there!'

Pope: 'You don't *have* to say that.'

Deakin: 'But why *shouldn't* you?'

Pope: 'But why *should* you, is my point. If I say to you there are two logical alternatives to something and I pick just one, then that is special pleading!'

Deakin: 'But there is no reason to believe it's different from anything I have described.'

Pope: 'Ah, not in *your* way of thinking, Tony. But in mine I can clearly see a new way, a *different* way – and not just me; others can, too! So you see, when you have two alternatives and you choose one, and *only* one because you say "I like that because it's the one I've always chosen," then that is special pleading and *philosophically*, it's a fallacy.'

Kilmister: 'But *that is what you are doing*, Viv. You have chosen *one* and then you say why should you ... [*noise and laughter*]

Pope: 'Oh no; no, no, *no!* I am trying to show you guys that by exploring *another* one, you break the *dogmatism* of the other. You see, when *everybody* is choosing just one way of thinking, it doesn't do any harm for someone like me to come along and irritate that whole community by saying you don't *have* to think like that, that there is something which has so far not been considered.'

Kilmister: 'But I think you've got the wrong target because I'm sure that most of us accept that yours is another way of thinking about it. You don't *have* to persuade us of that! But what is at issue here is, for example – if I change the argument slightly – you said that "light travels" is a non-empirical statement ...'

Pope: 'In *vacuo*?'

Kilmister: 'Yes, in *vacuo*'.

Pope: 'You see, I agree that light travels in the way Tony is saying ...'

Kilmister: 'That's what I was going to say. I mean, you said "non-empirical", so you're saying, there, that there isn't an empirical backing for it. Well, I want to ask you, what

would be an empirical backing better than the one Tony has just described? I mean, if you're asking for an empirical backing, then you must have an idea of what form it would be.'

Pope: 'Well, the answer is, of course – and this is where the Wittgensteinian twist comes in – This is very difficult because it involves a change of *Gestalt*. I mean, what I'm going to say now, interpreted in terms of *your Gestalt* it's going to be nonsense. You have to change the philosophical *Gestalt* ...' [noise]

Pope: 'Hey, I'm not doing a "Billy Graham" here! I'm simply saying, look, there is *another way*, guys, which, if you can look at it can make a lot of sense – as much sense at least as the other way. Okay, so I come across as a bit of an evangelist; but that's only by reaction. I just want to point out that there is another logically consistent way which – to reiterate what Adam (Parker Rhodes) said, is not the Western way.'

Parker-Rhodes: 'I'm not quite sure that that's what I said ...'

Pope: 'Well, whether it was or not, I'm not really concerned here. [laughter] I want to show that there is a peculiarly Western way of thinking which separates the object and the observer, so that something has to mediate between that object and ourselves. That has been, traditionally, the light-rays of Empedocles – you know, the "streaming- substance" that comes from the object to the observer. It is not an uncontested view because ...'

Parker-Rhodes: 'I never suggested that phenomenism and positivism were anything to do with Oriental thinking.'

Pope: 'Eh? What's that?'

Parker-Rhodes: 'I consider phenomenism *and* positivism to be thoroughly *Western* ways of thinking.'

Pope: 'Yes. Berkeleyan phenomenism. [noise] Well okay, you and I can discuss that sometime. Let me answer Clive's (Kilmister's) point here. The *other* way of thinking is this: instead of thinking of the light as travelling in space you do a *complete* inversion – and *that's* the difficult intellectual trick! You don't think of the light as travelling in space. You think of the space and everything that goes on in it as projected out of the light. The light comes first, *a priori*. You don't split a quantum-event between an emitter and an absorber. Anything you do like that's going to be purely theoretical (metaphysical or non-empirical). Any such space you may interpose between a quantum emitter and absorber can *never* be measured; because that means *splitting a quantum*, which you cannot do. So you have this new way of thinking, which is entirely self-sufficient. And you cannot judge a thesis by its *antithesis*. As I say, that's the logical fallacy of *ponendo tollens* if you say, "look, that new way doesn't fit with my way of thinking, so it's false." It really behoves you to *see* this other way of thinking and *understand* it first before you can judge it.'

Bastin: 'Look, now I'm going to put my foot in it a bit. There are several people here who value what you do and what you say, and accept that in your criticism that you insist on bringing up, you are indeed pointing to real troubles. Now, okay, let us take another step and say, alright, relativity theory does contain obscurities and complexities and actual inconsistencies and that in fact there is something very seriously wrong with it. Now you say "I can put an alternative" ...'

Pope: 'I say there *is* an alternative.'

Bastin: 'Well, I say that you have a hell of a long way to go before you get there!'

Pope: 'I don't think so!'

Bastin: 'Well you won't perhaps like me saying that, but I think that that is the case, that you have a long, long way to go before you can put a serious alternative. I believe that when you *do* get a serious alternative it will be a "killing". But that remains to be done.'

Pope: 'Well, and I'm worried about this: I have to say – and again I hesitate about this, because I have to say that I don't see that anybody is really *au fait* with this alternative. I mean, I've answered all the questions that are put to me. You see, it's a different *Gestalt*. It's got to be *understood*!' [See Analysis, Point 1.]

Bastin: 'Come on! You could be saying, here: "I'm a *philosopher*, I'm *not* doing science." You *dare* not say that! Wittgenstein said that. But then he bloody-well *didn't* do science! But you're *here*! You *do* want to do science ...'

Pope: 'Well yes, as I said to Vigier at the Toronto Symposium, I have a paper now out on gravitation.'⁵

Bastin: '... in some sense causing a dislocation of argumentation.'

Manthey: 'I think there's a great distortion here in talking about this *Gestalt*. You want us to conceive the world as if everything comes in proper time.'

Pope: 'No, not quite. There's a *duality*. What I'm saying is that you can look upon light as instantaneous or as travelling at the speed *c*, depending on what clock you use. [See Analysis, point 2.] The *information* travels at speed *c*. The "photon" doesn't. It *can't*.'

Manthey: 'I thought I was agreeing with you there! That's why I'm a bit surprised at the vehemence of your answer. It strikes me that your whole point that it doesn't travel anywhere ... You are saying that we must "move our butt" into the frame of the photon.' [See Analysis, Point 3.]

Pope: 'No, I don't say quite that. What I am saying is that if you are to *understand* that then you must "move your butt" into (the theory) at least *temporarily*.'

Manthey: 'Fine, then what I'm trying to say is, a) I think that's an excellent idea, b) I've been trying to do that for years and, c) that has resulted in my concept of co-occurrence, which has the same properties as the photon. Okay, so if we were to sit in your *Gestalt* then we would start looking at the world as this universe of co-occurrences on various scales, and that's it.'

Pope: 'That would be an interesting thing to try. But if people don't try it, they won't know whether it works or not.'

Manthey: 'But if you want everyone to go from where they are to where you want them to be, then they have to look at the world that way!'

⁵ See Footnote 2

Pope: 'Just for the moment, to give it a try. Once they've got it – and I can tell them when they've got it – *then* they can judge.'

Manthey: 'Isn't it true, though, that you'd be ecstatic if everybody, once they got there, stayed there?'

Pope: 'Not if I suddenly found there was something wrong with it.' [*laughter*]

Etter: 'I wanted to ... kind of put a challenge. Now to me, the presenting symptom as the doctors would say, that physics presents to philosophy, the most drastic kind of criticism, or sign of pathology, is the following. There can be situations in which A is possible and in which B is possible but in which A-or-B is impossible. Now that strikes me as something that philosophy's got to deal with. Now much as I like the notion of a good social phenomenism, well worked out and thought through, I don't see what that could make of the presenting symptom.' [See Analysis, Point 4.]

Pope: 'Well, it would get rid of a lot of bullshit. We could get rid of that in great big gobs! And things would be a lot, lot clearer. Or, as Berkeley said, "We kick up a dust and complain we cannot see." And there has never been a greater "dust" than there is at the moment! I saw that in those other conferences I've been to. They're never going to get anywhere. It's useless. It's like having an ashtray on a motorbike. Things don't work!'

Deakin: 'I'm going to make one short remark. Yes, if you manage to get astride of the photon and into its coordinate system, then you can ride it and it takes no time at all to get to ...'

Pope: 'But I'm not talking about that. *You* do that, *I* don't do that. I don't seek to get astride a photon ...'

Deakin: 'Just let me finish ...'.

Pope: '... Who the hell gets astride a photon?' [See, again, Analysis, Point 3]

Deakin: 'Right! Now I can't get any of my radar engineers to buy "photons". They just don't do it. What they do is just practise. This practice that I describe is just a convenient parlance to speak of the speed of light. Now we know there are mysteries. For example you can have an electronically ganging radar where no mechanical thing moves and the beam is still shifted about – and you can interpret it as if the beam was shifting about. And yet you can calculate that the end of this so-called "beam" is moving faster than light. But what you are actually dealing with is the shifting pattern of phases. That's the only reality. You're actually setting it up at the antenna, saying "You now, Fred, then you, George, now, Charlie," and then these fronts come out at different phases and they combine-up and produce this bloody beam! And fifty kilometres away the object is illuminated.'

Pope: 'Yes, but there are no *photons* doing that!'

Deakin: 'No *photons* ...'

Pope: 'My point, my point! ...'

Deakin: 'There's a *phase* that travels faster than the speed of light.' [See Analysis, Point 5.]

Pope: 'I've already said, you see, that *signals* go at the speed of light. *Signals* do. But even they don't go *in vacuo* because there are no waves in a vacuum. How can anything *wave* in a vacuum?'

Constable: 'An energy goes across a vacuum.'

Pope: 'Where have you seen an energy go across a vacuum?'

Deakin: 'But if it's got photons in it, it *isn't a vacuum!*'

Pope: 'Oh, Boy! That's like the Cartesian (ontological) argument for the existence of God, that God is perfect so if he didn't exist he wouldn't be perfect, therefore God exists! You can't argue (for the existence of photons) like that!' [Bell rings to end discussion].

Bowden: 'We're going to have to stop this now.'

Pope: 'Right. Just trying you out, guys!' [laughter and applause].

Analysis (numbered as referred to in text)

Point 1. In subsequent discussion with Bastin he explained that he thought I might ducking my resolve to get involved in physics by refusing to answer *physicists'* questions. However, as I explained to Kilmister, later, there may be no *experimental* way of choosing between *philosophical* theories and paradigms. How, for instance, would one choose *experimentally* between the Copernican and Ptolemaic interpretations of planetary motion, since the observational/experimental data are the same in both systems. Yet who would claim that the Gestalt-shift into the Copernican mode of astrophysics 'has no relevance for physics'? It was the same, I said (in answer to a question Kilmister had put to me earlier) with the famous Fizeau-Michelson toothed-wheel experiment to determine the value of *c*. Since the value of *c* as determined by that apparatus is exactly the same, whether you interpret it as Einstein's 'speed of light' or simply as Bondi's 'factor' for converting length and time, there was no way in which the experimental data could distinguish between the two interpretations. In fact, I said, all the standard optical experiments to determine propagation, reflection, refraction or whatever are evidence as much of the one interpretation as of the other. So, *a propos* Bastin's later point in the dialogue about me, as a philosopher, wanting to do science, I should have said that in order to even things up, in ANPA at least, some physicists there should want to do philosophy – not the old and out-dated 'philosophy' which physicists tend to think of as such, but the proper, up-to-date philosophy which, as I said in my talk, takes off with the *later* Wittgenstein and supersedes Positivism and Berkeleyan subjectivism.

On this same point, I have to disagree with Bastin's comment to me, over coffee, that philosophy is simply 'clear thought'. We were interrupted, otherwise I should have said that philosophy is much more than just that, that it is clear thought *plus* the sort of *creative imagination* which Copernicus showed in shifting from one 'clear-thought' paradigm to another – and which Wittgenstein showed in shifting from his earlier way of thinking, in his *Tractatus*, to the later and radically different way he revealed in his *Investigations*.

Point 2. Manthey says that I want to conceive – and for everyone else to conceive – the world 'as if everything comes in proper time.' However, that is more like what Tom Phipps

(Jr.) wants than what I want. The world, as I conceive it, comes in *both proper time and relative time*. The *proper time* of a body is what the observer *sees* as registered by the body's clocks in travelling some observational distance, and the *relative time* is the time he also *sees* to be taken by the body in travelling that same observational distance as registered by the clocks of the observatory. That is to say, the observer *sees* the two *different* times running 'concurrently'. It is the same relative motion in both cases, while, as the latter tends to c the former tends to infinity (instantaneity). So it can be said, without contradiction, as I explained in my talk, that the so-called 'speed of light'(or of 'photons') is *both finite and instantaneous*. However, I did not pick up on that, over-concerned as I was to offset any charge of dogmatism on my part, which is why the dialogue diverges at that point.

Point 3. Here I should have said that 'the frame of the photon' is a phrase without meaning because it is axiomatic in Relativity that light (hence the 'photon' – allowing that fiction) has the 'speed c ' in *all* physical frames of reference. So there is no conceivable physical frame of reference in which the photon can be regarded as stationary.

Point 4 Aware that time was running out I did not really answer Tom's question. What I should have said is that what he regards as the 'pathological' state of science, where we can have theory A and theory B but not theories A or B (i.e., $\sim(A \vee B)$) has to be due to a misidentification of 'A' and 'B'. The cure that Normal Realist philosophy offers, is 'properly to interpret theses A and B.' For instance, if A is Relativity Theory and B is Quantum Theory, we can have a situation (in phenomenalism, as I say) where we have *both A and B* (i.e., $A \cdot B$), and even $((A \supset B) \cdot (B \supset A))$, or $A \equiv B$. The 'bullshit' mentioned in my answer to Etter, in the dialogue, refers to the proliferating attempts to prescribe quack remedies for rather than curing the 'presenting symptom' he mentions.

Point 5 I obviously failed to pick up, here, on Deakin's word 'faster' (my dictaphone seems to hear better than I do!) My answer should have been that this 'faster than light' phenomenon is no more than a kinematical illusion, like the motions of the stars which, in those great circles they seem to traverse in one cycle of the earth's rotation, might be said to 'travel' many times faster than the speed of light. The fact that those motions of the stars or of Deakin's swinging radar beam are non-real (i.e., purely kinematical or non-dynamical) means that for the purposes of physics they may be discounted. Having missed what Deakin had said about 'faster than light', however, my next point was designed to answer *as if* he had said 'at the speed of light'.

/Ends

GRAVITY OUT OF INERTIA

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ABSTRACT

The work of this paper is presented as an illustration of a newly developed, and fundamental, approach to the derivation of gravitation theory. For the purposes of the illustration, the development is set wholly within the classical context of the conservation laws of Galileo and Newton applied to a *finite* and *discrete* model Universe, and it shows how classical gravitational action can be expressed in terms of a metric formalism which arises purely from a consideration of momentum conservation within this model Universe.

1 INTRODUCTION

The following analysis, which is performed within the classical context of the conservation laws of Galileo and Newton, is intended as an *illustration* of a particular approach to the development of gravitation theory which can be readily adapted to the conservation laws of modern physics, and its potential relevance to the general ANPA programme - the reconstruction of physics out of the combinatorial hierarchy - lies in the fact that it shows how a description of gravitational processes can be obtained purely from an appropriate consideration of global momentum conservation. In other words, gravitation is seen to arise out of the process which defines *inertial mass* - the momentum conserving interaction. The possible significance of this to the ANPA programme arises from that work of Noyes in which he shows how inertial mass determinations of elementary particles by the measurement of scattering angles in collision processes can be given an interpretation in Bit-String physics. He argues that if Bit-String physics can accommodate the notion of inertial mass, and if, ultimately, gravitational processes can be expressed purely in terms of inertial processes then gravitational processes will automatically fall within the purview of Bit-String physics.

The reason for developing the approach in the classical framework is simply that of its (relative) conceptual simplicity, and the fact that a familiarity with the analysis in this classical context is likely to be an invaluable aid to understanding its nuances when developed within the modern context.

2 EMPIRICAL CONSIDERATIONS

Apart from the potential relevance to the ANPA programme, there are other reasons which amply justify continuing investigation into gravitational processes, and these largely lie in the fact that the, so called, *Standard Model* - essentially, the Big-Bang model of cosmology - faces serious difficulties arising from modern observations. These are

described, briefly, below.

In practice, and in very broad terms, any *Cosmological Principle* is a general statement about the nature of the distribution of material in the hypothesised Universe, and the underlying gravitation theory of the cosmology concerned then predicts the evolution of the material distribution which conforms to this principle. In consequence, the testing of a cosmology can be partitioned into

- *Testing the assumed Cosmological Principle;*
- *Testing of the predicted dynamical evolution.*

In the following, we shall consider these two aspects of testing in turn, referring the discussion to the *Standard Model*, where appropriate.

(a) Testing Cosmological Principles

Since a *Cosmological Principle* is generally a statement about the distribution of matter in the Universe, then the most basic requirements for the testing of such principles are observations of the actual matter distribution over the largest possible volumes. The fundamental empirical tool which makes this a realistic endeavour is the knowledge that, out to fairly small distances at least, galaxy redshifts vary, in a statistical sense, linearly with distance - this is Hubble's Law. Hence, given the angular coordinates of objects on the sky, the measured redshifts of these objects allows an estimation of their spatial distribution. The *Standard Model* is based on the *Cosmological Principle* that material in the Universe is distributed homogeneously, and so the practical assumption of adherents to this model is that *on a large enough scale* this homogeneous nature will become revealed. The large quantity of raw data that has become available over the past fifteen years or so has made possible correspondingly intensive studies of the distribution of galaxies over this period; for example, we cite the surveys of Huchra et al [1983], Giovanelli and Haynes [1986], De Lapparent et al [1988], Broadhurst et al [1990], Da Costa et al [1994] and Vettolani et al [1994] etc. These surveys have failed to reveal any tendency towards the expected homogeneity and, to the contrary, have revealed large scale filaments, sheets, superclusters and voids; to quote Baryshev et al [1995]:

One of the most important issues raised by these catalogues is that the scale of the largest inhomogeneities is comparable with the extent of the surveys, so that the largest known structures are limited by the boundaries of the survey in which they are detected.

It is possible, of course, that the homogeneity required by the Friedmann models will reveal itself on yet larger scales than are presently observed; however, the serious point must be made that specifying such a *Cosmological Principle* without providing some objective criterion concerning the scale at which it becomes true makes it untestable, and therefore takes it beyond the remit of the scientific endeavour - it becomes merely a statement of faith. However, even if we make this 'act of faith', the observed large scale structures create a paradox for the *Standard Model*: the linearity of the redshift law predicted by this model is a *consequence* of the assumed homogeneity of the matter distribution; however, whilst it is true that many empirical studies (eg Peebles [1988] or Mould et al [1991]) have confirmed this linearity over smallish distances, this empirical confirmation has been well within the scale of the massive inhomogeneities revealed by the deep space surveys. The paradox can be resolved by introducing large amounts of dark matter which is homogeneous on the scale of

a few Mpc, but one then comes into conflict with the estimates for the amount dark matter in galactic haloes required to obtain the flat rotation curves of the spiral galaxies.

(b) Testing Dynamical Evolution

The evolutionary tests of any cosmology are extremely problematical for the reason that they invariably rely upon specific theory-dependent interpretations of the galactic redshift - for example, as an expansion phenomenon, or as a tired-light phenomenon, or as some intrinsic age-related property of matter etc. The situation is made even more complicated by the fact that many, apparently anomalous, galactic redshifts have been observed which appear to indicate that, Hubble's Law notwithstanding, galactic redshifts - and especially those of high values - are not necessarily indicators of distance scales; Arp [1989] gives many examples in this class and, to illustrate the point, we mention, very briefly, two, quite distinct, types of example which have been the subject of recent publications.

In a recent study, Rodrigues-Williams and Hogan [1994] analysed the overdensity of QSOs in the directions of foreground galaxy clusters and concluded that the observed overdensity cannot be accounted for by any cluster lensing model, and is implausible in any conventional model of cosmic mass distribution. It is difficult to avoid noting that this result is consistent with the idea that the QSOs studied are physically associated with the galaxies which were initially presumed to be in their foreground.

Secondly, we quote the study of Moles et al [1994] of the galaxies N0450 and U0807; these are nearly superimposed on each other on the sky, but have quite distinct redshifts which, in the standard interpretation, simply implies a chance alignment of the two objects. However, the Moles study concludes that the two objects display such comprehensive evidence of mutual interaction that it is almost inconceivable to imagine they are not interacting.

In addition to these apparent anomalies, which pose problems to the interpretation of high redshifts as necessarily being distance indicators, and therefore potentially raise questions for the expansion hypothesis, there is another apparent phenomenon which poses difficulties to the interpretation of low redshifts as a phenomenon of expansion - this is the *quantized redshift* phenomenon. Tift [1976,1980,1984,1990] has long been making claims for the reality of a 72km/sec and 36km/sec redshift quantization phenomenon, and his claims have generated considerable dissension, but not much reasonable discussion. However, in a recent independent study Napier and Guthrie [1995] have tested Tift's specific hypothesis, *that quantization at 72km/sec and 36km/sec exists in the redshifts of low redshift spirals*, in a statistically rigorous manner. They used a redshift sample of about 100 objects which are independent of Tift's various samples, and which were chosen on the basis of well defined, and unbiased, selection criteria. They conclude that Tift's basic hypothesis is confirmed at the level of virtual certainty for the sample analysed. If this phenomenon is real, then it becomes very difficult to understand galactic redshifts in terms of the expansion hypothesis of the *Standard Model*.

To summarize, the problems which exist for the *Standard Model* are of three types:

- *The deepest modern redshift surveys have failed to reveal any tendency to homogeneity and, to the contrary, have revealed structures on scales that are limited only by the extent of the surveys;*
- *There are apparent redshift anomalies which suggest that high redshifts are not necessarily indicators of distance, and thereby create potential problems for the expansion hypothesis;*

- *There is growing evidence that the quantized redshift phenomenon for low redshifts is, in fact, real; this apparent phenomenon is very difficult to reconcile with the idea of redshift as an expansion phenomenon.*

For all of these reasons, it appears prudent to continue questioning the fundamental basis of gravitation theory, and that is the primary purpose of this paper.

3 OVERVIEW

The presented work divides naturally into two distinct parts. The first part, occupying §4 to §17, can be considered as a purely *conventional* modelling exercise in classical mechanics in the sense that an uncontroversial qualitative definition of gravitational action (as that action through which the Weak Equivalence Principle and Newton's Third Law must be satisfied) is given, and the tools of classical mechanics are then applied to derive - without additional assumptions - the corresponding mathematical model. This model is uniquely defined up to the specification of an undetermined function, $U(r)$ say, and has the following properties:

- *Gravitational action is described in terms of a metric theory;*
- *Because the formalism derives solely from an appropriate consideration of momentum-conservation, there is an automatic and rigorous identification of inertial and gravitational mass;*
- *The equations of motion are scale invariant under arbitrary changes of scale up to the specification of $U(r)$;*
- *Newton's Universal Theory arises as the special case corresponding to the specification $U(r) = ar + b$ for constants a and b ;*
- *The gravitational constant, γ , arises as a constant of integration, with the property that, according to the model, if $\gamma = 0$ then no motion of any kind is possible. That is, there is no kinematics and no dynamics and physics becomes trivial;*
- *According to the model, specifying gravitation is equivalent to specifying the relationships that exist between distance scales and time scales.*

As has been stated, to this point, the whole exercise can be considered as a straightforward modelling exercise of a purely conventional kind in which the only physical content is that provided by the qualitative definition given to the gravitational action.

The second part, occupying §18 to §24, begins when *qualitative* arguments are used to give physical meaning to the, hitherto, uninterpreted function $U(r)$; this step allows a cosmology to be defined, and (in the present context) is a necessary step towards obtaining insight into the fundamental nature of gravitational process. The interpretation given to $U(r)$ derives from its role in the formalism, and is that it represents a *mass-distribution* function, specifying the totality of mass in a given volume. The main consequences arising from this second part are:

- *Mach's Principle in its strictest possible interpretation is automatically satisfied. That is, in the absence of mass, concepts of measurement in space and time are undefinable so that kinematics and dynamics are similarly undefinable;*

- *In an isotropic Universe which has bounded energy everywhere, then mass is distributed in a self-similar, or fractal way, with fractal dimension two.*

This latter conclusion is in exact conformity with modern deep space observations, and suggests that the interpretation given to $U(r)$ is correct. However, there are reasons - discussed in the text - for believing this interpretation might not be the whole story, in the sense that the 'totality of mass' which it quantifies for any given volume should also necessarily include the 'mass of the material vacuum' as well as ordinary condensed material. At this stage, the development becomes speculative, and is summarized and ended there.

4 QUALITATIVE DEFINITION OF GRAVITATION

The most general statement of what constitutes 'gravitational interaction' within the classical framework of the physics of Galileo and Newton can be considered given by

- C1 Test particle trajectories are independent of the specific mass values of the test particles concerned;*
- C2 Linear momentum is conserved through the interaction;*
- C3 The acceleration of any particular massive particle in a closed system of such particles is along the line connecting the particular particle to the centre of mass of the whole closed system.*

Condition C1 is a statement of the Weak Equivalence Principle, which reflects characteristic gravitational phenomenology as interpreted within the context of classical physics, whilst conditions C2 and C3 represent the realization of Newton's Third Law. It is well known that there are two distinct ways of modelling the effect of the Weak Equivalence Principle given by either making the forces felt by gravitating particles proportional to their masses, or confining the trajectories of massive particles to the geodesic lines of a spacetime geometry. The first of these, of course, embodies the classical Newtonian approach, whilst the second embodies the modern Einsteinian approach.

However, there is a third way, and much of the interest in the presented work can be considered to lie in the fact that it shows how the Weak Equivalence Principle can be incorporated into the classical framework using a *geometric* approach which is independent of notions of 'spacetime' and which derives solely from a consideration of momentum conservation. The result is a general metric theory of classical gravitation which has a very rich structure, and functions extremely effectively within its classical context.

5 A MODEL UNIVERSE

Suppose a model Newtonian Universe is defined so that

- *it consists of a finite number of material particle ensembles having respective inertial masses $m_1 \dots m_N$;*
- *the relative velocities of the mass-centres of these N particle ensembles, measured with respect to the global centre-of-mass, are, respectively, $\mathbf{v}^1 \dots \mathbf{v}^N$ at some time t ;*
- *the distribution of the ensembles is such that, statistically, there are no preferred directions, measured from the global centre-of-mass, for the location of the mass-centres*

of the ensembles - this restriction is not necessary for the general method, but allows a relatively simple illustration of it.

Then, measured from within any frame (inertial or otherwise), there is the Newtonian relationship

$$m_1 \mathbf{v}^1 + m_2 \mathbf{v}^2 + \dots + m_N \mathbf{v}^N = 0 \quad (1)$$

between the velocity vectors of the whole class of ensemble mass-centres in the model Universe. Since the velocities are measured with respect to the global centre-of-mass, then position coordinates, $\mathbf{r} \equiv (x^1, x^2, x^3)$, can be measured with respect to any frame which is stationary with respect to the global centre-of-mass. Furthermore, if all measurements of $\mathbf{v}$ and $\mathbf{r}$ are understood to be made relative to this global mass-centre, then any theory developed will be invariant wrt to transformations between arbitrary coordinate frames. This restriction has simplifying consequences, and is assumed to apply from here onwards, unless otherwise explicitly stated. With this restriction, then (1) integrates directly to give

$$m_1 \mathbf{r}^1 + m_2 \mathbf{r}^2 + \dots + m_N \mathbf{r}^N = 0. \quad (2)$$

Now suppose we conduct a thought experiment on this model Universe which consists of

- *imagining the material of the model Universe to be partitioned into two parts consisting of the ensemble of mass m_1 and mass-centre position $\mathbf{r}^1$ and the remainder, having total mass $M = m_2 + m_3 + \dots + m_n$ and mass-centre position $\mathbf{R}$. In this case, (2) becomes*

$$m_1 \mathbf{r}^1 + M \mathbf{R} = 0 \quad \rightarrow \quad \mathbf{R} = -\frac{m_1}{M} \mathbf{r}^1,$$

so that the orbit of the mass-centre of M is the anti-orbit of the mass-centre of m_1 where each orbit, of course, is referred to an objectively defined origin, the global centre-of-mass;

- *imagining that the motion of m_1 , defined by $\mathbf{r}^1$ as a function of time, is random, but with well defined statistics.*

If the only information available for this system was the motion of the mass-centre of m_1 or of M (but not both), measured over suitably long periods of time, then the only conclusion that could possibly be drawn would be that the motion appeared to be random, with an estimate of the statistics given. However, if information on the motions of *both* the mass-centres of m_1 and M was available, then it would be immediately obvious that the two motions were in concert about some well defined point - which has already been defined as the global centre-of-mass. The experiment can be imagined repeated with m_1 replaced, in turn, by $m_2, m_3, \dots, m_n$ with the final conclusion that, regardless of the apparent randomness of individual motions, the totality of motions are in concert about the global centre-of-mass and that - because of the way the model Universe was originally defined - there are no preferred directions definable with respect to this point. In other words, the model Universe would appear to behave *as if* governed by some objectively defined *spherical influence* which constrains the class of all possible motions to satisfy (2).

All we know about this *spherical influence* is that it *appears* to exist; we take the appearances literally, and represent this *spherical influence* in terms of the unspecified scalar function $U(r)$. In the following sections, we shall show how, using this function as the fundamental object, the gravitational interaction defined according to the three conditions C1,

C2 & C3, can be described in the finite and discrete model Universe as a metric effect. Because of its central role in this analysis, $U(r)$ is referred to as the *reference scalar* in all that follows.

Before continuing, it is possible to make general observations about the likely structure of the theory to be developed by observing symmetries of (1) and (2):

- *Firstly, it is readily seen that (1) is invariant with respect to arbitrary transformations of the 'time' parameter, so that this parameter in (1) cannot be identified automatically with physical time; consequently, it is to be expected that such a symmetry should arise at some point in the development of any theory based primarily upon (1). This is the case, but the symmetry is broken by the incorporation of C3.*
- *Secondly, it is easily seen that (2) is form invariant under the scaling transformation $r \rightarrow \lambda r$, for arbitrary constant λ . Because of the central role of (2) in deducing the existence of the reference scalar, $U(r)$, it seems likely that this function must be constrained to be form invariant under the same transformations of scale. This condition is not imposed a priori, but it transpires that the equations of motion derived hereafter only imply global momentum conservation (condition C2, or equation (1)) if the reference scalar is constrained by the scale-invariance symmetry.*

We leave the task of giving $U(r)$ a physical interpretation until later in the paper and, in the meantime, treat it as a convenient analytical object used to facilitate the analysis.

6 GEOMETRY AND THE REFERENCE SCALAR, $U(r)$

In §5, it was shown how the global momentum conserving interaction, (2), could be used to infer the existence of a scalar function, $U(r)$, which serves to represent the generality of all possible motions allowed by the momentum conservation condition. In the following, we show how this function can be used to represent the evolution of the individual trajectories involved in the interaction, given that this interaction is gravitational in the sense of §4.

Consider the trajectory, r (remember, r is measured with respect to the global centre-of-mass), of the mass-centre, $\mathcal{M}_0$, of an arbitrarily chosen member of the set of particle ensembles which make up the model Universe; in general, such a trajectory will not be confined to any particular surface, $U(r) = k$, but will cut across such surfaces with the consequence that this trajectory can, under a certain general condition, be described in terms of change in these surfaces. The general condition required is that any change, dr , in the instantaneous position, r_0 , of $\mathcal{M}_0$ can be put into a 1:1 relationship with the corresponding change, dn , in the normal-vector, n_0 , to the surface $U(r) = k$ at the position of $\mathcal{M}_0$. If this circumstance can be assumed to hold, it will follow that the change, dr , in the instantaneous position of $\mathcal{M}_0$ can be represented directly by the corresponding change, dn , in the normal vector between the positions r_0 and $r_0 + dr$. Since, of course, normal-vectors here are defined on the level-surfaces $U(r) = k$ of the *unspecified* function, $U(r)$, then the phrase 'change in this normal-vector' implies, in particular, 'covariant change in this normal-vector' and, in general, the idea that all operations performed in this analysis are performed on the geometry of the surfaces $U(r) = k$, which is unspecified since $U(r)$ is also unspecified.

To see, in detail, how a particle trajectory can be described in this way, first note that,

since $n_a \equiv \nabla_a U$, $a = 1, 2, 3$ defines normal-vectors to the surfaces $U(r) = k$, then

$$dn_a = \nabla_j(\nabla_a U)dx^j \equiv g_{ja}dx^j \text{ where } g_{ab} \equiv \nabla_a \nabla_b U, \quad (4)$$

gives the covariant change in n_a along an infinitesimal trajectorial arc dr (it is assumed at this stage that the geometrical connections which are necessarily required to give this latter expression an unambiguous meaning will be defined in due course). It is clear from (4) that dn_a is in a 1:1 relation with dx^a if g_{ab} is non-singular and uniquely defined everywhere. Given this condition, then dx^a is provided with a unique representation in terms of dn_a , and dn_a is provided with a unique representation in terms of dx^a . It follows from these considerations that the scalar product $ds^2 \equiv dn_i dx^i$ can be interpreted as a scalar measure of dx^a defined relative to locally specified geometrical properties of the surfaces $U(r) = k$. However, from (4)

$$ds^2 \equiv dn_i dx^i \equiv g_{ij} dx^i dx^j$$

so that if we want this measure to behave sensibly in the sense that $ds^2 = 0$ only when $dr = 0$, then we must replace the condition of non-singularity of g_{ab} by the condition that it is strictly positive (or negative) definite; in the physical context of the present problem, this will be considered to be a self-evident requirement.

Now suppose p and q are two arbitrarily chosen point coordinates on the trajectory of the chosen particle, and suppose the latter expression is integrated between these points to give the scalar invariant

$$I(p, q) = \int_p^q \sqrt{dn_i dx^i} \equiv \int_p^q \sqrt{g_{ij} dx^i dx^j}. \quad (5)$$

Then, in accordance with the foregoing interpretation, $I(p, q)$ gives a scalar record of how the particle has moved between p and q defined with respect to a geometry defined on the surfaces $U(r) = k$.

Now suppose $I(p, q)$ is minimized with respect to choice of the trajectory connecting p and q , then this minimizing trajectory can be interpreted as a geodesic in the Riemannian space which has g_{ab} as its metric tensor. Given that g_{ab} is defined in terms of $U(r)$ - the inferred existence of which depends purely upon the general fact of global momentum conservation and not at all upon any specific inertial-mass values - then the existence of the metric space, and of geodesic curves within it, is likewise explicitly independent of any specific inertial-mass values. It follows that the identification of the particle trajectory r with these geodesics makes the trajectory *independent* of specific particle inertial masses, and can be considered as the modelling step defining that general subclass of trajectories which conform to that characteristic phenomenology of gravitation defined by condition C1 of §4.

7 THE CONNECTION COEFFICIENTS

In §6, it was assumed that the geometrical connection coefficients could be defined in some sensible way. To do this, we simply note that, in order to define conservation laws (ie to do physics) in a Riemannian space, it is necessary to have a generalized form of Gauss's divergence theorem in the space. This is certainly possible when the connections are defined to be the metrical connections, but it is by no means clear that it is ever possible otherwise. Consequently, the connections are assumed to be

metrical and so g_{ab} , given at (4), can be written explicitly as

$$g_{ab} \equiv \nabla_a \nabla_b U \equiv \frac{\partial^2 U}{\partial x^a \partial x^b} - \Gamma_{ab}^k \frac{\partial U}{\partial x^k}, \quad (6)$$

where Γ_{ab}^k are the Christoffel symbols, and given by

$$\Gamma_{ab}^k = \frac{1}{2} g^{kj} \left(\frac{\partial g_{bj}}{\partial x^a} + \frac{\partial g_{ja}}{\partial x^b} - \frac{\partial g_{ab}}{\partial x^j} \right).$$

8 THE METRIC TENSOR IN TERMS OF THE REFERENCE SCALAR

It can be shown how, for an arbitrarily defined reference scalar, U , (6) can be exactly resolved to give an explicit form for g_{ab} in terms of such a general U : Defining

$$\mathbf{r} \equiv (x^1, x^2, x^3), \quad \phi \equiv \frac{1}{2} \langle \mathbf{r}, \mathbf{r} \rangle \quad \text{and} \quad U' \equiv \frac{dU}{d\phi}$$

then it is found that

$$g_{ab} = A \delta_{ab} + B x^a x^b, \quad (7)$$

where

$$A \equiv \frac{\alpha_0 U + \alpha_1}{\phi}, \quad B \equiv - \left(\frac{\alpha_0 U + \alpha_1}{2\phi^2} \right) + \frac{\alpha_0 U' U'}{2(\alpha_0 U + \alpha_1)}.$$

for arbitrary constants α_0 and α_1 .

9 EQUATIONS OF MOTIONS: PRELIMINARY FORM

The geodesic equations in the space with the metric tensor (7) can be obtained, in the usual way, by defining the Lagrangian density

$$\mathcal{L} \equiv \sqrt{g_{ij} \dot{x}^i \dot{x}^j} = (A \langle \dot{\mathbf{r}}, \dot{\mathbf{r}} \rangle + B \dot{\phi}^2)^{1/2},$$

where $\dot{x}^i \equiv dx^i/dt$, etc., and writing down the Euler-Lagrange equations

$$2A\ddot{\mathbf{r}} + \left(2A'\dot{\phi} - 2\frac{\dot{\mathcal{L}}}{\mathcal{L}}A \right) \dot{\mathbf{r}} + \left(B'\dot{\phi}^2 + 2B\ddot{\phi} - A'\langle \dot{\mathbf{r}}, \dot{\mathbf{r}} \rangle - 2\frac{\dot{\mathcal{L}}}{\mathcal{L}}B\dot{\phi} \right) \mathbf{r} = 0, \quad (8)$$

where $\dot{\mathbf{r}} \equiv d\mathbf{r}/dt$ and $A' \equiv dA/d\phi$, etc. By identifying particle trajectories with geodesic curves, this equation is now interpreted as the equation of motion for a single particle satisfying condition C1 of §4; it has yet to be considered how conditions C2 & C3 of §4 can be satisfied by this equation.

10 FORM INVARIANCE PROPERTIES

The equations of motion, (8), are form-invariant under the arbitrary time-scale transformations $t = \sigma(\tau)$, and under the arbitrary distance-scale transformations $r = g(R)$, where $r^2 = \langle \mathbf{r}, \mathbf{r} \rangle$, $R^2 = \langle \mathbf{R}, \mathbf{R} \rangle$. For the present purposes, the first of

these invariances is the most significant, and so only this one is shown:- the equations (8) are the Euler-Lagrange equations deriving from the Lagrangian density $\mathcal{L}$, and this density is positively homogeneous of the first degree in $\dot{x}^i$. It follows directly that the corresponding action integral is invariant with respect to arbitrary transformations of the 'time' parameter. This property of the system (8) has a direct bearing on the ability of the system to accommodate the condition C3 of §4, and this point is discussed in detail in §12.

11 GLOBAL MOMENTUM CONSERVATION

It can be shown that condition C2, which requires global momentum conservation, is satisfied if the reference scalar, $U(r)$, is constrained to satisfy

$$\alpha_0 U(r) + \alpha_1 = \gamma_0 \left(\frac{r}{r_0} \right)^{\gamma_1}, \quad (9)$$

where α_0 & α_1 are arbitrary constants as in (7), γ_0 & γ_1 are further arbitrary constants, and r_0 is some constant scale length introduced to ensure that the dimensions of γ_0 are independent of the value of the index, γ_1 .

12 EQUATIONS OF MOTION: FINAL FORM

The equations (8), with the reference scalar constrained by (9), can now be considered to satisfy conditions C1 & C2 of §4, but not C3, which states that particle accelerations are directed through the system centre-of-mass. However, if the equations of motion are referred to the centre-of-mass frame, which they are in this case, then this latter condition simply means that the equations of motion must have the general structure

$$\ddot{\mathbf{r}} = G(t, \mathbf{r}, \dot{\mathbf{r}})\mathbf{r},$$

for scalar function $G(t, \mathbf{r}, \dot{\mathbf{r}})$. In other words, (8) satisfies condition C3 if the coefficient of $\dot{\mathbf{r}}$ is zero, so that

$$\left(2A'\dot{\phi} - 2\frac{\dot{\mathcal{L}}}{\mathcal{L}}A \right) = 0. \quad (10)$$

At face value, the imposition of this condition would appear to overdetermine the system. However, as has already been noted in §10, the equations of motion (8) are invariant with respect to arbitrary transformations of the time parameter. A direct, and well known, consequence of this circumstance of arbitrariness is that the corresponding Euler-Lagrange equations form a linearly *dependent* set (eg Rund, 1973), so that it becomes necessary to specify extra conditions to close the system. A similar circumstance arises in General Relativity theory when the equations of motion are derived from an action integral which is formally identical to (5). In that case, the system is closed by specifying the arbitrary time parameter to be the 'proper time', so that

$$d\tau = \mathcal{L}(x^j, dx^j) \rightarrow \mathcal{L}(x^j, \frac{dx^j}{d\tau}) = 1,$$

which is then considered as the necessary extra condition required to close the system. Furthermore, in this particular case, $\mathcal{L}^2 = 1$ is readily shown to be a first integral of the Euler-Lagrange equations.

So, with these remarks in mind, the equations (8) can be seen to form an incomplete set which can be considered closed by the addition of equation (10). This latter equation is readily written as

$$\frac{A'}{A}\dot{\phi} = \frac{\dot{\mathcal{L}}}{\mathcal{L}} \rightarrow \mathcal{L} = k_0 A,$$

for arbitrary constant k_0 (remembering $A' \equiv dA/d\phi$) and, as in the corresponding General Relativity case, above, can be considered as that equation which removes the pre-existing arbitrariness in the 'time' parameter by *defining* the relation between time and distance scales to within the specification of $U(r)$. From this latter equation, we have

$$\mathcal{L}^2 = k_0^2 A^2 \rightarrow A < \dot{r}, \dot{r} > + B\dot{\phi}^2 - k_0^2 A^2 = 0, \quad (11)$$

which, as for the General Relativity case, $\mathcal{L}^2 = 1$, is readily shown to be a first integral of the Euler-Lagrange equations, (8), when modified by (10); this assertion is verified in §13.

13 EQUATIONS OF MOTION: POTENTIAL FORM

From §12, when (10) is used in (8) there results

$$2A\ddot{r} + \left(B'\dot{\phi}^2 + 2B\ddot{\phi} - A' < \dot{r}, \dot{r} > - 2\frac{A'}{A}B\dot{\phi}^2 \right) r = 0, \quad (12)$$

which has the first integrals

$$A < \dot{r}, \dot{r} > + B\dot{\phi}^2 - k_0^2 A^2 = 0, \quad r^2 \dot{\theta} = h.$$

Using the first of these two integrals to define the potential function, V , in the usual way as $V \equiv - < \dot{r}, \dot{r} > / 2$, then it is easily shown that (12) can be expressed as

$$\ddot{r} = -\frac{\partial V}{\partial r} \hat{r}, \quad V \equiv -\frac{1}{2} < \dot{r}, \dot{r} > = -\frac{k_0^2 A}{2} + \frac{B}{2A} \dot{\phi}^2, \quad (13)$$

for unit vector, $\hat{r}$, and where

$$A \equiv \frac{\alpha_0 U + \alpha_1}{\phi}, \quad B \equiv -\left(\frac{\alpha_0 U + \alpha_1}{2\phi^2} \right) + \frac{\alpha_0 U' U'}{2(\alpha_0 U + \alpha_1)}, \quad \phi = \frac{1}{2} < r, r >.$$

This verifies the original assertion that $\mathcal{L}^2 = k_0^2 A^2$ is a first integral of (8).

Since the equations (13) satisfy conditions C1, C2 & C3 of §4, they model the complete phenomenology of gravitation in a classical model Universe.

14 THE GRAVITATIONAL COUPLING CONSTANT, k_0^2

The constant k_0^2 arose as a constant of integration in the integration of (10), which is the condition guaranteeing C3. The application of this condition removed the arbitrariness in the time parameter which exists in (8) and, according to our interpretation, defines the relationship between time and distance scales to within the specification of $U(r)$.

To understand the meaning of k_0^2 , it is sufficient to simply put $k_0^2 = 0$ in (11), to give

$$A < \dot{r}, \dot{r} > + B\dot{\phi}^2 = 0,$$

which rearranges as

$$(A + 2B\phi)\dot{r}^2 + Ar^2\dot{\theta}^2 = 0,$$

after using $\phi = r^2/2$. Using (7), it is straightforward to show that the three eigenvalues of g_{ab} are A , A and $A + 2B\phi$. Consequently, since g_{ab} must be positive (or negative) definite (a requirement imposed in §6, and easily found to be guaranteed by the condition $\alpha_0 > 0$), then A and $A + 2B\phi$ are either both positive or both negative. It then follows immediately from the above equation that $\dot{r} = r\dot{\theta} = 0$. In other words, in a model Universe for which $k_0^2 = 0$, there can be no motion of any kind - there is a universal *stasis* and physics becomes trivial. Therefore dynamical relationships between particles in the model Universe can only be defined when $k_0^2 \neq 0$, and it follows that this parameter plays the role of a *gravitational coupling constant* between the objects in the model Universe; that is, k_0^2 is either to be identified directly with the gravitational constant, γ , or is directly proportional to it.

15 A PRELIMINARY ANALYSIS OF POSSIBLE CASES

Remembering that, by the remarks of §11, global momentum conservation requires the reference scalar to have the most general form $\alpha_0 U + \alpha_1 = \gamma_0(r/r_0)^{\gamma_1}$, then the most trivial possible specification is given by setting $\gamma_1 = 1$ so that $\alpha_0 U + \alpha_1 = \beta_0 r$, where γ_0/r_0 has been replaced by β_0 for convenience. Through (7), this corresponds to the metric tensor

$$g_{ab} = A\delta_{ab} + Bx^a x^b = \left(\frac{2\beta_0}{r}\right)\delta_{ab} + \left(\frac{1 - 4\alpha_0}{2\alpha_0 r^3}\right)\beta_0 x^a x^b, \quad (14)$$

where α_0 and β_0 are still unknown parameters. To understand the significance of the parameter α_0 , we note, from (7), that the invariant distance measure is given by

$$ds^2 \equiv g_{ij}dx^i dx^j = A((dx^1)^2 + (dx^2)^2 + (dx^3)^2) + Br^2(dr)^2.$$

If α_0 is such that $B = 0$, then it is clear that the magnitude of ds^2 depends only on the magnitude and *location* of the elemental vector, $dr \equiv (dx^1, dx^2, dx^3)$; by contrast, if α_0 is such that $B \neq 0$, then the magnitude of ds^2 is dependent on the *direction* of dr as well - that is, the value of ds^2 varies according to whether or not dr is directed so that $dr \neq 0$.

Consequently, it can be concluded that the case $B = 0$ corresponds to the case of isotropy of local measurement conditions about arbitrary points in the space, whereas the case $B \neq 0$ corresponds to isotropy only in those directions corresponding to $r = \text{constant}$, and picks out the radial direction as a special direction. It is clear from (14) that $B = 0$ only if $\alpha_0 = 1/4$.

16 THE CASE OF LOCAL ISOTROPY: NEWTONIAN GRAVITATION

Empirical considerations inform us that the barycentre of the solar system can be considered, to a very good approximation, as the origin of an inertial frame. With this justification, we consider the solar system barycentre as the centre-of-mass of our model Universe so that this 'Universe' consists of just our solar system. Furthermore, the classical point source model is readily identified with spherically symmetric and locally isotropic physics so that, from §15, we have $\alpha_0 = 1/4$ giving $A = 2\beta_0/r$ and $B = 0$. Substitution of

these in the potential function of (13) gives

$$V = -\frac{k_0^2 \beta_0}{r},$$

giving, for the equations of motion,

$$\ddot{r} - \frac{h^2}{r^3} + \frac{k_0^2 \beta_0}{r^2} = 0, \quad r^2 \dot{\theta} = h.$$

These equations are to be interpreted as the equations of motion of a particle with displacement r with respect to the barycentre of the solar system when the reference scalar is considered defined by $\alpha_0 U + \alpha_1 = \beta_0 r$.

If m represents the mass of this particle, and if M represents the mass of the remaining part of the system, then these equations become identical to the classical Newtonian two-body theory when $k_0^2 \beta_0 = \gamma M^3 / (m + M)^2$, where γ is the gravitational constant. However, by the remarks of §14, it is already known that k_0^2 must be proportional to the gravitational constant, γ ; consequently, we can deduce the result $\beta_0 \propto M^3 / (m + M)^2$. Since $\beta_0 \equiv \gamma_0 / r_0$ (cf §15), this implies directly that $\alpha_0 U + \alpha_1 \equiv \gamma_0 (r/r_0)^n$ represents some form of mass function; this point is considered in more detail in §18.

To summarize, Newton's Universal Law of Gravitation can be considered as the special case of the general classical metric theory, given by (13), which arises when the reference scalar is specified according to $\alpha_0 U + \alpha_1 = \gamma_0 r / r_0$, and when local measurement conditions about arbitrary points in space are constrained to be isotropic by the specification $\alpha_0 = 1/4$.

17 THE ANISOTROPIC CASE: $B \neq 0$

It was shown in §15 how $B \neq 0$ corresponded to the case of anisotropy in the radial direction. It is manifestly the case that, in fact, conditions in the region of a gravitational source like the sun are anisotropic in this way (if only because real sources are invariably *extended* distributions), and so it might be expected that this case might provide a more realistic model of gravitation in general. As was noted in §16, isotropy is implied when $\alpha_0 = 1/4$ and so anisotropy is implied for all $\alpha_0 \neq 1/4$. Since the classical conic orbits arise from the isotropic case, it is interesting to ask if the excess perihelion advance observed for planetary in the solar system can be accounted for by the manifest anisotropy of the solar gravitational source.

It is found that setting

$$\alpha_0 = \frac{1}{4} - \frac{3\gamma M}{2R_m c^2},$$

where R_m is the mean radius of Mercury's orbit, gives the observed advance of perihelion for that planet. In other words, the observed excess perihelion advance of solar system orbits can be consistently ascribed to the effects of anisotropy in local mass conditions.

18 COSMOLOGY: THE MASS DISTRIBUTION FUNCTION

Since the reference scalar, $U(r)$, always occurs in the form $\alpha_0 U(r) + \alpha_1$ then, for convenience, we define $M(r) \equiv \alpha_0 U(r) + \alpha_1$ and use the form $M(r)$ for the remainder of this paper. Remember also that, in §11, it was shown how the condition of global momentum

conservation constrains this function to satisfy

$$M(r) = \gamma_0 \left(\frac{r}{r_0} \right)^{\gamma_1}, \quad (15)$$

for arbitrary constants $\alpha_0, \alpha_1, \gamma_0, \gamma_1$ and scale constant r_0 .

So far, $M(r)$ has been treated as a purely mathematical object in the description of gravitation in a model Galilean Universe, and it has been shown how the most simple possible assumption of its form leads to Newtonian gravitation. However, if we are to progress to a cosmological *theory* - as opposed to what has hitherto been a descriptive process - then $M(r)$ must be interpreted in physical terms, and a means of defining its explicit form given. Let us firstly review what is known about this function.

Reference to the equations of motion, (13), or to the Newtonian analysis of §16, shows that the function $M(r)$ (in its original form $U(r)$) plays the basic role of determining the (gravitationally driven) dynamics of any chosen material ensemble in the model Universe; that is, in the conventional parlance, $M(r)$ plays the role of the 'attracting mass'. Additionally, remembering how this function was introduced via a consideration of the *global* behaviour of the finite collection of discrete material particles that make up the model Universe, we can conclude that the scalar measure of 'attracting mass' represented by $M(r)$ has a global aspect; that is, $M(r)$ is to be interpreted not as a *density* function which gives the value of a scalar quantity at a *point* in physical space, but rather as a *distribution* function which gives the value of a scalar quantity in a *volume* of physical space. Consequently, the most direct interpretation of $M(r)$ is that it defines the amount of mass inside a sphere of radius r , centred on the system centre-of-mass - that is, the reference scalar, $U(r)$, forms a basis for the definition of a *mass distribution function*, $M(r)$.

19 MACH'S PRINCIPLE

Regardless of the rectitude of the precise definition given to $M(r)$ in §18, its identification as *some kind* of quantitative measure of mass is certain, and this has an extremely interesting consequence: specifically, in §12 it was noted that the imposition of condition C3 through equation (10) had the effect of defining the relationship between time and distance scales within the theory (prior to this step, there was no *physical* time in the theory) whilst the relationship $ds^2 = g_{ij}dx^i dx^j$ defines the invariant distance measurements for the theory. If $M(r) = 0$ (that is, if there is no mass), then these definitions disappear, and there is no concept of physical time or of invariant distance measurement within the theory - there is no physics. This circumstance corresponds directly to the strictest possible interpretation of Mach's Principle, according to which it is impossible to conceive empty physical space.

20 AN ISOTROPIC UNIVERSE

The general invariant distance measure in the model Universe is derived from (7), and given by

$$ds^2 = A\delta_{ij}dx^i dx^j + Br^2 dr^2.$$

For the reasons discussed in §15, the requirement for isotropy of local invariant measurement conditions in arbitrarily located neighbourhoods requires the equation $B = 0$ to be satisfied

which, using the definition of B given in §8 and the identity $M(r) \equiv \alpha_0 U(r) + \alpha_1$, implies

$$-\frac{M}{2\phi^2} + \frac{M'M'}{2\alpha_0 M} = 0$$

$$\rightarrow M' = \pm \sqrt{\alpha_0} \left(\frac{M}{\phi} \right).$$

Using $\phi \equiv r^2/2$, this gives

$$M(r) = \gamma_0 \left(\frac{r}{r_0} \right)^{\pm 2\sqrt{\alpha_0}},$$

where α_0 , γ_0 and r_0 are undetermined parameters. It is easily seen how this form is a special case of (15), so that the imposition of measurement isotropy about arbitrary spatial points automatically implies the conservation of global momentum; however, the converse is not true, since this latter form of $M(r)$ constrains the value γ_1 in (15) to be $\pm 2\sqrt{\alpha_0}$, where α_0 is the arbitrary constant appearing in the definition $M(r) \equiv \alpha_0 U(r) + \alpha_1$.

Noting that $M(r)$ has been interpreted as the amount of mass in a sphere of radius r then, if a real singularity is to be avoided at the origin, the positive square root must be chosen and so, finally,

$$M(r) = \gamma_0 \left(\frac{r}{r_0} \right)^{2\sqrt{\alpha_0}}. \quad (16)$$

This latter equation represents the most general mass distribution allowed in the cosmology of the present theory which is consistent with the requirement for isotropy of local invariant distance measurements about arbitrary points in the space.

We now show that if the potential energy of the system is to be everywhere bounded, then α_0 can be determined. From (13), (remembering that $B = 0$) the potential is given by

$$V = -\frac{k_0^2 A}{2} = -\frac{k_0^2 M}{2\phi}$$

$$= -\left(\frac{k_0^2 \gamma_0}{r_0^{2\sqrt{\alpha_0}}} \right) r^{2(\sqrt{\alpha_0}-1)}. \quad (17)$$

If the potential is to be finite at $r = 0$, then we must have $\sqrt{\alpha_0} - 1 \geq 0$. Now consider the situation for $r \gg 0$: the model Universe of the present theory was initially defined to contain a finite number of massive particles and therefore there will always be a finite value of r which will encompass all of these particles. However, whilst the number of particles is finite, it is unbounded, and so R^* , the encompassing radius, is also unbounded. It follows that if the potential, V , is to be bounded in an arbitrarily defined finite model Universe, within which all particles are contained within the sphere $r \leq R^* < \infty$, then we must have $\sqrt{\alpha_0} - 1 \leq 0$. Consequently, taking the two conditions together, if V is to be bounded for all $r \leq R^* < \infty$, we must necessarily have $\sqrt{\alpha_0} = 1$. It follows, from (16), that the reference scalar - or mass distribution function - in a model Universe which is defined to guarantee the isotropy of local measurement conditions about arbitrary points in the space, and to have bounded potential throughout the material distribution, is given by

$$M(r) \equiv \gamma_0 \left(\frac{r}{r_0} \right)^2. \quad (18)$$

It now follows, from (17), that the potential in this model Universe is given by

$$V = -\gamma_0 \left(\frac{k_0}{r_0} \right)^2 \equiv \text{constant}.$$

The constancy of the potential simply implies that this Universe is in gravitational equilibrium.

21 GRAVITATIONAL EQUILIBRIUM

It is shown in this section that the basic model, outlined in §20, implies that equilibrium can only occur when

within any sphere contained wholly within the finite model Universe, and centred anywhere, then material is distributed according to

$$M(r) = \gamma_0 \left(\frac{r}{r_0} \right)^2,$$

where γ_0 is an arbitrary constant, r_0 is a scaling constant and r is the distance to the centre of the chosen sphere.

This amounts to saying that the material within a finite model Universe which is in equilibrium is distributed *fractally* with fractal dimension *two*. We make a comparison with observation in §22.

We begin by noting that, according to §20, the metric, g_{ab} defined at (7), of the model Universe reduces to

$$g_{ab} = \frac{2\gamma_0}{r_0^2} \delta_{ab}$$

when that Universe is defined to guarantee the isotropy of local measurement conditions about arbitrary points in the space, and to have everywhere bounded energy¹. Since this flat metric holds in the mass-centre frame, it holds in every frame which is inertial with respect to the mass-centre frame. Referring now to the original covariant definition of g_{ab} , given at (6), which, because of its covariant formulation, is true in all frames, and noting that $\Gamma_{bc}^a \equiv 0$ in all flat spaces and is therefore true in any frame which is inertial with respect to the mass-centre frame, then we obtain

$$\frac{2\gamma_0}{r_0^2} \delta_{ab} = \frac{\partial^2 U}{\partial x^a \partial x^b} = \frac{1}{\alpha_0} \frac{\partial^2 M}{\partial x^a \partial x^b}$$

which integrates immediately to give

$$M = \frac{\alpha_0 \gamma_0}{r_0^2} (x^i - x_0^i)(x^j - x_0^j) \delta_{ij}, \quad (19)$$

where x_0 is a constant of integration.

The question to be asked now is 'what value must x_0 assume?' Given there is only one special point in the model Universe - the global centre-of-mass - then there are only two possibilities:

¹ In passing, we notice again that if $\gamma_0 = 0$, so that, according to (18), there is no mass, then $g_{ab} = 0$ implying the collapse of physics. See §19

- either the point x_0 is constrained to be the coordinate position of the global centre-of-mass as seen from the new frame, with the effect that M at (19) is just $M(r)$ at (18) expressed in this new frame;
- or the point x_0 is not constrained in this way, in which case it can be chosen arbitrarily, since there is no other way of distinguishing between points in the space.

The first possibility would be the necessary choice if $M(r)$ at (18) represented some kind of density function which specified the value of some physical quantity at a particular point in physical space; however, in §18, it was concluded that $M(r)$ is not a density-type function, but a distribution-type function where the values assumed by the scalar $M(r)$ represent global measures of mass over volumes of space. This has the consequence that x_0 is not necessarily constrained to represent the coordinate position of the global centre-of-mass so that, in the absence of any further constraints, it can be chosen arbitrarily. The convenient choice is $x_0 = 0$ for all frames, and so, in an obvious change of notation, (19) becomes

$$M(r) = \gamma_0 \left(\frac{r}{r_0} \right)^2, \quad (20)$$

where r represents the radial distance to the coordinate origin of an arbitrarily situated frame. This amounts to the statement that mass is distributed in a self-similar, or fractal, fashion with a fractal dimension of two. Of course, since the model Universe is finite, the result $M(r) = \gamma_0(r/r_0)^2$ can only hold within a sphere which is wholly contained within the model Universe.

22 A FRACTAL UNIVERSE: THE EVIDENCE

We have shown how the simple interpretation of the reference scalar as the matter distribution function in a finite and discrete model Universe which is to be isotropic about the global mass-centre, and which is to have finite potential energy everywhere, leads directly to the self-similar mass distribution satisfying $M(r) \approx r^2$, where r is measured from any centre.

A basic assumption of the *Standard Model* is that, on some scale, the Universe is homogeneous; however, in early responses to suspicions that the accruing data was more consistent with Charlier's conceptions of an hierarchical Universe [Charlier, 1908, 1922, 1924] than with the requirements of the *Standard Model*, de Vaucouleurs [1970] showed that, within wide limits, the available data satisfied a mass distribution law $M(r) \approx r^{1.3}$, whilst Peebles [1980] found $M(r) \approx r^{1.23}$. The situation, from the point of view of the *Standard Model*, has continued to deteriorate with the growth of the data-base to the point that, [Baryshev et al, 1995]

...the scale of the largest inhomogeneities (discovered to date) is comparable with the extent of the surveys, so that the largest known structures are limited by the boundaries of the survey in which they are detected.

For example, several recent redshift surveys, such as those performed by Huchra et al [1983], Giovanelli and Haynes [1986], De Lapparent et al [1988], Broadhurst et al [1990], Da Costa et al [1994] and Vettolani et al [1994] etc have discovered massive structures such as sheets, filaments, superclusters and voids, and show that large structures are common features of the

observable Universe; the most significant conclusion to be drawn from all of these surveys is that the scale of the largest inhomogeneities observed is comparable with the spatial extent of the surveys themselves. So, to date, evidence that the assumption of homogeneity in the Universe is realistic does not exist. By contrast, evidence for the fractal nature of the matter distribution is becoming increasingly strong; for example, Coleman et al [1988] analysed the *CfA1* redshift survey of Huchra et al [1983], and found $M(r) \approx r^{1.4}$ for this sample; subsequently, the *CfA2* survey of Da Costa et al [1994], which is an extension of the *CfA1* survey out to about twice the depth, has been analysed by Pietronero and Sylos Labini [1995] to reveal $M(r) \approx r^{1.9}$. The pencil beam survey data accumulated in ESO Slice Project [Vettolani et al, 1994], which reaches out to 800 Mpc, has been similarly analysed [Pietronero and Sylos Labini, 1994] to conclude that, within this data, the distribution of galaxies conforms to the fractal law $M(r) \approx r^2$ up to the sample limits and, according to Baryshev et al [1995], this same result of fractal distribution of dimension ≈ 2 has been found in the analysis of other deep redshift surveys such as those of Guzzo et al [1992] and Moore et al [1994].

To summarize, for more than twenty years evidence has been accumulating that material in the Universe appears to be distributed in an hierarchical, or fractal way - in direct opposition to the requirements of the *Standard Model* - and the results of the most modern deep and wide angle surveys are consistent in suggesting the distribution law $M(r) \approx r^2$, valid about arbitrarily chosen centres. This empirical law, which describes a self-similar mass distribution of fractal dimension two, *is in direct conformity with the mass distribution law derived, for a Universe in dynamic equilibrium, in this paper.*

A point which must be emphasised is the fact that, contrary to what was until recently commonly believed, local isotropy about every point in a distribution *does not* imply homogeneity in that distribution. Homogeneity only follows from local isotropy everywhere when the distribution concerned is analytic, or regular. [Mandelbrot, 1982; Pietronero & Tosatti, 1986].

23 THERMODYNAMIC EQUILIBRIUM: THE EVIDENCE

According to the presented theory, a Universe in gravitational equilibrium must have a self-similar matter distribution of fractal dimension two; the observations give strong credence to such a matter distribution, and therefore, within the present theoretical context, give strong credence to the idea of a Universe in gravitational equilibrium. However, if the Universe is in gravitational equilibrium, then it is to be expected that it will also be in thermodynamic equilibrium with respect to the various energy sources within it. The following summarizes a review given by Assis & Neves [1995] of the evidence supporting the argument that the cosmic ray flux, the cosmic background radiation (both extragalactic sources) and our own galaxy's starlight field are in thermal equilibrium.

One of the earliest (if not the earliest) predictions of a background temperature to space, and estimations thereof, is that of Guillaume [1896] who used Stefan's Law [1879] to calculate the equilibrium temperature, arising from stellar radiation, of an inert body placed in the interstellar space of contemporary understanding; this was equivalent to calculating the 'temperature of space', and the figure arrived at was $5.6^\circ K$. A similar black-body calculation was given by Eddington in 1926 (reprint 1988), and he arrived at the figure $3.18^\circ K$, calling it explicitly the 'temperature of interstellar space'.

It was known by 1928 (Millikan & Cameron) that cosmic rays have an extragalactic origin and, subsequently, Regener [1933 or 1995 for an English translation] calculated the equilibrium temperature of an inert body (having the necessary dimensions to absorb cosmic rays) which is placed in a 'sea' of cosmic radiation, and found this to be $2.8^\circ K$. Regener went on to argue that, because of the extragalactic origin of cosmic rays, and because of the (assumed) extreme weakness of starlight in inter-galactic space, then $2.8^\circ K$ must be the 'temperature of intergalactic space'.

The earliest *Hot Big Bang* predictions for the existence of the CBR with a black-body spectrum were given by Alpher & Herman [1949] and Gamow [1952,1953], and these authors estimated the 'temperature of space' variously in the range $5^\circ K$ to $50^\circ K$; After the observations of Penzias & Wilson [1965], we are now aware that the CBR does exist as an additional extragalactic energy field, with a temperature of $2.7^\circ K$.

So, there are at least three independent sources of energy - galactic starlight, cosmic rays and the CBR - which have been used to estimate the 'temperature of space', giving answers which suggest that the three sources are in near thermodynamic equilibrium. In addition, Sciama [1971] has pointed out that the turbulent energy density of interstellar gases and the energy density of the interstellar magnetic field is similar to that of the aforementioned sources, and so the net picture is entirely consistent with the idea of a Universe which is in an approximate thermodynamic equilibrium.

24 SPECULATION AND THE ROLE OF THE VACUUM

Prior to §18, $M(r)$ was considered as simply an abstraction of the observed constraints on the particle-ensemble motions, and served merely as an aid to calculation; this is effectively how Newton viewed his law of 'Universal Gravitation' for which, famously, he 'posed no hypothesis'. However, in §18, $M(r)$ was given the interpretation of a global measure of the amount of mass contained in a specified volume, and it was subsequently shown how this interpretive model led to an exact conformity with the distribution of condensed material which appears to be observed in deep space surveys.

This interpretation of $M(r)$ leads to an interesting problem: specifically, it was shown in §16 how the identification $M(r) = \gamma_0(r/r_0)$ led directly to Newton's Theory of Universal Gravitation, known to be extremely accurate within our solar system; but this specification of $M(r)$ for local gravitational effects does not appear to be consistent with the given interpretation of this function in the conditions of the solar system - that is, the amount of mass in a sphere of radius r (centred on the solar system barycentre) does not appear to vary as $M(r) = \gamma_0(r/r_0)$.

However, we have ignored the possibility that the mass of the material vacuum - first predicted to exist by Nernst [1916]², and first predicted to have detectable consequences by Casimir [1948] - might have a fundamental role to play in gravitational phenomena. It has been speculated by many that ponderable material somehow condenses out of the vacuum - Nernst [1912,1937] was perhaps the first - and, in this case, it might be expected that relationships exist between material condensates and the material vacuum from which they condense. For example, it might be that material condensates create gradients in the physical properties of the material vacuum in their near vicinity. One can then conceive the idea that, in an isolated system like that of our sun, these gradients are such that the distribution of

²As the zero point radiation having energy density at frequency ν proportional to ν^3

total mass (vacuum mass plus condensate mass) behaves as $M(r) = \gamma_0(r/r_0)$. According to this view, when condensed cosmological material is observed distributed according to $M(r) = \gamma_0(r/r_0)^2$ and therefore in equilibrium according to the presented theory then, on the same scale, the mass of the material vacuum must also be distributed in the same way, if the equilibrium is to be maintained.

Referring to §5 in which, we remember, $M(r)$ (in the form of $U(r)$, the reference scalar) was first introduced as a *spherical influence* which *appears* to exist, then it is possible to understand gravitational action in terms of the response of condensed material to local vacuum gradients - perhaps as the process which causes condensed matter to 'evaporate' back into the vacuum. Whatever the realities of the situation, it seems reasonable to expect that, given the material vacuum exists and has mass, then it will exert gravitational influences according to the form of its distribution.

25 CONCLUSIONS

It has been shown how a minimal statement of gravitational phenomenology, given within the context of Newton's Third Law, leads to a metric theory of general gravitational processes in which the metric is defined in terms of the geometry on the level surfaces of a phenomenologically introduced scalar function, $M(r)$, of unknown form and, initially, unknown meaning. The theory is scale-invariant up to the specification of this $M(r)$ and the process of specifying it defines the explicit relationship between time measurement and distance measurement in the theory; it is this relationship between spatial and temporal measurement which represents a particular theory of gravitation. For example, the most elementary specification, $M(r) = \gamma_0(r/r_0)$, leads to Newton's Universal Theory so that, in consequence, Newton's Universal Theory becomes a theory which is *derived* from within the context of Newton's Third Law.

The function $M(r)$ is initially without a physical interpretation, but is finally given physical meaning as the 'mass distribution' function within the model Universe; since the relationships between temporal and spatial measurements are *defined* in terms of this function then, in effect, the matter distribution determines the physics of the space and time measurement processes - a result which coincides with the strictest possible interpretation of Mach's Principle.

A cosmology is derived by imposing the *Cosmological Principles* of the isotropy of local invariant measurement conditions together with an everywhere bounded potential energy; this cosmology gives a *unique* specification of the mass distribution function (that is, without disposable parameters) and, according to this function, material in the Universe is distributed according to $M(r) = \gamma_0(r/r_0)^2$ about an *arbitrarily* chosen centre in the model Universe. This prediction is in exact accordance with the most recent analyses of modern wide-angle and deep pencil-beam surveys, [Baryshev et al, 1995] according to which the distribution of matter about arbitrary points appears to behave as $M \propto r^2$. To summarize, the presented theory is unique in making *predictions* of the form of material distribution, and it appears to have made predictions which are in exact accordance with the observations.

Finally, the theory gives reasons for believing that the phenomenologically introduced function, $M(r)$, should be considered to represent the distribution of *vacuum mass* in a given volume, in addition to the ponderable condensed mass.

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A NEW WAY OF SCIENCE, A NEW TYPE OF RATIONALITY

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1. INTRODUCTION

I should like to plead the thesis: Contemporary Science transforms itself to a new stage of rationality. I will call this stage of rationality „constructive rationality“. In my opinion we can distinguish two types of rationality in the previous development of science. There were the Socratic or contemplative type of rationality and Galilei or mechanical type of rationality. If we want to explain both Socratic and Galilei types of rationality the best way to do it is explanation by the means of conceiving of truth. The conceiving of truth is always germane to rationality.

2. SOCRATIC RATIONALITY

The truth consists in a coherence of thought processes in the Socratic type of rationality. We can ascertain the coherence of thoughts by the answers to antagonistic propositions. The thoughts have to satisfy the demands of logical consistency if we want to identify them as true propositions. What do I mean? Greek philosophers from the classical time have assumed that the basic method for cognition of truth are the pronouncements of oracles and dialogue. The first method is acknowledgment of the will of Gods by the means of Pythic ecstasy, the second one involves the use of the human mind. In modern times we can see how this method has been transformed into forms of intuition and logic operations. I should like to devote my attention to the second form. About intuition and the role of dreams in scientific research I spoke here in the ANPA meeting six years ago (Pstružina, 1989).

When we are in the process of dialogue we can unveil something that is hidden behind words. The words can have different meanings; different words can have the same explanation. Gottlieb Frege pointed out on this fact (Frege, 1936).

„Words, words, words“ Hamlet said, but what is hidden behind words? Perhaps you remember the scene from Hamlet when the players were coming to Elsinor and they played the drama: „The Murder of Gonzago“ and where Hamlet was added several verses (Shakespeare, 1984). Behind the words something was hidden and Hamlet was not watching the stage but the face of his uncle - the King of Denmark. Hamlet knew how his father was murdered, the ghost of his father told him, but he needed the proof, and using the words of drama he could get this proof. It could help him. The words of drama could be the means to discover the reality of world.

Greek philosophers assumed that dialogue is an adequate method for discovering of truth. They were not interested in real things or occurrences but by the thoughts that were represented by words. Occurrences were the mutual activity of people and Gods. For example Achilles was killed by the dart of Parides. The direction of the dart was regulated by Appolon. The Greek's Gods entered into the plot, but they had been hidden, they had not been seen.

Theory was the means for the unveiling of the will of Gods that had been hidden behind of occurrences. The word *theoria* is compounded from „*theos*“ (God) and „*orein*“ (behold). Things and occurrences are veiled by the darkness of appearances and words and we can penetrate to the essence of them by dialogue. The Greek word „*aletheia*“ (truth) involves „*Lethe*“, the river of forgetfulness. *Aletheia* means to unveil or be reminded of something that we have known when we had been in Hade's realm; how to speak about Parmenides or when we our souls had lived among ideas, what is Plato's conception.

The modern continuation of this tradition starts with Cartesian philosophy. R. Descartes believed, that if we want to know something about the world we must start with the investigation of our thought processes or scientific method. Wittgenstein's method of logical investigation of scientific statements is a continuation of this tradition.

The world has been, from the point of view of the contemplative type of rationality, tied together with the life of people. The cognition of the world has not founded the possibility for total control and manipulation. I think, we are living at the time when a new form of Greek dialogue is emerging. It is dialogue that is carried out on electronic networks.

I hope this dialogue could breed a new Socrates too.

Participation in dialogue unveils what is covered by silence. Dialogue is a performance of opinion and everybody exposes himself or herself to the opinion of other people. When we take a part in dialogue we cannot say „it is not our business“ because we have emerged from the shadow and we are forced to give answers. The answers might be from time to time very painful. A pain can arise from the fact that we can hear something that we did not know about ourselves. Thinking is by the means of dialogue opened to critical screening. Thinking is opened to the world. Speech is the basis for understanding and for agreement. We can hear nothing without speaking and we must hear the answers even they are painful, if we want to understand how the world is.

We are living in the world where all thing are off-the-shelf products for usage. Words are also pre-prepared for certain situations and we learn to use the correct words for a certain situation. We have to expose the world to the same questions again and again when we want to know the truth about both the world and ourselves.

But one question still survives when we are in the contemplative type of rationality and it is the question of whether reality is the same as a speech and the logic of our thought processes.

3. GALILEAN RATIONALITY

The truth is correspondence between things and scientific findings in the Galilean type of rationality. This type of rationality with dominant with Renaissance and after. This does not mean that the contemplative type of rationality was finished or even nullified but only that a new dominance has been established in the scientific approach to investigation of reality.

We can illustrate the difference between this type of rationality and the contemplative type of rationality by the different approaches to knowledge that were carried out by Giordano Bruno and Galileo Galilei (Namer, 1975). They both vindicate the same conclusions. The universe is unlimited. But for Giordano Bruno this thesis was underlaid by the belief that God as an unlimited Being cannot create an universe that is limited. Galilei asserted the same statement but he was needed the proof for such assertion. He found this proof by means of a telescope. It was needed to see that the universe is Copernican. For him the truth is not hidden in thesis or words but in reality and we can see how the world is by means of experiment and measurement.

The truth is hidden in reality and experiments are tools for discovering truth. We must torture reality and reality must give the answers to our questions. It is not important what we think about reality, what Aristotle or Thomas Aquinas or the cardinals thought about reality but it is very important how we can penetrate to reality. The truth is hidden under a veil of appearances; and the processes of knowledge are uncovering or peeling off the veil. The truth is inside of things and we can distinguish them. Both experiment and measurement are basic methods for a knowledge of truth.

Scientific findings about things have to be verifiable by experimental work that consist of measurement or by logical verification. The propositions of science have to be verifiable. The verification is carried out by experiment or by logic. Logical verification is from this point of view a new position for contemplative type of rationality.

Reality is calculable and measurable. Reality is number-like in this type of rationality. (God was the best mathematician.) The findings of science enable control and manipulation of reality. Knowledge is power; how to control the world. This thesis we know from F. Bacon's time. Manipulation objects leads us to this critical point in our history. We have particular knowledge of the parts of reality but we know little or nothing about the whole. Man is part of the process of calculation and manipulation too, N. Machiavelli postulated that.

Scientific knowledge splits the world into objects and subjects in this type of rationality. Science by this means splits what is inseparable. The new unification of science in the sense of consolidation of objects and subjects is impossible if science uses only experiment and measurement. Galilei's type of rationality leads us to the stage of the grief of Reason, similarly to Gribojedov's drama.

4. CONSTRUCTIVE RATIONALITY

I think modern science is currently approaching a constructive rationality. The task of modern science does not consist of unveiling or discovering truth that is hidden in things but in the construction of a new reality that never before has existed.

If I had to give some examples of such a new form of reality then such examples could include gene manipulation, plastics, chips or virtual reality. We can ask

what is the truth of this new form of reality and what this new form reality represents. In my opinion the new reality represents our imagination and the truth of the new reality consist of a construction of new reality germane to the conditions that are needed for the realization of these new things.

And again, this new type of rationality does not mean that the preceding form of rationality has finished or even been nullified but only that a new dominance has been established in the scientific approach to the investigation of reality.

The first step of scientists in construction is the choice of how reality could be. This step is elaborated from the functioning of reality and from the conditions that we know. But we have not to know all the detail or all mechanisms of how the reality works. We can use the black box method similar to S. Freud's conception of subconsciousness or N. Wiener's method of input and output.

The next steps of science consist in the realization of these imaginings. We can return to ideas about the same object and we can constitute other objects and hence consequently another truth. These, our imaginings are not Plato's ideas but ideas that are inner to our minds.

I mean ideas that are not entirely independent but ideas that are dependent on the conditions of their occurrence. The construction of new things must always be based on scientific findings but the truth of some theory is also a construction from our ideas and findings. Such construction of truth has not been done arbitrarily, or even capriciously; but scientists must define the conditions that are necessary for the validity of truth.

The Popperian method of falsification supposes that all propositions of science must be testable and all propositions that are not contradictory by means of logic or observations are true. Truth is not only unique one but there could be many true propositions about the same reality; how do there exist many worlds, for example micro-world and macro-world, worlds of dreams, of virtual reality and so on? Feyerabend's thesis about proliferation of hypotheses is correct in my opinion (Feyerabend, 1989).

An observation of reality of the world does not proceed to a construction but every construction proceeds to an observation. Even experiments are constructions. We can find only such observations that we are looking for. A construction is founded by ideas and experience, is only a base and does not mediate stimula for formulation of scientific hypotheses. Of course, experience is inferred from some form of observation but the role of observation in modern science is changed. What does observation mean in today's science? J. Fodor gave such example: A professor delivers his lecture in Australia, his assistant is looking at a TV in American laboratory while the apparatus records the motions of rats. On the next conference the professor will talk: „We observed such motions of rats that prove ...“ and so on. Who has been observed and what?

What are really the findings of reality? There are several shots from bubble chambers, several fragments of bones from the Gobi-fora desert, several statistical particulars, etc. We construct the mosaic that we call „theory“ from these fragments of findings; such a story is authentic, credible and verifiable or testable. We create science in the discourse that is practiced under the methodological rules. Therefore it is not important to know the truth but to be in stake of truth.

Every alternative scientific approach is realized in specific discourse; we can characterize the development of science as the transition from one discourse to another and every alternative discourse constructs its truth. We construct the new world from the parts of reality and we construct the new truth from the parts of findings. Such work is compilation or composition or combination or construction. But this compilation must respect the wholeness.

Why can we speak about the narrative character of modern science? We construct reality in a similar way to the construction of stories. Every story needs characters and plot and science is looking for a chain of events, too. This looks like the working of memory. Memory is not only remembering but also forgetfulness and forgetfulness has the same importance as remembering in our life. Forgetfulness cleans our memory of many details and enables to us to reconstruct our life as a story. Similarly, science must also take no account of many details and peculiarities and take into consideration only such facts that are coherent. Of course, it is only the skeleton of a story how nature is and the best case is if we can include into the story all the details we know; but we can create different theories by using the same findings. Writers use this method very often and they can create several different stories from the same facts (for example historical facts). We can find the same principle in nature, too. Soot and diamonds have the same essence get they are different things. But we need a blank mind from time to time if we want to create original plot. The claim on originality is very relevant. Imitation is the way to decadence. Both examples are forms of construction of truth.

However I cannot agree with Foucault's (Foucault, 1994) thesis that the author of narration is redundant, that science is formed by the trans-subjective milieu, or by universal structure. In my opinion the subject is not only the dominant factor but also is inseparable from scientific work because science is not only knowledge but also responsibility for results. Trans-subjective structures are always very anonymous. On the other hand the science is very particular.

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ELEMENTARY PARTICLE OR 'WAVICLE'? SEEING THROUGH THE QUANTUM FOG

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*Without this continual attempt to penetrate to an underlying reality,
science would be a sterile, passive and rather unemotional activity.*

Jim Baggott

*I cannot believe that it is impossible to make sense of the physical
world. I think that it is the task of physics to do precisely that.*

Roger Penrose

FOREWORD

Human beings are a curious lot. They want to see the light, and are hardly ever content with a mere inkling as to *how* things work. They can go to great lengths to gather *why* things work - and most of all, *why* they do exist at all.

My aim here is twofold. It is to pay tribute to the quintessential human thirst for meaning and understanding; *and* to improve our intuitive grasp of the world. (There is a widespread belief that intuition does not sit well with quantum physics: I want to prove this wrong!)

What follows rests on one leading thread. This thread is the overarching notion of *parallable* quantum wave, which we are about to discover. Through it, I hope to show that science is more than a cookbook. (The trick could just be to pick out the right concepts from the start!)

This paper tries to bolster a better grasp of such hitherto murky and hazy topics as: wave-particle duality, measurement and objectivity, quantum uncertainty, quantum nonseparability; and... the conscious brain. (*The last point - about consciousness - will be taken up in Appendix A.*)

Modern physics - whether quantum mechanics (QM), quantum field theory (QFT) or general relativity theory (GRT) - is fraught with pitfalls and wobbly ends. Yet I believe and wish to show that there may still be room for a consistent "new realism" within physics.

The present endeavour is rather speculative in nature. It is something of a philosophical digression that points to possible new avenues of thinking about the world we live in. Most of the issues at stake will only be broached, in what is by no means a thorough, let alone rigorous, account.

One last point: although it contains no particular difficulty, this article is primarily intended for the reader with a prior knowledge in physics.

INDEX TERMS: Quantum physics, Measurement problem, Conscious brain, Cognitive iceberg

PART I. THE QUANTUM HOAX

Section I.1. Facts of leg-pulls?

The basic - and odd - features of the quantum world are particularly well captured by the famous Young's two-slit experiment, where the behaviour of a particle as small as an electron depends on whether one or two slits (some good distance away) are available to it as it passes through the screen. In some mysterious way the (*pointlike?*) electron encodes information about a comparatively vast structure in its neighbourhood, and responds accordingly.

Let us see more precisely what happens. In this experiment, light passes through a narrow chink O after which it encounters a first screen containing two slits A and B and finally reaches a second screen where it is observed. When both the slits are open, we get on the second screen - the detecting one - interference fringes (i.e. a zebra pattern of alternating dark and light bands). The bands are the result of light rays going through both slits and interfering with each other, like the waves of water rippling from pebbles dropped into a pond. They establish the wave nature of light, which behaves as if each photon had split and followed both paths simultaneously.

The zebra pattern disappears, though, if the experimenter does something to identify which slit any particular photon goes through. Having this knowledge means that the particle of light cannot have gone through both slits, a prerequisite for the stripes. The cumulative pattern appearing on the second screen is the same, regardless of whether light is sent through the slits one photon at a time or in shaft.

How can we understand this? To start with, can we know what goes on as an individual

photon flies across the first (two-slit) screen? To find this out, researchers have put detectors right after the two slits. These additional detectors monitor the photons, revealing whether each photon passed through one slit or another. Now lo and behold: knowing about a photon's behaviour at the slits makes the interference pattern vanish. And, upon switching off the detectors, the zebra stripes return!

This mind-boggler is puzzling on several grounds. Let us think it out. Whether or not photons create the zebra pattern is determined by the behaviour *at* the double-slit screen. How can what photons do at the slits be affected by a detector that they encounter *after* the slits? (This harks back to Wheeler's delayed-choice experiment.) Even if experimenters slip in the detectors after the photon has passed the slits, the photon still 'knows' what to do. Its past behaviour - at the slits - is made conform to whether or not it 'should' make the zebra pattern.

As explained in [Davies and Gribbin, 1992], p 171, with electrons and holes instead of photons and slits, "the electrons not only know whether or not both holes are open, they know whether we are watching them, and they adjust their behaviour accordingly. (...) Unless someone looks, nature herself does not know which hole the electron is going through. (...) Hence, each photon or electron must somehow *individually* take account of whether both or only one hole is open. But how can they do if they are indivisible particles? On the face of it, each particle can only go through one slit. Yet somehow the particle 'knows' about the other slit."

Einstein wrote: "The eternal incomprehensible thing about the world is its comprehensibility." With the above, we have no qualms about such a "comprehensibility"! Indeed, at its very heart, QM is inexplicable, as most physicists would confirm. One of its prominent founding fathers, Niels Bohr, went as far as to declare: "If anyone says that he can think about quantum mechanics without becoming dizzy, that only shows that he has not understood anything whatever about it." Decades later, Richard Feynman wrote: "Nobody really understands quantum theory."

A. Fine explains in a telling way the reason for this feeling of queasiness which QM may inspire: "Quantum theory is the blackest of black-box theories: a marvellous predictor but an incompetent explainer". (*Reported by J.T.Cushing.*)

For all its weirdness however, QM has passed all the possible tests and every reality check you care to name, with flying colours at that. Besides, it successfully provides the foundations for all our understanding of the molecular, atomic, nuclear and subnuclear realms. It definitely cannot be dismissed out of hand. The four basic physical postulates of QM are, in its standard version named the Copenhagen interpretation (adapted from [Schiff, 1988], p 46):

1. In case a quantum system is not observed, it can be represented by a wave function (or a state vector), often noted $|\psi\rangle$, which belongs to an abstract (Hilbert) space. The evolution of $|\psi\rangle$ is given by a wave equation (usually Schrödinger's equation).
2. Each dynamical variable that relates to the motion of a particle or a micro-system can be represented by a linear operator A , to which is associated a linear eigenvalue equation: $A|a_i\rangle = \alpha_i|a_i\rangle$, where $|a_i\rangle$ is the eigenvector of A corresponding to the eigenvalue α_i .
3. One or another of the eigenvalues α_i is the only possible outcome of a measurement of the dynamical variables represented by A .
4. The likelihood p_i that a measurement on the system $|\psi\rangle = |\psi(r, t)\rangle$ with respect to the variable or "observable" at hand yields the result α_i is proportional to the square of the magnitude of the projection amplitude, or to that of the amplitude of overlap, of $|\psi\rangle$ with $|a_i\rangle$. (In short: $p_i = |\langle a_i|\psi\rangle|^2$.)

[All the state vectors are normalised to unity, as is customary in QM. The projection operator Π_i corresponding to $|a_i\rangle$ projects $|\psi\rangle$ into the eigenstate: $\Pi_i|\psi\rangle = |a_i\rangle\langle a_i|\psi\rangle$. We plainly have: $A(\Pi_i|\psi\rangle) = A(|a_i\rangle\langle a_i|\psi\rangle) = (A|a_i\rangle)\langle a_i|\psi\rangle = (\alpha_i|a_i\rangle)\langle a_i|\psi\rangle = \alpha_i(|a_i\rangle\langle a_i|\psi\rangle) = \alpha_i\Pi_i|\psi\rangle$.]

In standard QM, there are at least two other basic postulates. One is the highly objectionable Copenhagen Cut Postulate, formulated in Section I.2; the other is necessary to explain the division of particles in two categories, i.e. into (solitary) fermions and (gregarious) bosons. It relates to the phenomenon of quantum indistinguishability, presented in Section II.1.

Finally, a short note on terminology. In order to keep it disambiguating, I shall occasionally introduce (very simple) new words and phrases. For instance, since in QM all particles are associated with waves, "wave-particle" or wavicle strikes me as better labels than "particle", if we are to eschew potential confusions when dealing with subatomic objects. I shall also, for the sake of shortness, use at times the words "quantum" or "quantal" rather than "quantum-mechanical". The same goes for "mingled" instead of "superposed" (about quantum

states).

Section I.2. The observer-creator-wizard

It is widely acknowledged that the basic mystery of QM revolves around either the measurement or the superposition problem. Quite simply, a quantum measurement is an observation of a microsystem, aimed at measuring a physical quantity (be it its position, momentum, spin or polarisation, or any further "observable"). It always involves a macroscopic measuring apparatus, and a conscious observer. I introduce the superposition principle first.

"You can add quantum states just as you can add waves": that is all there is to the superposition principle! At the quantum level, if one superposes (i.e. combines or intermingles linearly) two "things", one gets yet another "thing". Innocuous though it sounds, it is actually very odd.

I find it convenient to make a distinction concerning quantum observables: I split the physical quantities into H-observables and N-observables (more on this in Appendix B). The former refer to the energy, whilst the latter includes any non-energetic variable (e.g. the momentum, the spin, etc.). Likewise, I distinguish H-operators (i.e. the energy operators, or hamiltonians) and N-operators (non-hamiltonians). Furthermore, I shall occasionally deal with {H-pure, H-mingled} states on the one hand, and {N-pure, N-mingled} states on the other hand, where:

- *H-pure* means that the system has a sharp, or definite, energy level (i.e. it is in an energy eigenstate - duly normalised to unity as I implicitly assume everywhere here)
- *H-mingled* means that the system has a fuzzy, or ill-defined, energy value (i.e. it is a superposition of different energy eigenstates)
- *N-pure* means that the system has a sharp value with respect to a N-observable (i.e. it is in a N-eigenstate corresponding to a N-eigenvalue)
- *N-mingled* means that the system has a fuzzy value with respect to a N-observable (i.e. it is a superposition of different N-pure states)

As we shall find out shortly, there is a silent core assumption in QM, to the effect that 'real' micro-systems (e.g. particles, atoms) can only possess definite values for their observables. (This is well exemplified by the two-slit and the Stern-Gerlach experiments, for position and spin respectively.) We can make it explicit by expressing it through the (Standard) Quantum Existence Criterion, which reads: *A quantum system is real whenever it has a sharp value with respect to a given (e.g. measured) observable. It has no genuine physical existence and is a mere set of abstract potentialities any otherwise.*

We are led to the outlandish image of a microworld split in two subcategories: that of the "fuzzy", and that of the "sharp". Spooky mingled states are thought of as *clusters of overlapping unphysical potentialities*, and pure states, as representing *actual physical systems*. The abysmal gap between them is bridged by the act of measurement which, unlike anything else, causes the (ghostly) fuzziness to give way to an ever sharp and definite (i.e. physically real) result.

The cornerstone of what I dub the "quantum hoax" (that is: of the prevailing "Copenhagen interpretation" of QM) is the standard theory of measurement. Here is a rough outline of this theory. In Bohr (or Copenhagen) interpretation of QM, the observer plays a central role. He is the agency behind the occurrence of *any* wave function collapse. Such a collapse can be defined in the following terms: (from [Flood and Lockwood, 1995], glossary):

According to the standard interpretation of [QM], the effect of carrying out a measurement or observation on a quantum mechanical system is to collapse the wave function, which in general will take the form of a superposition of states [*initial mingled state*] corresponding to the various possible results of the measurement, into just one [*final pure state*] of the states making up the superposition, i.e. that corresponding to the result of the measurement.

Taken to the extreme, QM seems to tell us that "nothing is real unless we look at it, and it ceases to be real as soon as we stop looking." In the face of it, should we invoke the role of the (*unphysical?*) mind of the human observer, as put forward by Eugene Wigner?

Indeed, this view that there is no objective reality (in the sense of an external world independent of our observation of it) does not add up, for a number of reasons. A first intimation that this interpretation may have gotten badly thrown off track is the fact that, as pointed out by Roger Penrose (in [Flood and Lockwood, 1995], p 60), the collapse "must always take place sometimes *prior to* an observation being made."

So, the observation-bred reality would actually be created *before* the observation that

creates it? This clearly doesn't make any conceivable sense. Now I bring into focus the measurement set-up. It comprises the apparatus *plus* the observer:

- The apparatus is always *macroscopic*; it includes an analyser (which somehow prepares the object to be measured) and a detector (which plainly detects)
- The observer is conscious, so as to be aware of what goes on (some argue that a human mind is mandatory)
- The microsystem to be measured - with respect to either a N-observable or a H-observable - is generally bereft of any definite value prior to the act of observation (... *yet remind Penrose's remark!*)
- The measurement result is always definite, or sharp (this entails that the act of measurement has prompted a so-called "collapse of the wave function" on the microsystem, from its initial mingled state to a pure final state)

Analysers can be many different things. Birefringent calcite crystals make natural analysers, acting on the polarised light. Other examples are: beam-splitters and half-silvered mirrors (they lead to mingled states superposing the reflected and the transmitted partial waves); potential barriers (that are conducive to tunnelling effects *if* a collapse occurs); and so on. As for detectors, natural specimens are human and animal eyes! (To be specific: the retina is a detector while the crystalline lens is an analyser.)

One may wonder: What is so special about measurements? The trite answer is: "an awful lot". The conundrum is (at least) threefold:

- The measuring apparatus, on interacting with the (mingled) microsystem which it is about to process, should become mingled in its own right before detection. How is it, then, that such a macroscopic superposed state never takes shape?
- The "collapse" *cannot* be accounted for as a normal, if extremely complicated, interaction between the microsystem and the instrument. Clearly, we miss out on something - what is it?
- Why should the measurement procedure yield an ever definite outcome?

(Indeed, the measurement puzzle is deeper than that. For instance, it is compounded by the strange phenomenon of "non-separability" or "supraltness", outlined in Section III.1.)

The first point is that an account of measurement which fully respected Schrödinger's equation would tell us that the measuring instrument passes at some stage in an "entangled" state akin to a macroscopic superposition of states. However, we know from experience that measurements always keep the macroscopic apparatus in a determinate state throughout.

To sort this problem out, Niels Bohr introduced a 'cut' between the observed *micro*-system and observing *macro*-system. I shall henceforth refer to it as the "mic-mac cut" (a shorthand for "microcosmos-macrocosmos cut"). Its formal justification is the **Copenhagen Cut Postulate**, which posits that: *Whilst at the microphysical level matter obeys the laws of QM, it behaves classically - not by approximation but essentially - at the macrophysical level.*

This is a cheap (but efficient) way to rule out the unobserved mingled states that should form in the measuring instruments. The "mic-mac cut", as expressed by the above postulate, implies the following statement: *All macroscopic objects (including measuring devices) must be characterised by commuting operators exclusively.* This in substance asserts that macro-objects, being classical, are devoid of quantum "fuzziness" (no uncertainty principle holds for them).

With that, we can readily grasp why the mic-mac cut is emphatically *not* acceptable, for various reasons. To begin with, it is a completely *ad hoc* and totally unwarranted assumption (which makes for poor physics!). Furthermore, *macroscopic* quantum phenomena *do* exist, e.g. superconductivity, superfluidity, laser emission and so on.

But this is not all. Recall the famous photon-in-a-box thought experiment presented at the sixth Solvay Congress in 1930. [Gibbins, 1989] reminds us (p 69) that Bohr "argued as follows. First, and as usual, [*sic!*] apply the indeterminacy [i.e. uncertainty] relations to the apparatus thoroughly." Gibbins adds (p 70): "Bohr's argument is again that a *consistent and thorough* application of the indeterminacy relations yields no contradiction."

Amazing! This alleged contradiction-free argument is in flat contradiction with the Cut Postulate! Admittedly, "Bohr was not happy with his victory over Einstein." (Bohr, to be sure, was having the cake and eating it too...) The question is now: *How is it that no physicist (Bohr included) ever came up with a full-blown Copenhagen-friendly version of Bohr's argument?*

The answer is: because there is no such 'Copenhagen-friendly' solution. *Ergo...* the standard interpretation, which hinges on this untenable mic-mac cut that precludes just that, leads to a devastating contradiction. In the wake of this discussion, we are left with a rather inauspicious alternative:

- Either there is no "mic-mac cut"; and QM appears to be inconsistent (since macroscopic mingled states akin to the Schrödinger's cat "dead-alive" entangled states should arise prior to any measurement, and it *cannot* explain why they actually do not arise)
- Or there is such a cut... and this provides a loophole whereby it is *always* possible to foil the uncertainty principle, by making use of a suitable macro-object to bypass it. This possibility again means that QM is inconsistent.

Section I.3. One probability too many

We have learnt that QM harbours notions that defy everyday experience, and that it brings about a host of ideas that threaten our very notion of reality. For instance, we came across this view that at the quantum level things exist only as quantum wave probabilities, till an act of observation by a human mind "collapses the wave function" and temporarily makes things real. (As soon as we stop looking, the electron dissolves back into a spreading pool of probability waves.)

The idea here is that quantum particles are somehow linked to spread-out *unphysical* (pilot?) waves, where the size (or magnitude) of the wave at any particular spot is related to the likelihood of finding the particle there. This defines the Copenhagen interpretation of QM.

The quantum wave is "not a wave of substance, but only one of probability". Strange... *yet true?* Personally, I am far from convinced. This scepticism can be justified by going back to the formal underpinnings of the alleged probability wave. Using the Dirac bra-ket notation again, with the bras ($\langle\psi|$) and kets ($|\psi\rangle$) normalised to unity, we get:

$$P(r, t) = \langle\psi(r, t)|\psi(r, t)\rangle = |\psi(r, t)|^2$$

It reads - for the particle of matter or radiation represented by the wave function $|\psi\rangle = |\psi(r, t)\rangle$ - something like: "the square of the absolute value of the wave function at a given space-time point yields the probability (or more accurately: the position probability density) for finding the particle inside an infinitesimal volume surrounding it, upon observation".

More broadly, the wave function generalised as state vector predicts the likelihood of obtaining a given experimental result (e.g. a spin value, an energy level) relative to a microsystem; be it a subatomic particle, an atom, molecule, ion, etc....

Hereafter are two arguments that, I believe, deal a death blow to the probability interpretation of the wave function. The first one bears on the "ultra-relativistic" particles; the second one hinges on the inexistence of pointlike particles.

The "ultra-relativistic" case, to begin with. (A speck of matter is *ultra-relativistic* when its speed is close or equal to that of light. Photons are prime examples of this.) One can show that any massive particle accelerated near the speed of light has no observable position to an accuracy greater than its Compton wavelength (i.e. $(\Delta x)_{\min} \sim \lambda = h/mc$, where m is the rest mass). In a similar vein, photons have no definite position whatever.

The unsettling upshot of this nonvanishing lower bound of the position spread (or, in the accuracy of a position measurement) is that it is impossible to interpret $|\psi(r, t)|^2$ as before: the probability gloss is thus proved wrong for the ultra-relativistic particles.

Now, when we look at the arguments expressed in favour of the probability wave interpretation, we realise that the ultra-relativistic or otherwise nature of the particle is never taken into account. The reasoning is always conducted regardless of any speed consideration. This is clearly exemplified by Young's experiment: the reasoning and the conclusions are *exactly* identical, whether we use ultra-relativistic photons (with slits) or low-speed electrons (with holes).

The reasoning, being fallacious in the ultra-relativistic sub-case, is irretrievably flawed in all cases. So we have spotted a fly in the ointment, and this fly - a super-fly that whizzes across space at superlative high velocity! - is shrewd and obnoxious enough to topple the whole thing.

But this is not all: there is another killer bug in the edifice. To nab it, let us go back to the heart of the probability wave picture. Recall that it hinges on the statement that:

$dp(r, t) = P(r, t).d^3r = |\psi(r, t)|^2.d^3r$ is the probability that, if we measure the location or position of the particle at time t , it will be found within the differential volume d^3r surrounding r .

It is a matter of course that, were not the observed particle - at least at the time of measurement - strictly pointlike, it would possess a nonvanishing spread. Then it would not be possible to find it inside an arbitrarily small volume (d^3r), and the above statement would grossly be groundless and meaningless. We ought therefore to read it as a claim that the wave function is ultimately defined *with respect to some pointlike entity*:

$$P(r, t) = |\psi(r, t)|^2 = F\{\text{pointlike particle}\}, \text{ where } F\{x\} \text{ means: "function of } x\text{".}$$

So, in order for the probability-wave-talk to make sense, the measured particle *has to be pointlike* (this is why we stumbled on a problem in the ultra-relativistic case). In fact, one can read in textbooks that in standard theory, "particles are mathematical points with zero size." (Loosely speaking, the particle would then be some kind of Dirac δ -function idealisation.) This (generally silent) assumption cannot be eschewed; and it is a very awkward and worrisome one to make.

Going back to the experimental data however, don't we ever obtain (e.g. in the two-slit experiment) a highly *localised* spot on the detecting screen? Isn't it a clear token that a real *pointlike* particle crashed and thus came into being, however fleetingly, at that spot? Well, we know from our discussion of the ultra-relativistic case that this alleged "experimental evidence" has to be taken with a (large) pinch of salt.

It is interesting at this stage to go back to the avowed antagonism of the ghostly "potentialities" (prior to and away from an act of measurement) versus the "actuality" (of a so-called "measurement result"); because it ultimately rests on this idea of a full-fledged classical, pointlike, particle thought to appear - in less than a jiffy - whenever a measurement is carried out.

In substance, what is being said is the following. One identifies, for a quantum system in a given state with respect to an operator A : $\{A\text{-pure} \equiv \text{real}\}$ and $\{A\text{-mingled} \equiv \text{ghostly}\}$ (A is *wontedly taken as the position operator*; but since position is nothing special at all, A can really be any H -operator and N -operator alike.)

Let there be a pair of "incompatible" or "complementary" observables linked to the couple $\{A, B\}$ of non-commuting operators. As we know from the uncertainty principle (Section III.1), for a system in the state $|\psi\rangle$ we have:

$$\{|\psi\rangle \text{ is } A\text{-pure}\} \Rightarrow \{|\psi\rangle \text{ is } B\text{-mingled}\} \text{ and } \{|\psi\rangle \text{ is } B\text{-pure}\} \Rightarrow \{|\psi\rangle \text{ is } A\text{-mingled}\}$$

It means that a state that possesses a clear-cut value for one physical quantity will accordingly have a fuzzy value for complementary quantities. In other words, any particle which is 'real' as regards A (resp. B) is so only at the otherworldly price of being 'ghostly' with respect to B (resp. A)! (For instance, an electron which is 'real' momentum-wise is automatically 'ghostly' position-wise; and vice-versa.)

Clearly, the above criterion for 'reality' versus a 'ghostliness' is not even remotely consistent. Since it goes hand in hand with the "probability wave" interpretation, we conclude anew that the latter has to be dismissed. What is altogether clear is - as I already pointed out (e.g. [Ransford, 1994a]) - that pure and mingled states are exactly on the same ontological footing. There is no question of one being any more 'real' than the other: both mixed and pure states are arrant physical entities.

Let me emphasise that there is no pointlike particle, *whatever the circumstances*. Even when detection "localises the particle", there is no particle! If we set out to pin down this "localised particle", we shall *not* find it. We shall only find "wavy" entities, everywhere and - with the fleeting exception of the paral phases (see Section II.3) - *everywhen*. There is tellingly no room for pointlike objects in the formal theory.

To conclude: When shorn of its faulty pointlike particle idea, the probability wave interpretation becomes empty and shaky. It has therefore to be outright discarded; so we can (lightly) sum up the foregoing with a slogan: **No particle, ever. No probability wave, either!**

No particle and no probability wave: what is left the wave-particle duality? And, more generally: of the broader basket of Bohrian complementarity? Perhaps little more than hot air. As Einstein wryly observed, "the metaphysics of complementarity is a 'tranquillising philosophy' which explains nothing." (*Reported by [Gribbins, 1989], p 83*)

Despite what I believe is a cogent and compelling evidence against it, the probability wave fallacy has managed to become part and parcel of the widely accepted standard interpretation of QM. And this is really what strikes me as the most stunning facet of the whole thing.

But, as Gertrude Stein would no doubt have had it, a hoax is a hoax is a hoax...

My first order of business was to deflate the prevailing interpretation of QM, by bringing to the fore several of its (rather acute) shortcomings. This is a bit pointless however, unless I have something better up my sleeve. This brings me to the second order of the business, which is to outline a tentative "new realism" built on the wave picture (*not* a naive matter wave at that, but a wave with an attitude...).

I am a realist because I contend that quantum waves are real in a full-blown physical sense. Yet they are emphatically *not* the whole picture; because these waves can suddenly vanish to give way to something else - this "something else" being what I call "paral" (it is no longer matter-as-usual). Put differently, they are *parallable*, as we shall find out shortly.

PART II. THE WAVE-PARTICLE MONGREL, OR 'WAVICLE'

Section II.1. And yet, nature is probabilistic!

As we have just made it out, the quantum hoax is one probability too many, or at least one too far. It undeservedly tars the wave function with the brush of an unphysical likelihood! However, probabilities are not to be discarded altogether: they are commonplace indeed at the quantum level. The question is to understand where they arise from and where they lurk.

QM yields probabilities, and as a rule does not promise more when it comes to measuring anything quantal. It turns out that, for all practical purposes, the three basic ingredients of QM are, for an operator A matching any (observable) physical quantity:

1. The eigenstates (that form a complete orthonormal set) and eigenvalues:

$$\{|e_i\rangle\} \text{ and } \{\alpha_i\}, \quad \text{where: } A|e_i\rangle = \alpha_i|e_i\rangle$$

2. The probability of a quantum event (i.e. a "jump" or transition from $|i\rangle$ to the eigenket $|f\rangle$):

$$p_{i \rightarrow f} = |\langle f|i\rangle|^2 = |\langle f(r, t)|i(r, t)\rangle|^2 \int d^3r \quad [= p_{i \rightarrow f}(t)]$$

3. The expectation value $\langle A \rangle$ of the observable:

$$\langle A \rangle = \langle A(t) \rangle_\psi = \int |\psi(r, t)| A |\psi(r, t)\rangle^2 d^3r$$

Furthermore, the mathematical "standard deviation", which is a measure of the spread in the values of the outcomes, is called the *uncertainty* in A (e.g. in position). It is denoted by ΔA and defined by: $(\Delta A)^2 = \langle (A - \langle A \rangle)^2 \rangle = \langle A^2 \rangle - \langle A \rangle^2$ (In other words, ΔA is the square root of the expectation value of $(A - \langle A \rangle)^2$.)

Most of these notions are statistical or probabilistic in nature. They attest to a built-in 'probabilism' existing at the micro-level, and raise the issue of their origin. (Granted, they are not a mere effect of our ignorance.) To figure out where they stem from, we must go back to the basics - and spell out what the two quantum evolution laws are, when they hold.

In QM, two distinct laws govern the change of state of a system. The first one complies with the *least action principle*, whose writ runs as long as the system is not disturbed by an act of measurement. This "least action" motion, or "wavy" motion, is described by a wave equation (e.g. Schrödinger's equation). The second law is the wave function, or wavepacket, collapse. It comes into play whenever the system is subjected to a measurement. Here is a brief portrayal of both of them, pinpointing their differences:

LEAST ACTION: *In case the least action motion, or "wavy" movement, rules throughout, a physical evolution is:*

smooth, deterministic, reversible, relativistic, time-protracted, wave-like, conservation-law-abiding, and... 'supralness-blind' (cf. later, in Section III.1).

COLLAPSE: *In the event of a collapse, the evolution becomes:*

sudden, probabilistic, irreversible, a-relativistic, fleeting, particle-like, conservation-law-violating, and... 'supralness-dependent'.

Besides, as I already pointed out [Ransford, 1994a]: "A paral phase [i.e. a collapse] is unlike usual matter. (...) Here are [two] of its original and distinctly unphysical features: i) It is intrinsically *non-computational*. Hence no equation - whether nonlinear or otherwise - can ever encapsulate it. (This trait can possibly be linked to the Gödelian undecidability); ii) It brings about a *nondeterministic new element of reality*, whereby the [probabilistic] 'choice' of a specific [measurement] outcome is made."

The two laws portrayed above could hardly have been more dissimilar! A collapse is a micro-level event *par excellence*, whereas a "wavy" evolution is a rather bland 'nonevent'. It is actually possible to interpret the onslaught of a collapse as a change of state of matter (and really, of *psychomatter*, as suggested in Appendix A).

Due to this tentative shift in perspective (or in "paradigm"), I prefer to coin the word **paralling** for the collapse of the wave function. I also label it a **paral phase**; **paral** being the name of this alternative state of (psycho-?)matter. (Roughly speaking, paral is to matter what ice is to water: the same basic stuff, but under a different guise.)

It should be noted that the a-relativistic character of the collapse or paralling is in a way made spectacularly manifest in the "E.P.R. paradox", which brings into play nonseparability (or *supralness*: see further down). It has been firmly established by a number of researchers (for some references, see [Brown and Harré, 1990], page 97 footnote 6). It *does not* clash with relativity, since Einstein's theory is valid for *plain matter* only - it does not concern paral in the least. (It exclusively concerns *matter-energy propagation*, as described by the wave equation.)

Interestingly enough, this instantaneity of the paralling opens the daunting possibility of faster-than-light tunnelling. This effect - whereby under the right conditions the tunnelling photons arrive, on average, before those that travelled through the air - has recently been experimentally confirmed at Berkeley, in 1995, by R. Chiao and colleagues. This oxymoronic "superluminal light" allowed by the paral phase in connection with quantum tunnelling is a reality!

I must add a few words over the "wavy", or least action, motion. By far the commonest wave equation used to describe this evolution is Schrödinger's equation, valid for spinless and slow-moving wavicles. In the Schrödinger picture (where the time-dependence is concentrated on the state vectors, the operators staying put), the mathematical expression of the time-independent Schrödinger's equation is:

$$H|\psi(t)\rangle = i\hbar(\partial/\partial t)|\psi(t)\rangle \quad (\text{II.1.a.})$$

It is formally equivalent to:

$$|\psi(t')\rangle = U(t', t)|\psi(t)\rangle \quad (\text{II.1.b.})$$

where $U = U(t, t') = \exp(-iH(t - t')/\hbar)$, and: U is unitary with H hermitian (as usual, H is the energy operator, or hamiltonian). By (II.1.b.) we see that any two states related by the wave equation are linked by a unitary transformation: one can call them *uni-equivalent*. Now the general solution to (II.1.a.) is:

$$|\psi(t)\rangle = \exp(-iHt/\hbar)$$

In the time-independent case of a free system, for which $H(t) = H$, this solution becomes:

$$|\psi(t)\rangle = |\psi(r, t)\rangle = \sum_n a_n \exp(-itE_n/\hbar) |e_n(r)\rangle \quad (\text{II.1.c.})$$

where: $H|e_n(r)\rangle = E_n|e_n(r)\rangle$; a_n constant; all state vectors normalised to unity.

This entails that only two behaviours are available for a free or isolated quantum system, as long as the writ of the least action principle runs: either it remains stationary (in case it is initially in a H -pure state - where it is bound to stay); or it ever oscillates (if it is originally - and at all times - H -mingled) *Absolutely no other type of least action or 'wavy' evolution is allowed.*

We readily conclude that phenomena such as the spontaneous decay of an isolated system *do breach* the least action principle. I shall further investigate this point in the next Section.

Before embarking on it, I want to underscore that the wellspring of quantum probabilities is the paralling exclusively. *Ex-clu-si-ve-ly!* It should be clear (see the above portrayal) that the least action motion has nothing to do with them. This being so, there is absolutely no reason why we should thrust the quantum probabilities onto the wave function itself.

Section II.2.

How widespread is the collapse?

As I see it, most of the conceptual pitfalls of QM arise from the wrong-headed tenet that the paralling (aka the "wave function collapse") is inextricably bound up with the quantal measurement (involving a measuring device and a conscious observer). This stance leads straight to such riddles as Schrödinger's cat, Wigner's friend, Wheeler's delayed-choice, and countless other conundrums of that ilk.

A number of facts call it into question. For instance, there appears to be striking similarities between, say, spontaneous (i.e. measurement-free) quantum jumps and paral phases, as portrayed above. Some nuclei, especially the rarer isotopes, tend to split spontaneously into each other. This type of decay is glaringly at variance with the smooth and well-behaved "wavy" motion. Its irreversible and probabilistic suddenness is strongly reminiscent of the paralling.

More generally, a whole range of phenomena (and in fact, *any* particle-like quantum event!) bear the hallmarks of the paralling. This is a first and potent intimation that the standard theory of measurement may have gone astray. My challenge here is to "de-ghettoise" the paralling, i.e. to argue it out of the measurement ghetto into which it is currently locked. It is appropriate at this stage to recall the experimentalist's standpoint, expressed in [Braginsky and Khalili, 1995], p 19:

So long as one avoids the issue of measurements, one can regard the wave function ψ for a quantum particle as an ordinary classical wave that occupies a certain region V of space. Radical differences from a classical wave show up, however, when experimenters try to "chip off" a part of the wave - i.e., when they insert a calorimeter-type measuring device in some piece of the region V , and with it try to absorb the portion of the wave's energy that lies there.

If the wave were classical, then the calorimeter would absorb all the energy that falls in it, while leaving all the wave energy outside it unaffected. Not so for the wave function of the quantum particle. Inside the calorimeter there must be either all of the particle, or none of it. However, one cannot predict precisely

whether the particle is in the calorimeter or not. One can only give a probability for it being there.

This "either all of the particle, or none of it" points to what I call *quantumhood*. **Quantumhood** is the constraint whereby a photon cannot be "decohered", i.e. divided up into *non-mutually-interfering* parts. Broadly speaking, quantumhood [Ransford, 1994a] keeps subatomic wave-particles from splitting up - and conversely, from blending together - in a haphazard way (so they are said to be *quantised*). This very feature is enough to make the objects of the microworld differ markedly from those of our macroworld.

How to "de-ghettoise" the paralling? It can be done by showing that the spontaneous (i.e. observationless) decay of a free unstable system is necessarily prompted or fuelled by a paralling phase. This, it turns out, is children's play. (It makes it all the more surprising that, as far as I know, it has not been done before: could that be a case of "ideological blindness to the obvious"?)

The demonstration is in two stages. The first one is an elemental piece of logical reasoning, which goes by the breathtakingly beautiful Latin name of *modus ponendo tollens*. The second one offers a criterion that pinpoints at least some instances of measurement-free parallings.

Stage one. QM allows for two mutually exclusive evolution laws, here labelled A and B for convenience. (A stands for the deterministic "wavy" motion whereas B stands for the probabilistic paralling.) Accordingly, a system unfurling over a definite timespan $[t, t']$ will be either 'A-tuned' throughout, or 'B-tuned' at some stage - but not both ways simultaneously. It follows that if we manage to show, by any appropriate means, that a given evolution over $[t, t']$ is not 'A-tuned' throughout, then we shall know with certainty that it has been 'B-tuned' somewhere along the way.

Stage two. Let us consider a free-moving system, whose wave function is $|\psi(r, t)\rangle$. Since it is isolated, hence *measurement-free*, by the standard postulates of QM its evolution is admittedly "wavy" (that is, 'A-tuned' at all time). More specifically as we recall from Section II.1, it is described by the time-independent Schrödinger's equation:

$$H|\psi(t)\rangle = i\hbar (\partial/\partial t)|\psi(r, t)\rangle \quad (\text{II.1.a.})$$

whose general solution can be taken as:

$$|\psi(r, t)\rangle = \sum_n a_n \exp(-itE_n/\hbar) |e_n(r)\rangle \quad (\text{II.1.c.})$$

(where, as before: $H|e_n(r)\rangle = E_n|e_n(r)\rangle$; a_n constant; all state vectors normalised to unity)

As we pointed out in Section II.1, it follows that, for any 'A-tuned' system, two and only two time-changes are available. *Either* the microsystem will oscillate endlessly *or* it will remain forever stationary. To sum up: if an isolated system is strictly 'A-tuned' throughout a given evolution, it oscillates or, as a limiting case, stays put - *to the strict exclusion of whatever else*.

Examples of such 'throbbing' systems are aplenty. One is the ammonia molecule: its coherent oscillations (due to tunnelling) between the two energy minima eigen-states lead to the phenomenon of "ammonia inversion resonance". Another example is the strangeness oscillations of the neutral kaons. Most generally, any unstable system, being H-mingled, will therefore oscillate - until, that is, its eventual *paralling-fuelled decay*, as we are about to see now.

By the foregoing, a decay transition - being neither stationary nor oscillatory - is not 'A-tuned': from *Stage one* we then infer that the decay is 'B-tuned'. In other words, it is "paralling-fuelled". (This is further evidenced by its obvious features: it is sudden rather than smooth, it is irreversible and, as stressed later on, time- and channel-probabilistic. Here are the trappings of a paral phase!)

To conclude, a free unstable system which undergoes a decay transition does so through a paral phase - *regardless of a measurement*. Now, an induced decay exhibits much the same features as a spontaneous one: they both belong to the category of 'B-tuned' events. This brings me to contend that *any* decay - whether induced or spontaneous - stems from a paralling. By stretching this line of reasoning a bit farther, this claim can be made to bear on any quantum jump or transition (i.e. the Compton and photoelectric effects, ionisation and self-ionisation, electron or photon emission and absorption, inelastic scattering, and so forth...).

Now a big question: *Why isn't the wavicle fine with its sole wavelike motion?* What on earth is the point of an additional evolution law, i.e. the paralling? My short answer is: *Were it not for the paralling, nature would be inconstituent*. To make it clear, the best way is to tell a story. Therefore, here is a story, a tale of Lilliputian proportions (adapted from [Ransford, 1993]):

Once upon a time, good old Dwight became a Lilliputian. He became so small that even the wavicle - this minute wiggly cloudlet - took on the size of an apple with respect to him. Dwight, probably to while away his time, soon grabbed nearby tweezers, as they can be found even in the Lilliputian universe. Armed with this tool, he tried his best to pinch the wavicle asunder. (This is in principle *always possible*, since the wavicle is spread out: only a pointlike wavicle could definitely never be torn to pieces.)

Whack!...That's it: Dwight ripped it for good. He then went boasting around that he had divided up the wavicle into two disconnected parts. Cheers for this outstanding feat! *Cheers?* Not quite so indeed, for Dwight is a damn liar. He didn't tell the truth. Really, he failed to split the wavicle up in two halves. No doubt his deed sharply modified the shape of the wavicle, but it didn't divide it altogether.

Apples do not comply with quantumhood: nothing prevents them from being torn to bits, at will. But quantumhood is an airtight constraint for the wavicle, which thus *cannot* be pulled apart anyhow. This makes a huge difference indeed! (It actually accounts for most of the oddities of the quantum world.)

How, then, did the wavicle respond to the nipping? You guessed it: instead of letting itself be torn asunder, it underwent a paralling. Dwight, by insisting on splitting the wavicle, forced it into a paral phase. What happened next? Something most extraordinary indeed: instead of obtaining two half-wavicles as he would have gotten two half-apples, Dwight collected *one* piece only - which therefore contained the whole wavicle. So the latter, in its final state, became wholly concentrated (or 'reduced') on one side of the tweezers. Dwight, by his deed, prevented it from remaining on both sides - *unless precisely it would have been split against quantumhood...*

In a nutshell, the wavicle reacted *as a whole* to the threat of the nipping weapon, in order to vindicate quantumhood. This all-out response momentarily endowed it with distinctly corpuscular (or particle-like) features. This is why it could be confused with a particle then. One may wonder on which side of the tweezers the wavicle ended up in its final state. The answer is that this is utterly unpredictable. Only the *likelihood* to find it on one side or the other can be worked out.

Dwight's anecdote highlights a major finding: *no least action evolution whose end-state would go against quantumhood is ever allowed in nature.* This is the single most important feature which tells the micro-objects apart from the macro-objects. We can come across half-apples, but never across half-electrons, threefold protons or pion-muon mongrels...

When quantumhood is threatened (e.g. when a photon is in danger of being shattered to smithereens, on hitting a detecting screen), a paralling is likely to set in (within a given timespan). (Remember: "So long as one avoids the issue of measurements, one can regard the wave function ψ for a quantum particle as an ordinary classical wave (...). Radical differences from a classical wave show up, however, when experimenters try to "chip off" a part of the wave.")

The paralling deftly saves the day, by removing the initial threatening situation while preserving quantumhood. (The "threatening situation" is the *paralling situation* of Section II.3.) A key point here is that, whatever the *paralling situation* confronting a microsystem, the onset of a paral phase, and the selection of its actual outcome, spring from *within* the system itself (i.e. it is an *endo-determined* 'choice' or 'decision', as it were).

Now we are in a position to understand why, in the two-slit example, any attempt to find out which of the two slits each photon goes through destroys the characteristic two-slit interference pattern. In order to find this out, we need to detect which of the two slits any photon goes through - and this clearly amounts to a position measurement... which quite expectedly elicits a paralling! In this case, any photon eventually hitting the screen and being registered there tells a new story: it underwent *two successive paral phases* on its way to the screen. (The first one happened upon passing across the slits; the second one occurred on the final screen.)

Section II.3. The paralling postulate

My approach, as far as paral phases are concerned, rests on the following. For simplicity's sake, I take - in the Schrödinger picture (time-dependent states, time-independent operators) - the familiar isolated system whose hamiltonian H has a two-level discrete nondegenerate energy spectrum. The initial state (at time $t = 0$) of the system is $|\psi_0\rangle$.

By the superposition principle of QM, we can expand it in the complete set of the (orthonormal) energy eigenvectors. In the Dirac bra-ket notation, it reads:

$$|\psi_0\rangle = |\psi(0)\rangle = a_1(0)|e_1\rangle + a_2(0)|e_2\rangle \quad (|a_1|^2 + |a_2|^2 = 1)$$

where: $H|e_n\rangle = E_n|e_n\rangle$ ($n = 1, 2$), and $E_1 > E_2$ (energy levels for the eigenkets $|e_1\rangle$ and $|e_2\rangle$).

If either $a_1 = 0$ or $a_2 = 0$, $|\psi\rangle$ is H -pure and stays put (stationary state). If $a_1, a_2 \neq 0$, $|\psi\rangle$ is H -mingled and will oscillate as long as the system complies with the least action movement. It will do so according to:

$$|\psi(t)\rangle = a_1(t)|e_1\rangle + a_2(t)|e_2\rangle$$

where, by the wave equation: $a_n(t) = a_n(0) \cdot \exp(-itE_n/\hbar)$ ($n = 1, 2$)

It will therefore oscillate at frequency ν_{12} given by: $h\nu_{12} = E_1 - E_2 = \hbar\omega_{12}$ (where h is Planck's constant; $\omega_{12} = 2\pi\nu_{12}$ and $\hbar = h/2\pi$).

Whenever $a_1, a_2 \neq 0$ (H -mingled state), the system is actually faced with an alternative.

Either, at time t (before any decay), it carries on with its "wavy" motion, *or* it undergoes a paralling. In the second horn of the alternative, it will leap to a new state. The only end states allowed are the energy eigenstates $|e_1\rangle\langle e_1|\psi(t)\rangle$ and $|e_2\rangle\langle e_2|\psi(t)\rangle$. Whether one or the other will actually be chosen is, as we know, a matter of randomness. The respective likelihoods are given by the formulas: $p_1(t) = |\langle e_1|\psi(t)\rangle|^2$ and $p_2(t) = |\langle e_2|\psi(t)\rangle|^2$ ($p_1 + p_2 = 1$)

However the actual world is slightly more complicated. In QM much as anywhere else, "allowed" does not mean "available". (Wealth is generally allowed, but not necessarily available!) There is a number of physical laws and constraints to be reckoned with, which concur to narrow the range of what is physically achievable. Some of these constraints are the conservation laws. There is also *quantumhood*, introduced in the last Section.

Let us for definiteness consider a H-mingled system made up of a set of superposed H-eigen vectors $\{|e_\alpha\rangle\}$. As already said, any of these $|e_\alpha\rangle$ is *a priori* allowed, in the sense that it can be selected as the outcome (i.e. the actual end state), should a paralling occur. But it may happen *not* to be simultaneously available for the system at hand. If so (i.e. $|e_\alpha\rangle$ is allowed yet unavailable), I say that the H-eigenket is *unreachable*. The matching jump or transition is then *thwarted*.

Conversely, an evolution to a *reachable* H-pure state is by definition feasible in practice; the corresponding paralling-fuelled transition is *unthwarted*. By the same token, a H-mingled system for which all the jumps towards the H-pure states are put on hold is itself *thwarted*. Despite its "H-mingledness", a thwarted system will stay in its initial state $|\psi\rangle$, to oscillate forever (unless, of course, new influences change the setting).

An example of thwarted systems is a neutron in a nucleus. A free neutron will decay spontaneously (with a lifetime, or average lifespan, of roughly 15 minutes). Inside a nucleus, its decay is thwarted due to Pauli's exclusion principle (mentioned in Section III.1). A decaying neutron yields a proton, the problem being that this proton has nowhere to go, and so cannot come off! (The proton is a fermion, and all the available sites for it in the nucleus are already occupied: hence all these final states prove unreachable for it.)

These ideas have already been presented, in my ANPA 16 paper [Ransford, 1994a]. They are summed up by the Paralling Postulate hereafter:

The Paralling Postulate

=====

1. A physical system is in a **paralling condition** iff it is in a H-mingled (aka H-superposed) state. A paralling condition becomes a **triggering condition** whenever this H-mingled state includes at least one *reachable* H-pure state.
2. For want of a triggering condition, no paralling or paral phase - or else, no collapse - can ever happen. Then the system evolves in keeping with the principle of least action, and displays wave-like features. (It will be *stationary* or *oscillatory* depending on whether it is H-pure or H-mingled respectively.)
3. In the event of a paral phase, the quantum waves momentarily vanish - whereupon the system breaches the principle of least action and exhibits particle-like properties.
4. A paral phase destroys the initial wavefunction, to beget a new one. The new wavefunction (or state vector) is the final state of the system. We may call it the **paral outcome**, or the **paral product**.
5. A paral outcome is always:
 - a) a pure (or eigen) state of the hamiltonian of the initial system; and, to be more accurate:
 - b) any of the *reachable* H-pure states included in the initial H-mingled state.
6. The initial time of a paralling, and the picking out of a specific paral product, are given by probability laws. These laws can be derived from the *amplitudes of overlap* $\langle e_\alpha|\psi\rangle$ which link the initial H-mingled state $|\psi\rangle$ to its member H-eigenstates $\{|e_\alpha\rangle\}$.
7. Regardless of its H-status (i.e. of its being either H-mingled or H-pure), a system is ever teeming with a welter of *aborted* paral phases, which can be described as *virtual* transitions.

In the above, no room is made for a macroscopic measuring device and a conscious

observer: 'macroscopic-ness' is altogether immaterial, the human observer turns out to be utterly irrelevant. The reality being observed is no longer created; it is only drastically changed - further to the onset of a triggering condition - from a mingled to a pure state.

As I pointed out in my [Ransford, 1994a] article and as can be shown from a detailed account of the influence of the detector, the only feat achieved by the observer is to prompt a (previously 'thwarted') paral phase, which actively and explicitly involves the system being observed, in one the detector's unstable microsystems. This, by the way, translates into what I call the **Swap Principle**, which reads: *Whatever the measurement carried out on a microsystem, it always boils down to give rise to the quantum jump of an appropriate system towards an eigenstate of its hamiltonian.*

This principle is easy to understand by considering (yet again!) the two-slit experiment, which explicitly amounts to observe the position of, say, the incoming photon (position measurement = N-measurement). However, the nub is what happens in the microsystem on the detecting screen's surface that eventually "catches" or localises the photon. As far as the overall microsystem-cum-photon is concerned, what really occurs is an irreversible transition from an initial unstable (and therefore H-mingled) state to a stable final state: this clearly amounts to a H-measurement.

I call a **swapper** any of the "appropriate systems" referred to in the above statement of the Swap Principle. (The underlying idea is that, in order to single out the H-measurement underpinning any N-measurement, we must first find out what relevant microsystem must be "swapped" or interchanged with the explicit measured system.) To use the Swap Principle, one can proceed in two steps:

1. Specify the appropriate system - i.e. the *swapper* - with which to "swap" the system to be measured as the fulcrum of the theoretical analysis. (In the two-slit example, the *swapper* includes two obvious "entangled" or coupled sub-systems: the formerly thwarted microsystem on the photosensible detecting screen which eventually localises the photon, and the photon itself.)
2. Shift from the initial A-representation (where A is a N-operator) to a H-representation with respect to the *swapper*. (This is readily done by using the expansion formula relative to the swapper's energy eigenkets: $|\psi(r, t)\rangle = \sum_{\alpha} |e_{\alpha}\rangle \langle e_{\alpha}|\psi(r, t)\rangle$, where S_{α} is the generalised summation symbol as defined in [Schiff, 1988].)

Several comments are in order:

1. The theoretical derivation of the probability laws (*point 6 above*) is a wonted procedure routinely carried out in the study of unstable systems. It leads to the notion of lifetime, i.e. of average timespan before decay. Experimentally measured lifetimes range from tiny fractions of a second up to millions of years. This in turn paves the way for radioactive datation, a technique widely used in archaeology and elsewhere, and acknowledged to be reliable.
2. The demise of the macroscopic instrument as a key item for a paralling to arise entails that the "Copenhagen cut postulate" (Section I.2.) is no longer necessary: I discard it without further ado. (Soon we shall see why the measuring apparatus is not in a macroscopic mingled state - as it should apparently be according to the sole wave equation.)
3. In the new picture a paral outcome (aka a "measurement result") may comprise a number of wavicles, and not necessarily just one. An example: in an excited-state to ground-state atomic transition, the emitted photon is *also* part of the paral outcome.
4. We are now in a position to grasp that the 'odd quantum probabilities' eventually turn out to make good *classical* sense. Quite plainly, we add the amplitudes rather than the probabilities themselves for independent quantum events because... they are not, after all, independent! (Recall Young's two-slit experiment in Section I.1: The truly independent events: "photon flying through slit A" and: "photon flying through slit B" *only* take shape *providing* the additional detectors elicit an extra paral phase, which then occurs before the one triggered by the detecting screen. Now, these *really* independent events do not lead to the interference stripes, whereby the overall probability is just the sum of the partial probabilities corresponding to them. On the other hand, the zebra pattern relates to the *inexistence* of independent partial events - no additional detector is there to create them - which implies that we add the probability amplitudes *without* any clash with the classical rules. This vindicates the rules of *classical* probabilities throughout. There is no quantum probability... and no *quantum logic* either!)
5. What goes on during a paral phase? What happens *then*? The received wisdom comes up with a very blunt answer: "It is forbidden to ask such silly questions. They are meaningless." I think on

the contrary that they may steer us towards a whole new world (which could be that of awareness, as I ponder in Appendix A).

6. Point 7 (of the Paralling Postulate) has far-reaching consequences that make full sense within the framework of *relativistic* QM (that is to say, of QFT; to wit, quantum field theory).

7. The Paralling Postulate, as stated above, leans on the implicit assumption that the physical system at issue is *unsupralled* (see Section III.1). To take supralness (aka nonseparability) into account, the present formulation has to be emended. It needs to be recast in the density matrix formalism; and fitting new concepts of *local* and *supral* H-status, with respect to the individual supralled subsystems, have to be defined (in relation to trace operators, etc.).

I shall not go any further on these many issues, and shall just add a few quick remarks. The first one addresses two as-yet unanswered questions: (a) "Why is the outcome of detection always sharp?"; and: (b) "How can a measurement choose (between several *reachable* results)?"

In essence, the paralling converts a mingled (or superposed) state into a pure (or eigen) state. This accounts for the shift to a definite measurement result, and away from the initial fuzziness (i.e. "mingledness") with respect to the physical quantity at stake, and this settles question (a).

As for the origin of the (probabilistic) 'choice' of the measurement result (question (b)), is there an underlying mechanism at work (e.g. due to "hidden variables" of sorts)? Is it on the contrary a sheer matter of blind randomness? My answer is that this 'choice' is by no means a-causal, even though it is nondeterministic. I surmise that it is *endo-causal* or *endo-determined*, and that this 'choice' springs from the paral. (Again, more on this in Appendix A.)

Now I can address another poorly understood fact, which is that a measuring device is never found in a mingled state before detection. Recall that the key item of the measurement set-up is the detector. In turn, the key feature of, say, the detecting screen in Young's experiment is that its surface is chock-a-block with (mutually unrelated) thwarted microsystems. Being thwarted, they are unstable and H-mingled - and therefore ill-defined!

But this fuzziness does not show macroscopically, because there is no overall entanglement (i.e. wide-scale *coherent* quantum relatedness). They never form a globally coherent mingled state - which therefore is not observed. One should also take note that, as I pointed out in [Ransford, 1994a], the thwarted systems on the detector's surface become unthwarted when coupled to the observed system. The detecting device does precisely this: *It uses the incoming system to create triggering conditions for some of its erstwhile thwarted systems.*

Thus when the paralling is carried out, it duly involves the incoming system (which turned into an "entangled" sub-system of the new unthwarted system), to the effect that we can re-interpret the whole process as a N-measurement on it (instead of a H-measurement on the swapper).

However, macroscopic mingled states are not precluded, since nothing rules them out. On the contrary they are bound to arise whenever the microscopic subsystems are combined into forming a coherent (entangled) whole (superconductivity and SQUIDs provide examples of this).

It is worthwhile going back at this juncture to the probabilities which appear through the paral phases (Paralling Postulate, point 6). Two aspects are to be considered: the temporal and the non-temporal ones. I shall delve for a (very) little while in both of them. I make implicit use, at the outset, of the Swap Principle, to convert any N-measurement into an appropriate H-measurement involving the relevant *swapper*. (In other words, I shall deal with swappers - simply referred to as the "systems" - exclusively.)

Let us assume, to simplify, that the energy eigenstates of the system (i.e. the swapper) under scrutiny are all *reachable* and nondegenerate. From the paralling postulate and given the expansion formula: $|\psi(r, t)\rangle = \sum_{\alpha} |e_{\alpha}\rangle \langle e_{\alpha}|\psi(r, t)\rangle$, we draw the conclusion that the outcome of a paral phase, happening at the instant *t sharp* (quantum uncertainty notwithstanding):

- can correspond to any one of the H-pure states $|e_{\alpha}\rangle$ (where $a_{\alpha} = \langle e_{\alpha}|\psi(r, t)\rangle$, the *amplitude of overlap* between $|\psi\rangle$ and the H-eigenket $|e_{\alpha}\rangle$, verifies: $a_{\alpha} \neq 0$);
- is predictable in the weak sense that the likelihood for any particular $|e_{\alpha}\rangle$ to be selected as the actual outcome is known, and equal to the (*intensity of*) *overlap* $p_{\alpha}(t) = |\langle e_{\alpha}|\psi(r, t)\rangle|^2$.

From this we can derive:

- A time-averaged probability law: $p_{\alpha} = \int_T p_{\alpha}(t).dt$, where T is the spell of the process
- A probabilistic partial time-law, inferred from $p_{\alpha}(t)$, which yields the partial transition rate per unit time corresponding to the projection operator $|e_{\alpha}\rangle \langle e_{\alpha}|$ (the total transition rate is then obtained by taking the whole *reachable* spectrum into account)

(For guidance on the detailed calculations, the reader can for instance consult some of the

textbooks mentioned in the "References and Further Reading" section, e.g. [Cohen-Tannoudji et al., 1987], [Schiff, 1988], [Merzbacher, 1988], etc.)

As I already pointed out, a paral outcome may comprise - unlike the conventional "measurement results" - more than one wave-particle. A striking example of this is afforded by a K^+ meson (or kaon), which is acknowledged to have a whole array of decay channels, such as:

$$K^+ \longrightarrow \{\mu^+ \nu_\mu, e^+ \nu_e, \pi^0 \mu^+ \nu_\mu, \pi^0 e^+ \nu_e, \pi^+ \pi^0, \pi^+ \pi^+ \pi^-, \pi^+ \pi^0 \pi^0\}$$

where $\{\mu^+ + \nu_\mu, \pi^+ + \pi^0, \pi^+ + \pi^+ + \pi^-, e^+ + \nu_e \text{ and } \pi^+ + \pi^0\}$ account respectively for {64%, 21%, 5.6% and 4.9%} of the observed decays, with the fitting lifetimes. (Taken together, these channels provide the set of the *reachable* eigenstates of the hamiltonian of the undecayed kaon...)

This dovetails neatly with the general framework proposed here: for one, a free kaon is unstable because it is H-mingled. For two, an example of paral outcome is $\{\mu^+ + \nu_\mu\}$: it is one of the $|e_a\rangle \langle e_a| \psi\rangle$ above. [Technically, this state is understood to belong to the vector space that is the tensor product of the Hilbert spaces relative to both μ^+ and ν_μ .]

It (almost) goes without saying that this mode of analysis can be recast in terms of the S-matrix approach (the net effect of a paral phases amounts to an *inelastic* scattering). The likeness is conspicuous: the S-matrix element $S_{if} = \langle f | S | i \rangle$ is, in much the same way as above, the probability amplitude for the final state $|f\rangle$ resulting from the initial state $|i\rangle$... by means of a *paralling-fuelled* transition. The relationship between S and the *initial* hamiltonian, H, of the whole interacting system can be made explicit by comparing the formula for the amplitudes of overlap, namely, $\{\langle f | S | i \rangle\}$ and $\{\langle e_a | \psi \rangle = (E_a)^{-1} \cdot \langle e_a | H | \psi \rangle\}$.

My closing remark is about the unstable systems. (At it stands, their conventional treatment is not theoretically correct; but I shall not develop this point here.) I want to underline the following: if the decay, as it oft happens, goes hand in hand with an emitted photon (in which case the energy spectrum includes a *continuous* part), a proper handling of the whole process ought to make use of the tensor-product Hilbert space that comprises the undecayed system Hilbert space and the photon Hilbert space as subspaces. Incidentally, this is a first step towards a full-fledged Fock space (and towards mingled states comprising *ill-defined* numbers of particles, since the decay modifies this number; I shall call them the "Fock-mingled" states in Section III.2.)

PART III. SOME FURTHER INSIGHTS

Section III.1. Waves, uncertainty, and belonging

We are by now familiar with the idea of a parallable wave. (Recall that it is a shorthand for a "parallable interfering quantum wave"; perhaps a wave of... *psychomatter*!). We saw that the quantum wave is *parallable* in that it is prone to be paralled, in which case "matter" momentarily vanishes to become "paral".

All this has already been said. I want here to convey a rough notion of some other aspects of the "parallable wave" picture. The first aspect I take up is Heisenberg's uncertainty principle. The question is: What do the uncertainty (aka indeterminacy) relations exactly mean? For me they are simply what meets the eye, i.e. *spread relations*.

We know (cf. Section I.3) that a wave packet is a bunch of mutually interfering individual waves. The waves interfere either constructively or destructively, depending on whether they are in phase or out of phase. If we work that out quantitatively, we arrive at the Heisenberg inequality (e.g. $\Delta x \cdot \Delta p \geq \hbar/4\pi$ or $\Delta x \cdot \Delta k \geq 1/4\pi$, where k is the wave number as given by de Broglie's relation: $p = \hbar k$). Examples of uncertainty relations are:

- (III.1.a) $\Delta p_x \cdot \Delta x \geq \hbar/2 = \hbar/4\pi \quad (\sim \hbar) \quad \text{where } \{p_x; x\} = \{\text{momentum; position}\}$
- (III.1.b) $\Delta N \cdot \Delta \Phi \geq \hbar/2$, where $\{N; \Phi\} = \{\text{number of field quanta; phase of the quantum field}\}$
- (III.1.c) $\Delta E \cdot \Delta t \geq \hbar/2$, where $\{E; t\} = \{\text{energy; time}\}$ (*however, time is not an observable!*)

As we see, there is a whole family of such inequalities. The general formula is given by the generalised uncertainty relation:

$$\Delta A \cdot \Delta B \geq 1/2 \cdot |\langle [A, B] \rangle|, \quad \text{where } [A, B] = A \cdot B - B \cdot A \quad (\text{commutator bracket})$$

(ΔA is given by: $(\Delta A)^2 = \langle (A - \langle A \rangle)^2 \rangle = \langle A^2 \rangle - \langle A \rangle^2$; $\langle A \rangle = \langle A(t) \rangle_\psi = \int |\psi(r,t)|^2 A(r,t) d^3r$)

Any two operators linked by an uncertainty (i.e. spread) relation as those above are said to be *noncommuting*, because they verify $[A, B] \neq 0$. They represent couples of *incompatible* observables, or of *complementary* physical quantities. So we have different ways to express the same thing:

complementary physical properties $\Leftrightarrow$ incompatible observables $\Leftrightarrow$ noncommuting operators

The uncertainty relations can be derived from a theorem named the Schwartz inequality: no physics is involved, only mathematics. The exact physical interpretation however depends on the relation at issue. The full physical sense of the Heisenberg principle is recovered when we recall that "uncertainty" is a misnomer for "spread", and that this "spread" stems from the (parallable) wave nature of the wavicle. It then becomes clear that what this principle says is that there are *mutual compensating mechanisms* at work at the micro-level.

As for the time-energy relation (III.1.c), it can be interpreted in at least three different ways; in connection with either measurements, decays, or virtual processes. This inequality ($\Delta E \cdot \Delta t \geq \hbar/2$, that is to say: $\Delta E \cdot \Delta t \sim \hbar$) can be alternatively interpreted at least in three different ways; in connection with measurements, decays, and virtual processes:

- Given a system with an energy spread, or width, ΔE , the time available for an energy measurement on it is of the order $\Delta t \sim \hbar/\Delta E$.
- The mean lifetime, or average "life expectancy", τ , of an exponentially decaying unstable system of energy width $\Delta E = \Gamma$ is given by $\tau \sim \hbar/\Gamma$. (Recall that unstable systems have a large energy width due to their "H-mingledness": there is a broadening of the unstable energy levels corresponding to the excited states of the atom, associated with the possibility of spontaneous emission.)
- In QFT (see Section III.2), the quantum vacuum is just one particular state of a quantum field. It is the quantal state in which no field quanta are present (or "excited"). Since the number of quanta is sharp, we know by eq. (III.1.c) that the vacuum fluctuates. This gives rise to virtual phenomena, whereby "off-mass-shell" particles (for which $E^2 \neq p^2c^2 + m^2c^4$, m being the rest mass) and live flittingly on "borrowed" energy (lifespan of order $\Delta t \sim \hbar/\Delta E$ for "borrowed" energy ΔE). Likewise, a virtual photon of energy ΔE will pop in and vanish after travelling an average distance of $c\Delta t \sim \hbar c/\Delta E$. (If this photon, after being emitted by an electron, is then reabsorbed by another electron, it will give rise to a recoil, which feels like a repulsive force: this accounts for the electromagnetic repulsive force!)

Similarly, it turns out that the amount of energy a photon needs to "borrow" for tunnelling through a potential barrier depends only on the height of the barrier, not on its thickness. The same goes therefore for the amount of time during which it can keep this energy; so that any photon tunnelling through a barrier has to do it in the same time, *no matter how thick the barrier*. By taking thick enough a barrier, the experimentalist can check the possibility of faster-than-light tunnelling.

Finally and to sum up, it appears that the fuzziness exhibited at the quantum level arises from two main features:

- The inherent spread of whatever distribution relates, on a one-to-one basis with each wave, to a bunch of mutually interfering waves (forming a wavepacket or wave function).
- The probabilistic demeanour that attaches specifically to the paral phases (including the aborted - i.e. 'virtual' - ones).

Now I turn to the issue of belonging. At the bottom of the "new realism" advocated here lies the notion of *parallable* wave. These quantum waves do not just roam free, on their own, through spacetime. They comply with a number of basic requirements, e.g. they must interfere with other waves in a way that upholds quantumhood. As a consequence, any (unparalled) quantum wave must, at any time, belong to a definite wavicle.

This begs a lot of questions. For instance, one may wonder: *How can an individual wave 'know' to which wavicle it belongs? And, should such a knowledge become objectively unattainable, what would then happen?* The first thing to notice is that an individual wave (e.g. $\Psi_{\text{indiv}} \propto \exp\{-i/\hbar(Et - pr)\}$ or $\Psi_{\text{indiv}} \propto \exp\{i(kr - \omega t)\}$) belongs to a given wavepacket or wave function iff - i.e. *if and only if* - it interferes with *some* of the other waves of this cluster of waves. (This is self-evident from the mathematical expression of a wavepacket.)

I said: "with *some*", and not: "with *any*" or: "with *all*". Why? This is because of *non-separability*, also called *non-locality*, which I now introduce. I christen this phenomenon *supralness*, for reasons highlighted in Appendix A.

Supralness refers to the existence, between an arbitrary number of microsystems *and regardless of their mutual distances* (they can be light-years away!), of a supral interaction, or link. Such a supral link seems to imply a "spooky action at a distance", as Einstein put it, whereby "by the mere fact of observing *here* you affect something *there*." In other words, *supralness* means that the parallings of different wavicles are interdependent and simultaneous. (Technically,

supralled wavicles are best depicted by a collective density matrix with nonvanishing off-diagonal elements.)

Supralness is better known under the popular "EPR paradox". This "paradox" concerns a pair of *non-separable* or *supralled* photons flying apart in opposite directions, and which must always have opposite values of polarisation (they form a "singlet state"). Therefore, if we measure the value for one photon, we instantly know the value for the other photon... even though no measurement is made on this other photon, and both these values did not exist beforehand.

In term of the theory, these twin photons, tagged γ_1 and γ_2 , do not have individual wave functions or state vectors. To understand why, let us consider the following. When the photons are *unsupralled*, they can be described as: $|\gamma_1\rangle = a_1|\uparrow\rangle_1 + b_1|\downarrow\rangle_1$ and $|\gamma_2\rangle = a_2|\uparrow\rangle_2 + b_2|\downarrow\rangle_2$ (where $|\uparrow\rangle_n$ and $|\downarrow\rangle_n$ - $n = 1, 2$ - stand for the two polarisation eigenstates of γ_n ; $|a_n|^2 + |b_n|^2 = 1$ as usual)

The collective wave function (in the tensor-product Hilbert space) for these *unsupralled* photons is then: (III.1.d) $|\gamma_1\rangle|\gamma_2\rangle = |\Psi\rangle_{12} = (a_1|\uparrow\rangle_1 + b_1|\downarrow\rangle_1)(a_2|\uparrow\rangle_2 + b_2|\downarrow\rangle_2)$

It is a factor comprising two terms, each concerning one photon individually. Contrariwise, the collective wave function for a *supralled* pair is:

$$(III.1.e) \quad |\Psi\rangle_{12} = 2^{-1/2}(|\uparrow\rangle_1|\downarrow\rangle_2 + |\downarrow\rangle_1|\uparrow\rangle_2)$$

It is non-factorable, which amounts to say that the photons γ_1 and γ_2 can no longer be described separately. Lest there be any confusion, I emphasise at once that relation (III.1.e) can be shown to bear on the possible paral outcomes exclusively - *not* on the "wavy" motion at all. Therefore, as long as the whole system strictly obeys the least action principle, supralness does not show. This is fortunate for physics: were it not so, no workable local theory could be arrived at, and that would forcibly toll the knell of this messy discipline!

Let us now examine what happens in the event of a polarisation measurement performed on, say, γ_1 (and γ_1 only). We learn from (III.1.e) that the collective wave function is a mingled state comprising two eigenstates, namely, $|\uparrow\rangle_1|\downarrow\rangle_2$ and $|\downarrow\rangle_1|\uparrow\rangle_2$. By the measurement or projection postulate (or really: *by the paralling postulate together with the swap principle*, as explained in Section II.3), we deduce that these N-pure states are the only possible end states for the pair, should a paralling occur.

As a consequence, if the observation yields the result $|\uparrow\rangle_1$, it shows that the paral outcome of the *supralled* pair as a whole is now $|\uparrow\rangle_1|\downarrow\rangle_2$. In other words, the paral phase on γ_1 instantaneously results in a matching paral phase for γ_2 , which brings it in the pure state $|\downarrow\rangle_2$.

Since supralness operates regardless of distance (γ_1 and γ_2 can be *wide* apart when the measurement on γ_1 is made!), it stands *prima facie* in wary regard of relativity. With hindsight however, it appears that, granted that supralness leaves the least action movement aside, there is no open conflict here: no matter-energy propagation, or transfer, is ever involved with it.

The *supralled* photons above have no individual wave function, they must be taken together. However, it is wrong-headed to infer that they now form one single particle. The reason is that there is no mutual interference between γ_1 and γ_2 : in that respect, they have firmly kept their separate identity. (*This is why we cannot say, just by "looking" at it, whether a given particle is unsupralled or, instead, is supralled to either one or seven thousand other particles!*)

Yet this notion of "specific identity" (or of "primitive thisness", to borrow the expression from Redhead) is far from crystal-clear in the quantum world, as exemplified by identical particles. Such is nature that the collective wave function of identical particles must be either totally symmetrical or totally antisymmetrical (with respect to particle permutations). This is a source of supralness, as we see from the expression of the collective state vector for two identical particles:

- antisymmetric case (for fermions): $|\Psi_a\rangle_{12} = 2^{-1/2}(|+\rangle_1|-\rangle_2 - |-\rangle_1|+\rangle_2)$
- symmetric case (for bosons): $|\Psi_s\rangle_{12} = 2^{-1/2}(|+\rangle_1|-\rangle_2 + |-\rangle_1|+\rangle_2)$

(We successively have, as expected: $|\Psi_a\rangle_{21} = -|\Psi_a\rangle_{12}$ and $|\Psi_s\rangle_{21} = |\Psi_s\rangle_{12}$. Identical fermions, unlike bosons, cannot occupy the same quantum state: this is expressed by Pauli's exclusion principle, and is a direct consequence of indistinguishability.)

Nature does not allow any other type of wavicle than fermions and bosons. And this really accounts for another basic postulate of QM. It turns out that this "fermion-boson split" rids physics of a worrying (and consistency-threatening) indeterminacy that would otherwise show up whenever two identical particles collide. (The outcome would be unpredictable; but not in the fashion of the usual quantum randomness.)

This brings us to the last item of this Section, which is the *kith-kin* criterion. "Kith and kin": friends and siblings, as it were. Both these categories of relationships entail a kind of belonging: belonging to a circle of friends, and belonging to a community of relatives. Likewise I

metaphorically define *kin-waves* and *kith-waves*. They differ in the type of belonging which binds them: *kin-waves* are mutually interfering; *kith-waves* are mutually supralled (conversely, there also exist mutually *kinless* and *kithless* waves).

Below a certain distance, *wave kinship* is 'garbled' when identical particles are crammed together (just because there is absolutely no intrinsic means for nature herself to keep track of which wave belongs to which wave-particle!) Ultra-cold atoms are a case in point: they have very little momentum spread, so by the uncertainty principle their wave functions spread out very broadly, by atomic standards. When they spread to the point of overlapping significantly with the wave functions of neighbouring atoms, all the atoms involved merge.

The normal way to distinguish identical atoms is by position, momentum, spin component, or any relevant property. When they are moving so slowly that momentum no longer distinguishes them, and wave functions have spread so broadly that they can no longer be clearly said to be in different places, absolutely *nothing* tells them apart.

The atoms are confused too: because of the basic ambiguity as to where their individual waves belong, they all start to act as one. They lose their identities, and only a single collective wave function, generalising (III.1.e), can be ascribed to them. This gives rise to a so-called Bose-Einstein condensate, which is therefore a collection of atoms all in one quantum state.

In principle, such condensates could be big, allowing quantum behaviours normally confined to the scale of individual atoms to operate at far larger scales - thus leading to *macroscopic* quantum effects against the *Copenhagen Cut Postulate*, as superfluidity and superconductivity.

Phenomena like these point to the existence of an "indistinguishability threshold", whereby the sense of kinship is beclouded when identical particles are close to less than this (momentum-dependent) distance. This suggests the notion of *kith-kin kernel*, which would be an innermost 'area' such that, within it, two properties concur: (a) wave kinship is blurred when identical wave-particles have overlapping kernels; and (b): no trespassing of the kernel is allowed for *fermions*; any attempted trespassing elicits a reaction (typically, a scattering) on the particle's part.

If so, then the *physical* size of a fermionic wave-particle would be akin to the actual width (or rather, to the minimum breadth or *lower bound*) of this *kith-kin kernel*. It would bear no direct relation to the spread Δx , which can be taken as the *mathematical* size of the (unsupralled) wavefunction. There are hints that the Compton wavelength, λ , has something to do with this. (Recall that $\lambda = \hbar/mc$, where m is the rest mass; $\lambda \sim 10^{-13}$ cm for an electron.) A variety of experimental data point to its relevance, as for instance:

- Identical particles keep their separate identity when their coordinates do not overlap significantly. They lose it when their mutual distance is of the order of the Compton wavelength.
- Ultra-relativistic wavicles have no position defined to an accuracy greater than their Compton wavelength.
- In QFT a *local* field theory must satisfy a condition called *microscopic causality*. And there is a very significant experimental agreement with this at least at distances comparable to the Compton wavelengths of the elementary particles.

But reality is slightly more complex, as evidenced by the possibility to split (yet not "decohere") any wave-particle into a reflected and a transmitted parts after passing through a suitable analyser. For such a widely strewn wavicle, there is no notion of *kith-kin kernel* (or else, it has to be suitably emended). Other facts that a thorough *kith-kin* criterion should spell out are:

- As a rule, supralness is produced when particles come very close together and interact; it is destroyed by paral phases only (in fact, it is fully wiped out by prompting parallings corresponding to a so-called complete set of compatible observables).
- Apparently, the "sense of belonging" with respect to quantum waves is not determined by consideration of 'neighbourhood-blurred' identity only. It has been established that even atoms which never came into contact (from the perspective of classical Newtonian physics) can become supralled.

Another point worth stressing, with respect to wave kinship, bears on the virtual processes. We remember from the paralling postulate that there is a basic identification:

$$\{\text{Virtual transition or process}\} \equiv \{\text{aborted paral phase or paralling}\}$$

Now, the point is that a virtual particle does not keep track, either, of its initial identity; in the sense that the particle that gave rise to it is totally forgotten (and indeed, *where and how could this information be encoded?*). For example, any virtual photon stemming from a given electron harbours no special relationship with this electron. It can thus indifferently be reabsorbed by any

neighbouring electron as well. (As we know, this process yields the electromagnetic interaction.)

To conclude, the *kith-kin criterion* was necessary because the existence or otherwise of a separate wave function is no faithful criterion to define an individual particle. One can formulate it as follows:

- An unparallel *individual* particle is a whole cluster of kin-waves (what characterises these waves is their ability to "cohere" or interfere mutually)
- A *supral* aggregate of unparallel particles is a group of particles (as defined above) whose respective waves are kith-waves.

There would be far more than the foregoing to say about the "kith-kin" affair; but I leave it here for the time being. The point is that it is possible to develop a deeper understanding of the microworld by going all the way down to the parallable interfering quantum wave.

Section III.2. Wavicles, or field excitations?

Nature is both quantal and relativistic: of this there is evidence aplenty. And, when we bundle together quantum mechanics and special relativity theory (SRT), we arrive at a field theory which incorporates wholly new aspects, not included in QM (e.g. antimatter). In this Section I set out to touch on quantum field theory (QFT for short), with no ambition to go beyond a light scratching of the surface.

Much to anyone's surprise, there is a deep and challenging mismatch between QM and relativity, whether SRT or GRT. Indeed, when we mix quanta and relativity, the broth turns sour and makes for a theory of dizzying complexity. The resulting formalism is unwieldy and - to add insult to injury - mired in infinite quantities.

How is it that any attempt at a full-blown relativistic quantum theory seems bound to face such setbacks? My guess is that the answer lies (at least in part) in the twists and turns of the quantum wave itself. For, in the *parallable* wave picture, two wayward features are thrown in. Namely, the discontinuity and the intrinsic a-relativistic nature of the paral phases.

Ironically, in standard QFT the paralling is sidestepped and even downright ignored. It is swept under the rug of the so-called second quantisation. "Field quanta" may dash in and out of existence, but no one wants to know how. This does not prevent the real-world paral phases to be there, hale and hearty as ever, and wink back at the theory with a vengeance. No wonder then that, particularly at the high energies where paral phases are rife, QFT sours.

The first issue that I bring into focus bears directly on the wavicle: *Can the (nonrelativistic) QM wave-particle idea be made into a relevant one in QFT?* At first glance, it seems that it should not be the case, and that the field idea is poised to replace it for good.

The crux is that wavicles are regarded as mere quantum fields excitations, often referred to as "quanta". In QFT, one finds raising (or emission) and lowering (or absorption) operators - their action changes the number of quanta - along with occupation number operators derived from them. These operators act in Fock spaces; which generalise the quantal Hilbert spaces for states with a variable and indefinite number of wavicles, i.e. of "excited field quanta".

This leads to the awkward possibility of superposing field states with varying numbers of quanta. These "Fock-mingled" states as I dub them are states for which the actual number of wavicles is not fixed - and more to the point, *does not exist!* Herein lies a major conceptual hurdle, unheard-of in QM. If we think of any object as being positively here or definitely not here, we are in for trouble. The "Fock-mingled" state seems irretrievably at odds with the elementary particle idea.

This, added to quantum indistinguishability and to the slippery nature of some highly unstable and exceedingly short-lived hadronic resonances, seriously call into question the quantum particle concept. It is no wonder that for most physicists today, "the truly fundamental material entities are no longer considered to be particles, but *fields*. Particles are regarded as disturbances in the fields, and so have been reduced to a derivative status." (Paul Davies)

I shall not develop, let alone rough out, the technical niceties underpinning these philosophical issues. I shall only express my own "hunches", as they arise in agreement with the proposed "new realism" grounded in the *parallable* wave idea:

- *Unparallel* quantum waves can be regarded as fields, and as such can be recast in a (classical) field-theoretic framework.
- Hidden (i.e. measurement-free) paral phases are widespread indeed, in the full interacting arena of real-world physics; and they hold the key to the puzzling "Fock-mingled" states. Furthermore, they spawn discontinuity, randomness and a-relativism in QFT. Thus they rift

the fabric the theory, and foil any attempt at achieving a consistent global (i.e. nonperturbative) portrayal.

- Short-distance physics is high-energy physics. In the high-energy range, a frenzy of paral phases - brought about by collisions, etc. - takes shape. They make the conventional formalism go haywire!
- These crazed parallings spew forth all manner of new (and 'fickle') wavicles. These sharply discontinuous events - along with the nonvanishing spread of the (un-point-like) quantum waves - may go a long way towards explaining why any "smooth and steady" theoretical framework is bound, in the limit of the very small distances, to fall headlong in the murky slough of divergent quantities.
- All the paralling-bred phenomena are inherently a-relativistic. This might eventually spell out why theoreticians are at great pains to ever come up with a consistent covariant formalism: there just isn't such a thing! (For instance, no adequate covariant position operator can be specified in QFT; this must also be traced to the spread-out quantum wave.)
- The vacuum is *not* a genuine physical medium. It is just a shorthand way to cope with several features of "empty" space, which really is brimming with quantum waves coming from near and afar alike. (In the parallable wave picture, any chunk of space, regardless of whether there is any wavicle *explicitly* inside it, is teeming with unparalled quantum waves whose local density of presence is, as a rule, immeasurably low.)
- According to the paralling postulate, *point 7*, aborted parallings *do* arise in such a quantum-wave-laden "emptiness". This may throw an illuminating new light on the whole vacuum fluctuation affair.

In order to further illustrate the new mode of reasoning put forward by the "quantum wave realism", I briefly mention Haag's theorem. This theorem, first published in 1955, establishes, roughly speaking, that there is no consistent mathematical formalism in which *interacting* quantum field theory can be couched. This worrying result, that "at least appears to call into question the mathematical foundations" of interacting QFT ([Teller, 1995], p 115), is in fact grist to my mill as it should now be clear.

Indeed, as we can read in [Stern, 1994], p 508, "Haag's theorem states that the unitary transformation: $\phi_{in}(t) = U(t, -\infty) \cdot \phi_{in}(t) \cdot U(+\infty, t)$ between the effectively free in-fields and the interacting Heisenberg fields is not strictly consistent with Poincaré invariance." [*The Poincaré group transformation has the following general form: $x^\mu = \Lambda^\mu_\nu x^\nu + a^\mu$. It combines translations $\{a^\mu = \text{translation vector}\}$ and Lorentz transformation $\{\Lambda^\mu_\nu\}$.]*

This result is currently viewed as a potentially shattering difficulty laying bare a major flaw in the theory. Not so within the *parallable* wave interpretation - where it amounts to no more than my statement, recast in a field-friendly way, that "All the uni-equivalent states of an isolated system share the same H-status" (see [Ransford, 1994a], and Appendix B).

In short, when the fields do not interact (free theory), they form isolated systems and their respective states remain uni-equivalent throughout. If on the contrary they interact (as they ever do in real-world physics), paral phases do occur and, as a consequence, the uni-equivalence is lost. This in essence is the pith and marrow of Haag's theorem - and this is nothing new to worry about!

Section III.3. Gravitation revisited

In the same spirit as before, I shall venture a few words on gravitation, to give an inkling of what the *parallable* wave idea might bring, by way of new modes of thinking. Again, a meaningful assessment of the perspective adopted here would require an in-depth study, which is out of question here.

As we know, gravity does not fit well into our understanding of fundamental physics. Unlike any fundamental interaction described in QFT, it can be understood as pure geometry: Einstein proposed that gravitation is not strictly speaking a force, as Newton had assumed, but a distortion, "warping" or bending, in the geometry of spacetime. This results from his Equivalence Principle, which posits the equality between gravitational and inertial mass.

Even though gravity can be described properly as a field, we are, with it, a far cry from standard QFT. Indeed, Einstein's field equations [EFE] relate the metric of spacetime to the mass-energy (and pressure) distribution. As a result it turns out that gravity - and ultimately, the presence of matter - 'curves' or 'warps' spacetime.

Physicists have embarked on constructing a gauge theory of gravity, which leads to an

explanation of the force of gravity in terms of the exchange of a field boson (dubbed the graviton), for it is possible to interpret Einstein metric as a quantum field associated with a massless spin 2 particle. (The graviton has to be massless to account for the infinite range of gravity; it has to be spin 2 because gravity is attractive and never repulsive.)

The trouble is, the Lagrangian describing spin 2 particles is not renormalisable. (As a rule, QFT deals with renormalisable spin 1 fields.) For this reason, quantum gravity cannot be made into a conventional local gauge theory. This negative result leaves us in our predicament!

In the face of it, other (and bolder) attempts have been undertaken, spurred on by formidable hopes - but inevitably leading, it seems, to disappointing outputs. The main attempts go by the name of super-symmetry and super-string theory. Super-strings are hypothetical objects that (unlike wavelces) have length but no width. (We take note in passing that, having no width, these superstrings are incompatible with the parallable wave picture.)

Going back to *classical* GRT, a (far-reaching) puzzle which besets it is pinned down by the so-called Mach's Principle. As evidenced by the famous Newton's bucket and Foucault's pendulum experiments, one can in principle say whether an object is accelerating or not, with no direct reference to anything external. The odd yet inescapable conclusion is that the contrast between uniform and nonuniform motion is deep: *Whereas uniform motion is relative, nonuniform motion is intrinsic.*

What makes non-accelerating systems so special? What defines zero acceleration? Mach's principle provides an answer, by postulating that inertial frames are not unaccelerated in an absolute sense, but rather, that they are unaccelerated relative to some suitable average of all the matter in the universe, its far-flung galaxies included. (However, why and how it should be so has not been even remotely settled.)

Einstein was very much in favour of Mach's views on inertia. The problem, as pointed out in [Davies and Gribbin, 1992] p 90, is that: "In general, the theory of relativity is *not* consistent with Mach's Principle". This is exemplified by the model put forward by Kurt Gödel in 1949 (cf. [Gödel, 1981], p 389 ff.), who found a solution to Einstein equations [EFE] that describes a rotating universe.

The trouble is that, in the Machian perspective, the concept of the universe rotating as a whole - including its far-off galaxies! - is utterly meaningless. (*Relative to what can the universe as a whole be said to rotate?*) As a consequence, Mach's principle remains an experimentally compelling but theoretically elusive, and even dubious, concept. More pointedly, the deep problem of the origin of inertia is still left dangling...

Can we get out of this deadlock, and how? My suggestion is that a solution does exist, and that it must be sought by infusing *substance* to GRT; that is to say: by explicitly taking into account the actual nature of matter. And this brings me back to the *parallable* wave picture. Can it provide useful insights? In a nutshell, the parallable wave throws in two main aspects [Ransford, 1994a]:

- Gravitation originates in the unparalled quantum waves
- Paral is stark a-relativist, and is therefore thoroughly alien to gravitation

If correct, these two statements put a new gloss on the gravity field. As I pointed out in my ANPA 16 paper, if it is true that gravitation originates in the unparalled quantum waves, then "the averaging-out of *all* the quantum waves coming from the *whole* universe present - however dimly - in any small volume of space, may lead to the notion of *intrinsic* zero acceleration." This conceptually bears out Mach's principle (the detailed calculations have to be carried out).

There are two ways to figure this out. Either "we reckon that these waves, being extended physical waves, accordingly stash extended matter-energy which ultimately caters for the gravity field's potential energy." Or else, "we make out that these waves locally twist or 'warp' space-time, as seen from any other incoming quantum waves."

With this notion that the unparalled quantum wave yields gravitational effects, one views a wavelce of mass m as the source of a gravitational field. More accurately, this field emanates from it in proportion to $dm(t) = m \cdot |\psi(r, t)|^2 \cdot d^3r$ (where $|\psi(r, t)|^2 \cdot d^3r = |\langle \psi | \psi \rangle|^2 \cdot d^3r$ is the local density of presence of the wave packet).

Due to the local presence of this wave-borne infinitesimal mass dm , the fabric of spacetime would be locally distorted, to the effect that any particle immersed in the resulting geometrical field would have its least-action pathways accordingly deformed. Or, to put it differently: *it would feel a force* - that of gravity! - inside the invisible halo of influence surrounding the massive wave-particle. This is how I propose to understand that "matter tells space how to bend; space tells matter how to move." (Wheeler)

The semi-classical exercise outlined hereafter gives a first intimation that the analysis proposed here may be on the right track [Ransford, 1994a]:

Let us consider two elementary particles. The first one - henceforth denoted (O, m) - is a regular quantum particle, taken for simplicity's sake to be a Gaussian wavepacket centred in O . Its mass, m , is assumed to be spread out according to the local density of presence given by the wavepacket. The second particle is a classical point-like one of mass m' , located in O' . The exercise consists in calculating the gravity potential energy corresponding to the second particles (O', m') , as immersed in the wavepacket-induced mass density of (O, m) . The upshot is that the find a potential energy of the Newton law of gravitation!

(For the reader willing to try his hand at the mathematics involved, here are three hints on how to arrive at the force potential matching the direct inverse square law of Newtonian gravitation: (a) given the symmetry of the problem, we work in the spherical polar coordinates centred in O ; (b) we use Gauss' theorem transforming a volume integral into a closed surface integral; (c) we perform an integration by parts, and we notice that one of the two integrals obtained vanishes.)

Now I turn to the second aspect of the approach sketched out here: it bears on the *paralled* quantum wave. Really, when quantum waves are *paralled*, they are no longer waves at all. They give rise to a *whit* of *paral* - as opposed to the bit of matter it was when un*paralled*.

The main idea here is that *paral* has nothing to do with gravity: it is downright *gravityless*! (By this I mean that it neither contributes nor responds to gravitation.) Its concomitant 'switching off' of gravity has a number of consequences, already hinted at in [Ransford, 1994a]:

The perspective that gravitation may stem from quantum waves provides a new insight into the very early universe. According to the standard cosmological model of the big bang theory, the awesomely hot and dense universe of the beginning played host to a frenzy of high-energy collisions. As a result its particles - or rather, 'wavicles' - were prey to ever-recurring triggering conditions. These wavicles would thus ceaselessly hop, as it were, from a *paral* phase to the next. The overall picture is that our early universe was home to a large-scale *primordial paralling*. (...) If so, gravitation was then drastically weakened by all the quantum waves 'wiped-out' by this *paralling*.

This being said, *paral* is an as-yet totally uncharted territory. We do not even know what the average (and presumably, probabilistic) duration of a *paral* phase is. It is likely that a better knowledge of what *paral* stands for would markedly improve our knowledge of quantum gravity (especially near the Planck scale), our understanding of the very early universe (home to the *primordial paralling*); and more broadly, of the spacetime singularities affair (could *paral* manage to keep at least some of them at bay?).

APPENDIX A. A WHIFF OF CONSCIOUSNESS

The im-im model, in a nutshell

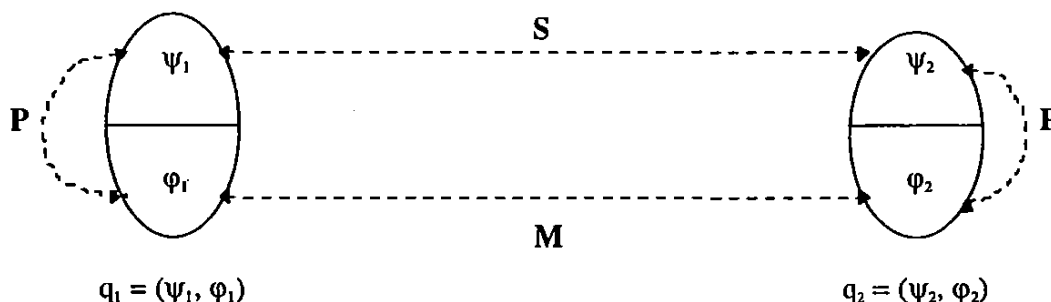
The 'im-im' approach rests on the im-im assumption, which claims that consciousness is both *immanent* and *immaterial* (or nonmaterial). Hence its name. Quite simply, its being immanent means that it is a content of our universe. It enjoys a worldly existence of its own. Its being nonmaterial means that it is definitely and distinctly un-matter-like. It behaves in its own idiosyncratic way, unconstrained as it is by the laws of matter (thus we can define, say, neither its weight nor its height).

These two statements put together lead to the notion of psychomatter. With it, we are invited to understand consciousness as arising from a specific "mindstuff", which somehow is embedded in matter. I label psychism, or - better still - 'psi', this content (or "mindstuff"). As its very name suggests, psychomatter is bi-dimensional. We can readily derive from this fact the existence of new, non purely material interactions. This is clearly exemplified by the *M-P-S diagram* hereafter, which is the linchpin of the im-im approach.

The drawing represents two elementary particles, say q_1 and q_2 . (They are depicted as two tiny beads, which is an outrageous oversimplification! But I do not need to be more accurate here.) According to the 'im-im' hypothesis, these particles are made up of a bi-dimensional stuff, namely psychomatter (instead of sheer matter). Henceforth I can write them down as:

$q_1 = (\psi_1, \phi_1)$ and $q_2 = (\psi_2, \phi_2)$. (Here ψ_i [$i = 1$ or 2] tags the 'psi', or psychic, part of the "wavicle" q_i whereas ϕ_i stands for its material, or physical, part - I propose to name *wavicle* any sub-atomic particle of psychomatter.)

Given their twofold nature, q_1 and q_2 are prone to three different types of interactions. This brings us straight to the heart of the *M-P-S* affair:



We successively have:

M: material interaction (between ϕ_{11} and ϕ_{212})

P : paral interaction or phase; or else, **paralling** (between ψ_{1i} and ϕ_{1i} , $i = 1$ or 2)

S : supral interaction, or link (between ψ_{11} and ψ_{212})

Quite frankly however, something is amiss. There is a snag somewhere. For - if matter is psychomatter - how is it that, by and large, this doesn't usually show? To square this blatant fact, I am led to surmise that *the 'psi' part of psychomatter is overwhelmingly dormant, or latent*. Therefore most of the time it is a-lurking, cosily hidden in a state of utter lethargy. It is fast asleep! This being so psychomatter is, quite understandably, bound to be confused with a purely material stuff: hence the widespread dismissal, amongst scientists, of anything but matter in our universe. Clearly, plain matter is in the eye of the beholder...

Now, psychomatter comes under two alternative (or 'complementary') guises. They are: **matter**, which is psychomatter whose 'psi' is latent; and **paral**, which is psychomatter whose 'psi' is active or "off-latent". If the 'psi' is normally latent, what may at times 'wake' it out of its latency? Clearly, we have to look for some kind of "awakening device". We have seen (cf. [Ransford, 1994a], and the main text) that the sought-after "device" is a sort of specific catalytic conditions, which are the **paralling conditions**.

Furthermore, I assume that *matter* can be defined as being whatever complies with the principle of least action; whereas *paral* is characterised by a degree of *endo-determination* (i.e. a kind of "self-will" - which factually manifests itself as an *indeterminism*). From this one can readily conclude that, if the M-P-S diagram (or the im-im model) is correct, then quantum theory - carefully divorced from its current *interpretations* - teaches that:

- The **paralling** or **paral phase** is identical with the 'wavefunction collapse'
- **Supralness** (i.e. the fact to be suprally linked) is identical with non-separability (aka non-locality)
- The **paralling conditions** generalise the measurement processes.

The cognitive iceberg

Now I turn to the conscious brain. *What is consciousness*, to begin with? Big question: far too big to handle. There are so many possible ways to answer... and in the process, to explain consciousness away. I shall not embark on such a virtually bottomless issue, huge and slippery as it is. Besides, my concern here is not consciousness as such, but rather the conscious brain ("*Why is the brain the organ of awareness?*").

Consciousness, as it is fairly clear, is a mongrel concept. It conjures up an endless variety of ideas and realities. One thing I can yet venture to say is this: *consciousness stems from supralled paral* - or equivalently, *from paralled supral*. How is that? Easy to see: to start with, matter - or else, *psychomatter whose 'psi' is latent* - is ever unconscious. Whatever its supral spread, it can develop no significant awareness of its own.

Only paral - i.e. *psychomatter with an "off-latent" or active 'psi'* - is endowed with (varying degrees of) awareness. This awareness is exceedingly dim for an isolated particle which undergoes a paralling (such an unsupralled "wavicle" is bound to remain a dimwit!). By any standard, it becomes tangible only when the supral extension, or breadth, of the underlying paral is somewhat large and complex.

There are actually two phenomena involved here. One is a *threshold effect*, according to which awareness becomes significant, or intense, only when the "supral spread" of a given lump of

paral is important enough. It must reach at least a certain threshold, below which we can hardly speak of awareness or consciousness proper. (It is much like a light, which can go from nigh-invisible to subdued to scorching white, depending on its threshold or production level.)

The other phenomenon is a *pattern effect*, whereby the subjective contents of awareness (i.e. sensory percepts, emotional affects, thought concepts...) depend directly and very precisely on the way a given aggregate of paral is "patterned" or suprally structured. (*More about this soon, with the notion of suprel.*)

With the above I believe one can start to address the question of the conscious brain. But does the foregoing fit in in any sensible way with the conscious brain, as it is known (with increasing accuracy) to-day? Does it match the data, as far as we can tell? The main 'im-im' proposals in this respect [Ransford, 1995b] go as follows:

- The brain is the organ of awareness because it can yield a ongoing stream of wide-scale paralld supral.
- The brain achieves this feat because it contains a thick network of (*en*)paralling microbiological structures, which beget paralling conditions for incoming ions, molecules and the like, within specific brain microsites.

I call these micro-structures - which collectively generate (supralld) paral - the **paralgens**. If 'im-im' is correct, they hold the key to the conscious brain. (*We are not totally clueless regarding their exact nature: for instance, a number of brain paralgens are likely to be found in some of the so-called synaptic NMDA ion channels...*)

I now introduce the **cognitive iceberg**. It is a conceptual model which purports to explain sensory perception or "cognition", along with some of its most striking features. (It does not address other aspects of consciousness and cognition. Other conceptual models, not considered here, will cater for them.)

Like any iceberg, the cognitive one comes with a tip and an underwater part. Let us consider the underwater part, first. It is where the sensory information (be it visual, auditory, tactile, etc...) is converted into unconscious 'psi' information or else, into *suprels*. (A *suprel* is a unit, or bit, of suprally-encoded information; in keeping with the pattern effect, its content varies with the shape or 'topology' of the underlying supral pattern.)

The tip, on the other hand, is where the *suprels* are subsequently sent - within, say, the cerebral cortex - on to an array of paralgens. There they are *enparalled*, upon which their information content (which was hitherto dormant, or latent) flashes into the mind. This is how they eventually contribute to our overall awareness. (Remember: *consciousness* - sensory or otherwise - is *paralled supral*.) In short, the cognitive iceberg consists of:

- An *underwater part*, which yields *suprels* that encode incoming sensory data. (At this stage, these input-specific *suprels* remain *unconscious*, for want of enough paralgens to enparal them.)
- A *tip*, where these *suprels* are enparalled, e.g. by gushing through some paralgens. (Here the *suprels* are turned into paralld supral: they likewise become *felt* or *aware* or *consciously perceived*. Their subjective contents are given off and enter the mind.)

Let me add three points:

- As soon as they squirt out of the paralgens, the *suprels* become unconscious again (they are no longer enparalled). They thus go back, unless they are destroyed somewhere down the path, into the unconscious underwater part. There they are stashed-in-hiding, waiting to be possibly re-enparalled (or *renparalled*) - maybe years later! In other words, *they are memorised*. This roughly accounts for our mental memory (i.e. our "declarative memory"). A concise formula expresses this idea: *our memory is supral in essence*.
- According to 'im-im' the sensory information, though initially processed at some length in the underwater part, becomes aware only at a later stage, within the tip. This may elucidate two well-researched but as yet poorly understood facts, namely: a) Only a puny proportion of the brain processes does contribute forthwith to awareness (*since whatever goes on outside the tip remains unconscious*); b) Only the end-product of the brain processing may become conscious at all. (*This finding can be branded the upshot problem. Again, it boils down to the fact that paralgens are typically found inside the tip, so they come into effect only at the latter stage of the brain processing.*)
- Not quite unexpectedly, supralness is a likely answer to the so-called "binding problem". (The **binding problem** refers to the puzzling oneness and holism of the normal conscious self - as opposed to the fragmentation and separateness of the underpinning brain processes.)

It is worth emphasising that supralness gives a strong holistic flavour, or 'aura', to our minds,

which are immeasurably more than what goes on at the elemental, microbiological level. (Again, I voluntarily narrow my focus, here, to sensory awareness or aware perception. This is but a tiny bit of the broader picture of consciousness, which goes way beyond the cognitive iceberg and would require other conceptual schemes, not considered here.)

APPENDIX B.

PARALLING AND TRIGGERING CONDITIONS

(This Appendix is an excerpt of my ANPA 16 paper (1994a), where I changed "mixed" state into "mingled" state. "Mixed" was a very poor and confusing choice indeed, since it is wontedly used in a different sense!)

I.1. The setting

Virtually any of us knows that quantum mechanics deals with wave functions and more generally, with state vectors. In the language of the theory, they represent elementary particles and other quantum systems.

Quantum mechanics also deals with *observables*, which refer to properties that can be observed or measured on a quantum system, such as its position and velocity. Any observable is matched in the theory to a specific operator.

An *operator* is an abstract entity; it operates on a Hilbert vector space, it has eigenvalues and eigenvectors (or eigenstates). All this is widely known. It can easily be found in any textbook on quantum theory, and needs not to be developed here. For my own purpose, I shall highlight the few points hereafter:

a) Observables relate to such physical attributes as : position, energy, speed (or rather, momentum), polarisation, spin, angular momentum and the like. I shall henceforth put them in two separate categories, namely: the *H-observables* and the *N-observables*. The former observables relate to energy; whilst the latter relate to any other *nonenergetic* physical quality (such as position and momentum).

I likewise distinguish *H-operators* and *N-operators*. Quite simply, H-operators are Hamiltonians, whereas N-operators are the remainder (e.g. position operators).

b) Broadly speaking, a wave function can put on two guises, with respect to a given observable: it is either in a pure state, or it is in a mingled one. A quantum system is in a *pure state* regarding an observable *iff* (i.e. if and only if) it occupies an eigenstate of the matching operator. (It has therefore a sharply defined value for this observable, this value being the apposite eigenvalue.)

Conversely, a system in a *mingled state* (oft called a *superposed state*) is a linear combination, or superposition, of such pure states. It has no clear-cut value for the observable concerned. In that sense a mingled state is rather ill-defined, or fuzzy. In this paper, the distinction between *H-pure* and *H-mingled* states on the one hand, *N-pure* and *N-mingled* states on the other, is pivotal (these various states relate to the H-operators and N-operators, or observables, respectively). The fact of being H-pure or H-mingled can be called the *H-status* of a state (hence this H-status is twofold: pure or mingled).

c) A quintessential quantum feature is "quantumhood". *Quantumhood* refers to the battle-tested utter impossibility to split or conversely, to blend or coalesce, state vectors in a haphazard way. For instance, it flatly rules out half-photons and proton-neutron hybrids or compounds; which cannot be found in actuality. (Quantumhood can also be labeled as the 'quantisation constraint'.)

d) Quantum theory allows for two - and only two - evolution laws; which are:

- The *wavelike movement*, or 'wavy motion' in short. It complies with the mathematical principle of least action.

- The *wave function* (or *state vector*) *collapse*. I find it convenient to also call it the *paralling*, or the *paral phase*. It is a sudden non-deterministic event, in sharp and unmistakable contrast with the smooth and tractable 'wavy' (i.e. least action) motion.

An exclusively 'wavy' evolution - from a given initial state to a given final state - is said to be *unitary*. Accordingly, these two end-states (that is, both initial and final) can be said to be *uni-equivalent*. It plainly means that there is a time-reversible unitary transformation which links them up mathematically (in the so-called 'Schrödinger picture', for the detail).

I.2. Two useful findings

Two simple results will prove of paramount importance in this paper. They are:

a) *All the uni-equivalent states of an isolated system share the same H-status.* It means that any free system in a H-pure (or conversely, in a H-mingled) state will remain such as long as its evolution is exclusively wavelike.

Let us briefly see how this can be proven. First we consider a free system, taken in a H-pure (resp. H-mingled) state at a certain time. Then we show that it is contradictory to assume that there exists a H-mingled (resp. H-pure) uni-equivalent state for the system.

The contradiction is easily arrived at. It stems from the fact that this assumption implies that the norm of the state vector - which is always equal to one - is unlawfully made to differ from unity at some other time. (Alternatively, we can readily derive this result from the plain fact that H-pure states are stationary.)

b) *Wavelike motion can be breached only by paral phases.* This result is straightforward. For a given evolution, the least action movement is either enforced throughout, or it is breached. (Obvious enough, isn't it ?) The point is, when breached, quantum theory provides only one alternative (namely, the collapse, or *paralling*) - which thus becomes mandatory. This ascertains that the wavelike motion is breached *iff* at least one collapse occurs during an evolution.

These two findings will be instrumental in reaching two conclusions, which are:

- i) The paral phase is far more pervasive than is commonly held. It is not locked up in the sole quantum measurement, or observation, processes.
- ii) The alleged {wave / particle} duality is spurious. Indeed, it harks back to the {pure state / mingled state} duality; which in turn mirrors the {collapse / wavy motion} duality.

I.3. The dilemma

As we saw, nature did not content itself with just one evolution law (namely, that of the wavy motion, which is overwhelmingly predominant). Instead, it introduced a second law. *Why is it so?* The answer lies in the rather unexpected fact that the wave function is potentially *self-contradictory*. This startling fact stems from two attributes of the wave function which are inherently conflicting. These attributes are:

- a) The extension or 'spreadedness' - and more generally the dispersion, spatial and otherwise - of the interfering quantum waves; belonging to the same elementary particle (or *wavicle* as we might call it lest it be confused with classical particles).
- b) **Quantumhood** or the quantisation constraint; which prevents the wave function from being split (and/or mingled) haphazardly.

It is a matter of course that a log can always be chopped *because* it is not point-like. The extension of the wave function similarly entails that it can always be split. However, such a splitting is anathema: it is ruled out by quantumhood, and therefore never arises. What happens, then, if we insist on splitting the wave-function? This is a key question, properly answered by means of the Paralling Postulate (*see the main text*). Here I set out to explore the theoretical meaning of the sentence: "*we insist on splitting the wave function*". (It will ultimately turn out that performing a measurement is a way to do so.)

To that end, let us go back to the H-status of an isolated system. If this system is in a H-pure state, it will never leave it spontaneously (it is acknowledged to be in a stationary state). Under such a circumstance, quantumhood is not threatened. Consequently, the system can smoothly proceed on, by means of its wavy motion. If the system is originally in a H-mingled state, the situation is utterly different (as we know, such a system overlaps with more than just one eigenstate of its Hamiltonian). It is in danger of being split, *against* quantumhood; and here is the snag. Let us see why.

A basic feature of Hamiltonians is that their eigenstates are prone to absorb, or 'swallow' as it were, bits of neighbouring wave functions (this is what makes the energy operators so special). Accordingly, a H-mingled state always tends to be severed by the various H-eigenstates involved. They somehow conspire to absorb it, in proportion to the related intensities of overlap. This 'H-driven' tendency puts quantumhood in jeopardy. The contradictory nature of the wave function is then made explicit. In essence, this situation is what I call a *paralling condition*. Thus, *a system is in a paralling condition iff it is in a H-mingled state*.

A paralling condition tends to elicit a collapse, or a *paralling* (hence its name). Yet this necessary condition is not sufficient. To show and exemplify this, let us scrutinise an isolated

atom taken in its ground (i.e. not excited) state.

In fact, such an atom may be in a H-mingled state, which explicitly involves its peripheral electrons with the relevant energy levels. However and by definition, it cannot spontaneously leave this state (for want of the right particle to absorb or to emit). It cannot jump into, or reach, any of its H-pure states... which therefore prove *unreachable*.

When a H-mingled state includes at least one *reachable* H-pure state, a paral phase may actually occur. More accurately, it will definitely take place, sooner or later. Given enough time, it is bound to happen. The system is then said to be in a *triggering condition* (which is but a particular case of a paralling condition).

We now gather that "insisting on splitting a wave function" translates theoretically into: "putting a system in a triggering condition". The degree of insistence (i.e. the intensity of the paralling condition) can be worked out from the appropriate amplitudes - or else, intensities - of overlap.

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DOES NATURE APPROXIMATE A CLASSICAL IDEAL?

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ABSTRACT

This paper addresses the question: can the behaviour that characterizes ideal Classical Dynamics (CD) be expressed in Quantum Mechanical (QM) terms? Subsidiary to this question are others: is this ideal expression followed, in nature, or is it only approximated?; if the latter, what sort of approximations obtain?; what regularities or laws are implied?; are they recognisable as belonging to CD?

We begin by proposing a QM expression that purports to describe 'ideal classical behaviour' for a set of commuting observables. This so called Ideal is then specialised to apply to the coordinates of a classical particle. The Hamiltonian operator obtained by solving the resulting equations (in terms of coordinates and momenta) turns out to be unrecognisable. The cause is soon apparent: such behaviour, to be observed, requires infinite energy. Two well known classical Hamiltonians, however, are shown to be able to approximate the equations; in other words nature does, sometimes, approximate the ideal. A general approximating condition is proposed that may be like that followed by nature. This condition is shown to require, firstly, that a particular hierarchy of integrals shall exist and, secondly, that their variation (with respect to the Hamiltonian) shall vanish. The first few solutions of this hierarchy of (variational) problems, are quoted: they determine a recognisable Hamiltonian archetype. The significance of this result is discussed. Limitations and weaknesses in the argument are surveyed.

1. THE IDEAL

1.1 The Classical Ideal

The ideal classical dynamic is *smooth, continuous and, in principle, predictable*. The predictability depends upon accurate measurement of initial conditions combined with a reliable deterministic theory.

1.2 The Quantum Ideal

To express these ideas, in the language of QM, consider a symmetric (hermitian or self-adjoint) operator A which represents an observable. Measure A at time $t = 0$, to give eigenvalue $a(0)$ and eigenstate $\phi(0)$; then measure A again at time $t = t_1$, to give eigenvalue $a(t_1)$ and eigenstate $\phi(t_1)$. At intermediate times

$$(1) \quad 0 < t < t_1$$

the state ϕ is supposed, according to the standard model, to evolve as

$$(2) \quad \phi(t) = U\phi(0); \quad U = \exp(-iHt / \hbar); \quad \hbar \equiv (\text{Plank's constant}) / (2\pi)$$

where H is a symmetric operator called the Hamiltonian.

If, for all $t_1 > 0$,

$$(3) \quad \phi(t_1) = \phi(t_1); \text{ an eigenstate}$$

then A is said to be *classical with respect to $\hat{H}$* . Given condition (3) the second measurement is *never uncertain*; it is the eigenvalue corresponding to an eigenstate $\phi(t_1)$ which depends only on $\phi(0)$, t_1 and H .

2. A CLASSICAL COMMUTING SET

2.1 Necessary and Sufficient Conditions

Consider a commuting set of symmetric operators

$$(4) \underline{A} \equiv \{A_1, A_2, \dots, A_n\}$$

which characterize some system. Then, for every element of $\underline{A}$ to be classical with respect to H , it is both *necessary and sufficient* that

$$(5) A_j(t)A_k = A_k A_j(t); j, k = 1, 2, \dots, n$$

where

$$(6) A_j(t) \equiv U^\# A_j U; \text{ see (2); Heisenberg representation; } \# \text{ denotes Hermitian transpose}$$

2.2 Some Implications

Strictly, the conditions (5) are sufficient only when the eigenvalues of each A_j are distinct. If the eigenvalues are proper (the spectrum is discrete) the evolution (2) must be in steps. But this is contrary to the requirements of *smoothness and continuity*. We conclude that the elements of $\underline{A}$ must have *continuous spectra*. In consequence we regard (5) and (6) as defining classical behaviour rather than (2) and (3).

2.3 A Little Algebra

The commutator

$$(7) D_{jk} \equiv i(A_j(t)A_k - A_k A_j(t)); j, k = 1, 2, \dots, n$$

can be expanded in powers of time t

$$(8) D_{jk} = \sum_{\alpha} t^{\alpha} K_{\alpha jk} / \alpha!; \alpha = 1, 2, \dots$$

where

$$(9) K_{\alpha jk} \equiv i(V_{\alpha j} A_k - A_k V_{\alpha j})$$

$$(10) V_{(\alpha+1)j} \equiv i(HV_{\alpha j} - V_{\alpha j}H) / \hbar; V_{1j} \equiv i(HA_j - A_j H) / \hbar$$

from which

$$(11) K_{(\alpha+1)jk} = i(HK_{\alpha jk} - K_{\alpha jk}H) / \hbar + i(V_{1k}V_{\alpha j} - V_{\alpha j}V_{1k})$$

This recurrence relation is sometimes more convenient, for calculation, than the definition (9).

2.4 Consequences

The conditions (5) refer to successive measurements. First, the simultaneous measurement of *all* the elements of $\underline{A}$ and second, the simultaneous measurement of *all* the elements of $\underline{A}(t)$. The precision of these measurements is limited by an algebraic inequality known as the *generalized Heisenberg uncertainty principle*

$$(12) \text{ var}(A_j) \text{ var}(A_k(t)) \geq \langle D_{jk} \rangle^2 / 4; \text{ var}(A) \equiv \langle A^2 \rangle - \langle A \rangle^2$$

Notice that (5) is equivalent to

$$(13) D_{jk} = O \Leftrightarrow K_{\alpha jk} = O; \text{ null operator; see (8)}$$

Further, if we require to approximate classical behaviour only for a short time ($t \rightarrow 0$) after the first measurement then, the condition

$$(14) K_{1jk} = O$$

is sufficient. By adding more of the conditions

$$(15) K_{\alpha jk} = O; \alpha = 2, 3, \dots$$

we merely extend the duration of the approximation.

3. IDEAL PARTICLES

3.1 QM Notation- Coordinates and Momenta

Following the usual practice the state of a system of one or more particles is represented by a function $\phi(\underline{q}, t)$, on a Hilbert space, where the set of independent variables

$$(16) \underline{q} \equiv \{q_1, q_2, \dots, q_n\}$$

is (usually) chosen to be the aggregate of particle coordinates (defined over suitable domains). The Fourier transform of ϕ , over $\underline{q}$, $\varphi(\underline{p}, t)$ forms another (dual) Hilbert space with basic variables

$$(17) \underline{p} \equiv \{p_1, p_2, \dots, p_n\}; \quad -\infty < p_j < \infty; \quad j = 1, 2, \dots, n$$

φ is also taken to be (a different) representation of the state. The sets of operators $\underline{P}$ and $\underline{Q}$ defined, on their respective Hilbert spaces, by

$$(18) \underline{P}\varphi = p\varphi; \underline{Q}\phi = q\phi$$

can be shown to satisfy

$$(19) Q_j P_k - P_k Q_j = i\hbar \delta_{jk}; P_j P_k = P_k P_j; Q_j Q_k = Q_k Q_j$$

The scaling constant $\hbar$ is inserted as part of an identification of $\underline{p}$ with the momenta (for free particles, at least).

Note that the elements of $\underline{P}$ and $\underline{Q}$ all have continuous spectra.

3.2 Hamilton's Equations in QM

ϕ is normed; and the usual argument purports to show that ϕ evolves as at (2). The velocity A^* of a symmetric operator A can then be defined by

$$(20) \quad d\langle A \rangle / dt \equiv \langle A^* \rangle$$

to give

$$(21) \quad A^* = i(HA - AH)$$

In particular we note that the statements

$$(22) \quad \underline{P}^* = i(H\underline{P} - \underline{P}H) / \hbar; \underline{Q}^* = i(H\underline{Q} - \underline{Q}H) / \hbar$$

are formally equivalent to Hamilton's equations; hence the identification of the elements of $\underline{P}$ as momenta. It can be shown that, if F is a symmetric operator function of the $\underline{P}$ and $\underline{Q}$ then, the operations

$$(23) \quad F_{,j} \equiv i(FQ_j - Q_jF) / \hbar; F_{,k} \equiv i(P_kF - FP_k) / \hbar$$

are formally equivalent to *partial differentiation, with respect to P_j and Q_k respectively, the order of $\underline{P}$ and $\underline{Q}$ factors being preserved*. The rule for the differentiation of a product also holds under the same condition. These rules are most easily demonstrated for polynomial operator functions; but they also hold for a wider class.

3.3 The Classical Particle

In classical physics the particle is conceived as a Euclidean point with inertia. In the course of its motion this point is supposed to move smoothly and predictably. It is clear, therefore, that if the conditions (5) have any validity, as defining *classical behaviour*, then they should apply to the coordinates of a particle. That is, we may choose

$$(24) \quad \underline{A} \equiv \underline{Q}$$

3.4 Solution for the Hamiltonian

We now solve the operator equations (13) for H given (24). The first of the equations has a unique solution

$$(25) \quad K_{1jk} = 0 \Rightarrow H_{,j;k} = 0 \Rightarrow H = P_m b^m + c$$

where the b^m and c are either pure functions of $\underline{Q}$ or constants; also, from now on the Einstein summation convention is in force until further notice. Therefore it is necessary to raise suffices on certain symbols. For example

$$(25a) \quad Q_j \rightarrow Q^j; V_{\alpha j} \rightarrow V_{\alpha}^j; K_{\alpha jk} \rightarrow K_{\alpha}^{jk}; b_j \rightarrow b^j$$

This does not imply, however, that these symbols necessarily have tensor properties. Substitute the expression for $\underline{H}$, at (25), and we find that (15) holds; see (9) and (10). In other words *all* the

conditions (13) hold. So (25) is the most general form of the Hamiltonian that ensures that $\underline{Q}$ is classical.

3.5 Disaster! H is Linear in $\underline{P}$

H is linear in $\underline{P}$ with coefficients pure in $\underline{Q}$. This implies that the velocity and higher time derivatives of $\underline{Q}$ are functions of $\underline{Q}$ alone; see (21). No such Hamiltonian is recognizable in classical physics.

The reason why no such behaviour is observed is that it is not observable. The conditions (2) and (3) require that a measurement of A shall be exact, instantaneous and drawn from a continuous spectrum; so infinite information is conveyed in infinitesimal time. In practice all measurements involve *finite* energy, quantum disturbance, frequency, time and information; so the variance of a *dynamical variable* is *never* zero. This suggests that, at least for a particle,

$$(26) \quad D^{jk} \neq 0; \text{ see (7), (12) and (24) }$$

4. CLASSICAL HAMILTONIANS THAT APPROXIMATE THE IDEAL

4.1 A Free Newtonian Particle

The Hamiltonian

$$(27) \quad H \equiv 1/2 \sum_j W_j^2 / m; j = 1, 2, 3$$

represents a free Newtonian particle of mass m , with *cartesian* momenta and coordinates

$$(28) \quad \underline{W} = \underline{P}; \underline{X} = \underline{Q}; n = 3$$

We find that

$$(29) \quad D^{jk} = \delta_{jk} h t / m; \text{ Kronecker delta}$$

Now apply condition (12) to two successive measurements of X^j both of which achieve a standard deviation σ . Then

$$(30) \quad \sigma^4 \geq (h t / m)^2 / 4 \Rightarrow t \leq 2 \sigma^2 m / h$$

For example suppose that $\sigma = 10^{-6}$ metres and $m = 5 \times 10^{-6}$ kilograms, then $t \leq 3 \times 10^9$ years. We conclude that, in principle, the position of a laboratory mass can be determined, to good precision, over a very long period; (in this example 2/3 the age of the earth). Such behaviour is, surely, *classical*.

4.2 The Harmonic Oscillator

The Hamiltonian

$$(31) \quad H = 1/2(W^2 / m + sX^2); m, s > 0 \text{ scalar}$$

represents a one dimensional harmonic oscillator. We find that

$$(32) \quad D = (h t_0 / m) \sin(t / t_0); t_0 \equiv (m / s)^{1/2}; \text{ suffices on } D \text{ ignored}$$

So D is bounded for all t . Now the Harmonic oscillator is sometimes used to model radiation. Suppose, for example, that we define the period $2\pi t_0$ in terms of a wavelength λ and the mass m in terms of the photon energy. Thus

$$(33) \quad 2\pi t_0 \equiv \lambda / c; mc^2 \equiv h / t_0; c \text{ is the speed of light}$$

Then substitute D into (12); and consider, again, two successive measurements of X with standard deviation σ . We find that (12) holds, for all t , only if

$$(34) \quad \sigma \geq 2^{-1/2} \lambda / (2\pi)$$

which relates the wavelength, directly, to the precision with which position can be measured.

5. A CRITERION OF APPROXIMATION

5.1 D of Finite Order

The above calculations suggest that nature may approximate the ideal under suitable conditions. We do not know the criterion of approximation used by nature; but, it is reasonable to suppose, any criterion capable of getting close will yield roughly correct results. This is the philosophy adopted, for example, when, in the absence of knowledge about error statistics, we use the *least squares* criterion, to fit a curve to data, in favour of the *maximum likelihood* criterion.

Thus, to express the degree of inequality between the RHS/LHS of (5), we could use the criterion

$$(35) \quad \text{tr}(D^2); \text{ suffices ignored for simplicity; see (7)}$$

where the symbol D is used for the matrix representation, of the operator D , with respect to some basis. Because D is symmetric (35) is the sum of squares of the moduli of all the elements of D ; it is also the *sum of the squares of the eigenvalues of D* . So (35) is invariant (independent of the basis).

5.2 D of Infinite Order or With a Continuous Spectrum

The criterion (35) is satisfactory when D has a finite number of eigenvalues (is of finite order). In practice D may have an infinite number of eigenvalues or it may have a continuous spectrum. In either case the trace may not converge. It is better, therefore, to adopt the *mean square spectrum* of D denoted by

$$(36) \quad \langle\langle D^2 \rangle\rangle; \text{ mean square spectrum of } D$$

as a criterion. Notice that, when the order of D is finite, (36) reduces to

$$(37) \quad \text{tr}(D^2) / N; \quad N \text{ is the order of } D$$

5.3 Lemma- The Trace of a Symmetric Operator

(37), as it stands, is not of much use; we need an explicit formulation that holds in the general case. Consider first an operator that has a *finite* trace. Suppose that we agree to express the Hamiltonian and associated operators in terms of only cartesian momenta and coordinates

$$(38) \quad \underline{P} \equiv \underline{W}; \underline{Q} \equiv \underline{X}$$

where we can then use the representations

$$(39) \quad W_j \phi \equiv -i\hbar \phi_{,j}; \quad X^k \phi \equiv x^k \phi; \quad x^k \text{ is scalar}$$

It can be shown that if $F(\underline{W}, \underline{X})$ is a symmetric operator function, with a *finite trace* but an *infinite order*, then

$$(40) \quad \text{tr}(F) \approx \int_{-\infty}^{\infty} \dots \int_{-\infty}^{\infty} F(\underline{w}, \underline{x}) \prod_{r=1}^n (dx^r dw_r / (2\pi\hbar)); \text{ not summed over } r$$

where the scalars $\underline{w}$ and $\underline{x}$ replace the operators $\underline{W}$ and $\underline{X}$ respectively.

5.4 Lemma- The Mean Spectrum of a Symmetric Operator

The same argument that leads to (40) also requires that

$$(41) \quad \langle\langle F \rangle\rangle \approx \lim_{\substack{u \rightarrow \infty \\ v \rightarrow \infty}} Lt \int_{-v}^v \dots \int_{-v-u}^u \dots \int_{-u}^u F(\underline{w}, \underline{x}) \prod_{r=1}^n (dx^r dw_r / (4uv))$$

This expression is valid *only* when the *trace does not exist*. The approximation in (40) and (41) is of the type

$$(42) \quad \sum_{k=-\infty}^{\infty} f(k) \approx \int_{-\infty}^{\infty} f(s) ds; \quad k \text{ is an integer}$$

5.5 Conditions to be Satisfied

In anticipation, that for many of the operators with which we are concerned the traces do not exist, we use the form (41) rather than the form (40). Accordingly, we impose the conditions

$$(43) \quad \langle\langle D^2 \rangle\rangle \text{ shall be finite;}$$

$$(44) \quad \delta \langle\langle D^2 \rangle\rangle = 0 \text{ where possible}$$

The variation at (44) is of all free parameters and functions associated with H ; its purpose is to make a *unique* choice which, we hope, *minimises* $\langle\langle D^2 \rangle\rangle$.

By virtue of (8) we can expand D^2 in powers of t . Thus

$$(45) \quad D^2 \equiv \sum_{\gamma=2}^{\infty} L_{\gamma} t^{\gamma}; \quad L_{\gamma} \equiv \sum_{\alpha+\beta=\gamma} K_{\alpha} K_{\beta} / (\alpha! \beta!); \quad \gamma = 2, 3, \dots$$

It follows that, because t is positive but otherwise arbitrary, the conditions (43) and (44) become

$$(46) \quad \langle\langle L_{\gamma} \rangle\rangle \text{ shall be finite; } \gamma = 2, 3, \dots$$

$$(47) \quad \delta \langle\langle L_{\gamma} \rangle\rangle = 0 \text{ where possible}$$

When we use (41) to express (47) for given γ , as a problem in the calculus of variations, we impose the usual caveats about what happens to the variations at the limits of integration.

6. RESULTS

6.1 $\gamma = 2$ - The Hamiltonian

We get

$$(48) \quad L_2^{jk} = (K_1^{jk})^2; K_1^{jk} = \hbar H_{:j;k}; \text{ treat } H \equiv H(\underline{w}, \underline{x}) \text{ as scalar}$$

To give a meaning to the variational problem (47), in the case $\gamma = 2$, we consider variation of the $\underline{w}$ integral prior to taking limits $u, v \rightarrow \infty$; see (41). We get

$$(49) \quad H_{:j;j;k;k} = 0$$

So H is a polynomial in $\underline{w}$, of order not exceeding 3, with coefficients that are functions of $\underline{x}$. It follows that, by the method of their construction, the $V_\alpha^j, K_\alpha^{jk}$ and L_γ^{jk} are *all polynomials* in $\underline{W}$ of finite order. We conclude that for the integral (46) to be finite when the limits ($u, v \rightarrow \infty$) are taken, its kernel L_γ^{jk} must be of *zero order*. Therefore H is of *second order* in $\underline{w}$. Thus

$$(50) \quad \begin{aligned} H(\underline{w}, \underline{x}) &= 1/2 a^{rs} w_r w_s + b^r w_r + c \\ \Rightarrow H(\underline{W}, \underline{X}) &= 1/2 \underline{W}_r a^{rs} \underline{W}_s + 1/2 (b^r \underline{W}_r + \underline{W}_r b^r) + c \end{aligned}$$

where the factor 1/2 is inserted by convention. Notice that in defining the *operator* $H(\underline{W}, \underline{X})$ we have, firstly, used the QM rules for going from CD scalar equations to QM operator equations

$$(50a) \quad a + b \rightarrow A + B \text{ and } ab \rightarrow 1/2(AB + BA)$$

and, secondly, made the substitution

$$(50b) \quad 1/2(\underline{W}_r \underline{W}_s a^{rs} + a^{rs} \underline{W}_r \underline{W}_s) \rightarrow \underline{W}_r a^{rs} \underline{W}_s$$

The operators on either side of (50b) are not, of course, equal; but they differ only by a function of $\underline{Q}$ which has been included in the operator $c(\underline{Q})$.

6.2 $\gamma = 2$ - Consequences

Hamiltonians of the form (50) are capable of describing the motion of a particle under electromagnetic forces in a gravitational field; in this role they are called Super Hamiltonians [4]. For example, when we set

$$(51) \quad b^r = 0; c = 0$$

Hamilton's equations (see (50)) are identical to the geodesic in a Riemannian metric with fundamental tensor a^{rs} . By assuming that

$$(52) \quad \det(a^{rs}) \neq 0; \text{ usual for a metric tensor}$$

we form a non-singular relation between velocity and momentum. We shall show, however, by continuing the analysis of the L_γ^{jk} , that the present theory requires the a^{rs} to be *constants*; gravity, in the sense of GR, is thereby excluded.

Because L_γ^{jk} is a function of $\underline{Q}$ alone (47) reduces to a variational problem (prior to taking limits $u, v \rightarrow \infty$) in $\underline{Q}$ alone. In the case $\gamma = 2$ the condition (47) cannot be applied because it leads to

$$(53) \quad a^{rs} = 0; \text{ all } r, s$$

which takes us back to the linear Hamiltonian!

Note that Hamilton's equations give

$$(54) \quad X^j X^{k*} - X^{k*} X^j = i\hbar a^{jk} \Rightarrow H_{,j;k} = a^{jk} \Rightarrow (50); X^{j*} \equiv V_1^j$$

which, from some points of view, is a statement of Newton's Second Law; it is also (in various forms) the start of the Feynman-Dyson/ Tanimura/ Kauffman-Noyes papers [1],[2],[3]. Note, however, that Tanimura, Kauffman and Noyes do not, necessarily, require X^j and X^{j*} to be symmetrical operators. That is they do not, necessarily, claim to be doing QM.

6.3 $\gamma = 3$ - Metric Coefficients

We get

$$(55) \quad L_3 = 1/2(K_1 K_2 + K_2 K_1)$$

$$(56) \quad K_1^{jk} = \hbar a^{jk}; K_2^{jk} = \hbar(a^{rs} a_{,s}^{jk} + a^{ks} a_{,s}^{jr} - a^{js} a_{,s}^{kr}) W_r + \text{a function of } \underline{X}$$

Because L_3 must be independent of $\underline{W}$ the coefficient of W_r in K_2^{jk} must vanish. This implies

$$(57) \quad a_r^{jk} = 0; \text{ metrical coefficients constant}$$

Using (57) the full expression (56) becomes

$$(58) \quad K_2^{jk} = \hbar(a^{kr} b_{,r}^j - a^{js} b_{,s}^k)$$

Because the metrical coefficients are constant the space is flat and the metric is entirely consistent with the use of cartesian coordinates. Given (58) condition (47) is satisfied identically.

6.4 $\gamma = 4$ - Newtonian Particle Only- For Simplicity

In order to simplify the next stages of the calculation we reduce (50) to the special case

$$(59) \quad H = 1/2 \sum_{r=1}^3 W_r^2 / m + c(\underline{X}); \text{ particle in a scalar potential}$$

We get

$$(60) \quad L_4 = (K_1 K_3 + K_3 K_1) / 3! + (K_2)^2 / 4$$

$$(61) \quad K_1^{jk} = \delta_{jk} \hbar / m; \quad K_2^{jk} = 0; \quad K_3^{jk} = -\hbar c_{,j,k} / m \Rightarrow L_2 \text{ is constant; } L_3 = 0$$

Note that L_4 is a function of $\underline{X}$ alone, as required, being proportional to $c_{j,k}$. The integral $\langle\langle L_4 \rangle\rangle$ therefore depends only on the values of the $c_{j,k}$ at the limits of integration; if these are not to be varied then condition (47) is satisfied identically.

6.5 $\gamma = 5$ - Newtonian Particle Only- For Simplicity

We get

$$(62) \quad L_5 = (K_1 K_4 + K_4 K_1) / 4!; \quad K_2 = 0; \text{ see (61)}$$

$$(63) \quad K_4 = -\hbar \sum_{r=1}^3 (W_r c_{j,k,r} + c_{j,k,r} W_r) / m^2$$

Because L_5 must be independent of $\underline{W}$ the coefficients of W_r in K_4^{jk} must vanish.

$$(64) \quad c_{j,k,r} = 0 \Rightarrow K_4 = 0 \Rightarrow L_5 = 0$$

That is c is a second order polynomial in $\underline{X}$. So, given (64), (59) represents a *multidimensional linear oscillator*. The integral $\langle\langle L_5 \rangle\rangle$ vanishes because its kernel does so; see (47).

Concerning the convergence of the series for D^{jk} and the existence of the mean spectra $\langle\langle L_\gamma \rangle\rangle$ (see (8) and (41)): note that the K_α^{jk} are zero for even α and constant for odd α ; the constants are functions of the (constant) coefficients in c . Thus the mean spectra all exist and, although it may be necessary to place some restriction on the coefficients in c , convergence of the series *can* be ensured; see, for example, (32).

6.6 A Function of the Cartesian Coordinates

Suppose now that the array $\underline{A}$ consists of a single function (for simplicity)

$$(65) \quad A = A(\underline{X})$$

of the cartesian coordinates $\underline{X}$; there is then only one difference operator D to be considered. Given the Hamiltonian (59) we get

$$(66) \quad K_1 = \hbar \sum_{r=1}^3 (A_{,r})^2 / m; \quad K_2 = 1/2 \sum_{r=1}^3 (W_r K_{1,r} + K_{1,r} W_r) / m$$

L_2 is independent of $\underline{W}$. But L_3 is independent of $\underline{W}$ only if K_2 is likewise. Thus

$$(67) \quad K_{1,r} = 0 \Rightarrow K_1 \text{ is constant; } L_2 \text{ is constant; } K_2 = 0; \quad L_3 = 0$$

Notice that these results are independent of the potential energy function $c(\underline{X})$.

Condition (67) (that K_1 is constant) *selects* functions A which satisfy (46) for $\gamma < 4$; at least for small duration t , until the higher order terms of (8)/(45) become significant, these functions may therefore be said to *characterise the system*. They could be used as *coordinates*. For example any A linear in the $\underline{X}$, with constant coefficients, satisfies

$$(68) \quad A_{,j,k} = 0; \text{ for all } j, k$$

and therefore satisfies (67). The polar and azimuthal radii defined, respectively, by

$$(69) \quad R \equiv (X_1^2 + X_2^2 + X_3^2)^{1/2}; \quad \rho \equiv (X_1^2 + X_2^2)^{1/2}; \text{ suffices lowered for clarity}$$

also satisfy (67).

The appearance of the Euclidean distance, in this way, is intriguing. Although we state that the X are 'cartesian coordinates' there is nothing explicit in the theory that imposes a metric. The coefficients a^{rs} , in (50), are called 'metrical coefficients'; but this is only an analogy derived from the fact that, given (50), Hamilton's equations have the appearance, under appropriate conditions, of geodesic equations. The structure of R and ρ appears to relate to the quadratic dependence of (59) on $\underline{W}$; but (59) is only a specialisation of (50). R and ρ seem, therefore, to be *fundamental* to the theory.

6.7 The Polar Radius

In order to pursue further the relationship between the *polar radius*

$$(70) \quad A \equiv R \Rightarrow K_1 = h / m; \text{ see (66)}$$

and the Hamiltonian (59) we need to calculate the K_α for $\alpha > 2$ given (70). This is tedious when carried out, by purely symbolic methods, as heretofore; easier progress is possible using *representations in polar coordinates*. Firstly note that (59) transforms to

$$(71) \quad H = -\frac{h^2}{2m} \left[\frac{\partial^2}{\partial r^2} + \frac{2}{r} \frac{\partial}{\partial r} - B / r^2 \right] + c; \quad r^2 \equiv \sum_{j=1}^3 x_j^2$$

where, with polar angle θ and azimuthal angle ϕ ,

$$(72) \quad B \equiv - \left[\frac{1}{\sin \theta} \frac{\partial}{\partial \theta} \left(\sin \theta \frac{\partial}{\partial \theta} \right) + \frac{1}{\sin^2 \theta} \frac{\partial^2}{\partial \phi^2} \right]$$

and $h^2 B$ represents the *square of the angular momentum*. Note also that B commutes with both r and $\partial() / \partial r$ and so

$$(73) \quad V_1 \equiv i(HR - RH) / h = -\frac{ih}{m} \left[\frac{\partial}{\partial r} + \frac{1}{r} \right]; \quad R \equiv rI$$

Therefore

$$(74) \quad H = 1/2 m V_1^2 + f; \quad f \equiv 1/2 B(h/r)^2 / m + c$$

from which it quickly follows that

$$(75) \quad V_2 \equiv i(HV_1 - V_1H) / h = -f' / m; \text{ where the prime denotes } \frac{\partial}{\partial r}$$

and, because K_2 vanishes,

$$(76) \quad K_3 \equiv i(V_1 V_2 - V_2 V_1) = -\frac{h}{m^2} f''$$

Similarly

$$(77) \quad K_4 = -\frac{\hbar}{m^2} [V_1 f''' + f''' V_1]$$

The operators K_3 and K_4 are *unbounded* and they possess neither *trace* nor *mean spectrum*; the same is therefore true of L_4 and L_5 . These statements appear to hold whatever the choice of the function $c(\underline{X})$. Although there are obvious formal similarities, between the expressions for K_3 and K_4 given at (76) and (77) and those given at (61) and (63), we may not simply abolish f''' (by analogy with (64)); f is an operator function of both the coordinates *and* their conjugate momenta.

The method of approximation, by which we hope to identify operators that are characteristic of classical systems, seems, in this very important case, to have failed (at least for the higher order K_α). But the failure may be in the *model* (59)/(71) rather than the *method*. The trouble is twofold. Firstly, B has no mean spectrum because its eigenvalues are

$$(78) \quad j(j+1); \quad j = 0, 1, \dots$$

Secondly, the factors r^{-n} for $n > 2$ do not have mean spectra on account of their behaviour as $r \rightarrow 0$. We would expect, however, that no particle in the universe can actually achieve an infinite angular momentum about any point; B must be bounded in some way. Also, for the situations to which (59)/(71) has been applied (a *light* particle orbiting in a central scalar field created by a *heavy* particle), the condition $r \rightarrow 0$ implies that the particles have *coalesced*. This situation may be possible; but it is surely outside the scope of the model.

7. COMMENTARY

7.1 The Choice of Coordinates

We have arrived at (41) by using the representations (39). These representations are appropriate to cartesian coordinates and momenta in the sense that they have *doubly infinite continuous spectra*. Thus, by expressing the lemma (41) in terms of the operators $\underline{W}$ and $\underline{X}$, we may have *ensured* (see (57)) that the metrical coefficients are constant. In this way we are open to the usual jibe that 'all attempts to derive CD (including gravity) from QM end up with flat spaces'.

It appears, therefore, that, in order to make further progress, lemma (41) must be expressed in terms of *general* coordinates. Now the representations of the $\underline{P}$ in the Hilbert space of functions $\phi(\underline{q})$, where the operators $\underline{Q}$ are interpreted as curvilinear coordinates, can be deduced using the properties of the Fourier transform; see (18). These representations involve the determinant of the metric tensor (because we insist that the integral of $|\phi(\underline{q})|^2$, over the space of curvilinear coordinates, is always unity). It follows that the *form* of (41) must depend on the metric tensor as well as the choice of coordinates.

We might hope that, by using coordinates other than cartesian, we would invoke not only more interesting metrical coefficients but also more interesting electrical forces: see, for example, (64). Unfortunately more formidable difficulties appear from another quarter.

7.2 The Model of Quantum Mechanics

In Sections 3.1 and 3.2 we make it clear that we are using the standard QM formalism associated with and leading to Schrodinger's equation. Various difficulties attend:

- Some would say that the formalism is appropriate only to the non-relativistic case; but see [1] and [2]. If so, we must always restrict the scope of our interpretations.
- There is a question as to whether (21) and (22) are valid for curvilinear coordinates in a (possibly) curved space.
- The true status of the time t , in the standard formalism, is obscure; likewise the status of H . Not the least of the difficulties is that there appear to be mathematical inconsistencies; (but that is another story!).
- It seems sensible to suppose that the (localised) subsystems of a system should be characterised by their own time measures; and, that if the subsystems are in different places, the time measures should differ. Questions then arise: what time measure should be used for observation of the *whole* system?: how does that measure relate to those of the subsystems? This problem arises as soon as we consider more than one particle.
- Some of the difficulties may be due to our assumptions about the continuity of space-time and/ or the smoothness of the evolution of the state. For example, the time and, possibly, the coordinates may be *discrete*. Alternatively the time may be *continuous* but the evolution may be *fractal*. In either case we might get rid of the some difficulties at the cost of great complexity.

These remarks alone suggest that the foundations need rigorous scrutiny and, perhaps, refurbishment before one may proceed further.

7.3 Mathematical Difficulties

Whatever expressions are finally obtained for the K_α the series (8) must be shown either to converge or to terminate; see, for example, (29) and (32). If it is otherwise then the results may be invalid. Apart from the tedium of working out the details, this is the chief weakness of the method based on the series of conditions (46) and (47). N.B. an operator series may be said to converge if the sum (operator) is bounded.

This difficulty is at its most acute in the discussion of the polar radius; see Section 6.8. After the first two the *individual terms* of the series for D appear strongly divergent. This behaviour is blamed on the model (particle in a central scalar field) rather than the method; some sort of normalisation might do the trick!

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PROCESS, SYSTEM, CAUSALITY, AND QUANTUM MECHANICS

A psychoanalysis of animal faith.

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Chapter 3. Process and system: the mathematics.

ABSTRACT

I shall argue in this paper that a central piece of modern physics does not really belong to physics at all but to elementary probability theory. Given a joint probability distribution D on a set of random variables containing x and y , define a *link* between x and y to be the condition $x=y$ on D . Define the *state* S of a link $x=y$ as the joint probability distribution matrix on x and y without the link. The two core laws of quantum mechanics are the Born probability rule, and the unitary dynamical law whose best known form is the Schrodinger's equation. Von Neumann formulated these two laws in the language of Hilbert space as $\text{prob}(P) = \text{trace}(PS)$ and $S'T = TS$ respectively, where P is a projection, S and S' are density matrices, and T is a unitary transformation. We'll see that if we regard link states as density matrices, the algebraic forms of these two core laws occur as completely general theorems about links. When we extend probability theory by allowing cases to count negatively, we find that the Hilbert space framework of quantum mechanics proper emerges from the assumption that all S 's are symmetrical in rows and columns. On the other hand, Markovian systems emerge when we assume that one of every linked variable pair has a uniform probability distribution. By representing quantum and Markovian structure in this way, we see clearly both how they differ, and also how they can coexist in natural harmony with each other, as they must in quantum measurement, which we'll examine in some detail. Looking beyond quantum mechanics, we see how both structures have their special places in a much larger continuum of formal systems that we have yet to look for in nature.

INTRODUCTION: COUNTING SHEEP.

Once upon a time there was a sheep farmer who had ten small barns, in each of which he kept five sheep. When asked how many sheep he had altogether, he replied "many", for people in those days counted on their fingers, and no one had ever thought of counting beyond ten.

Every morning he would drive his sheep over the hill and through the woods to their pasture, where they assembled in five fields, ten sheep to a field. The farmer, who was of a reflective bent, saw here a curious and beautiful law of nature: "Ten barns each with five sheep, and then five fields each with ten sheep!" Unfortunately this law did not always hold, and when the wolves howled on the hill at night, it failed quite often. The farmer had an explanation for this: "The howling of the wolves greatly upsets my sheep, and the laws of nature, like the laws of man, are often disobeyed when agitated spirits prevail". The farmer realized that to make his law universal he would have to modify it thus: "When tranquillity reigns, ten of five turn into five of ten."

We today who know arithmetic would say that the farmer's law, though true enough in his particular situation, isn't a very good law by scientific standards. It needs to be "factored" into two laws, the first being the simple and very general law that $xy = yx$ and the second a more complicated and specialized law having to do with sheep and wolves. The farmer was indeed aware that $xy = yx$, at least in the case of 5 and 10, but what he could not see is that the essential condition for xy to be yx has nothing to do with sheep or wolves or tranquillity but is simply that the total number of sheep remain constant. One reason he couldn't see this is that he lacked any conception of the total number of his sheep; that's because in those days there were no numbers beyond ten, just "many".

There are three morals to this tale. The first is that it's not enough just to ask whether a law is right or wrong - we should also ask whether it gets to the point. The second is more subtle: If the point escapes us, maybe it's because we lack the raw materials of thought needed to even conceive of it. The third is not subtle at all: learn to count!

We have learned to count beyond ten sheep and even beyond three dimensions, but we still are under a very stifling conceptual limitation in not being able to count beyond the two types of phenomena that we call *classical* and *quantum*. This paper will set these two among many more. It will do this by teaching us some new ways to count cases, such as how to keep counting when the count goes below zero! This will provide us with the raw materials for thought we need to clearly see some crucial points that quantum philosophy has so far missed, notably the significance, or rather the insignificance, of the wave function, and the essentially acausal nature of quantum processes.

Quantum mechanics has revealed many puzzling patterns in nature, perhaps the most puzzling being the EPR correlation. The explanations we hear for this phenomenon all too often resemble the farmer's spirit of tranquillity. We'll see that we can "factor" a

simple piece of probability theory out of physics that makes sense of things like EPR in much the same way that $xy = yx$ makes sense of the farmer's sheep counts. What's basically new here is that quantum phenomena in general can be represented as simple large number phenomena whose laws belong to the arithmetic of case counting.

But what about non-locality? The collapse of the wave front? Quantum measurement? How does the present paper fit in with the more familiar ways of interpreting the formalism of quantum mechanics? Let me briefly address this question.

It was recognized quite early that quantum mechanics bears the earmarks of a purely statistical theory. The Schrodinger equation looks very much like the equation governing a Markov process, and we actually get the Schrodinger equation if we multiply the generator of a self-adjoint continuous Markov chain by i . Now Markov processes belong entirely to the theory of probability - there's no physics in them at all. Could it be that quantum mechanics does for mechanics what statistical mechanics did for the theory of heat? Can mechanics, and space and time along with it, be reduced to the statistical behavior of something simpler and more fundamental, or perhaps even to theorems in the bare science of probability itself?

Two obstacles have stood in the way of such a simplification.

The first is that probabilities can't be imaginary. It turns out that we can define imaginary probabilities in terms of negative probabilities, but that wouldn't seem to be of much help - have you ever counted fewer than 0 cases? But then again, come to think of it, have you ever seen a pile of gold bricks with fewer than 0 bricks in it? And yet the mathematics of negative piles of gold bricks has become indispensable for keeping accounts, especially government accounts. So why not "keep accounts" with negative probabilities? This first obstacle doesn't look like it ought to stop us in our tracks for long, and if it were the only obstacle, it probably wouldn't.

The second has proved more obstinate. We saw that the Schrodinger and Markov equations have the same form, but we must next ask, do they govern the same quantities? The answer appears to be no. The numbers that in the Markov equation are probabilities, are the square roots of probabilities in the Schrodinger equation. This little disparity has for sixty years kept alive the notion of the "wave function" whose "amplitude" squared is probability.

And this little disparity, this seeming technicality, has been the logjam that has kept quantum philosophy circling in the same stagnant pool of inadequate ideas for the last sixty years. The present paper aims to break up that logjam.

Let's assume we have overcome obstacle 1 and can now work within an extended theory of probability where cases count negatively as well as positively. As we'll see, most of the math in this extended probability theory is the same as in the all-positive theory. Now suppose we write down the Schrodinger equation as a formal Markov chain, in which

the differential transition matrix operates on a state vector. The numbers in this vector are the amplitudes of the "wave function", which we would now like to think of as probabilities. The well-known problem with this construction is that the basic rule for quantum probability, the Born rule, says that these amplitudes must be squared to give the probabilities that are actually observed in quantum measurement.

However, and now we are coming to the key idea, this chain of transition matrices we call a Markov chain is only one of many ways to represent the joint probability distribution that is the Markov process itself. Another is by a chain of joint probability matrices linked by conditioning events of the form $x=y$. We define the *state* S of such a link as the probability matrix on x and y with the link removed. A state is called *quantum* if the distributions on x and y are identical, and a chain is called *quantum* if its states are quantum and its matrices are unitary. Now here is the punch-line: in a quantum chain, the unlinked probabilities on x and y behave like quantum amplitudes, while the linked probabilities, which are the diagonal entries of S , are the squares of these amplitudes, and hence behave like the probabilities they are supposed to be. There is no wave function, only probabilities, positive and negative.

The link method embeds quantum mechanics in the mathematics of Markov processes in such a way that quantum amplitudes are represented by *unlinked* probabilities and quantum measurement probabilities by *linked* probabilities. As we'll see, the same embedding maps classical Markov chains into isomorphic representations of themselves. In linked chains of any kind, linked probabilities are quadratic in unlinked probabilities, but in classical chains one of the factors is constant and factors out, so probability is in effect linear in amplitude, i.e. the classical "state vector" is the usual probability vector. If this sounds a bit cryptic, don't worry, it will all be spelled out in detail.

Most discussions of the meaning of quantum mechanics these days seem to be about the problem of the "collapse of the wave function." In link theory this problem simply vanishes, since there is no wave function to collapse. Imagine if the Eighteenth Century caloric were still hanging around as the official theory of heat: we'd be chronically plagued by ever more complicated theories explaining the collapse of the "caloric field" when you measure an atom's energy. What a relief to get away from the spell of such nonsense!

This large-number explanation of quantum mechanics raises two basic questions: Large numbers of what? and Must we buy it?

The answer to the first question is implicit in the above discussion, but needs to be said simply: The things we count large numbers of are *cases*. Simple arithmetic reveals that the core quantum laws, in a generalized form, are features of any probabilistic system whatsoever. Von Neumann's formulation of the Born probability rule $\text{prob}(P) = \text{trace}(PS)$ holds at every connection between the parts of such a system, and the dynamical rule $S'T = TS$ governs every part that is connected at two places.

I brought up caloric to draw a parallel between our present situation and the situation in physics when it was discovered that the laws governing heat could be interpreted as statistical laws of atomic motion. However, there is a big difference. In the case of heat, the statistical theory sat on top of the Newtonian theory of motion, whereas in our case there is no underlying empirical theory at all. Probability theory is just the arithmetic of case counting, so the generalized quantum laws are like $xy = yx$ in that their truth is assured, the only empirical issue being where and when they apply.

The answer to the second question is no, we don't have to. However, the same can be said about the arithmetical explanation of five fields with ten sheep each. It's logically possible that when true tranquillity reigns, the gods always make sure that every field contains ten sheep (presumably the age of true tranquillity is long since past). It's also logically possible that the non-local "guide wave" explanation of quantum phenomena is the right one. With both sheep and quantum, the arithmetical explanation makes so much more sense that it would be most malicious of the gods to reject it just to save our old habits of thought.

We'll see that there is another reason to prefer the arithmetical explanation, which is that, as our discussion of Markov processes suggests, it also applies to classical things like computers. This at last enables us to make sense of quantum measurement, which has always been a great mystery. Quantum and classical now stand revealed as two "shapes" made of the same stuff, so there is nothing more mysterious about their both being parts of the same process than there is about round wheels and square windows both being parts of the same car. The radical path also leads to a good Kantian solution of Hume's problem, which is that of finding causality in the order of succession, and we'll see that the choice between acausal and causal/classical thinking is to some extent a choice of analytical method, like the choice between polar and rectilinear coordinates.

But then comes the big question: What about the other shapes? The ones other than quantum or classical that we have never before imagined, and therefore never thought to look for in nature? We'll briefly touch on the big question, but it calls for a much bigger answer than I can give here, or now.

CHAPTER 1. PROCESS AND SYSTEM: AN OVERVIEW.

The term 'animal faith' in my subtitle is taken from the title of Santayana's book "Skepticism and Animal Faith"; it refers to what gets us through the day and keeps our thought processes going even through our spells of radical doubt. Santayana was concerned to delimit animal faith and to contrast it with other, presumably higher things. My agenda here is quite different and is closer to Kant's: it is to articulate and transform into explicit principles the animal faith implicit in certain of our concepts that play a key role in science. Once this is done, once these principles become explicit, we'll find that they take on a new life of their own, and are full of surprises. First, they reveal the simple mathematical structure that unifies quantum and classical. But then they strangely turn

against themselves, revealing their own limitations, and even rather impolitely suggesting that perhaps we should find better ways to get through the day. This paper, however, is about their more cheerful messages.

Like Freud's "talking cure"; my analysis will start by paying attention to how we talk about everyday things, and then go on to explore subterranean labyrinths in search of hidden meanings. But it is not a search for what has been repressed, or for what has fallen into the unconsciousness of habit, but for that ancient animal heritage of "know how" that we, as thinking people, unconsciously draw upon in formulating our most sophisticated thoughts.

Lest this sound too ambitious, let me add that I am confining my analysis to what underlies certain commonplace scientific words, notably: *information, part, place, event, variable, process, procedure, system, input, output* and *cause*. The unconscious beliefs we are searching for are those that belong to the smoothly working hidden machinery behind the easy flow of thoughts in which these words occur. To put it another way, without certain implicitly held principles these commonplace words, which are indispensable in any discussion of scientific matters, would be quite meaningless, and these are the principles I am trying to capture and articulate.

We shall see that, when these principles are precisely articulated, they become a tightly knit system that has some surprising consequences, including, as mentioned, the two 'core laws' of quantum mechanics. More generally, the consequences of an analysis of animal faith are of two kinds - call them Kantian and Freudian; the Kantian kind reinforce our animal faith, while the Freudian kind force us to question it.

Let's begin our analysis with information.

Suppose you see something and write down what you see; this is a *datum*. *Data* is what has been seen and noted. To qualify as *information*, however, data must have some element of surprise. If you know that A is going to happen, seeing it happen may be gratifying or reassuring, but it is not *informative*. Thus *information* is what you have seen, contrasted with what you *might* have seen. When you *describe* something, you normally supply a number of connected pieces of information, so *description* is information broken up into *connected parts*.

These very simple and ordinary observations are beginning to give hints of a pattern, but they come to a halt with the difficult word '*parts*'. It's curious how in the long and contentious history of speculations about what the world is made of, almost all the debate has been about what are its *ingredients*, and almost none about the more basic problem of what it means for these proposed ingredients to fit together as parts. Taking ordinary material things apart and putting them together is such a natural activity that we blithely extend it to entities of every kind, without ever imagining that this might lead to problems.

And this is certainly the case with information, as when we speak confidently of "partial information", "the whole story", etc. No doubt we are at ease with such talk because the parts and wholes of information so often coincide with the parts and wholes of the language that conveys it. But underlying this linguistic idea of part and whole there is a deeper level of meaning, and excavating that deeper level will be the main task of our analysis.

First of all, we must carefully distinguish between two kinds of part whole relationship: that which we find in space and space-time, and that which we find in material structures like buildings and computers and atoms. Let's call the first relationship *extension* and the second *composition*. The parts of an extension are its *regions*, and the identity of a region, i.e. that which makes it different from other regions, is its *place*. The parts of a composition, on the other hand, are its *components*, and the identity of a component is not its place but its *type*. Components are *interchangeable* parts; you can duplicate them, remove them from their places and use them elsewhere, etc. But a region, far from being an interchangeable part, is by its nature unique; the people of Palo Alto can leave California, even the buildings of Palo Alto can leave California, but Palo Alto itself can't leave California since it's a *place* in California.

Matter, as we currently conceive of it, is composed of elementary particles, the most perfectly interchangeable of parts imaginable, which makes matter a composition par excellence, which is to say, its essential self is nowhere. There are those today who speak of making matter out of space or space out of matter, but this is nonsense; you can't make extension out of composition nor composition out of extension. Those who claim to have a theory of *everything*, even if they are right, have only found half of the Holy Grail; the other half is a theory of *everywhere*!

The material objects we encounter in everyday life are always both regions and components; they have both places and types. Thought is a constant dialogue between extension and composition. Practically speaking, the important distinction to keep in mind is between parts that don't keep their identity when you remove them from their context, like random variables in a joint probability distribution, and those that do, like logic gates in a computer. Only the latter can be used to *construct* things.

In mathematics, the dialogue is between geometry and algebra. Think of analytic geometry, which is just such a dialogue. In pure Euclidean metric geometry, we start out with a homogeneous space whose places are *points*, i.e. we can only identify them by *pointing* to them as "here" or "there" or "there" etc. In order to keep track of these points, we move into algebra. We do this by giving each point a unique identity as a composition of an x-vector, a y-vector and a z-vector; this identification scheme is known as a coordinate system. Notice that vectors themselves are not regions or places; you specify a vector as a certain *type* of thing that can be found anywhere. Notice also that we must carefully specify just what it means for one vector to be a part, i.e. a component, of another; this particular kind of part-whole relation is what defines linear algebra as opposed to other kinds of algebra. Finally, notice that giving coordinates to points does not

entirely get rid of *here* and *there*; thus the dialogue continues since we must locate the 0 vector *here*, and we must then point "there", "there", "there" in the directions of x , y and z . Nor can we completely get rid of *this kind* and *that kind* in geometry, since geometric structure involves numerical ratios of distance, so we have *this* and *that* number, and onward to *this* and *that* figure, etc.

How does the distinction between extension and composition apply to informational structures?

Information accumulates as a progressive *extension* of the "body of knowledge". Incoming items of information build on each other, qualify each other, and in general, can only be understood in relationship to their "neighbors". We say "*this* happened, and then *this* happened and then *this* happened .." etc., where in each case to know *what* happened we must know something about what happened previously. From time to time, though, we *abstract* an item of information from its place and give it a kind of autonomy by turning it into a story, a design, a warning, an example, a rule, a law, a procedure etc. that can be retold or reused in other contexts; in short, we turn it into a component. One way to analyze a process is to systematically reconstruct it from components that have been so abstracted; we'll call this a *reductive analysis* and the resulting composition a *system*. Let's reflect a bit on the distinction between process and system, which will play an important role in interpreting our mathematical results.

The word 'process', in its most general sense, means something that 'proceeds or moves along'. But the word also has the connotation of *procedure* as in *due process*. Thus a process of a certain kind refers to that which is allowed to happen, or which can happen, under certain specified conditions. This is how we shall construe the present technical meaning of the word. If the possibilities under the specified conditions are assigned probabilities we'll call the process a *stochastic process*. More exactly, a stochastic process is a joint probability distribution on a set of so-called stochastic variables. In Chapter 3, we'll focus on Markov processes, which are the simplest and best known stochastic processes, and we'll see that the core quantum laws as they occur in Markov processes take the form most familiar to physicists.

A system is a process analyzed into interchangeable parts. A good system brings order to a complex whole by portraying it as a regular arrangement of a small variety of such parts. For stochastic systems the paradigm case is a Markov chain, which is a "chain" of connected copies of a single part called a *transition matrix* T_{ij} . We can think of T_{ij} as a representation of a stochastic variable j whose probability distribution $p(j)$ is a function of a free parameter i . Connecting means *assigning* the free parameter i to the stochastic variable j of the prior transition matrix, thereby turning the numbers T_{ij} into conditional probabilities. If we assign the free parameter of the first component to an *unconditioned* variable, then chaining transmits the definiteness of the first variable down the line to create a joint probability distribution on all the succeeding variables, and the resulting process is a Markov process.

The contrast between process and system is roughly that between extension and composition. A process is extended, its regions being *events*. Like many English nouns ('noun', for instance), the word 'event' can be taken either in the definite or the indefinite sense; an event can be something in particular that happened, in which case it has its unique place in space-time, or it can be a particular *kind* of happening, as when we speak of "the event heads" in probability theory. One way to describe a process is as an arrangement of events in the second sense, which we can think of as labelling its extended parts by certain of their qualities; another word for this is *map-making*. A map is a composition of essences, or predicates, to use the modern term, but the parts of the process that these essences identify need not be removable components, so a map does not in general represent the process itself as a composition.

Thus we see that there are two ways to analyze a process, or any other extended whole: We can *map* it; this is the way of the naturalist, and also of the explorer, the historian and the astronomer. Or we can *reduce* it to a heap of autonomous parts, which we then reassemble into a composition that has the same map the naturalist would draw. Nowadays we often hear about the naturalist as the good guy and the reductionist as the bad guy. But building things and taking things apart belong to life as much as exploring and drawing pictures. When the reductionist goes bad, it's usually because the naturalist hasn't given him a good enough map; what the reductionist then recreates from his storehouse of interchangeable parts may resemble the naturalist's whole, but the essence of the original is missing.

To give to a part the autonomy of a component, we must usually do more than just copy it as it appears in place. Rather, we must "de-install" it from its original context, which is to say, we must transform it into a new entity whose features are no longer conditioned by place. To reductively analyze a process is to de-install its partial regions, at least in our imagination, turning them into items that can stand alone, and that can be duplicated in such a way that the duplicates can be reassembled into a duplicate of the original whole. Reductive analysis gives the observer a more penetrating gaze, which sees not only what and where things are but where and how they come apart.

The metaphor of de-installing and re-installing components should be a vivid one for anyone who has had to wrestle with the problem of de-installing an APP from Windows. In fact, de-installing a computer program is literally a special case of the operation I shall describe here of *disconnecting* a component. We'll go into all this in detail later, but for now let it just serve as a reminder of how much hard work goes into seeing our ordinary world as full of *things*. This hard work is largely unconscious, and is based on "know-how" dating from our dim animal past. Our problem today is that, with the progress of scientific thought, we have wandered into domains where this unconscious skill no longer serves all our needs; for instance, it falls far short of telling us how to de-install a quark from a nucleus, or an event from a time loop, or for that matter, how to de-install a thought from the stream of consciousness. Thus it has become essential to dredge up and articulate the principles we unconsciously rely on in this work of de-installing

things, and consciously learn to do it better, for otherwise we risk disastrous encounters with things we can't imagine and therefore can't see.

The naturalist, in contrast to the reductionist, sees things in place; he sees what and where they are in the context of the process as a whole. This involves seeing extensional separations and boundaries, but it also involves seeing how things *function* within the whole. . Actually, as we'll see, functional structure is best described in terms of the following three-place relation:

Separability. We say that *b separates A and C*, or that *A and C are separable at b*, if fixing *b* makes the uncertainty or indefiniteness as to what is the case with *A* independent of the uncertainty or indefiniteness as to what is the case with *C*.

The most familiar example of this notion is the separability of the future from the past by the present: If we know everything about the present, then getting more information about the past does not give us any more information about the future, and vice versa, or so we suppose. (Remember, we are now examining our presuppositions, not looking for objective truths). Note that this says nothing about the determinism of the future by the present - all it says is that the past and future, however else they may be related, only "communicate" with each other via the present. Which brings up another familiar example: the separability of two communicating parties by their line of communication. If *A* and *C* are talking by phone, tapping their phone line can tell you everything that's going on between them; whatever else is going on with the two of them at the time is going on with each of them independently.

Another term for separability is *conditional independence*. In the theory of stochastic processes the two terms have essentially the same meaning, though we'll keep them both to refer to slightly different mathematical formulations (see Chapter 2.)

Independence, unconditional and conditional: Events *A* and *C* are called *independent* if the probability of *A&C* is the probability of *A* times the probability of *C*. *A* and *C* are called *conditionally independent* given condition *B* if they are independent in the probability distribution conditioned by *B*, i.e. if $p(A\&C|B) = p(A|B)p(C|B)$. If for some random variable *x*, *A* and *C* are independent given condition $x=k$, where *k* is any value of *x*, then we say that *x separates A and C*. In the above telephone example, if we regard *b* as a random variable, then *b separates A and C*.

Since every probability distribution is conditioned by something, there is a sense in which all independence is conditional. The relation of independence, like every notion of being different or separate, is really a three-term relation: *A* and *C* are independent in a certain context *B*. We have just considered the case where that context is the result of placing a certain condition on a probability distribution. But the more familiar case is that in which the context is the result of *removing* a certain condition on a probability distribution. Notice that although there is only one way to impose a condition, there are many ways to remove a condition; this is one reason why there are so many different ways

to represent a given process as a system. The converse of "deconditioned" independence is:

Conditional dependence: We say that A and C are *conditionally dependent* given B if $p(A \& C | B)$ is unequal to $p(A | B)p(C | B)$.

A very important kind of conditional dependence is that in which the condition B is of the form $x=y$, where, without B , $A \& x$ is independent of $y \& C$. This is called *linking*, and we'll return to it shortly. There is a fundamental theorem, which will be proved in Chapter 3, that relates linking to separability:

Disconnection theorem. Process $A \& x \& C$ is separable at x if and only if there exists a process in which $A \& x$ is independent of $y \& C$ which reduces to $A \& x \& C$ under the condition $x=y$.

Speaking more generally, what makes separability so important is that it always coincides with the possibility of breaking a process into two de-installed components of some kind. We separate A and C by listening in on their phone line b , which is at the same time their connection and their common functional boundary, at least as far as their conversation goes. On the other hand we *disconnect* A and C , turning them into autonomous components, by *cutting* their phone line; this ends their conversation because their phone line is their separation boundary. When we mark the separation boundaries in a process, we may be functioning as naturalists, but we are also like the butcher with his blue pen marking the chops in a carcass.

To reconnect A and C so as to restore the whole process is to equate what is the case with b_1 and what is the case with b_2 , thus restoring the original situation of $A \& b \& C$. Just what it means to equate b_1 with b_2 turns out to be less obvious than it sounds, and in fact can be understood in two very different ways; we'll call these two ways *i-o connection* and *linking*. Understanding exactly how an *i-o connection* differs from a link is the key to understanding how quantum processes differ from classical processes such as phone conversations. Let's start with *i-o connection*.

We'll simplify slightly by supposing that A is talking to C on a one-way line b . If that line is cut, what happens to the signal voltage at the two cut ends b_1 and b_2 ?

The cut line b_1 from A is an *output*. This means, ideally, that what comes down the wire depends only on A , so the signal at b_1 is unchanged by the cut. The cut line b_2 from C is an *input*, however, and our description of the situation doesn't specify how inputs behave when they are disconnected. An open line will normally produce white noise, sometimes mixed with hum and faint radio signals. However, and this is a crucial point, the designer of a system usually doesn't have to take into account disconnected inputs; he only needs to think about how the system behaves when inputs are "driven" by outputs, whether from the user or from other components. To put it another way, the designer can

regard inputs as *free variables*; his components are *open processes*, analogous to open formulae in the predicate calculus.

Open processes are interconnected by equating inputs to outputs, which means *assigning* free variables to non-free or *bound* variables (assigning variables is a familiar concept for computer programmers.) A composition created in this way will be called an *i-o system*. If all the free variables of the components are assigned, we call the system *closed*; otherwise it's an *open* system.

We are on familiar ground here; this is how engineers and programmers are taught to think these days. Once again, here are the main ideas:

Input: A free variable in an open process.

I-o connection: The assignment of a free variable to a bound variable.

Output: The bound variable to which a free variable is assigned.

Disconnecting is just the reverse of connecting. More exactly:

I-o disconnection. A and C are disconnected at variable b means we are given two processes $A \& b_1$ and $b_2 \& C$ with b_2 free such that when b_2 is assigned to b_1 , the result is the original process $A \& b \& C$.

I-o system. A composition of connected open processes. Every i-o system represents a process, but we must be careful to distinguish this process from the system, just as we must be careful to distinguish a vector from its representation as a sum in a particular basis.

There's something not quite right about all this. After all, a free variable is only an abstract linguistic entity, whereas our cut wire, lying there on the ground and picking up white noise, remains very much a real material object. The parts of a telephone system are not "indefinite processes" - they are just as definite as houses and trees and stones. Or so says animal faith. There is in fact a way to describe the situation that's more in keeping with this bit of animal faith, which is to replace the concept of assigning a free variable by the concept of *linking* a so-called *white* variable.

White variable: A random variable on which there is a uniform probability distribution. In the case of a discrete variable, whiteness means that every value is equally probable, and hence the variable has a finite number of values, since their probabilities must add up to 1. A white real variable has a uniform probability density which must integrate to 1, so it's range is bounded.

Theorem: If x and y are independent random variables with the same range, and y is white, then the condition $x=y$ on their joint probability distribution does not change the distribution on x .

In our telephone example, let's now forget about free variables and think of the components $A \& b_1$ and $b_2 \& C$ as closed processes. Taken together, the two can be regarded as independent regions in the process $A \& b_1 \& b_2 \& C$. When we connect b_1 to b_2 , the result is a new process $A \& b \& C$. How does this new process differ from the old? The obvious answer is that it is just like the old one except for its behavior being restricted by the condition that b_1 equals b_2 . Connecting two wire ends means making their signals equal.

It is not immediately clear how this answer, which is couched in the physical language of broken wires, signal voltages etc., applies to processes as such. (Remember, a process, as we are now defining it, is simply a joint probability distribution on a set of variables.). However, the phrase "... restricted by the condition..." gives a strong hint. To say "the probability distribution D_1 is just like the probability distribution D_2 restricted by the condition C " means that you get D_2 from D_1 by conditioning all event probabilities in D_1 by event C . Thus our answer is basically this: To connect any two variables x and y , condition all of the probabilities of the disconnected system by the event $x=y$. This particular bit of animal faith brought to light will turn out to be the key to making sense out of quantum mechanics, so let's forthwith coin words that nail it down:

Links. Place a condition of the form $x=y$ on two variables x and y of a stochastic process, thereby creating a new process in which the unconditional probability $p(E)$ of any event E is $p(E|x=y)$ in the old process. This condition is called a link.

Link system: A stochastic process plus a set of links.

We'll go over all of this in considerably more detail in chapters 2 and 3. A summary of the relevant concepts, terminology and notation of probability theory will be found in Chapter 3, Section 1. Let's now briefly look at some high spots.

First, if x and y are independent and y is white, then the link between them behaves exactly like an i-o connection, with y acting like the input; this follows immediately from the above theorem. We'll show in Chapter 3 that we can neatly translate any i-o system into a link system which represents the same process and has essentially the same formal description. Engineers should have no problem adapting to link descriptions.

Second, and this is extremely important, we'll prove that this natural mapping of i-o connections to links does not go backward! Though every i-o system is equivalent to a link system, most link systems have no i-o equivalents, and even when one can make an i-o model of a link process, that model is usually exponentially more complicated than some link representation of the same process. Quantum systems are among those having no i-o counterparts, though quantum measurement systems do, as we'll see in Chapter 3, Section 7. If technology can learn to really deal with this larger class of non i-o systems, it will turn into a fundamentally new kind of enterprise.

Third, a crucial definition, that of a *link state*:

Link state: Given any link $x=y$, the *state* of that link is defined as the matrix representing the joint probability distribution on x and y without the link (actually, it's this matrix normalized by dividing by its trace, but that's a small detail.) This is the concept that does away with the quantum wave function, and hence with the problem of the collapse of the wave function and other related quantum nuisances.

In the introduction I briefly mentioned caloric. Heat, in the Eighteenth Century, was regarded as a fluid called caloric which was somehow related to the mechanical behavior of objects, but subject to its own non-mechanical laws. A series of experiments during the first half of the nineteenth century, starting with Count Rumford's crude qualitative observations, and ending with the definitive work of Joule and Helmholtz, convinced physicists that they didn't actually need to postulate a new substance called caloric, since heat is simply energy. When it was further realized that this energy is the random kinetic energy of molecular motion, thermodynamics became statistical mechanics.

History to some extent repeated itself in the early twentieth century with De Broglie's wave mechanics. His "matter waves" were at first thought to be waves in some new kind of fluid, the quantum analogue of caloric, but after Born discovered his quantum probability rule, they lost most of their substantiality, and physicists began to call them "probability waves".

Quantum mechanics, however, didn't go the way of thermodynamics. Because the so-called probability wave is really a wave of the square root of probability, "quantum caloric" didn't seem to be reducible to anything already known. And, alas, it is still with us, ignored by most working physicists, but hanging around the edge of physics in a kind of limbo where from time to time it is reworked and touted as a wonderful new discovery. But fortunately this unhappy state of affairs is almost over.

Here, in a nutshell, is how link states reduce "quantum caloric" to something already known, namely probability:

Suppose x and y are independent random variables. Then the probability that x and y will both have some value k is the product of the probabilities that they will have k separately, i.e. $p(x=k \text{ \& } y=k) = p(x=k)p(y=k)$. Let's call $p(x=k)$ and $p(y=k)$ the *unlinked* probabilities of x being k and y being k . Now suppose we impose the link condition $x=y$. The *linked* probability of $x=k$ is then the probability, conditioned by $x=y$, that both $x=k$ and $y=k$. The probability that x is k , as a function of k , is proportional to $p(x=k)p(y=k)$.

In short, linked probabilities are always quadratic in unlinked probabilities. If the distributions on x and y are identical, which is the quantum situation, then linked probabilities are the squares of unlinked probabilities. That's essentially all there is to it! Quantum amplitudes are not the intensities of some mysterious new fluid, but are simply

unlinked probabilities. One might ask "Probabilities of what?", but we'll see that we don't have to answer this question, just as we don't have to answer the question of what sets of things have numbers x and y in order to understand the meaning and truth of $xy = yx$.

A generalized form of quantum amplitudes will be found in any statistical situation whatsoever if we subject that situation to link analysis. Quantum amplitudes proper will show up if probabilities can go negative (we'll return to this in a minute) and we de-install components in such a way that the link states possess quantum symmetries; as we'll see in Chapter 3, this is always possible, though not always advisable.

Link states are quantum states represented as von Neumann density matrices. However, we can start in the more familiar way with quantum states as vectors over the amplitude field, and when we apply link analysis to the Schrodinger equation, amplitudes still turn out to be unlinked probabilities. Here, very roughly, is why this is so; the details are in Chapter 3.

In a continuous Markov chain, regarded as an i-o system, the probability distribution on the state variable can be represented as a vector which evolves in time according to the law $v' = T(v)$, where v is the distribution vector at time t , v' the vector at $t+dt$, and T the differential transition matrix. The Schrodinger equation, expressed in the language of Hilbert space, has exactly the same form, the difference being that the components of v are amplitudes rather than probabilities, and T is a unitary matrix rather than a transition matrix.

Let's now represent the Markov chain as a link system rather than an i-o system. The changing state vector is now the diagonal of a link matrix S that changes according to the law $S' = TST^{-1}$. If we think of S and S' as density operators, this is the von Neumann generalization of $v' = T(v)$. An immediate corollary is the Born probability rule in the generalized von Neumann form $\text{prob}(P) = \text{trace}(PS)$. The rule for turning an i-o system into a link system gives S the form $|v\rangle\langle w|$, where w is a "white" vector, i.e. it represents a uniform probability distribution, so it is only v that varies with time. The diagonal entries are quadratic in the entries of v and w , but since the constant entries of w are equal they factor out, so the probability vector is just v , the same probability vector that occurs in the i-o system.

Next, let's do the same with the quantum chain. Again we have $S' = TST^{-1}$, but now T is unitary. And again we have $S = |v\rangle\langle w|$, but now instead of w being white, we have $w=v$, which makes the diagonal entries into the squares of the entries in v , i.e. state probabilities are the squares of "amplitudes"! The amplitudes are no longer the values of some mysterious wave function but are simply the probabilities in the joint distribution on a pair of unlinked variables. Quantum caloric is nowhere in sight!

That takes care of the main obstacle to a statistical interpretation to quantum mechanics. The square law is no longer a reason not to regard probability waves as true waves of probability. It's true, the wave amplitudes aren't the probabilities that we

measure; rather, they are the probabilities that we *would* measure if the quantum process were disconnected at a separation point. But the same is true in a Markov chain, analyzed as a link process, the only difference being that in the Markov chain the output probabilities don't change when the output is linked to an input. In a quantum chain, "input" and "output" have identical distributions, and since unlinked state variables are independent, linking yields the unlinked probabilities squared.

There is still the problem of amplitudes going negative (they can also go imaginary, but as Mackey observed, the scalar i can be interpreted as a unitary symmetry in real quantum mechanics) [Mackey, 1963]. What could possibly be the meaning of negative probabilities? How can there be fewer than 0 things having a certain property P ? How can an event E occur less than 0 times?

These are not questions that can be answered by constructing some ingenious mechanism, since they involve logic itself. Earlier I compared negative probabilities to negative piles of gold, my point being that there is no problem in constructing a useful mathematics that incorporates them. But that doesn't tell us what they really are, and I now doubt very much that their deeper significance can be understood at all within the scope of science as we know it.

This could be enough to drive many people back to quantum caloric. But before you join the stampede, be aware that the logical mysteries in quantum mechanics don't come from the link state interpretation. They come from the bare fact of amplitude cancellation, and practically stare you in the face with EPR. How can it be that A is possible, and also that B is possible, but that $(A \text{ OR } B)$ is impossible? - that's the mystery of the two-slit experiment. Von Neumann was well aware of the logical strangeness of quantum phenomena, and tried to cope with it by means of a non-Boolean construction that he called quantum logic. This turned out to be a dead end, but other strange logics dating from the period of quantum logic show more promise. Enough of this for now, however. We'll return briefly to the logic of negative case-counting in Chapter 2, but it's too big a subject to do justice to here; suffice to say that the logical anomalies don't vitiate our simple mathematics.

So where do we stand with quantum mechanics? We've gotten rid of "matter waves" but the mystery of quantum interference remains, since to derive the Schrodinger equation in link theory does require negative probabilities, even in the process itself. But now, at least, the mystery of interference has become a *simple* mystery, and there is no longer any reason to think of it as belonging to physics in particular. Once one has really grasped how universal the two generalized quantum laws are at the case-counting level, it's as hard to take seriously the arcane "models" of quantum mechanics that abound today as it is to take seriously the ancient sheep farmer's "spirit of tranquillity" after one has understood that $xy=yx$.

But I'm afraid I'm getting somewhat ahead of my story. We started out to psychoanalyze animal faith, to dredge up some of the unconscious beliefs that underlie our

scientific thinking. "Wait a minute!", you protest, "I don't know about you, but I can't find any of this fancy mathematics in my unconscious!" Fair enough, I can't either. It's the analyst's job to propose, not to pronounce, and his proposals must always be confirmed by the analysand, who in this case is all of us. However, what we're looking for in our unconscious is certainly not fancy mathematics. Animal faith in itself, i.e. *in place*, can't even be put into words. To "find" it means to *articulate* something that does the same job, namely that of supporting our habitual modes of conscious thinking. In our current jargon, to articulate an item of animal faith means to *de-install* it, i.e. to transform it into a proposition that can stand alone. However, once articulated and understood, this new creation can then be put to the test by re-installing it in our intuition: Does it ring a bell? Do we think "Ah, yes, that's what I've always thought, even if perhaps I never quite said it that way."? If the items we have articulated pass this kind of test, then we can confidently move on to the interesting surprises.

Quantum mechanics is one such interesting surprise; another, which is the topic of Chapter 2, is a very Kantian answer to Hume's skepticism about the existence of causes. Kant's own answer was that we necessarily see causes everywhere because, as Bertrand Russell put it, we wear "causal-colored glasses" [Russell 1945]. By combining the philosopher von Wright's insight into how we *operationally* define causality with our analysis of process and system, we'll bring this statement up to date: We necessarily see *input-output systems* everywhere. Since input-output systems are closely related to computer models, it could be said that we necessarily see *computers* everywhere - we all wear computer-colored glasses!

If all this sounds like the grand march of progress, be aware that the two surprises above are not entirely harmonious. What makes our Kantian analysis of causality possible is the fact that the separability structure of a process only partly determines the form of the link matrix *S*, and we can show that it is always possible to choose a causal *S* (i.e., an *S* with an input). However, quantum *S*'s are *not* causal! Furthermore, and this is crucial, the choice of *S* for a given disconnection will, in general, affect the separability structure of the disconnected parts. Thus things that come apart neatly with one type of *S* may exhibit bad "non-localities" for another type of *S*. EPR comes apart very neatly with a quantum analysis, but any causal analysis compatible with the data will exhibit non-locality, as Bell's theorem shows. Thus, though we are always perfectly free to choose causal *S*'s and thus turn the universe into a giant computer, this computer may have a gosh-awful tangle of long distance wiring that could be eliminated by using other *S* forms.

Kant has had his say - now it's Freud's turn. Freud's project was not to justify our beliefs but to change our minds. He was, of course, working with people whose minds were very much in need of changing (which of course doesn't include you and me.) Still, he may be able to give us a few useful tips.

As long as our assumptions remain unconscious, we have no choice but to believe them, or rather, we have no choice but to act as if we believed them. The question of belief doesn't actually arise until we are able to contemplate and weigh alternatives. For this to

happen with some article of animal faith, we must first of all raise it up from the unconscious depths in such a way that it becomes a proposition, i.e. we must de-install it. Then one of two things may happen. We may take a clear look at this proposition and recognize its absurdity: "Wait a minute, my mother isn't really threatening to lock me in my crib if I try to go outdoors!", or whatnot. Or, we may see that the proposition is obviously true. If this happens, then another question arises: is this truth "objective", or does it depend on how you look at it?

How do we answer this last question? How do we find out whether something depends on how we look at it? One way is to see whether we can look at it differently. If we believe that all print is too fuzzy to read, we might try on glasses. Of course we may have to experiment to find the right glasses. Kant's error was to confuse our glasses with our eyes, so-to-speak.

The Kantian conclusion of Chapters 2 and 3 is that we all wear causal glasses, which turn out to be the same as computer-colored glasses. Does this mean that we are forever fated to see everything as a computer, or can we take these glasses off? As mentioned, "causal glasses" correspond to a particular kind of link matrix. We can work mathematically with very different kinds of link matrices, and indeed we must do so in order to practice quantum physics. Chapter 3 will make this very clear, and will point us in the direction of an expanded kind of science that works with the full range of acausal state matrices. But the question still remains: Can we, as human beings living our daily lives, ever take off our causal glasses?

For mathematicians, all things are possible. The experimenter who is willing to push hard enough may occasionally force nature to reveal its unnatural proclivities. But you and I, when we are just trying to get on with it, must constantly ask questions whose answers begin with "Because." That's our human nature. But what will be the effect on how we live and act if we can learn from the coming science how to go beyond "Why" and "Because"? What sort of beings might we someday become?

CHAPTER 2. HUME'S PROBLEM.

All modern discussions of causality begin with Hume. It was Hume who first clearly pointed out that merely knowing the regular succession of events does not tell us what causes what. The fact that B always follows A gives us no grounds for concluding that A "forces" B to happen. Why speak of causes at all, then? One school of thought says we should stop doing so; here is the young Bertrand Russell on the subject:

"... the reason why physics has ceased to look for causes is that, in fact, there are no such things. The law of causality, I believe, like much that passes muster among philosophers, is a relic of a bygone age, surviving, like the monarchy, only because it is

erroneously supposed to do no harm." [Published in *Proceedings of the Aristotelian Society*, 1912-1913, republished in *Mysticism and Logic*, Anchor 1957]

The mature Bertrand Russell, however, found out that like the rest of us he was unable to say very much outside of logic and pure mathematics without bringing in causes. Cause and effect pervades everyday life; the solution to Hume's problem can't be confined to the ivory tower of philosophy. A recent news item reported the discovery of a correlation in Mexico City between heavy chili pepper consumption and stomach cancer. For us chili pepper lovers, this was most unwelcome news: Do chilies cause cancer, or might there be some other explanation? What's at stake here is no mere philosophical abstraction; it's whether we have to stop eating chilies!

If there is no causal link between A and B, then, even if there is a high correlation between A and B, our wanting to bring about or prevent B is no reason to do anything about A. But if we learn that A causes B, or that it significantly increases the probability of B, then whatever power we may have over A becomes power over B too. The understanding of cause and effect is essential for practical life, indeed it is essential for our very survival, because it is the knowledge of means and ends.

This obvious fact of life has been curiously neglected by philosophers. The logician G. H. von Wright, however, was an exception; he actually went so far as to propose that the ends-means relation be used as the *defining criterion* of causality [*Causality and Determinism*, Columbia 1974]. That is, he would have us say that, by definition, A causes B if and only if A can be used as a means to B, and more generally, that there is a causal connection from A to B if and only if we can change B by changing A. Notice that this is an operational definition: it says that the presence or absence of a causal connection between A and B can be detected by performing a certain operation, which is to change A and notice whether there is a concomitant change in B. That is, of course, just how diet scientists would go about seeing whether chilies cause cancer.

Does von Wright's definition solve Hume's problem? It certainly gives causality an empirical anchor that cannot be neglected in any proposed solution. However, a very important question still remains: Is it possible to find formal patterns in the data itself that correspond to the causal connections revealed by the dispositional relation of means-to-ends? This is a difficult and subtle question, and the search for its answer ultimately takes us into territory where the question itself essentially disappears. First, though, there is a lot of work to be done closer to home.

The people who really live with causality are not philosophers but engineers, and curiously enough, they rarely use the word "cause". That's not because, as Russell said, "there is no such thing", but because they have developed a much richer and more discriminating vocabulary for the various causal structures and relationships they deal with. 'Variable', 'input', 'output', 'process', and 'function' are standard causal terms. Computer science has added some important newcomers such as, 'assignment', 'call', and 'subroutine'. Indeed, the concept of subroutine neatly revives the whole Aristotelian

foursome of causes: The material cause of a subroutine is the computer it runs in, the formal cause is its source code, the efficient cause is its call, and the final cause is the purpose it serves in the calling program.

It's clear from this brief survey of terms that, for the engineer, causality is an aspect of how something is composed, not of how it is extended. The engineer's job is to put together certain components into a system that realizes a causal process, i.e. a process having *inputs* that produce certain desired *outputs*. Furthermore, the components themselves are causal processes, and putting components together means using the outputs of certain components to causally control the inputs of others. As remarked in Chapter 1, the engineer does not normally concern himself with the behavior of unconnected inputs, and the behavior of input B connected to output A is simply the behavior of A in the absence of the connection, so B can be regarded as an indeterminate or *free* parameter. Thus, what the engineer designs, in our current jargon, is a *functional i-o system*; its components are what we called *open processes*. The functional dependence of outputs on inputs is called a *transfer function*. The concept of a transfer function clearly matches von Wright's concept of causality as the potential for the relationship of means to ends, so it makes sense to refer to functional i-o systems as *causal systems*.

In a causal system, so defined, the outputs of each component are functions of its inputs, and their composition by assignment of inputs to outputs is functional composition. However, we now know that, in the case of macroscopic components, the functional dependence of output on input is a large-number effect resting on the statistical behavior of atoms. A more accurate, and more general, account of the engineer's systems makes the output of a component into a *probabilistic* function of the input, with the causal transfer function replaced by a transition matrix of conditional probabilities; these are the i-o systems of Chapter 1. The composition of transfer "functions" is no longer functional composition but matrix multiplication, which reduces to functional composition in the special cases where the transition matrices contain only 0's and 1's. Having carefully noted the distinction between the mathematician's function and the engineer's transfer function, we'll now relax our language a bit and use the words "cause" and "causal" in connection with both. Thus we'll often refer to i-o systems as causal systems and to i-o states as causal states, especially in a context where we are distinguishing these from quantum systems and states, which are essentially a-causal. A more accurate qualifier would be 'statistico-causal', but it's too much of a mouthful.

Suppose an input is connected to an output - how do you know which is which? To find out you disconnect them; the output is the one that continues to do by itself what they did together when connected - another operational test of causality. But suppose you can't disconnect them, and suppose also that you can't manipulate the inputs to the system; all you can do is watch. Then how do you identify causality? Is there nothing we can learn about what causes what by just watching? Hume's problem is still with us.

Here's another take on Hume's problem: Are there patterns in the joint probability distribution of an observed process that mark the causal relations we would see if we were to experiment with its disconnected parts? To put it another way, do cause and effect belong to the process itself, or are they artifacts that result from taking the process apart?

The theory of link systems throws new light on this question. Recall the disconnection theorem, which says that a process can be disconnected or "de-linked" at x if and only if it is separable at x . This shows us a way to transform a process into a link system: First disconnect x into x_1 and x_2 , where x_2 is "white" (corresponding to an input), which produces two independent processes, call them A and B. Then link A and B by imposing the condition $x_1 = x_2$; this gives us a two-component link system exhibiting our original process. Now do the same for separating variables in A and B etc. until there are no separation points left. The result is a system which "factors" the original process into "prime" components that cannot be further subdivided. Recall that separability belongs to the process itself, i.e. it is intrinsic to the probability distribution. Thus the disconnection theorem reveals severe limitations on how we can construe the correlations in a process as causal. A highly correlated joint probability distribution need not have any separating variables at all, so causality requires something besides mere correlation in the data itself. We have at least a partial answer to Hume.

We'll see in Chapter 3 that the earmarks of causality in the data itself can actually be quite distinctive. Given the temporal order of the events in the process, it turns out that there is a unique prime factorization into components with white inputs. We learned in Chapter 1, and it will be proved in Chapter 3, that such systems are in natural 1-1 correspondence with i-o systems representing the same processes. Thus from a knowledge of the *given* alone, i.e. of the statistical behavior of the system without our interference, we can completely describe the i-o components we would get if we took the process apart at its (intrinsic) separation boundaries. The cause and effect relation, which we first located in the transfer functions and interconnections of the parts, is already *there* in the process itself!

OK, Mr. Hume, what do you say to that?

"Not so fast, sir; you were too glib with your prime factorization. Why should the x_2 's be white? And, if you regard what is *given* to our understanding to be only the *probability* of certain events, then on what basis do you bring in temporal order? If you must call upon such invisible help to reveal causality in the bare order of correlation, you do but reinforce my conviction that causes are phantoms of the imagination."

Good points. Let's translate them into the modern idiom. It will help to focus our ideas if we confine ourselves to processes in which the separating boundaries are all in a line; these are called *Markov processes*. Their definition can be stated in terms of a three-term relation among events called the *Markov property*, which is defined as follows:

Markov property: To say that the triple of events (A,B,C) have the Markov property means that they satisfy the equation $p(C|A\&B) = p(C|B)$, which we'll refer to as the *Markov equation*.

Markov process: A process in which the variables are indexed by a time parameter such that, at any time, if A is any event not involving future variables, and B is the state of the present, and C is any event not involving past variables, then A, B and C satisfy the Markov equation.

Recall that the probability $p(X|Y)$ of X conditioned by Y is defined as $p(X\&Y)/p(Y)$. Bearing this in mind, notice that if we multiply both sides of the Markov equation by $p(A\&B)/p(B)$ we get the equation $p(A\&C|B) = p(A|B)p(C|B)$. This relationship among A, B and C is known as *conditional independence*; in words, A is independent of C given B. Recall that in Chapter 1 we referred to separability as conditional independence; this equation defines what we were referring to. Event B separates events A and C means that, given B, the probability of A&C is the product of the probabilities of A and C. It turns out to be more convenient to define separability by a slightly different equation, which is gotten by multiplying both sides of the Markov equation by $p(A\&B)p(B)$.

Separability: To say that B separates A and C means that $p(A\&B\&C)p(B) = p(A\&B)p(B\&C)$.

If none of the joint probabilities of A, B and C are zero, then the Markov property, conditional independence, and separability are all logically equivalent. However, for conditional independence to make sense, $p(B)$ cannot be zero, while for the Markov equation to make sense $p(A\&B)$ must also not be zero. Since separability always makes sense, whatever the probabilities, it's the most convenient of the three to work with.. Such fine points needn't concern us now, however; the really important point is this:

Conditional independence and separability are symmetrical in A and C, which means that the defining property of a Markov process is symmetrical in time! Think about that for a minute.

The Markov equation seems to have a temporal arrow, since it's about conditioning of the future by the present and past, but we now see that this is an illusion. Our argument for causality being in the process itself rested on a theorem that assumes a given temporal order of events, and in the usual discussions of Markov processes, temporal order is taken for granted. But Hume's challenge was to find causality in the bare order of correlation. We now see that this bare order does not distinguish past from future, so it would seem that Mr. Hume is right: Cause and effect really are in the eye of the beholder.

Kant, who has been listening to this discussion with growing impatience, can restrain himself no longer. Since his style is notoriously obscure, I will freely translate his

remarks, with a little help from the eminent historian of philosophy Harald Hoffding [Hoffding, 1955].

"I gather that by 'process' you mean something that is accessible to experience. Now experience, in contrast to mere imagination, is a complex composed of elements some of which are due to the faculty of knowledge itself, while others are the result of the way in which this faculty is determined to activity from without. As I have shown, space and time are forms of our perception. For whatever the nature of our sensations, and however much they may change, the spatial and temporal relations in which their content is presented to us remain the same; a space or a time does not change, whatever be its filling out. It follows that the temporal order of the process is given, since it belongs to the *form* whereby the outer determinant of our experience of the process can manifest itself in experience *as a process*. Hume is correct that cause and effect are in the 'eye of the beholder', but wrong to imagine that we could see otherwise than as we do."

Kant's ambition was to describe once and for all the "forms" that belong to the "faculty of knowledge", leaving the "outer determinants" of experience in a limbo where they have become known as "things in themselves". Today we are more modest. For one thing, we no longer make his absolute distinction between the subjective and the objective; rather we see objectivity as a matter of degree. Also, instead of trying to directly analyze the faculty of knowledge, we often rest content with characterizing the contribution of the subject to an experience in terms of a "choice of viewpoint", and of the object as a particular feature of the experience that is invariant under the choice of viewpoint. The subjective-objective boundary is of course quite mobile, and depends on the range of possible viewpoints we are considering.

Even transposed into a modern setting, Kant's observations about time direction are telling. So far we have been taking the notion of process as simply *given*. But we must ask: given to what? If given merely to imagination, then of course processes can be anything we want them to be, and they can go backwards or forwards or any which way in time. But if we are empirical scientists, then processes are something given to *experience*, at least potentially, which means that in thinking about processes going backwards we must consider whether we could actually *observe* such a process. And here we must heartily agree with Kant that the answer is no, since experience itself has an inexorable temporal arrow, whatever its content. Indeed the process as something given to experience is given one piece at a time, and the order in which these pieces are given is precisely the temporal order that we find in the process itself. This being the case, what could it possibly mean that time goes backwards?

We've been describing the process itself as what is given, but here's something else to keep in mind: The time reversal of the *given* is the *taken away*!

Let's be more concrete. Suppose we are observing a process and writing down what we observe. Our observations accumulate in a diary which starts with 100 blank pages and progressively fills with writing. Now each letter that we write reduces the nominal

possibilities for what the diary *could* contain by a factor of 26; this is the Shannonian sense in which it constitutes information. Thus the process itself proceeds step-by-step alongside of a process of information gathering, which progressively reduces the vast initial range of possible states of the diary to one state, the chronicle of *what actually happened*.

We decided in Chapter 1 that it is not a particular chronology that we will call a process, but rather the conditions and rules that govern it. Let us then imagine an enormous series of repetitions governed by the same conditions and rules, each producing a diary of what actually happened. We now are given an enormous heap of diaries from whose statistics we abstract the joint probability distribution that we have been calling a stochastic process. The question is whether the stochastic process so abstracted exists as a thing-in-itself apart from its potentiality of "determining our faculty of knowledge to activity", namely, the activity that produces this heap of diaries.

The Kantian answer is no. The very *idea* of process is inseparable from the idea of a progressive accumulation of observed information in a diary. Such a progression has of course a time arrow, that of the progressive narrowing of a range of possibilities. Thus a Markov process backwards in time is nonsense. To be *given* a process without a time arrow is inconceivable, since the given-ness of a process is conceptually tied to the given-ness of successive stages of its presentation to a potential observer.

It took a lot of fancy footwork, but time and with it causality seem at last to have returned to their familiar shapes. Finally we are back safe in the bosom of animal faith. Or are we?

Kant says it's OK to believe what our animal faith tells us, because we can't help it - such beliefs belong to the very "faculty of knowledge". Darwin's theory of evolution also suggests that it's OK, but for a somewhat different reason, namely that our faculty of knowledge evolved to enable us to survive in an all-too-real world of hostile and dangerous "things-in-themselves". The two messages, despite their opposite philosophical starting points, are actually not all that far apart. Darwin's message applies an important corrective to Kant's, however, which is that in analyzing our faculty of knowledge we must take into account the special conditions that obtained in the environment where that faculty evolved. Thus, the belief that the world is flat is certainly "true enough" for creatures who never travel more than a few miles from where they are born, but is badly wrong for long-distance navigators.

Homo Sapiens is perhaps alone among the animals in being able to modify his animal beliefs in response to drastic changes in the environment, including those of his own making. Furthermore, and in this we humans are probably unique, our faculty of knowledge includes a faculty of abstract reasoning that can extrapolate to conditions far beyond our present environment. Sometimes this precedes a big move: think how we used every branch of modern science to prepare ourselves for exploring and colonizing outer space.

Kant lived in the Age of Enlightenment whose idols were Euclid and Newton, and rather naively incorporated their systems into his "intrinsic" faculty of knowledge, thus confusing a brief phase of human culture with something final and absolute. One wonders what he would have said about Einstein's theory of relativity. Kant thought it was impossible to experience space as non-Euclidean, and yet today we have good reason to believe that certain regions of space are quite non-Euclidean, and we know at least some of the ways in which this impinges on experience. The most drastic departures from Euclid involve warps not only in space but in time, producing loops where time closes back on itself. For the inhabitants of such a loop, if there be such, the future is also the past!

Needless to say, time loops pose a serious problem for our neat Kantian explanation of time-direction and causality. If the future is the past, which way does our experience go? How can there be a diary today about the future? In brief, *what happens?* If what happens is what could be experienced by someone involved in the happening, what would such experiences in a time loop be like? And what would it mean for experience itself to happen? Before getting further into this quagmire, let's briefly check in with the authorities to see whether time loops are anything more than wild science fiction.

We'll start with Stephen Hawking. Here is a recent item from the New York Sunday Times:

"October 1, 1995. In a U-turn that has sent shock waves through the universe, Professor Stephen Hawking, Britain's leading cosmic physicist, has accepted the possibility of time travel. Having ridiculed the concept for years, Hawking now says that it is not just a possibility but one on which the government should spend money."

Hawking gives a quick summary of the reasons for his U-turn in the preface to a new book called *The Physics of Star Trek* by astronomer Lawrence Krauss. First he addresses the problem of faster-than-light travel, a staple of space opera despite its being forbidden by special relativity, and concludes that "Fortunately, Einstein's general theory of relativity allows the possibility for a way around this difficulty." He then turns to time travel: "One of the consequences of rapid interstellar travel is that one could also travel backwards in time. Imagine the outcry about the waste of taxpayer's money if it were known that the National Science Foundation were supporting research on time travel. For that reason, scientists working in this field have to disguise their real interest by using technical terms like 'closed timelike curves' that are code for time travel. Nevertheless, today's science fiction is often tomorrow's science fact."

One of the scientists working in this field is the super-string theorist Michio Kaku. Here are two brief excerpts from his recent book "Hyperspace":

"In June 1988, three physicists (Kip Thorne and Michale Morris at the California Institute of Technology and Ulvi Yurtsever at the University of Michigan) made the first

serious proposal for a time machine. They convinced the editors of *Physical Review Letters*, one of the most distinguished publications in the world, that their work merited serious consideration."

"If time travel is possible, then the laws of causality crumble. In fact, all of history as we know it might collapse as well."

Whatever may be the prospects for a time machine, and I personally don't think they are very good, the conceptual problem posed by time loops in outer space is problem enough in itself. Even if we remain comfortably at home in our causal near-Euclidean space-time and never venture near a cosmic worm-hole, we still live in the same universe with such aberrations. In the larger picture, their time is of a piece with our own, and thus the question still remains: *what happens there?* Since this question can't be answered by a narrative, what kind of answer should we look for?

Here's another brief quote from Kaku. "However, the concentrations of matter-energy necessary to bend time backward are so vast that general relativity breaks down and quantum corrections begin to dominate over relativity." [ibid.] A complementary point is made in an article called "The Quantum Physics of Time Travel" by David Deutsch and Michael Lockwood in the *Scientific American*, March 1994: "Quantum mechanics may necessitate the presence of closed time-like curves. These, while hard to find on large scales, may well be plentiful at the submicroscopic scale, where the effects of quantum mechanics predominate."

These are very interesting points, since, even without time loops, quantum mechanics on the microscopic scale has always given a lot of trouble to those who ask "What happens there?". It seems that we are bracketed by temporal and causal weirdness from both the very small and the very large. The realm of causality and process, or at any rate due process, appears to be confined to a middle scale, like sentient life itself. But are we so perfectly balanced in this middle that the temporal weirdness at the extremes has no bearing at all on our situation?

Not so long ago we seemed to be bracketed by weirdness of another kind: Though pebbles are round and stars and planets are round, the Earth we live on is flat. Today we forget how incredibly hard it was for our ancestors to give up their flat Earth. The ancients managed to live precariously on a round Earth for almost nine centuries, but by the fourth century AD the Earth had become flat again, and flat even for educated people who knew their Aristotle, Ptolemy and Aristarchus. Curiously enough, it wasn't flattened by the barbarians invaders, but by learned scholars who were well grounded in Alexandrian mechanical technology which they used (or misused) to construct ingenious models of the cosmos as a planetarium [Munitz, 1965].

What drove our educated ancestors to such absurdities? Biblical faith is the usual suspect, but I think animal faith is the real culprit. Animal faith says that up is up and down is down, and never the twain shall meet. UP and DOWN are *Kantian-Darwinian*

categories, so deeply entrenched in our psyches, our language and our genes that they pervade all thought, as shown by expressions like getting high, feeling let down, being well grounded, etc. etc. The trouble with a round Earth is not that we can't imagine its shape but that we can't imagine a universe where unsupported things don't fall, and falling means *going down*.

The Aristotelian and Medieval solution to the problem of falling was to have everything fall towards the center of the universe, which was located in the center of the Earth. But after the Copernican revolution, the concept of falling once again became quite mysterious. Which way do things fall when they are well away from Earth, and why? It was Newton who solved this mystery by realizing that falling is not something that happens to objects in isolation but is a relationship between two or more objects. With this insight, falling was subsumed into another and very different concept, that of mutual attraction.

Past and future are even more ingrained in our genes, psyches, and language than up and down, so quantum superposition and black holes are even greater heresies for animal faith than a round Earth wandering around the Sun. Today it's not falling that is mysterious but *happening*. Does Newton's solution offer us any help in making it less so? Is it conceivable that we could subsume the concept of happening into some more general concept? Given the current astronomical evidence, this larger concept can't have an absolute temporal arrow anymore than Newton's concept could have an absolute arrow of up and down. The past and future have become poles in a relationship. But a relationship between what and what? And described how? We'll return to this in a moment.

We agreed with Kant that *happening* is an empirical concept. We then simplified the meaning of *empirical* into being able to keep a diary of observed events. Let's now concentrate on the concept of a *diary*.

To keep a diary, you start out with a stack of white paper and write down your experiences in the order in which they occur. As we noted above, each time you write something down, you narrow the set of possibilities for what could end up on the paper, i.e. you take away a subset of *cases*. Each item in your diary corresponds to a certain set of cases taken away, so the act of writing produces a sequence -C1, -C2, -C3, ... of such departing case sets.

This account of writing is incomplete, however, since it only shows cases departing, not cases arriving. Our description of your diary began after you had already acquired a stack of paper. And yet, properly speaking, your first act of diary keeping was to acquire that stack of paper, since it is blank paper that *supplies* the set of cases from which the members of C1, C2, C3 etc. are taken away when you write. Furthermore, this first act may well be repeated, since you may run out of paper before you are done. Thus a more complete description of your diary writing would go like this: +C1, -C2, -C3, +C4, -C5

etc., where a +C is a set of cases added to those at hand when you get a new sheet of paper, and a -C is a set of cases taken away when paper meets pencil.

Now let's address the question: what would it mean for a record of events to be reversed in time? What comes to mind is a movie run backward: spilled salt flying from the floor into its shaker, the Phoenix rising from his ashes etc. However, simply running the frames of a movie backwards does not reverse the crucial order of events that creates each frame, namely, light going from object to camera and then leaving its imprint on the blank film. A backwards movie is only a forward record read backwards; it's not the record of backwards events.

Let's approach our question on the more fundamental level of information and entropy, which is to say, on the level of departing and arriving cases. Once we take this course, its answer is immediate: Time reversal turns departing cases into arriving cases and vice versa. What goes backwards is not the order of movie frames or some other kind of "state descriptions" but rather the order of the case sets C1, C2, C3 etc., and this is accompanied by a reversal of their *signs*. Thus our diary above would go +C5, -C4, +C3, +C2, -C1.

Reversing past and future turns writing into paper and paper into writing!

Time reversal is indeed very simple, but it's also pretty weird. Here's another bit of weirdness. Seen forward in time, you take a piece of paper supplying 100 cases and write something on it that takes away 50 of them. Seen backward, you take a piece of paper supplying 50 cases and take away 100 of them! What is the meaning of the "remaining" minus 50 cases? It's certainly nothing familiar. However, this example shows that negative cases, and thus negative probabilities, can't be avoided in describing time-reversal, and if the astrophysicists are right about the circular warping of time, we will somehow have to make our peace with them.

In short, the generalized concept of *happening* we have been looking for, whatever else it may require, does require a generalized theory of probability in which we can countenance debts as well as surpluses of cases.

In Chapter 1 I mentioned that negative cases play havoc with logic. Let's very briefly see how this happens, since the mathematical "illogic" of negative cases is actually a useful tool.

Define the logic of a set of cases to be the set of all of its subsets. Consider the logic of a set of three cases. It has eight members, which can be arranged in a natural way as the vertices of a cube, called the *Boolean cube*. If we stand the Boolean cube on the vertex representing the null set 0, the top vertex becomes the universal set 1 containing all three members, i.e. the principle axis containing 0 and 1 is vertical. This same construction can be carried out with n cases to give an n-dimensional hypercube, which is also called a Boolean cube.

Ascending an edge of a Boolean cube (or hypercube) represents adding a case to the lower vertex, where parallel edges always add the same case. It follows that going down an edge represents taking away a case. Since writing a diary means successively adding and taking away cases, we can represent the course of writing by an edge path that starts with the vertex representing the initial case set.

Suppose now that we stand the Boolean cube upside down, putting 1 at the bottom and 0 at the top. Each step in the diary edge path then reverses its sign, so this transformation of the cube represents time-reversal. It also has a logical meaning, which is *negation*; more exactly, a self-reflection of the cube in the vertical direction maps each vertex into its negation.

Turning the cube upside down is only one of its many geometric symmetries [Etter, 1995a]. We can classify these according to how far they rotate the 0-1 arrow from its normal vertical orientation, the extreme being 180 degrees, as in negation. The intermediate angles represent partial time reversal. We can imagine the movable 0-1 arrow as a throttle that ranges from full speed ahead in time when it is pointing up to full speed astern when it is pointing down. If the throttle is exactly halfway between, the diary goes "sideways" in time, so-to-speak (this can only happen in a hypercube with an even number of dimensions.) Among such sideways transformations are square roots of negation, which we'll come back to in a minute.

It was our aim to come up with a concept of diary that is invariant under time reversal. The most natural interpretation of its invariant structure is a path on the Boolean cube, now understood as a purely geometric structure without an intrinsic up-down direction. This conception *relativizes* the logic of the diary, since the operations AND, OR etc. depend on the setting of the time throttle. Recall in Chapter 1 that we noted the weird logic of interference, where A is possible and B is possible but (A OR B) is impossible. Relativizing logic explains this puzzling situation: The particle that "interferes with itself" is in an eigenstate of an observable for which OR has a different meaning than it does in the frame of the classical apparatus.

Back to the square root of negation. When we base probability theory on Boolean geometry, this transformation gives us imaginary probabilities, just as negation gives us negative probabilities; link theory turns these into the imaginary and negative amplitudes of quantum mechanics.

Let me now go out on a limb and conjecture that the direction "sideways in time" is a direction in *space*. This could mean that the square root of negation is also behind the i of Minkowski space (see Chapter 3, Section 8 for more on this topic.) If so, it could be crucial for unifying our theories of space and matter.

Let me go even further out on that limb. First, I'll call on Pauli for a bit of support. Towards the end of his life Pauli had a mystical vision of the quantum i as the key to

unifying physics with psychology [Erkelens, 1991]. It turns out that complex quantum mechanics is mathematically equivalent to a real quantum mechanics (see Chapter 3, Section 6.) in which every object contains a particular two-state object, which has been called the Janus particle [Etter, 1995b], whose state is unmeasurable. My conjecture is that choosing between the two states of the Janus particle corresponds to choosing between the Heisenberg and Schrodinger representations. The fact that the absolute phase of amplitude is unobservable then means that quantum matter in itself (i.e. unmeasured) is not only symmetrical with respect to time direction but also with respect to *subject* and *object*. The differentiation of the world into particular pieces of matter, particular minds, and particular space warps can thus be understood as three different manifestations of the breaking of the primal symmetry represented by *i*.

As I said before, logic is really out of bounds for this paper. The last few paragraphs must have seemed pretty cryptic, but I wanted to give a bit of the flavor of things to come. We'll return to such speculations in the last section of Chapter 3, but until then our business will be with matters closer to home.

Time reversal does more than just change the sign of the events in our diary writing - it also reverses their order. Now it's reasonable to formalize the process of registering and recording information as an *i-o* system. As we saw, the laws of the underlying Markov process are symmetrical in time, which means that we could also represent it by an *i-o* system going backwards in time. But what sort of system would represent "sideways" time? Here we must turn from *i-o* systems to link systems. There turns out to be a whole spectrum of these ranging from full speed ahead to full speed astern, and it is those exactly half-way between that give quantum states (see Chapter 3, Section 6.) Note that the "state throttle" here is very different from the "phase throttle" that rotates the Boolean cube, and indeed it need not even involve negative probabilities.

I asked above whether *happening* might resemble *falling* in being a special case of a more general situation analogous to motion in a gravitational field. As we'll see, the mathematics of the state throttle supports this analogy. We should speak of *rising*, however, rather than falling, in the sense that events *arise* from the "ground" of past conditions. With the state throttle in full reverse, events arise backward in time from the ground of future conditions. In the more general case, events are "repelled" by both past and future, like helium balloons between two massive bodies. At half throttle, when the balloons are exactly balanced in between, we get quantum mechanics.

We've come a long way from our starting place, which was to respond to Hume's challenge to find causality in the "bare order of correlation". In effect, Hume's problem effectively disappeared, giving way to others more puzzling and serious. The problem with Hume's problem is that it assumes we are simply given a world where things happen. This is indeed true in everyday life, but when we start to think about what happens in time loops or atomic nuclei, we find that it's the concept of happening itself that is the problem. Causality, time direction, and history all seem to be bound up together in a way that makes it hard to say which is the more fundamental. This tightly

knit complex of ideas is a familiar one, and we have made some progress in analyzing it. But we have also discovered that it doesn't transfer very well to either the domain of the very large or of the very small. To make sense of modern physics seems to require that we give up not only causality and history but even logic. The next chapter will lay out in a more systematic way the kind of mathematical theory that must take their place.

CHAPTER 3. PROCESS AND SYSTEM: THE MATHEMATICS.

The main goal of this paper is to show that basic quantum mechanics can best be understood as a branch of the theory of probability. For this to happen, probability theory must be expanded in two ways: First and foremost, probabilities must be allowed to go negative. Second, and this is more a matter of technique than a change in our assumptions, the composition of transition matrices must be subsumed into a more general method of composition based on the linking of variables. We'll see that in this expanded theory, probability amplitude is not some new kind of "fluid" or physical field, but simply probability itself. By regarding it as such, we turn some of the most striking basic discoveries of modern physics into pure mathematics, thereby freeing them from their purely physical context to become tools available to every branch of science.. Just how much more of physics as we know it will follow in this course remains to be seen.

SECTION 1. Some Basic Concepts of Standard Probability Theory:

Event, sample space, case, variable, range, joint variable, logic, atom, probability measure, probability distribution, random variable, summation theorem, marginal, stochastic process, independence, conditional probability, conditional independence, separability, Markov property, Markov process.

Event. Following Feller we'll take *event* to be our basic undefined concept. We'll use the word in pretty much the ordinary sense. Like most English nouns, it can be either definite or indefinite. [Feller, 1950],

Sample space, case. A *sample space* is a set of mutually exclusive possibilities called *cases*. We say that an event *E* belongs to a sample space *S* if for every case of *S*, it makes sense to ask whether *E* is true or false. Another word for event is *proposition*. Examples: The event *Heads* belongs to the sample space whose cases are the possibilities Heads and Tails. The event *Seven* belongs to the sample space whose cases are the 36 possible throws of a pair of dice.

Variable. The term will be used in the scientist's sense, meaning something that can vary, rather than in the logician's sense of a place-holder for a term. A variable *x* is said to belong to a sample space *S* if for any value *k* of *x*, the event $x=k$ belongs to *S*. For example, let *S* be the set of all cases for ten coin tosses, and let *x* be the number of heads; then $x=1$, $x=2$, $x=3$... $x=10$. are events in *S* and *x* is a variable in *S*. More abstractly, a

variable is simply a function on S ; when we regard x in this way, the proper notation is $x(s)$. The set of all possible values of a variable is called its *range*.

Joint variable $x \& y$. Though variables are often numerical, we'll place no restrictions on what kind of things their values can be. For instance, a variable can range over all of the joint values of several other variables. The notation $x \& y$ will refer to a variable that ranges over all joint values of x and y .

There is an ambiguity in the term "joint value", which is whether or not it involves the order of things joined. Is $x \& y$ the same variable as $y \& x$? And is $(x \& y) \& z$ the same variable as $x \& (y \& z)$? Actually, there are two concepts of joining, which in everyday language we would render as "x and y" and "x, then y". The first and simplest, which is *unstructured* joining, is commutative and associative, so we can write $x \& y \& z \& \dots$ etc... without parentheses and with the variables in any order. The second, which is the one we'll use here, takes joint values to be *ordered* n-tuples; it has the decisive advantage over the first that variables of the same type are in natural 1-1 correspondence. There's actually a third concept called *list structured* joining that is very useful in constructing complicated link diagrams, but it belongs to a more advanced and specialized chapter of our theory.

Logic, 1 and 0, generating. Events can be combined with AND, OR and NOT, and a set of events closed under these operations will be called a *logic* (this use of the word 'logic' comes from von Neumann.) We'll abbreviate AND by $\&$ and NOT by $\sim$. A logic is of course a Boolean algebra. The *null* element of a logic, abbreviated 0, is the element $A \& \sim A$, which is the same for any A . The *universal* element, abbreviated 1, is the element $\sim 0 = (A \text{ OR } \sim A)$. Given any set S of events, the set of all events that result from applying AND, OR and NOT to the members of S is a logic, and it will be called the logic *generated* by S . Given a variable x , the logic generated by all events of the form $x=k$, where k is a value of x , is called the logic of x , and the members of that logic are called events *in* x . More generally, the logic of $x, y, z \dots$ is the logic generated by all events of the form $x=i, y=j, z=k$ etc., and the members of that logic are called events *in* x, y, z etc..

Atom. The atoms of a logic are the cases of that logic. To put it another way, an *atom* of a logic is defined as an event that cannot be further refined by any other event of the logic, i.e. A is an atom if, for any E in the logic, either $A \& E = A$ or $A \& E = 0$. Atoms are mutually exclusive possibilities, and the set of all atoms is exhaustive within the logic. In the logic of the variable x , the atoms are the events of the form $x=k$ for all k , while in the logic of $x, y, z \dots$ the atoms are the events of the form $\{x=i\} \& \{y=j\} \& \{z=k\} \dots$ for all $i, j, k \dots$. If the number of atoms is finite, then the subsets of atoms uniquely correspond to the events in the logic. The cases of the sample space are the atoms of its logic, so *atom* and *case* are basically synonymous terms. However, we'll generally reserve the word 'case' for atoms that we count, such as the atoms of the sample space.

Probability. Underlying all our more sophisticated ideas about probability is Pascal's classic definition of the probability $p(E)$ of an event E as the number of cases favorable to

E divided by the total number of cases. If the cases of a sample space can be regarded as equally probable, then this definition defines a so-called *probability measure* on the events of that sample space, satisfying the following three axioms:

- 1) It's additive: If E_1 contradicts E_2 then $p(E_1 \text{ OR } E_2) = p(E_1) + p(E_2)$
- 2) It's non-negative; for all E , $p(E) \geq 0$.
- 3) It's normal, i.e. $p(1) = 1$.

For now we are dealing with *classical* probabilities, which are never negative. For these we can always imagine an underlying sample space with equiprobable cases that define $p(E)$ according to Pascal's definition. In dealing with negative probabilities, the notion of an underlying case set must be generalized so that it can contain both positive and negative cases (The notion of negative set membership has been carefully investigated by mathematicians, and Blizard has shown that the Zermello-Fraenkel axioms can be generalized to allow for it [Blizard, 1990]). In this more general form, the case set has a very important role to play in link theory, since it enables us to distinguish among several quite different conceptions of independence; the details here will be found in future papers.

Probability distribution on x : A function defined on the range of x which for each k in that range has the value $p(x=k)$. The standard notation for this function is $p(x)$, which can be very confusing, since it conflates the scientist's and logician's two very different notions of variable. However, it's such a well-established tradition that we'll have to live with it; when you get confused, just remember to replace x by something that looks like a sentence.

Stochastic, or random variable: A variable on which is given a probability distribution. The qualifiers 'stochastic' and 'random' are only needed when a variable must be distinguished from variables of other kinds, like free variables. Since our results have nothing to do with randomness, 'stochastic' is the preferred qualifier.

Joint probability distribution on variables $x, y, z, \dots$ written $p(x, y, z, \dots)$ is defined as the probability distribution on the variable $x \& y \& z \dots$

Summation theorem and marginals: The summation theorem says that given a joint probability distribution on variables x and y , we can find the probability distribution on x alone by summing over all values of y ; i.e., $p(x=i) = \sum_j p((x=i) \& (y=j))$, or, in our abbreviated notation, $p(x) = (\sum y) p(x, y)$. This is a theorem we'll use repeatedly. A corollary is that a joint probability distribution uniquely defines the joint probability distribution on any subset of its variables; such partial joint probability distributions are called *marginals*.

Stochastic process. Define a *stochastic process*, or *process* for short, as a set of variables on which is given a joint probability distribution. In the usual definition it is also assumed that these *stochastic variables* involve events which can be ordered in time. This won't do for our present purposes, though, since, we need a new concept of process general enough to apply to time loops. We can start out by thinking of process variables as having at least approximate locations in the space-time of current physics. However, since we aim to eventually construct space-time out of probabilities, this is only a temporary expedient.

Independence. Events $E, F, G \dots$ are called independent if $p(E \& F \& G \dots) = p(E)p(F)p(G) \dots$. Note that for a set of events to be independent, it's not enough that every pair of events in that set be independent; independence is a joint condition of all the events. Two variables are called independent if events in the first are independent of events in the second. A set of variables are called independent if events in different variables are independent.

This definition pretty much captures our intuitive concept of independence as lack of correlation, although there is one thing about it that is not so intuitive: Suppose that either $p(E)=1$ or $p(E)=0$. Then for any F , $p(E \& F)$ is either $p(F)$ or 0; in either case it is $p(E)p(F)$. Such a "definite" E is independent of all events, including itself! This oddity will turn out to be important for the concept of probability space (see Section 8).

Given a finite sample space of equiprobable cases, there is another way to define the independence of E and F , which is that the sample space can be arranged as a rectangular set in which the cases of E are a vertical stripe and the cases of F are a horizontal stripe. For ordinary probability theory in which cases always count positively, the two definitions are equivalent. However, with both negative and positive cases, the two diverge, since $p(E)$ can be 0 even though its cases set is not "rectangular" with respect to that of F . It turns out that there is even a third definition of independence which applies to imaginary and complex probabilities (see Section 6) when these are defined in terms of real probabilities. The above product rule is the definition we'll adopt here, however.

Conditional probability. The conditional probability of E given F , written $p(E|F)$, is defined as $p(E \& F)/p(F)$.

Since a conditional probability results from dividing by $p(F)$, it is only well-defined if $p(F)$ is not 0. Negative probabilities are constantly producing 0's in weird places, so it's best to avoid conditional probabilities if there's an alternative; fortunately, there usually is.

Condition: An event C that is assumed to be true, thereby reducing the sample spaces to the set of those cases favorable to it. Axioms for a process are often given as conditions on a "free" process whose variables are independent. A very important kind of condition for our present work is the *link*, which is an event of the form $x=y$. The probability of E in the reduced sample space conditioned by C is $p(E|C)$ in the unconditioned sample space.

Conditional independence. Events E and F are called *conditionally independent* given C if they are independent in the sample space conditioned by C. The usual algebraic formulation of this relationship is $p(E \& F | C) = p(E | C)p(F | C)$. More generally, a set of events $E_1, E_2, E_3 \dots$ is conditionally independent given C if $p(E_1 \& E_2 \& E_3 \dots | C) = p(E_1 | C)p(E_2 | C)p(E_3 | C) \dots$

As we noted in Chapter 2, these equations becomes meaningless when the probability of the condition is 0. The following definition is equivalent to conditional independence when $p(B)$ is not 0, but also makes sense when it is 0:

Separability. Events A and C are called separable by event B if $p(A \& B \& C)p(B) = p(A \& B)p(B \& C)$. This follows from conditional independence if we multiply both sides by $p(B)^2$. The equation defining the separability of n events by B is derived by multiplying both sides of the equation for conditional independence by $p(B)^n$.

We say that variable y separates variables x and z if $p(x, y, z)p(y) = p(x, y)p(y, z)$, which is an elliptical way of saying that if for any values i, j, and k of variables x, y and z we have $p((x=i) \& (y=j) \& (z=k))p(y=j) = p((x=i) \& (y=j))p((y=j) \& (z=k))$.

Much of our work will be with Markov processes. Feller begins his chapter on Markov processes with the concept of Markov chain, which he defines as a series of trials in which the probabilities at each trial depend on the outcome of the previous trial, the rule of dependence being the same for all trials. He then goes on to say that a Markov chain is characterized by a matrix of conditional probabilities, called the *transition matrix*, together with an initial "state vector" giving the probability distribution at the initial trial.

There are problems with this definition. Part of what motivates it is the need to separate out the *law* of the process, given by the transition matrix, from the *boundary condition* on the process given by the initial state vector. And yet there is no reason why the initial vector should not contain 0's, for which conditional probabilities are meaningless. Thus the Markovian law of the process, which makes perfect intuitive sense whatever the initial state, cannot properly be described by a matrix of conditional probabilities. The so-called transition matrix must be defined in some other way.

The remedy for this and related confusions is one we have discussed at length in Chapters 1 and 2, which is to clearly distinguish between the process itself and its representation as a *system*. Feller began by describing a process and then switched over to describing it as a system, without noticing the change. Section 3 will be devoted to Markov systems, so here we'll define only the process. First a more general concept, which we've already met in Chapter 2:

Markov property: Three events A, B and C are said to have the Markov property if $p(C | A \& B) = p(C | B)$. Multiplying both sides by $p(A \& B)p(B)$, we see that this is simply separation, i.e. it says that B separates A from C

Markov process: A stochastic process whose variables can be arranged in a sequence such that each variable separates the variables before it from those after it.

Transition matrix: Abstractly, a matrix in which the sum of every column is 1 (just to confuse matters, such matrices are sometimes called *stochastic matrices*.) More concretely, the matrix of conditional probabilities from variable $x(t)$ to variable $x(t+1)$ in a Markov process.

SECTION 2. Processes.

Primary variables, secondary variables, join, global variable, independent parts.

Here are some useful technical terms:

Primary variables. In section 1, a process was defined as a set of random variables on which we are given a joint probability distribution; these variables will be called the *primary* variables of the process. We will assume that the primary variables are given in a certain linear order, which may or may not be related to the probability distribution.

Secondary variables. A secondary variable is a joint variable in a set of primary variables, called its *members*. We will assume that the members always occur in their natural order, so there is one and only one secondary variable for every subset of primary variables. We'll speak of secondary variables as also belonging to the process. Both primary and secondary variables will be denoted by small letters.

Variables. Unless otherwise stated, the word 'variable' will refer to either a primary or secondary variable of a process.

White variable: A variable on which the probability distribution is uniform.

Global variable. The secondary variable whose membership includes all primary variables. The process itself will be denoted by capitalizing the global variable, e.g., W is the process whose global variable is w .

Independent part, component. A sub-process U of W is called an independent part of W , or a *component* of W , if the primary variables of U are independent of all other primary variables of W . The primary variables of U are also called its *members*.

Prime. A process having no independent parts is called *prime*.

Prime factorization theorem. The set of prime components of a process is unique.

One often hears that the whole is more than the sum of its parts. What is generally meant is that the whole is more than the mere heap of its parts, which is true unless the

whole happens to be a heap of parts. A process is indeed the heap of its prime parts, which are in fact components, in the sense that the description of each does not require that we take into account the individual peculiarities of the others. However, we saw that one of the surprises of negative and imaginary probability theory is that there are several quite different definitions of independence, and thus the members of a heap share, as it were, the "nationality" of their heap, which involves the particular kind of independence that its members have from each other.

SECTION 3. Links and Cuts.

If you have been dozing through these definitions, that's OK - you can always go back to them. But now is the time to wake up. Pay attention! Here comes the key idea of the whole paper:

Link: Given a process W containing variables x and y , we *link* x to y in two steps.

Step 1: Apply the condition $x=y$ to W to obtain a new *conditioned* process W' with the same variables as W but with a new joint probability distribution W' in which the probability $p'(E)$ of an event E is $p(E|x=y)$ in W .

Step 2: Then modify W' by dropping the duplicate variable y .

We can think of a link as an operator on $L(W, x, y)$ on L , x and y that produces a new process W' , in which the probability of an event E is its probability in W conditioned by the event $x=y$, and in which the variable y is dropped. We'll write $W|(x=y)$ for this operation, i.e. $W' = W|(x=y)$; more generally, $W|LMN$ will mean applying links L , M and N to process W .

There is nothing in the definition of link that says x and y must be different variables; $W|(x=x)$ is a perfectly legitimate operator. What, then, does it do? It does nothing to the probabilities, since the condition $x=x$ is already in effect. It does, however, drop the second variable, namely x . To link a variable to itself simply means to drop it from W .

x and y can be either primary or secondary variables. If they are secondary, however, they must contain corresponding members that are also implicitly linked. Thus if $x = x_1 \& x_2$ then y must be of the form $y_1 \& y_2$, and the condition $x = y$ implies also that $x_1 = y_1$ and $x_2 = y_2$. This means that when y is dropped, we must also drop all of its primary members, and with them all other secondary variables containing any of these members, which are no longer needed, since they are duplicated by secondary variables with corresponding members in x . Dropping a variable is a bit more complicated than the name suggests.

The converse of linking could be called 'de-linking', but the term doesn't really fit, since it implies that there are two things to be de-linked, whereas in fact there is only one. To restore the original W , we must first turn x in two equal variables, and then remove the condition that equates the two. We'll call this operation:

Cutting. To *cut* the process W' at x means to produce a new process W containing a new variable y such that $W' = W|(x=y)$.

Unlike linking, cutting is not an operator, since there are an infinite number of ways to cut W' at x .

Equivalent cuts: Cuts on the same variable whose linking produces the same process.

SECTION 4. States.

Link state, density matrix, state of a variable, trace, propositions, projections, Born's rule, pure states, mixed states.

We now come to another key concept.

State of a link. Given a link $W|x=y$, the state of that link is defined as the joint probability distribution on x and y in W .

Equivalent states: States that result from equivalent cuts.

Density matrix: The joint probabilities on x and y arranged in a matrix with x as the horizontal index and y as the vertical. Following the quantum tradition, we'll use the terms 'state' and 'density matrix' interchangeably.

State of a variable x : The density matrix of the link $x=x$. Notice that the off-diagonal elements of this matrix are all 0, since it's impossible for its two indices to differ, and impossibility has probability 0! A more rigorous definition of the state of a variable is the diagonal matrix whose indices range over the range of that variable, and whose diagonal is the probability distribution on that variable. For instance, if x ranges from 1 to 3, then its state is the 3 by 3 matrix M_{ij} such that M_{11} is the probability that $x=1$, M_{22} the probability that $x=2$, M_{33} the probability that $x=3$, and all other entries are 0.

We'll find that the state of x encompasses all of the classical meanings of the word 'state', including Markov states and deterministic states like those of a computer. A Markov state is usually represented by a vector; this traditional vector can now be defined as the diagonal of the state matrix of the time-dependent random variable of the process (We'll see the details of this later.)

Trace. The trace of a matrix S , written $\text{tr}(S)$, is defined as the sum of its diagonal entries.

Important trace theorem: $\text{tr}(AB) = \text{tr}(BA)$. Important corollary: $\text{tr}(A) = \text{tr}(TAT^{-1})$, i.e. the trace of a matrix is invariant under all linear transformations.

Proposition: Literally, a synonym for event. (I am borrowing the term from von Neumann, who applied it to quantum variables.) There is a difference in connotation, though. Events are often described by noun phrases, whereas one always represents propositions by sentences. We can think of propositions as sentences in the predicate calculus whose free variables are assigned to process variables.

Predicate: A "propositional function" whose free variables are unassigned.. A predicate becomes a proposition when all of its variables are either assigned or quantified. A link, for instance, is a proposition resulting from the assignment of the equality predicate to a pair of variables.

Projection. In linear algebra, a projection P is defined as a linear operator satisfying the idempotent law $PP = P$. It can be shown that we can always find a basis in which such a P is represented by a diagonal bit matrix. For now we'll confine ourselves to diagonal projections, i.e. a *projection* will be defined as a matrix in which all off-diagonal elements are 0 and the diagonal elements are either 0 or 1.

Von Neumann showed that propositions about the measurement of a quantum observable x can be represented by projections that commute with the eigenvalues of that observable. This led him to an elegant generalization of the Born probability rule: If P is a projection representing a certain proposition, and S is a density matrix representing a quantum state, then, given S , the Born probability of that proposition is $\text{tr}(PS)$.

Representing predicates by projections. If $P(x)$ is a proposition about x , define the *characteristic function* of $P(x)$ to be the bit-valued function $B(x)$ which is 1 or 0 corresponding to whether $P(x)$ is true or false for x . The diagonal matrix P having $B(x)$ as its diagonal is the projection that we'll use to represent the proposition $P(x)$. We'll normally use the same letter P for both the predicate and the projection, and when the predicate is assigned, for the resulting proposition; it's usually clear from the context which of these things we mean.

If we multiply the projection P by the state matrix S of x , we clearly knock out all cases for which the predicate P is false, so the sum of the remaining entries in S is the probability of proposition P , i.e. $p(P) = \text{tr}(PS)$. This makes Born's probability rule start to look a bit less mysterious. However, what really demystifies Born's rule is that it holds not just for the state of a variable but for any link state. More exactly:

Generalized Born's rule. Let S be the link state of $x=y$ in process W , and let P be any proposition about x . Then the probability of P in the conditioned process $W|x=y$ is $\text{tr}(PS)/\text{tr}(S)$.

Proof. First note that the state S' of x in $W|x=y$ is S with all off-diagonal elements set to 0, normalized by dividing by $p(x=y)$. Since P is diagonal, PS is the result of multiplying the rows of S by corresponding diagonal elements of P . Thus the diagonal elements of S' are simply those of S divided by $p(x=y) = \text{tr}(S)$. Since we just saw that $p(P) = \text{tr}(PS')$, we conclude that $p(P) = \text{tr}(PS)$. QED.

Our result differs from von Neumann's by the factor $\text{tr}(S)$. Had we defined a link state as $S/\text{tr}(S)$, thus making $\text{tr}(S)$ always 1, we would have gotten von Neumann's result exactly. This was my original definition of state, and it works fine for classical and quantum states. However, we would like our definition of state to still make sense even as we push our theory into new domains where, because of the possible cancellation of plus and minus cases, the trace of S can be 0. For this reason, I believe that our present un-normalized state is preferable in the long run.

Pure states. Given a link $W|x=y$, if x and y are independent in W , the state of the link is called *pure*.

It follows directly from the definition of independence that the density matrix of a pure state has the form $|v\rangle\langle w|$ in Dirac notation. If w is white, the state will be called *causal*. On the other hand, if $w=v$, the state is quantum. Causal and quantum overlap in if w and v are both white, but in no other case.

Pure causal state. The state of a link between independent variables, of which the second is white.

Causal trace theorem. If S is pure causal, then $\text{tr}(S) = 1/n$, where n is the dimension of S .

Proof. To say that w is white means that for any i , $p(w=i) = 1/n$. Let S be the state of link $W|x=w$. Since S is pure, $p((x=i) \& (w=i)) = p(x=i)p(w=i) = (1/n)p(x=i)$. But $p((x=i) \& (w=i))$ is S_{ii} , the i 'th diagonal term of S . Thus $\text{tr}(S) = \sum_i S_{ii} = (1/n) \sum_i p(x=i) = 1/n$.

White connection theorem. Roughly: Pure causal links don't affect the component on the "output" side. More exactly, if x is in a component A that is independent of y , and y is white, then the link $W|x=y$ doesn't change the joint probability distribution on A .

Proof: Let z be the join of the variables of A other than x , i.e. $A = X \& Z$. Then $p(x,z,y) = p(x,z)p(y) = (1/n)p(x,z)$ in W . By the above trace theorem, $p(x=y) = 1/n$. Thus $p(x,z,y|x=y)$, which, ignoring y , is the joint probability distribution on x and z in $W|x=y$, is equal to $(1/n)p(x,z)/p(x=y) = p(x,z)$, which is the joint probability distribution on x and z in W . QED.

This theorem is the key to mapping the engineer's input-output systems into link theory. Remember, we asked the question: When an input is wired to an output, how do

you know which is which? The answer is: cut the wire and see which loose end is unaffected by the cut. This answer can be amplified a bit. If the wire is from box A to box B which is otherwise unconnected to A, then cutting it not only leaves the output of A unaffected, but all of A unaffected. This is essentially the content of the white connection theorem, if we regard the causal link as a connecting wire. The word "causal" means what it says!

Mixed states. Needless to say, pure states play a very important role in our theory. In quantum mechanics, impure states are called *mixed* states; this is because they can always be written as linear combinations of pure states. This turns out to be true quite generally of link states, so we'll borrow the quantum term *mixed state* for any state that is not pure. Let me point out, though, that the decomposition into pure states of a mixed classical state, such as one might find in a computer program, is generally quite artificial, since the pure components will seldom be classical and will often involve negative probabilities.

SECTION 5. Link Systems.

Disconnection, fundamental theorem of disconnection, link systems, proper systems, predicates as components, relational composition, abstract links and their states, contraction, product theorem, tensors, public variables, systems, transition matrices, causal boxes and systems.

We now come to a simple but profound connection between purity and separability which could be the key to unifying space and matter. First a definition:

Disconnection. To *disconnect* x from z at y means to cut their process at y in such a way that $x \& y$ becomes independent of $y' \& z$, where y' is the new variable produced by the cut.

Fundamental theorem of disconnection. Let W be the join of three variables x , y and z , i.e. $w = x \& y \& z$, where $p(y)$ is not 0 for any y . Then x can be disconnected from z at y if and only if y separates x and z . (Recall that separation means $p(x, y, z)p(y) = p(x, y)p(y, z)$.)

proof:

Suppose there exists a disconnection of W that produces a new process W' . Let y' be the new variable of W' , i.e. $W = W'|(y=y')$. To avoid confusion, let's use p for probabilities in W and p' for probabilities in W' . Then in W' we have $p'(x, y, y', z) = p'(x, y)p'(y', z)$. This implies that, in W' , x is independent of z for any values of y and y' . In particular, x is independent of z if $y=k$ and $y'=k$. Since this is true for any k , it is consistent with the linking condition $y=y'$, which is what turns W' into W . This shows that y separates x and z in W . Notice that going from disconnection to separation doesn't require the assumption that $p(y)$ is never 0.

Going backwards is a bit harder. Suppose y separates x and z in W . We need to construct a new W' containing x , y and z plus a new variable y' such that $x \& y$ is

independent of y' & z in W' , and such that $W = W'|(y=y')$. Separability in W means that $p(x,y,z)p(y) = p(x,y)p(y,z)$. Let W' be a process consisting of two independent parts $W1'$ and $W2'$, where $W1'$ contains x and y , and $W2'$ contains y' and z . Let $p'(x,y) = p(x,y)$. $p'(y',z)$ will be defined as a matrix that looks like the transition matrix $p(z|y)$ normalized by dividing by its sum. More exactly, if we substitute y for y' in $p'(y',z)$, we get $p'(y,z) = p(z|y)/n$, where n is the number of rows in the matrix $p(z|y)$. We must now show that under the condition $y'=y$, W' is W , which is to say, $p'(x,y,z) = p(x,y,z)$. First, we will use separability to separate out z in the joint probability distribution of W .

Lemma 1. $p(x,y,z) = p(x,y)p(z|y)$. Proof: Because $p(y)$ is never 0, we can freely speak of probabilities conditioned by y . Thus we can write $p(x,y,z)$ as $p(x,z|y)p(y)$. We can also write separability as conditional independence, i.e. $p(x,z|y) = p(x|y)p(z|y)$. Combining the two we get: $p(x,y,z) = p(x,y)p(y)p(z,y) = p(x,y)p(z|y)$. QED

Lemma 2. The joint probability distribution on x, y, y' and z in W' is $p(x,y)p(z|y')/n$. Proof: By the independence of $W1'$ and $W2'$, we have $p'(x,y,y',z) = p'(x,y)p'(y',z)$. Since $p'(x,y)$ was defined as $p(x,y)$ this becomes $p(x,y)p'(y',z) = p(x,y)p(z|y')/n$, where n is the number of rows in the transition matrix $p(y'|z)$. QED

Now let's link y and y' . This involves two steps: first we identify the variables y and y' , and then we normalize the resulting distribution by dividing by $p'(x=y)$. We see by lemma 2 that the first step turns the joint distribution on W' into $p(x,y)p(z|y)/n$. Now $p'(x=y)$ is the trace of the state matrix of the link, which we can easily see is $1/n$ (the steps showing this are spelled out in the proof of the causal state theorem in the next section.) Thus dividing by $p(y=y')$ yields $p(x,y)p(z|y)$, which by lemma 1 is $p(x,y,z)$. Thus $p'(x,y,z) = p(x,y,z)$.

QED.

The above disconnection theorem is what tells us how to mark a process for dissection, so-to-speak. First, we find a variable x , primary or secondary, which separates the process into two conditionally independent parts. We then disconnect the process at x into two unconditionally independent parts, and the repeat for these two parts etc. until we can repeat no more, at which point we have arrived at a prime factorization. These prime factors, as such, constitute a mere heap of independent parts. The disconnection procedure, however, leaves a record of past connections, a kind of wiring diagram, that can be used to reconstitute the original process.

The question arises whether such a prime factorization is unique. There are actually two questions here: first, whether the factors themselves are unique as processes, to which the answer is definitely no (except in the trivial case where the process has no factors except itself) and second, whether the prime "boundaries" are unique, or to put it another way, whether every factoring procedure ends up with the same sets of primary variables in its factors. The second question has no simple yes or no answer. Often it is

yes, but there are important cases (EPR, for instance) where a seemingly arbitrary choice in making the first cut will determine whether or not any further cuts are even possible.

Link system: A pair of processes W and W' together with a sequence of links on W that produce W' .

Proper link system. A link system in which all links are between different independent parts of W , and no two links involve the same variable.

Propriety theorem. In a proper system, the links commute, i.e. you can get from W to W' by applying the links in any order.

Proof. Divide the linked variables into two sets X and Y , where X contains the first variable of each link, and Y contains the second. Let x be the join of variables in X , y the join of variables in Y . Then the link $x=y$ clearly encompasses the links of all the members of X and Y , whose order is immaterial as long as they are properly matched.

The linking order can indeed make a difference if the linked pairs have variables in common. If you link y to z and then x to y , all three of x , y and z will be tied together and called x . However, if you first link x to y , y disappears, so you when then you try to link y to z , there is no process variable to link. This sort of thing cannot happen in a proper system, of course.

But suppose you need to link all three of x , y and z together? Indeed, this is exactly what an engineer does all the time when he "branches" an output to go to several inputs. Must the engineer then work with an improper system?

The answer is no. There are in fact several ways to branch properly. One rather interesting way is to extend link theory by treating predicates as "abstract" components which, when linked to random variables, become conditions on the process. Thus to connect x , y and z , we introduce the abstract component $x'=y'=z'$ plus the three proper links. The resulting more general theory merges stochastic composition with the predicate calculus, and may be of mathematical interest. However, much of what it accomplishes can be done in other ways; for instance, we can regard $x'=y'=z'$ as a stochastic process with three equal white variables, and the links $x=x'$, $y=y'$ and $z=z'$ also connect x , y and z . At any rate, abstract components won't concern us here. With one exception, that is; links themselves should definitely be regarded as abstract components. But they are the only ones we need for now.

Not having to worry about the order and grouping of links is such a plus that we'll assume that all our systems are proper. Under this assumption, there is a very readable and natural diagrammatic notation for link systems, which uses icons to represent component processes, and arrows between them going from x 's to y 's to represent links. Indeed, if we draw and label the icons right, we can often avoid variable names altogether, since the variables will be identified by their places.

Unfortunately, the practicalities of word processing, e-mail etc. make it expedient to have a linear backup notation. Boxes are written thus: $A[y_1, y_2, \dots; x_1, x_2, \dots]$, where the variables before the semicolon are the ingoing arrows, those after, the outgoing. A link is shown by an equation, so $A(y; x) (x=x') B(x'; z)$ links x in A to x' in B . If there is no need to reconstruct the particular lost variables in W , we can omit the equation and simply use the same letter in A and B , e.g. $A(y; x) B(x; z)$. Needless to say, diagrams are much easier to understand, and it's often best to turn this linear notation into actual diagrams rather than trying to decipher it as it is. There are more powerful diagrammatic notations than this for dealing with complex structures, but they aren't needed for the simple examples we'll be studying here. It's worth introducing one new concept from this extended notation, though:

Box: An icon that represents a component of a system, but that may also be a container, implicit or explicit, for a more detailed system. From the outside a box is to be regarded simply as a process, but it's permissible to look inside it for smaller components (think of the transistors inside an IC, or the electrons inside an atom.) In the next two sections I'll show all component icons as boxes, and sometimes refer to them as such.

Transformation: A two-variable sub-process T of a link system in which both variables are linked to others. If the variables of T are x' and y , in that order, and if $x=x'$ is the link to x and $y=y'$ is the link to y , then the link state (density matrix) of $x=x'$ is called the state *before* T , while the link state of $y=y'$ is called the state *after* T . T is called *causal* if x' is white.

Transformation product theorem. Roughly, to combine two successive transformations, multiply their matrices. More exactly, if T and U are transformations with variables x', y and y', z , then linking y and y' and then ignoring y produces the transformation nUT , where n is a normalizer.

Proof: Combine the summation theorem with the definitions of linking and matrix multiplication.

Corollary. Equivalent cut theorem. Suppose that cut $C1$ in W' produces the process $W1$ with variables x and y . Define $W2$ as the process that results from multiplying x by diagonal matrix D and y by D^{-1} . Then the cut $W', W2$ is equivalent to the cut $W', W1$.

Corollary. A pure state with no 0's in its diagonal is equivalent to a causal state.

Corollary. Born's rule gives the same probabilities for equivalent states.

SECTION 6. Markov Chains and Schrodinger's Equation.

The rule of law, boundary conditions, Markov chains, generators, time-reversed Markov chains, link chains, the link dynamical rule, differential Markov chains, , inverse Markov

chains, double boundary conditions, chains, sharp states, Schrodinger's equation, complex amplitudes, the phase particle, the Janus particle.

The ancient and Medieval worlds observed nature as carefully as we do, hoping as we do to discover what remains fixed in the midst of change. But science, as we know it, began when our focus shifted from changeless *things* to the changeless laws of *change itself*. The goal of "natural philosophy" then became to find those universal "laws of causality" that determine the future from the present. The amazing success of Newtonian mechanics convinced many people that this goal had actually been attained, and so went the scientific consensus for 200 years. But then came quantum mechanics.

What most shocked people about quantum mechanics was its seeming break with Newtonian determinism. Chance, seemingly exiled to the limbo of ancient superstition, had, like Napoleon returning from Elba, once again forced itself upon the civilized world. Could Chaos be far behind? The ideal goal of scientific theory had to retrench from predicting certainties to predicting probabilities. Our ultimate "law of causality" was an illusion; the best we could hope for was to find the ultimate rule giving the best *probability distribution* on the state variable $x(t+1)$ of the world at time $t+1$ as a function of the state variable $x(t)$ of the world at time t . Which brings us to *Markov chains*.

A Markov chain is a Markov process whose transition matrix is the same at all times t (see Section 1 for the definition of transition matrix.) Call this unchanging matrix G the *generator* of the process. Given G plus the state at time 0, we can calculate $p(x(1))$, $p(x(2))$.. etc., and also $p(x_1, x_2)$ and $p(x_2, x_3)$.. etc. It follows from the Markov property that we can calculate the whole joint distribution (this is a well-known theorem.)

A generator, in contrast to an initial state, is the *law* of a process. It's important to realize that not every Markov process has such a law. Given the Markov property alone, the matrix T of conditional probabilities between successive states can be an arbitrary function of time, in which case the distinction between law and initial condition, between "essence" and "accident", is artificial at best.

What happens to the generator if we reverse time? Except in very special cases, it does not exist. The transition matrices of a time-reversed Markov chain are almost always time-dependent. This makes the chain property a good indicator of time direction - score one more point against Hume! It's far from being a definitive indicator, though, since this reversed time-dependence is always lawful, and the reversed chain can always be embedded in a larger process with the chain property.

We have not yet mentioned systems; the concept of generator applies only to the process itself. However, it follows quickly from our results in Section 5 that we can always represent a Markov chain by a link system whose components, except for the first one, look and behave just like G . More exactly:

Causal link chain theorem. Given a Markov chain C with generator G and initial vector V , there is a link system whose first component is V and whose successive components all have the matrix $(1/n)G$, where n is the dimension of V etc., i.e. the number of values in the range of the state variables.

Proof:

Let x be the initial chain variable, and x', y be the variables (indices) of G . Then we must show that the joint distribution on x, y that results from assigning x' , the conditioning index in G , to x , is the same as that resulting from linking the joint distribution in x', y having the joint distribution matrix $(1/n)G$ to the independent variable x with the link $(x=x')$.

Recall that the sum of every column of a transition matrix is 1, so $(1/n)G$ is a joint distribution matrix in which the variable x' is white. Thus, by the white connection theorem, linking x and x' does not change the distribution on x , and so multiplies the i 'th column of $(1/n)G$ by $p(x)/p(x=y) = np(x)$. But this is just what you get when you multiply the initial vector by the transition matrix G .

QED.

Causal link chain: The link system derived from a Markov chain as above. When we don't need to carefully distinguish system from process, we'll usually call such a system a Markov chain too.

Causal transformation: A transformation T in which the probability distribution on the row index is white, i.e. all columns have the same sum. The transformations in a causal link chain are of course causal.

Theorem: The product TU of causal transformations T and U is causal.

Theorem: The link states of a causal link chain are of the form $|v\rangle\langle w|$, where w is white.

We have now mapped Markov chains onto link chains in such a way that their generators turn into causal transformations with the same matrices, except for a normalizing factor n . It was important to keep track of such *normalizers* in proving our basic theorems, but in the future it will make life easier to ignore them until it comes time to compute actual probabilities. This is almost always possible because, if we know any multiple of a probability distribution on x , we can divide by its sum to get the probabilities themselves. When it comes to dealing with negative probabilities, there may be no sensible normalizer at all. For instance, it can happen that even though all the case counts we need are well defined, the total case count is 0.

The passage of time in a Markov chain can be represented by a time-dependent transition matrix $T(t)$, where $T(t+1) = T(t)G$. By the product theorem in Section 5, this

same "transition law" (modulo a normalizer) is true in a causal link chain. The two concepts differ radically, however, when it comes to the meaning of the word 'state'. In a Markov chain, the states are what we have called the state vectors of the variables, which directly represent their probability distributions. These state vectors are indeed still well-defined in link theory, and have the same meaning. However, the word 'state' itself has been given a very new meaning, and now refers to the density matrix of a link. Since the state vector is the diagonal of the state matrix, probabilities are gotten by the rule $p(P) = \text{tr}(PS)$, as we saw in the last section.

In Markov chains, the law governing change of state, or as we'll call it, the *dynamical rule*, is $V' = T(V)$, where T is the time-dependent transition matrix, V the initial state, and V' the final state. Things are very different when we come to link states.

Link dynamical rule. Let T be a causal transformation. If the matrix of T has an inverse, then $S' = TST^{-1}$, where S is the state before T , S' the state after. If T does not have an inverse, then the most that can be said is that $TS' = ST$

Proof. Let x' and y be the variables of T , where the links to T are $x=x'$ and $y=y'$ for external variables x and y' . Now consider the join of the components other than T , and abstract from this the sub-process with variables x and y' . Cut the link $x=x'$. By the product theorem, the matrix S in the unlinked variables x and x' is UT . Similarly, the matrix S' in the unlinked variables y and y' is TU . We now have $S = UT$ and $S' = TU$. Thus $S'T = TUT = TS$. If T has an invertible matrix, then we can multiply on the right by T^{-1} giving the stronger rule $S' = TST^{-1}$.

The classical dynamical rule is the offspring of Newtonian determinism, and is based on a certain concept of explanation, which equates our understanding of a phenomenon with our ability to reliably predict it. With the return of chance, reliability becomes a matter of degree. The best laws are now those that give us the best estimates of probability. Nevertheless, the aim of classical explanation remains the same, namely reducing information about the future to information about the past. Thus the form of an explanatory law is that of a function that yields a probability distribution on the future as a function of a probability distribution on the present.

Notice that all this changes with the link dynamical rule. S' is only a function of S if T is invertible. Otherwise, the dynamical rule only specifies a certain relationship between future and past, in which the two enter symmetrically (note that if T is invertible, we can also write $S = TS'T^{-1}$.) With this shift come new questions about the very meaning of dynamical explanation. The traditional scientist asks *how* the past dominates the future, but never *whether*. But link dynamics reveals many other kinds of relational structure besides domination. We are beginning to see hints of how link theory could enable us to remove our "causal colored glasses".

Differential Markov chain: One whose generator makes a very small change in the state.

It greatly simplifies many calculations to write the generator of a differential Markov chain in the form $G = 1 + Hdt$, where 1 is the identity matrix. Ignoring second-order effects, we then have $G^2 = 1 + 2Hdt$ etc., so we can calculate $T(t)$ by integration rather than by repeated matrix multiplication. In differential Markov chains, it's customary to speak of H rather than G as the generator of the process. Let $\psi(t)$ be the state vector of the chain at t , and define $d\psi(t)$ as $\psi(t+dt) - \psi(t)$; then for any t we have $d\psi = G(\psi) - \psi = H(\psi)dt$. Many people have remarked on the resemblance between the equation $d\psi = H(\psi)dt$ and Schrodinger's equation. We'll now see that this resemblance is no accident.

First, note that $1 + Hdt$ is always invertable, its inverse being $1 - Hdt$ (we are ignoring second-order terms, so $(1 + Hdt)(1 - Hdt) = 1 - H^2dt^2 = 1$). Treating $1 - Hdt$ as a generator would give us our original Markov chain reversed in time!

But doesn't this clash with our earlier observation that the time-reversal of a Markov chain isn't a Markov chain? No, because closer inspection reveals that $1 - Hdt$ always contains negative "probabilities". Of course if we allow negative probabilities, then the answer is yes, and we have a useful tool for retrodicting state vectors. However, be careful here. Though the state evolution is reversed by the inverse process, the forward process itself, which includes the joint distribution on all variables, is not, since the inverse process always has negative joint probabilities.

Inverse Markov chain: A formal chain with a generator that is the inverse of a causal transformation.

If we encounter a time-reversed Markov chain, there are two things we can do in order to preserve the chain property: We can turn ourselves around in time, in effect putting the boundary condition on the future, or we can allow negative probabilities and work with the inverse chain, which means giving up studying the transitions and contenting ourselves with state vector dynamics. Suppose, though, that we encountered a chain C made up of two chains C_1 and C_2 , the one forward and the other reversed. Then it would seem that we only have the second option. However, link theory gives us a third, which is to put a separate condition on both the past and future of C , where the initial condition is white for C_1 , and the final condition is white for C_2 .

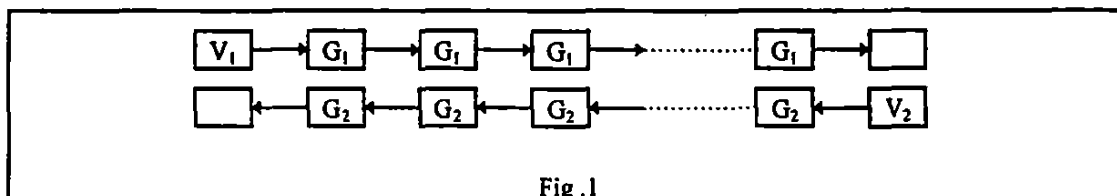
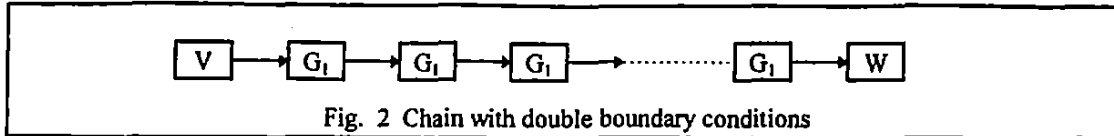


Fig. 1

The "generator" $G = G_1 \& G_2$ of C is of course not a causal transformation, so C is not a Markov chain. But G persists throughout the time of the process and thus should qualify as a law. What is essentially new is that in order to define the process as satisfying this law we must now be given *two* boundary conditions, one initial and one

final, and that without this final condition, the process has no law! This observation leads us to a basic generalization of the concept of Markov chain:

Chain: A link system given by a repeated transformation G called a generator, an initial component V and a final component W .



As we'll now see, quantum systems result from opening up our formalism to allow for both negative probabilities, and chains with double boundary conditions.

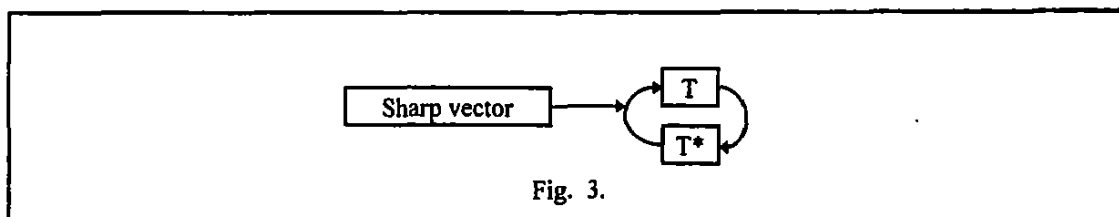
We've been focusing on transformations; let's now turn to states. As we saw above, the link states of a causal chain are of the form $|v\rangle\langle w|$, where w is white. We know from quantum physics that quantum density matrices are self-adjoint, which means that pure quantum states are of the form $|v\rangle\langle v|$. What kind of link chains have quantum states?

Suppose the initial link state of a quantum chain is $|v\rangle\langle v|$. By the dynamical rule, the second link state will then be $G|v\rangle\langle v|G^{-1}$. For the second state to be quantum, i.e. to be of the form $|v'\rangle\langle v'|$, we must have $\langle v|G^{-1} = (G|v\rangle)^* = \langle v|G^*$. (Recall that T^* is the adjoint of T , which for real matrices is just T with rows and columns reversed.) If this equality is to be true for any initial state vector $|v\rangle$, i.e. if we are to be able to separate the law of the process from its boundary conditions, then we must have $G^{-1} = G^*$. But a transformation whose inverse is its adjoint is a *unitary* transformation.

Thus we see that the generator of a quantum chain is unitary. One question remains; Given the initial vector $|v\rangle$, what must be the final state vector $\langle w|$? Let T be the product $GGGG\dots$ of all the G 's. Then $|w\rangle\langle w| = T|v\rangle\langle v|T^{-1}$, so $\langle w| = \langle v|T^{-1}$. We can write T^{-1} as $G^{-1}G^{-1}G^{-1}G^{-1}\dots$ which leads to a way of diagramming quantum chains that is very useful in analyzing quantum measurements, as we'll see in the next section. First an important definition:

Sharp vector, state. A vector is called *sharp* if it contains a single non-zero entry. A quantum state matrix S is also called *sharp* if it contains a single non-zero entry.

Since S is self-adjoint, its non-zero entry must be in its diagonal, which means it is a pure state whose vector components are also sharp. This leads to the following diagram of a quantum system which is prepared in a sharp state:



As with a differential Markov chain, we can write the equation of a quantum chain in the form $d\psi = H(\psi)dt$, where ψ is the state vector. But where is the i ? you ask. We still haven't arrived at physics yet; what we have been studying is a more general mathematical structure known as *real* quantum mechanics. As mentioned, Mackey showed how to get *complex* quantum mechanics as a special case of real quantum mechanics by introducing a (real) linear operator, which we'll call i , that commutes with every other operator. We'll take a slightly different route.

Instead of starting out with i as an operator, we'll start with a matrix representation of the complex numbers, where $a+ib$ is represented as a 2×2 matrix C_{ij} , with $C_{11} = C_{22} = a$, and $C_{12} = -C_{21} = b$. One can quickly verify that these matrices behave like complex numbers under addition and multiplication. A complex "probability measure" is then simply an additive measure on a Boolean algebra having these matrices as its values. Let x be a variable on which there is such a complex probability distribution $c(x)$. We can map this onto a real distribution on three variables x , i and j such that for any x , $p(i,j)$ is the i,j 'th entry of $c(x)$. With this mapping, a complex probability acquires a definite meaning in terms of real probabilities: it is a joint distribution on two binary variables i and j satisfying $p(i=1 \& j=1) = p(i=2 \& j=2)$ and $p(i=2 \& j=1) = -p(i=1 \& j=2)$. A complex variable x is then a process in x , i and j such that, for any k , $p(i,j|x=k)$ is a complex probability.

Suppose E and E' are independent events with complex probabilities c and c' . What, then, is the probability of $E \& E'$? We would of course like it to be cc' . However, if c and c' are independent real 2×2 matrices, i.e. independent joint distributions on i,j and i',j' , then combining E and E' produces a distribution $p(i,j,i',j') = p(i,j)p(i',j')$ which does not even make sense as a complex probability. This presents us with a choice: We must either give up trying to reduce complex probabilities to real probabilities, or else give up our old notion of independence, putting the complex product rule $c(E \& E') = cc'$ in its place. Since quantum mechanics uses this complex product rule in defining the inner product of two quantum objects, we'll take the second course, and ask what it means when translated into real probability theory.

To multiply c and c' means to *contract* j and i' , i.e., to link j to i' and then ignore j , or equivalently, since $cc' = c'c$, to contract i and j' . Thus to combine a complex process W with an independent complex process W' into a complex process $W \& W'$ we must always link a binary variable of one with a binary variable of the other. In quantum mechanics, an n -dimensional unitary transformation T can be thought of as operation on an object which is an entangled composite of an n -dimensional real object R and a two-dimensional real object J , where the entanglements satisfies the skew-symmetry rules above. The 2×2 pieces of T can be thought of as operators on J . Suppose T' operates on an independent $R' \& J'$. To bring T and T' into the same universe of discourse, we must link j and i' , thus multiplying the transformations on J times those on J' . But it only makes sense to multiply transformations if they operate on the same object, so we conclude that $J=J'$!

In other words, there is a particular two-state particle that belongs to every quantum object; this is what I referred to earlier as the Janus particle. That name was based on a conjecture relating its states to the equivalence of the Heisenberg and Schrodinger representations, so for present purposes let's give it another name:

Phase particle: The two-state particle that is common to all quantum objects when we interpret complex amplitudes as 2×2 real matrices.

As is well known, only the relative phase of quantum amplitude can be observed, the absolute phase being unobservable. This is equivalent to saying that the state of J is unobservable. Note that this means that "pure" quantum states, regarded as real states, are always mixtures with two-dimensional degeneracy. An interesting way to construct complex quantum mechanics is to start with real objects R_i , add J to each of them, and then confine ourselves to those states of $R_i \& J$ in which either J is independent of R_i (in the real sense), or is entangled in such a way that changing the state of J does not affect the probabilities in measurements of R_i .

Complex quantum mechanics: Real quantum mechanics where everything intersects in a single quantum bit whose state doesn't matter.

Janus particle: A hypothetical two-state particle belonging to all quantum objects, among whose symmetries are the reversal of the Heisenberg and Schrodinger representations. Let me here go on record with my conjecture that the Janus particle is the phase particle, and that resulting "subject-object symmetry" is the conjugation symmetry of quantum mechanics.

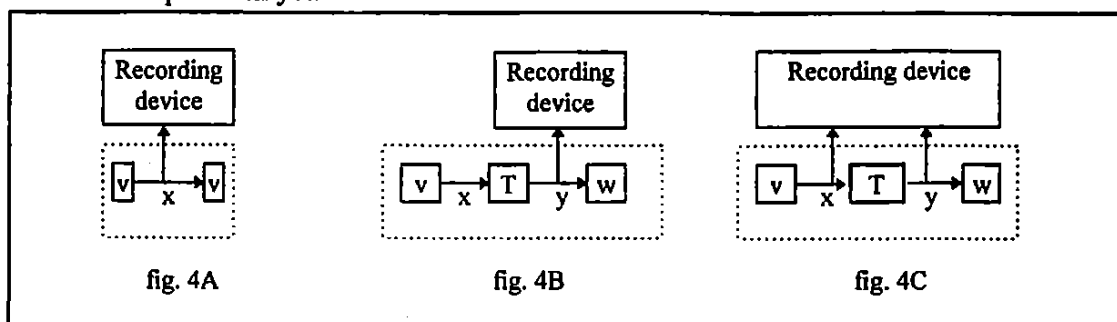
SECTION 7. The Quantum Measurement Problem and its Solution.

The measurement problem in link theory, the paradoxes of negative case counting, standard measurement theory, the projection postulate, the disturbance rule, the three axioms that define laboratory objects, functional objects, experiments, confined quantum objects, quantum time vs. laboratory time, prepared quantum chains, definite present and open future, the projection postulate in link theory, the collapse of the collapse of the wavefront, the confined quantum object is a laboratory object, the white state, time loops revisited, macroscopic matter.

The Born rule tells us that, for any proposition P and quantum variable x , the probability of a measurement of x satisfying P is $\text{tr}(PS)$, where S is the link state of x . The dynamical rule tells how the state S changes with the passage of time. So doesn't that wrap it up? What more is there to be said about quantum observation? In the standard Hilbert space approach, there is the problem of how to construe our "classical" measuring instruments as quantum objects, which led von Neumann to his famous construction of the movable boundary between object and observer. But this problem doesn't arise in link theory, since quantum and classical can perfectly well coexist in the

same joint distribution. Indeed, as we have seen, the very distinction between classical and quantum rests in part on arbitrary decisions about how to analyze a process. So what, then, is left of the infamous quantum measurement problem? Have we in fact solved it?

Not quite. In fact, the measurement problem takes a particularly acute form in link theory, as we'll now see. Fig. 4A shows a single complete measurement of a variable x in a pure state $|v\rangle\langle v|$; we simply draw an arrow from x to a recorder, linking x to an input variable of the recorder which is assumed to be white. So far so good; there's no measurement problem yet.



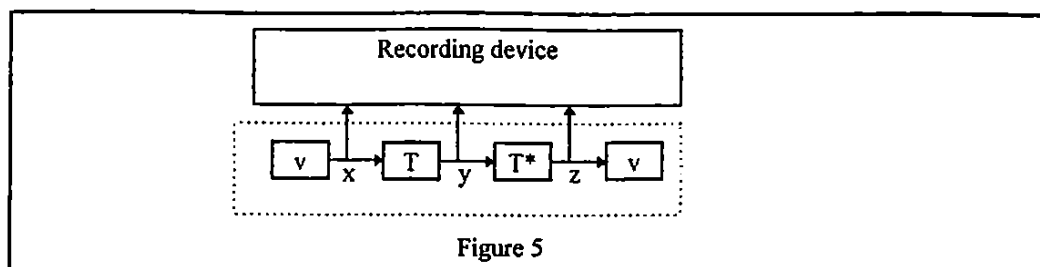
If we wait until time $t(y)$ to measure (fig. 4B), then the state will have been transformed into TST^* , and an arrow from y to the recorder will again give results in agreement with standard theory. Still no measurement problem.

But now let's consider two complete measurements, one before and one after transformation T (fig. 4C). Each of these measurements will give the same probabilities that it did when it was the sole measurement. However, that's not what the standard theory predicts. What went wrong?

Here's one thing that went wrong. Consider a *selection* from among all the records of just those records for which the first measurement had the value k . Within this subset, every event in x will have probability 1 or 0, so x is independent of all other events, and v , which we'll assume is not sharp, is replaced by a sharp vector v' . The result will be in most cases that the state of y is no longer quantum. In other words, measuring the object throws it out of a quantum state.

But we're in worse trouble than that. In the system of fig. 4C, it may well happen that some of the joint probabilities on x and y are negative, which means our recorder has recorded "negative" cases! So what's wrong with recording negative cases? you may ask. Remember, recorded cases are not just alternatives, they are *instances*; they are actual things, marks on a tape, whatnot. Suppose your closet contains ten positive white shirts and ten negative green shirts. If you reach into your closet for a shirt, you will come out empty handed, since there are 0 shirts there. On the other hand, if you reach in for a *white* shirt, you'll have ten to choose from. That's quantum logic.

To see the problem of repeated measurement more sharply, consider a third measurement on z which comes after a second transformation T^* (remember, T is unitary, so $T^* = T^{-1}$); this is shown in fig. 5.



As before, we'll run a long series of recorded experiments. Since $TT^* = 1$, x and z are perfectly correlated in this process. Since the recorder is white and therefore doesn't effect the probabilities of x , y and z , this perfect correlation will show up in the total set of records, i.e. x and z are equal in every record. But now consider the subset S_k of all records for which $y=k$. Since y separates x and z , we know that x and z are conditionally independent given y , which means that they are independent within S_k . Thus the records in S_k will in most cases show x and z as having different values! S_k is like your white shirts in the closet - it's a non-null subset of the null set. But real shirts and real records just don't behave like that. Obviously we've got to take another tack.

Up to now in this paper we have managed to avoid the logical anomalies that result from negative case counting by carefully steering away from them, the trick here being to think only about the arithmetic of ratios. But this won't work forever. Sooner or later we have to face the paradox of the white shirts. This is the paradox of the two-slit experiment seen straight on. With both slits open, the case counts cancel at the null in the "wave interference pattern" where the detector lies, and no electrons are detected. But with one slit closed, the "white shirt" electrons alone are selected, and they make it through. We'll see that the solution of the measurement problem involves transforming this seeming paradox into a mathematical account of what it means for the future to be *open*.

Since the predictions of the standard theory of quantum measurement are in close agreement with experiment, we must represent measurement by link systems that make these same predictions. To see just what is required, here is a brief summary of the standard theory, which is based on an assumption that von Neumann called the *projection postulate*.

Projection postulate. Given a state S and a proposition (projection) P , there is an ideal measurement M to test for the truth of P that leaves those things that pass the test in the state PSP/p , and those things that fail in the state $P'SP'/(1-p)$, where $p = \text{tr}(PS)$ and P' is the negation $1-P$ of P .

It follows directly that M leaves the object in the state $PSP + P'SP'$. Let Q be any observable with eigenvalues $q_1, q_2, \dots, q_n$, and let $P_1, P_2, \dots, P_n$ be the "projectors" of these eigenvalues, i.e., the (ortho) projections whose ranges are the eigenspaces of $q_1, q_2, \dots, q_n$. It is easy to show from the projection postulate that we can measure Q by performing a series of tests for, $P_1, P_2, \dots, P_n$. The object will only pass one of these tests, P_i , which tells us that Q has the value q_i . The ensemble of objects thus tested will be in the mixture $\sum_i P_i S P_i$, which gives us the general rule for the minimum "disturbance" of a state by a measurement.

Disturbance rule. Given an object in state S , the least disturbing measurement we can make of quantity Q is one which leaves the object in the state $\sum_i P_i S P_i$, where the P_i are the projectors of Q .

The disturbance rule gives us a way to prepare a beam of objects in any state S , which is to start with a beam in any state and measure it in a way that yields the components of S , and then select out a subset of objects from each component to give the proper weight of that component in S .

Practically speaking, the standard theory of measurement works very well, at least for simple systems. But the projection postulate has caused a lot of trouble for people concerned with quantum foundations, since, by "collapsing the wavefront", it messes up what is otherwise a beautifully clean and simple unitary dynamics. Since link theory has no wavefront to collapse, it ought to be able to do better, and indeed it does; in fact, I count its success in this regard as its major accomplishment to date. We'll see in a while what the projection postulate means in terms of linking. But first we need a description in our current language of the kinds of things that go on in a physics laboratory.

The first thing to be said about our idealized and simplified laboratory is that there is a clock on the wall. Someone, or something, is always watching that clock and logging every event, so that at the end of every experiment there is a diary of the experimental procedures and the outcomes of measurement.. The second thing to be said is that these experimental procedures can be repeated as often as necessary to obtain meaningful relative frequencies. Let's now characterize the *objects* in our lab.

Laboratory object. A process J in which we distinguish two kinds of *external* variables, called *input* and *output* variables. Each such variable exists for only one clock tick; for instance x could be the keyboard input to a computer at time t ; we'll write the time at which x occurs as $t\{x\}$. Input variables are those that we control, while output variables are those that we measure, or use to control inputs. J may have other variables besides its inputs and outputs, and it need not have inputs at all, though it must always have at least one output.

When we speak simply of *the input* to J , we'll mean all of its input variables together, or more exactly, the join of all these variables. Similarly for the *output* from J .

The input (output) *before* or *after* time t_1 is the join of input (output) variables x_i such that $t(x_i) < t_1$, or $t(x_i) > t_1$.

We'll assume that laboratory objects satisfy three axioms.

- 1) **Controllability.** The input is white.
- 2) **Causality.** Given the input before t , the output at or before t is independent of the input at or after t (no influences backwards in time.)
- 3) **Observability.** The output never has negative probabilities.

These three axioms broadly capture our common-sense notion of processes that we can observe and manipulate. Notice that they have nothing whatsoever to do with classical mechanics. Bohr characterized the laboratory as the place where Newtonian mechanics reigns, but this was a mistake. Newton's laws neither imply nor are implied by the above axioms; indeed they belong to a wholly different universe of discourse (that's one reason why the young Russell, who admired physics more than common sense, was so eager to jettison causality.)

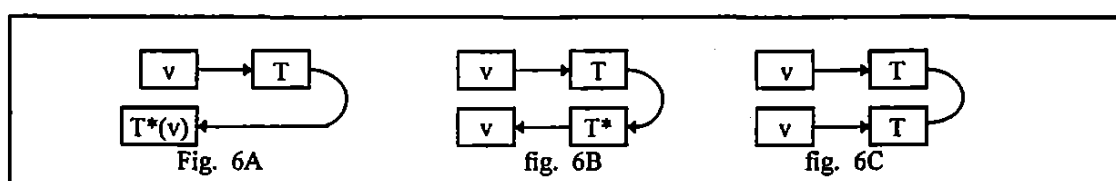
Functional object: One in which the output is a function of the input. The requirement of whiteness on the input imposes the condition on the joint distribution that all probabilities are either 0 or $1/n$, where n is dimension (total number of cases.) It's convenient to ignore normalization and represent such a joint distribution by the bit array which is the characteristic function of the object's function regarded as a relation.

Experiment: A link system whose components are laboratory objects, one of which is a functional object known as a *recorder*. The output of the recorder is a variable at the end of the process whose value is a record of all inputs and outputs to the object on which the experiment is performed. It is assumed that every input variable in an experiment is linked to a prior output variable, and from this it follows that the process represented by the experiment is itself a laboratory object.

Let's now turn to the construction of quantum objects. We'll show these as enclosed in a dotted box, as in the figures above. Our aim now is to design dotted boxes that enclose their quantum weirdness, so-to-speak., i.e., boxes whose in-going and outgoing arrows satisfy our definition of a laboratory object. Such confined quantum objects are, properly speaking, the true objects of scrutiny in a quantum physics lab.

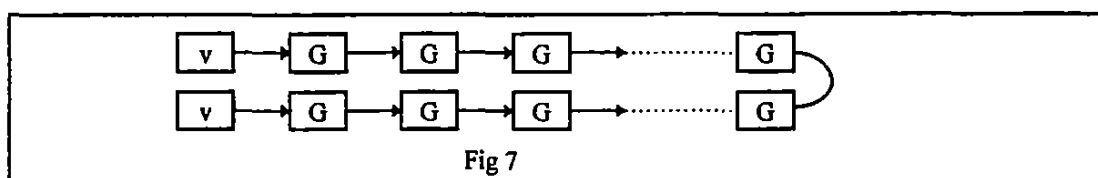
Notice that the dotted boxes in fig. 4 are confined quantum objects; they satisfies 1) and 2) because they have no inputs, and 3) because their probabilities satisfy Born's rule and are therefore non-negative. The dotted boxes in fig. 5 are not confined objects, however, since their outputs, i.e. the joint distributions on x , y and z , do have negative probabilities. Our challenge now is to redraw fig. 5 as a confined object that gives the right numbers.

There's one thing doesn't look right about box 5, which is that to prepare it in state $|v\rangle\langle v|$ requires placing a boundary condition both before time $t\{x\}$ and after time $t\{y\}$. If we think of these conditions as inputs from the lab, the second condition violates 2), the causality principle. This is assuming that the time sequences inside and outside the box are the same. But there is another possibility, and a more likely one, which is that "time" in the quantum domain is somewhat autonomous from laboratory time, or to put it more abstractly, that the chains of quantum separability that we compared to Markov chains are in themselves not temporal sequences at all. Under this assumption, it is reasonable to define a *prepared* quantum chain as one whose beginning and end are both at the initial laboratory time of the experiment (fig 6A).



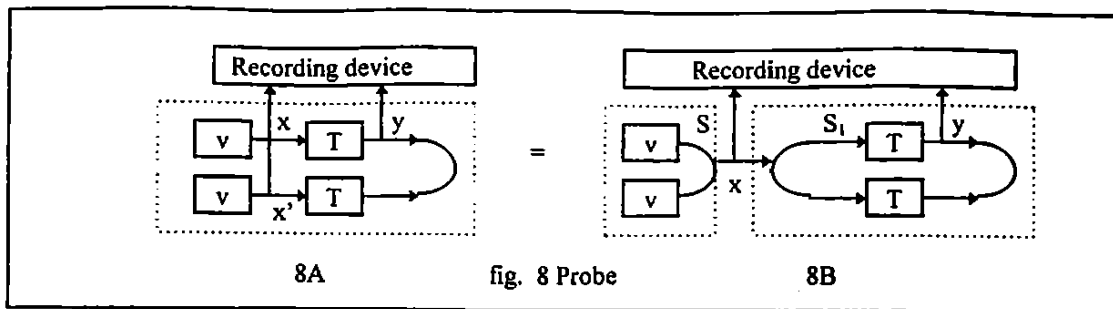
This construction looks more natural when we draw it with T^* factored out of w (fig. 6B). Reversing rows and columns of T^* turns 6B into the equivalent form 6C. Let's then take this as our picture of a chain prepared in a pure state; notice that the states S and S' are identical. More generally:

Prepared quantum chain: A parallel pair of identical unitary chains whose initial vectors are the same and whose final variables are linked. (fig. 7).



Suppose that when we measure x , the future measurement of y is indeterminate, in the sense that all outcomes are possible.. This indeed is our experience of time, which we express by saying that there does not exist at time $t\{x\}$ a record of the outcome of the measurement at $t\{y\}$. Thus it makes no sense to speak at $t\{x\}$ of the subset of records for which y has a certain value. But this implies that x and x' are linked at time $t\{x\}$ by the matrix $TT^* = 1$, which means that recording the value of x at $t\{x\}$ also records the value of x' at $t\{x\}$. Since we must sum over all values of y to evaluate probabilities at $t\{x\}$, this avoids the paradox of fig. 5.

Now what happens when we measure y ? In our earlier example, this measurement unlinked x and z , making them in fact independent.. However, in the present case, x' is already on record as having the same value as x , so it of course cannot become independent. How do we show this in our diagram? Simply by drawing a line between x and x' (fig. 8A). Recall that a recording device is a laboratory object having (white) inputs whose output at the final time is a 1-1 function of the history of its inputs.



Before y is measured, this line merely duplicates TT^* , so drawing it would be redundant. But after y has acquired a definite value, this line in effect remembers the measurement of x' , thus holding the fort against the disruption that would otherwise be produced by fixing y . Therefore there will be no records at all where x and x' are unequal, and the paradox of a non-null subset of the null set is avoided.

Choosing to measure x draws a line between x and x' as well as a line from x to the recorder. This is the link theory version of the projection postulate.

Probe x : Link x to x' and link x to the recorder (strictly speaking, this is a *complete* probe; we'll come to incomplete probes later.)

Projection postulate for a complete measurement. To make a complete measurement of the state of a prepared quantum chain at time $t\{x\}$, insert a link between x and x' and measure the linked variable x , i.e. *probe* x .

The choice of whether or not to probe x can be thought of as a double-pole switch. Just what do we mean by the term 'switch'? By a *single-pole switch* we mean a three-variable component $Sw[s,x,y]$ where s is a binary variable with values ON and OFF, $Sw[ON,x,y]$ is the identity matrix, and $Sw[OFF,x,y]$ is the white matrix in which every element equals $1/n$, n being the dimension of x and y . Notice that for unlinked x and y , s is white, since the sum of matrix elements in the ON condition is the same as that in the OFF condition. By a *double-pole switch* we mean a pair of single-pole switches whose s 's are linked.

The crucial question now arises whether this switch input satisfies axioms 1) and 2), i.e. whether it is white and causal. If so, then the dotted box qualifies as a laboratory object. To say that the switch is white means that the probability P of the process is unchanged by drawing the line. By the summation theorem, P is the sum of the joint distribution on x and x' . Drawing the line eliminates all off-diagonal terms in this sum. But since $TT^* = 1$, these off-diagonal terms are all 0 anyway, so the line doesn't affect P . This takes care of 1); we'll come to 2) shortly.

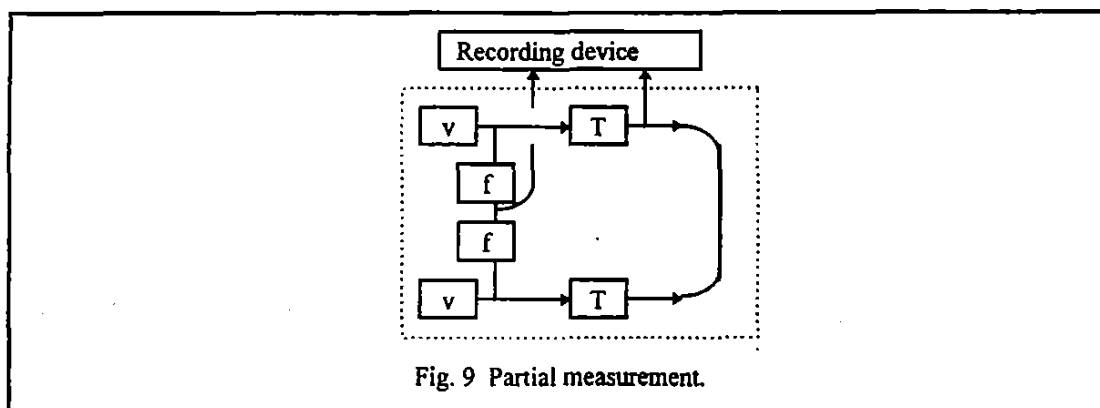
Looking at fig. 8B, we see that closing the switch breaks the diagram into two parts separated by x . The second part reduces to a circle, as far as x is concerned, so its presence at x is white. Thus the state S of the link from v to x is unchanged by the measurement of x ; it is still $|v\rangle\langle v|$, and the probability distribution on x is still the

diagonal of S , in accordance with Born's rule. But what is state S_1 ? Clearly it has the same diagonal as S . But, since $TT^* = 1$, its off-diagonal elements are all 0. S_1 is then the *mixed* state having sharp components weighted by the probability distribution on x . Thus we see that this change of state from S to S_1 satisfies the standard theory of measurement, and is indeed nothing else but our old friend the collapse of the wavefront! Of course there is now no longer a wavefront to collapse, so we'll have to find another name for it. Whatever we call it, though, it's the crucial event that links the micro to the macro, and it is brought about by the *passive* act of recording the state of x . Our probe does not disturb what it sees; its effect comes from seeing alone. (That seeing alone is sufficient to collapse the wave function was a point made by H. P. Noyes in the context of the double-slit experiment performed with low-velocity cannon balls, armor plate and mechanical detectors [Noyes 1980].)

Closing the "record" switch does not change the observed probability distribution on x , and in this sense it does not disturb the measurement. However, it does affect the state of y , which would be TST^* with the switch open but is TS_1T^* with the switch closed. From the point of view of the laboratory, closing the switch *causes* a change in the state of y . Inside the dotted box, though, there is nothing resembling cause and effect; indeed, nothing even *happens* inside the box, since closing the switch merely duplicates an existing link.

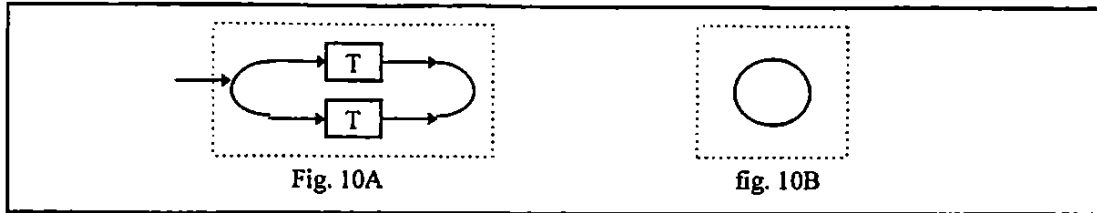
Measuring y is just like measuring x , the only difference being that S_1 is mixed (fig. 8B). Thus we see that throwing the switch at y doesn't affect the measurement of x , or the whiteness of the x switch, so the switch inputs satisfy 2), the causality axiom. This shows that the dotted box is a classical object, and the sequence of measured variables is in fact a Markov process.

So far we've only been considering complete measurements. An incomplete measurement of x is represented by a functional box $f(x)$ going from x to the recorder, where f is not 1-1. Recall that, in a complete measurement, connecting x to x' means that by recording x at $t\{x\}$ we have also recorded x' at $t\{x\}$. However, in the case of a partial measurement, it's not x but only $f(x)$ that gets recorded at $t\{x\}$, so this connection should go from $f(x)$ to $f'(x')$ rather than from x to x' (fig. 9).



This arrangement satisfies the standard general rule $S' = \sum_k P_k S P_k$ for the change of state due to a partial measurement, where the P_k are the projectors of the measured observable..

What about preparation? As remarked, a preparation is always equivalent to a selection based on a measurement. But we can also represent a preparation as an input to an *unprepared* system, i.e. a system in the white state (fig. 10A); clearly such an input is always white.



The white state, whose matrix is 1, has a unique role in quantum theory, since it is unchanged by measurement (i.e., if $S=1$, then $\sum_k P_k S P_k = \sum_k P_k = S$). The diagram of an unprepared and unmeasured quantum object is simply a circle (fig. 10B); we can think of it as representing pure entropy. Perhaps this is the only quantum diagram that makes sense in isolation from the classical world.

Our conclusion that the “collapse of the wavefront” requires an open future seems to reintroduce an absolute arrow of time. But this is an illusion, since an unprepared system is symmetrical with respect to past and future, whether it is measured or not. Time’s arrow comes not from quantum openness but from classical preparation, which we have conventionally placed at the beginning of the process, but which could just as well occur at the end, or at both the beginning and end, since it is equivalent to a selection of records. Thus our present analysis is consistent with our previous notion of the “state throttle” in a time loop.

The topic of measurement, especially partial measurement, deserves a much fuller treatment than I have given it here. However, even the present abbreviated account calls for a few more remarks concerning macroscopic matter:

In our analysis above, we have treated quantum and laboratory objects as essentially different kinds of things. And yet sooner or later we must face the fact that laboratory objects are made out of atoms, and try to render an account of just what this means. In standard quantum mechanics, when you assemble quantum objects you always get other quantum objects, and this indeed does create insoluble problems, since there’s no way to “bend” quantum-ness into anything else. The present approach doesn’t have that problem, of course, since it treats quantum-ness as a mode of analysis rather than an intrinsic property of things, and there is no reason why the appropriate mode of analysis should be the same at different scales. But we can hardly let things go at that, since there is a huge body of empirical knowledge about the relationship between micro and macro that needs to be translated into our present conceptual scheme.

In the last century, the laws of mechanics were thought to form a closed system that completely governs physical change, all other physical facts being relegated to the category of "accidents" of the world state. How, in such a scheme, is one to deal with the undoubtedly lawful character of thermodynamic events like heat flow? The nineteenth-century answer, which never quite satisfied anyone, was to impose a secondary layer of "laws among the accidents", so-to-speak, these being somehow based on the laws of probability. With the advent of quantum mechanics this idea becomes pretty far-fetched, since the equation governing quantum mechanical change has, except for the factor i , exactly the same form as the diffusion equation, which is one of the alleged laws among the accidents.

Let us hope that this classical two-layer concept of law is now well behind us. How, then, do we describe the relationship of micro to macro without it? Let's start at the level of process. Remember, in a process as such there are no states until you decide how to carve it up, only correlated variables, so it literally makes no sense to ask whether a process is quantum or causal. It does, however, make sense to ask whether or not its joint distribution contains negative probabilities.

Let's pretend for the moment that it doesn't. What we are then given is a huge number of micro-variables associated with individual atoms plus a much smaller number of macro variables associated with regions large enough to contain many atoms, the latter often closely corresponding to sums or averages of the micro variables. The macro-variables can be directly and repeatedly measured without disturbing them, and they seem to satisfy pretty well our axioms for laboratory objects, at least for simple inorganic objects.. We implicitly assume that spatial boundaries inside the object correspond to separation boundaries in our present sense - one sometimes speaks of this assumption as *non-locality*. Applying our fundamental theorem makes it possible to define states at these boundaries, thus turning the macro-behavior of the object into a composition. This is essentially how an scientist carves something up when he wants to make a computer model of it, and there is no doubt that it works very well.

The reason it works is that macro-behavior has a good deal of autonomy from what we assume about the behavior of the unmeasured micro-variables, and for most purposes we can ignore quantum effects completely, at least under average terrestrial conditions. It might be supposed that this is simply a consequence of locality plus the complexity of the processes the macro-variables are averaging over, and indeed it would be, if the micro-probabilities were always positive.

However, with negative micro-probabilities the situation is quite different. Consider a chain whose generator is periodic, i.e., whose n 'th power is the identity for some n . With only positive probabilities allowed, such a generator would have to be deterministic. But this is not true with negative probabilities, and physicists are well acquainted with periodic unitary transformations (indeed, any self-adjoint unitary transformation has period two.) Now a probabilistic chain of period n is of course separable at every variable, and would thus satisfy locality if it were strung through a material object.

However, its first and last variables are perfectly correlated, just as if they were directly linked! From the point of view of common sense, this is non-locality with a vengeance.

Clearly we need a new assumption that keeps such non-local "virtual links" from playing a significant role in macro-behavior. Physicists already have a name for what is probably the right assumption: *decoherence*. Since this concept is defined in terms of wave functions, it needs to be reformulated in the language of probability.. And it undoubtedly needs to be generalized to deal with matter at low temperatures, and perhaps with organic matter as well. Though all this could become technically complicated, it looks straightforward enough.

There is a much more serious problem which is not solved by decoherence, however, and that is to explain why negative probabilities remain confined to the micro-realm. What prevents them from "breaking out" into the everyday world and playing havoc with logic? We avoided this problem in the case of quantum measurements by assuming that making a measurement replaces certain "virtual links" due to quantum coherence by actual links. Something of the same sort must apply to the relationship between a macro-variable and the quantum average to which it corresponds. That is, a macro-variable, by being linked to the world at large, actualizes the virtual link on the quantum level between an average variable and its time-reversed dual. To put it more simply, macro-regions are *self-measuring*. Notice that measuring an average of many quantum micro-variables extracts only a minute fraction of the information in a pure joint state of these micro-variables, so these "macro-measurements" are essentially non-disturbing.

To summarize: If we assume decoherence plus the actuality of virtual quantum links from averages to their duals, macro-matter becomes classical.

But before we break out the champagne to celebrate the final triumph of logic and common sense, there are a few more questions. Why doesn't the time symmetry of the microscopic level carry over to the macro-level, at least to the extent of showing up as independent macroscopic boundary constraints on past and future? To what extent does our reasoning in terms of states bear on the process itself rather than on how we choose to carve it up? Might it be that hanging on to our causal framework, even for everyday objects, exacts a large price in complexity, thus blinding us to simple and essential forms in nature? And finally, why should we assume that we can always find ways to keep the irrationality of negative probability out of our experience?

SECTION 8. Things to Come.

This has been a long paper, but I fear it has only scratched the surface of its subject. I've concentrated here on the basics rather than on results. I believe this was the best

course, since these basics take some getting used to. But in the long run, it's results that win the prize. The history of science is littered with the remains of seductive theories that led nowhere, von Neumann's quantum logic being a recent case in point. The first place to look for results in link theory is obviously physics, about which I've said relatively little, so here, in conclusion, is a brief prospectus on several new items of physics that I see on the horizon.

First, some general remarks about space. So far, we've mostly been drawing diagrams rather than maps; in the dialogue between composition and extension, composition has had by far the louder voice. But the world doesn't present itself to raw experience as a neat arrangement of interchangeable parts - first we have to carve it up. And before we can carve it up, we have to map it, or "mark the carcass", as I put it earlier. The places where we mark are the places that separate the information in the extended structure, the boundaries through which correlation flows, and which, when rendered "immobile" by fixing the values of their variables, break the whole into independently variable parts. The question naturally arises as to how the structure of the probabilistic separating relation $p(A \& B \& C)p(B) = p(A \& B)p(B \& C)$ is related to more familiar geometric structures like metrics and topologies and linear orders.

Consider, for instance, a Markov process. We always start out by giving its variables in a certain order, but is this order contained in the probabilities or is it arbitrary? We saw that the arrow is arbitrary, so we are really talking about the between-ness relation. Let's define $B(x,y,z)$, read "y is between x and z", to mean $p(x,y,z)p(y) = p(x,y)p(y,z)$. We know that separation is equivalent to the Markov property, which holds for any x,y and z in the given order. Thus the question is whether $B(x,y,z)$ always excludes $B(y,x,z)$ and $B(x,z,y)$; let's call this the *order property*. If a process has the order property, then the linear order of its variables is indeed intrinsic to that process itself.

There are some obvious instances of Markov processes in which the order property fails. For instance, a sequence of independent variables is a Markov process, but since any one of the variables separates any two others, the variables can be rearranged in any order; intrinsically, they are a mere heap. At the opposite extreme is the case where the variables are perfectly correlated, i.e., where the transition matrices are permutation matrices. If you fix any variable in such a process, you fix all the others too. But, as we saw in Section 1, a fixed variable is independent of all others, so, again, any variable separates any other two. It may seem odd to think of Laplace's perfect Newtonian clockwork universe as a mere disordered heap of states, but there it is!

The true opposite of the independent variable case is the differential process given by a system where $G = 1 + Hdt$. If H is not 0 and there are no negative transition probabilities, such a process is always ordered. Hdt doesn't have to be truly infinitesimal to guarantee this, only rather small. If G is constant in time, the probabilities not only define an order but a metric, at least up to a scale constant. The local order of a continuous process is rather curious. Since it approaches the deterministic case in arbitrarily small neighborhoods, it is, so-to-speak, locally disordered.

That is, it would require arbitrarily many repetitions of the process to statistically determine the order of points that are arbitrarily close together; this is rather suggestive of the time-energy uncertainty principle.

The same kind of reasoning can be applied to higher-dimensional continuous processes, whose probability distributions in general contain the intrinsic geometry of the space that is used to define them, or at least its topology. Keith Bowden has suggested that if we apply suitable perturbing random fields to classical fields, we can recover their spaces from the resulting joint probability distributions. It would seem natural in a "probability space" for the local structure to be quite different from the global structure, which might have some bearing on super-string theory.

Now for something completely different..

What's the simplest random process of all? Surely it's a coin toss. Let V be a coin tossing process whose HEADS/TAILS variable is x , and define the *velocity* v of V as the probability of $x = \text{HEADS}$ minus the probability of $x = \text{TAILS}$ (we're not assuming it's a *fair* coin.) Notice that if you step one meter to the right or left every second according to the outcome of a toss of V , then v is your average velocity to the right in meters per second.

What's the simplest of all systems with more than one component? Surely it's a pair of linked coin tosses. Call these components V and V' , with binary variables x and x' and velocities v and v' . Prior to linking, V and V' are independent. Linking x and x' produces the single-variable binary process $U = V \& V' | x=x'$, which we can think of as a coin toss too. What, then, is the velocity u of U in terms of v and v' ?

Boost theorem. $u = (v+v')/(1+vv')$, i.e., taking the velocity of light be 1, the velocities of linked binary variables satisfy the relativistic addition law.

Proof: Let p and q be the probabilities of HEADS and TAILS for V , and similarly let p' and q' for V' . Then $v = p-q$ and $v' = p'-q'$, and from the definition of linking one can quickly verify that $u = (pp'-qq')/(pp'+qq')$. Thus we must show that $(pp'-qq')/(pp'+qq') = (p-q+p'-q')/(1-(p-q)(p'-q'))$. Now in fact these two expressions are not identical as they stand, but only become identical when we bring in the additional fact that probabilities add up to one, i.e. $p+q = p'+q' = 1$. The easiest way to take these conditions into account is to note that $v = (p-q)/(p+q)$ and $v' = (p'-q')/(p'+q')$ and substitute these expressions for v and v' in $(v+v')/(1+vv')$; the resulting expression then reduces to $(pp'-qq')/(pp'+qq')$. QED.

Applied to observer and object, the boost law implies the Lorenz transformation. Thus we find that the concept of linking, which before led us immediately to the heart of quantum mechanics, has now led us immediately to the heart of relativity!

There is still a lot of work to be done to relate the above theorem to the concept of "probability space" based on separability. One approach here may be to interpret "time lines" as binary Markov chains from which the LEFT-RIGHT variables are abstracted statistically. 1x1 space-time would then be the *indefinite* process that results from linking these velocity variables in an unspecified collection of such chains. Notice the formal resemblance here to our construction of complex amplitudes, which also resulted from linking an indefinite set of processes via a binary phase variable.

The question arises whether this resemblance is more than just an analogy. Could it be that at some fundamental level, the phase particle and the "velocity particle" are one and the same? Let's briefly consider where this would lead. Since in (complex) Minkowski space boosts are rotations of the complex plane, this identity would make the relativity of amplitude phase into a generalization of the relativity of motion. Even more important for the science of the future is that the conjugation symmetry of the phase particle would become the symmetry of v and $-v$, which is the symmetry that results from reversing object and observer. This supports my conjecture that the phase particle is indeed the Janus particle, and makes more sense of Pauli's mystical vision of i as the key to uniting physics with psychology, as described in Chapter 2. But of course we are now looking rather far ahead.

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AN INTRODUCTION TO SYNTHETIC FUNCTIONS

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First, a quite ordinary analytic function and its derivative are treated in a certain way to illustrate the approach. Then a particularly interesting function is described which, because it can only be treated in this way, I have called a synthetic function. Later an attempt at a formal definition of this concept is presented where the process is generalised.

KEYWORDS: Synthetic function, formal derivative.

1 INTRODUCTION

The synthetic method of generating a function and its derivative can be illustrated if we consider the trivial case of the real function $y = 2^x$ over the open interval $[0,1]$. We will call this function $g(x)$. Two properties of this function are:

$$\begin{aligned} \text{a)} \quad g(x/2) &= (g(x))^{1/2} & (\text{ie: } y^{1/2}) \\ \text{b)} \quad g(1-x) &= 2 / g(x) & (\text{ie: } 2/y) \end{aligned}$$

These two expressions can be used to generate the function $g(x)$ starting with a single known value of $g(x)$ for some specific x . Together they form a recursive definition of the function.

The use of the expressions: $(x/2)$ and $(1-x)$ generate values of the domain in the order:

$$1/2, 1/4, 3/4, 1/8, 3/8, 5/8, 7/8, 1/16, 3/16, \text{ etc.}$$

The corresponding values of $g(x)$ which are $g(1/2)$, $g(1/4)$, $g(3/4)$, etc, can in this particular case be generated from $g(1/2)$ by use of the right-hand halves of (a) and (b), ie: $y^{1/2}$ and $2/y$.

Since the above two formulae (a) and (b) are identities and the function $g(x)$ is analytic, we can differentiate them throughout to produce two more identities which in turn define the derivative $g'(x)$:

$$\begin{aligned} \text{a2)} \quad (1/2) \cdot g'(x/2) &= (1/2) \cdot (g(x))^{-1/2} \cdot g'(x) \\ \text{b2)} \quad -g'(1-x) &= -2 \cdot g'(x) / (g(x))^2 \end{aligned}$$

Rearranging gives:

$$a3) \quad g'(x/2) / g'(x) = (g(x))^{-1/2} \quad \text{and,}$$

$$b3) \quad g'(1-x) / g'(x) = 2 / (g(x))^2,$$

which is more convenient for generating values of the derivative $g'(x)$.

We are now in a position to consider a rather less trivial, and purely synthetic function.

2 A NON-ANALYTIC EXAMPLE

The example to be considered can be generated by the two expressions:

$$c) \quad f(x/2) = f(x) / (1 + f(x))$$

$$d) \quad f(1-x) = 1 / f(x)$$

To give an initial value of $f(x)$ we choose $x=1/2$ as before, and make the corresponding value $f(1/2)$ equal to 1. Using these two expressions we generate the arguments and their corresponding values, in the following order:

1/2	1/4	3/4	1/8	3/8	5/8	7/8	1/16	3/16	5/16	7/16	...	etc
1	1/2	2/1	1/3	2/3	3/2	3/1	1/4	2/5	3/5	3/4	...	etc

If we rearrange these in ascending order for values of the domain which have a denominator not greater than 16, with the limit points of the range included we get:

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	
	16	16	16	16	16	16	16	16	16	16	16	16	16	16	16	
0	1	1	2	1	3	2	3	1	4	3	5	2	5	3	4	1
1	4	3	5	2	5	3	4	1	3	2	3	1	2	1	1	0

The elements of the domain of this function can be represented in binary form and fall between zero and 1.

Referring back to (c) and (d) above, by differentiation we get:

$$c2) \quad (1/2) \cdot f'(x/2) = f'(x) / (1 + f(x))^2$$

$$d2) \quad -f'(1-x) = -f'(x) / (f(x))^2$$

Rearranging gives:

$$c3) \quad f'(x/2) / f'(x) = 2 / (1 + f(x))^2$$

$$d3) \quad f'(1-x) / f'(x) = 1 / (f(x))^2$$

Start at $x=1/2$ so that $f(x)=1$, and make $f'(1/2) = 1$

From this we first get: $f'(1/4) = 1/2$ and $f'(3/4) = 2$,
then: $f'(1/8) = 4/9$, $f'(3/8) = 4/9$, $f'(5/8) = 1$, $f'(7/8) = 4$

At the fifth level we get:

$$\begin{aligned} f'(1/32) &= 16/25 & f'(2/32) &= 1/2 & f'(3/32) &= 16/49 & f'(4/32) &= 4/9 \\ f'(5/32) &= 1/4 & f'(6/32) &= 8/25 & f'(7/32) &= 16/49 & f'(8/32) &= 1/2 \\ f'(9/32) &= 16/49 & f'(10/32) &= 8/25 & f'(11/32) &= 1/4 & f'(12/32) &= 4/9 \\ f'(13/32) &= 16/49 & f'(14/32) &= 1/2 & f'(15/32) &= 16/25 & f'(16/32) &= 1/1 \\ f'(17/32) &= 1/1 & f'(18/32) &= 8/9 & f'(19/32) &= 16/25 & f'(20/32) &= 1/1 \\ f'(21/32) &= 16/25 & f'(22/32) &= 8/9 & f'(23/32) &= 1/1 & f'(24/32) &= 2/1 \\ f'(25/32) &= 16/9 & f'(26/32) &= 2/1 & f'(27/32) &= 16/9 & f'(28/32) &= 4/1 \\ f'(29/32) &= 4/1 & f'(30/32) &= 8/1 & f'(31/32) &= 16/1 & f'(32/32) &= 1/0 \end{aligned}$$

3 TOWARDS A GENERAL DEFINITION OF SYNTHETIC FUNCTIONS.

Take two sets X and Y then a function F can be defined over X and have values in Y . That is X will be the domain of the function F and Y its range. To define a synthetic function we need both a method of generating the elements of the domain X , and a method of generating the corresponding elements of the range Y .

A way to achieve this suggested by the last section is to generate the domain starting from some fixed element by the use of two functions 'd' and 'D' which map the domain to itself. A corresponding pair of functions 'r' and 'R' is used to generate the range by also mapping that to itself.

It is important that any periodic points of these two mappings have the

same period. A periodic element 'p' of some mapping 'f' of a set S into itself of period 'n' is an element such that 'f' applied to 'p' repeatedly until it has been done 'n' times yields the value 'p'. This means $f(f(f(\dots p \dots))) = p$, where f() is done n times. The fixed points z of 'f' (elements with period of 1) are particularly important, ie, elements such that $f(z) = z$.

Let the function 'd' be such that $d(d(x)) = x$, that is, it is its own inverse. Similarly, let $r(r(y)) = y$ or 'r' is its own inverse too. Let the functions 'D' and 'R' have no periodic elements so that they be can be applied any number of times without returning to the original argument.

Now for our definition of F. F is to be such that:

$$1) F(D(x)) = R(F(x))$$

$$2) F(d(x)) = r(F(x))$$

Let z be a fixed element of 'd', ie: $d(z) = z$. Then $F(z)$ should be chosen such that it is a fixed element of 'r', ie, $r(F(z)) = F(z)$. Starting with 'z' it should be possible to generate the whole of the domain of F, ie X, and starting with $F(z)$ the whole range of F, ie Y should be generated. If the generation of Y does not create any element more than once then F is invertible. If we let $F(z) = s$, then the order of generation would be:

z, D(z), d(D(z)), D(D(z)), D(d(D(z))), d(D(D(z))), d(D(d(D(z))))), etc and:

s, R(s), r(R(s)), R(R(s)), R(r(R(s))), r(R(R(s))), r(R(r(R(s))))), etc

Differentiating 1) and 2) throughout, gives respectively:

$$3) D'(x).[F'(D(x))] = F'(x).[R'(F(x))]$$

$$4) d'(x).[F'(d(x))] = F'(x).[r'(F(x))]$$

Re-ordering gives:

$$5) F'(D(x)) / F'(x) = R'(F(x)) / D'(x)$$

$$6) F'(d(x)) / F'(x) = r'(F(x)) / d'(x)$$

Since this derivative is defined in an unusual way we call it a 'formal derivative'.

A starting value is needed in order to generate the derivatives $F'(x)$. The value we choose is the fixed point 'z' of 'd'.

Now, $r(F(z)) = r(s) = s$.
From (6) it follows that:

$$r'(F(z)) / d'(z) = F'(d(z)) / F'(z) = 1$$

so, $r'(s) / d'(z) = 1$ (since $F(z) = s$)

ie, the derivative of r at its fixed point 's' has the same value as the derivative of d at its fixed point 'z'.

The Method of Synthetic Functions enables exact definitions of non-analytic functions, including some very erratic cases such as those which are fractal in character.

Sign and Space

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Abstract

The theme of this paper is the relation between formal systems and our concepts of space. I start with examples like the quaternions where three dimensional spatial concepts and their formal structure are right on top of one another. This leads to discussion of the nature of quantum mechanics and to the role of networks - where the space and the formal structure are coincident. When I say that the space and the formal structure are coincident I mean that there is a functor producing geometry/topology from the network/process background while that same background can be seen as an essentially linguistic process. This leads to a discussion of self-reference/paradox and how the role of "quantum nets" (where one sums over products of internal tensor weights) are a natural extension to circularly interconnected nets avoiding the difficulties of recursion. These ideas, in turn lead downward to the fundamental consideration of the role of making a distinction in the creation of space or formal structure.

1 Introduction

Sign and space are not separate. Space, as we describe it, is made from signs. Signs must occur in some space. This essay is an exploration of the interlocking nature of these concepts.

The essay begins with a discussion of the role of continuity and set theory in the description of space. In the first section we contrast the points of view of ancient Greek geometry with present day mathematics. The modern appearance of symbols that represent completed infinities and are regarded in direct correspondence with these infinities is highly significant for our present models. The first section does not discuss fractal geometry, but it is this idea of space as self-referential, self-replicating that gives new geometry its peculiar action. The next section discusses the algebraisation of space with emphasis on Hamilton's

quaternions. The fourth section takes the point of view of graphs and networks, culminating in quantum networks equipped with states and amplitudes. Here space and formal structure are coincident in a new way with the space itself providing a garden of branching paths whose averages lead to geometry, topology and the precursors of physical observation. The last section returns to the concept of distinction where we began.

This essay is a sketch of a larger program of ideas and relationships. It is a descendant of my 1982 article [3] and it is intended to have descendants of its own.

2 Reflections on Space

We wish to study the relationship between the concept of space and the way properties of spaces cohere in sign and symbol. In the geometry of Euclid this correspondence is quite apparent. In that geometry there is no model of space apart from the relations that define it. We know from the Euclidean axioms that there are points and lines, that two points determine a line and three points determine a plane. We know that in a plane there is a unique line through a given point parallel to another line. But it is not given that space is an aggregate of points, or that a line is an ordered set of points. The Greeks of Euclid's time did not have our set theoretic ideas. The familiar language of diagrams for Euclidean geometry has a particular charm since it illustrates these relations with great accuracy. Each diagram is both a symbol for the geometric relations, and it is a model for them (when drawn with care). In this sense there is no fixed boundary between Euclidean geometry, the spaces that it describes and the symbols that are used to depict these spatial relations.

As a sign/symbol a line is an integral form. It is the line that divides a plane. It is the form that lies evenly

upon itself (Euclid's description). It is the abstraction of our act of dropping a plumbline or tracing the shape of a ruler. In our modern point of view the Euclidean line consists in an aggregate of points. It is not simply determined by these points; it *is* these points. We continue to draw a line and use the symbology even after it has become the symbol not for just a geometric entity but for a completed infinity.

The modern point of view is powerful and alarming to the degree in which it skirts paradox and inscrutability. Consider the question of how many points must be on that line. We agree on the concept of a continuous geometrical line of infinite length, or of a continuous line segment of definite finite length. We further concur in the idea that if that line is covered by a collection of intervals of finite length then the length of the line is necessarily less than or equal to the sum of the lengths of the intervals in the cover. (A covering by intervals means that a certain collection of intervals has the property that every point on the line is in one of the intervals in the collection.) With these agreements, let us suppose that the number of points on the line is countable. That is, let us suppose that there is a list of the points that is in one-to-one correspondence with the positive integers 1, 2, 3, ... Let the points be indicated by $p_1, p_2, p_3, \dots$. Then we can assign an interval around each point p_n of length $\epsilon 2^{-n}$. The sum of the lengths of these intervals is equal to ϵ . This contradicts our assumption of the infinite length of the line and of the specific finite length of a segment. The inevitable conclusion is that the number of points on the Euclidean line (if it be an aggregate of points) is uncountable.

In this argument for the uncountability of the continuum there is no specification of the actual point set that constitutes the line. Consequently, we have no way to deduce a cardinality other than knowing that it is a cardinality higher than $\aleph_0$, the first infinite cardinal. It is common to model the continuum with the standard real numbers, the Cauchy sequences of rational numbers. These reals are proved to be uncountable by the famous diagonal argument of Cantor (of which we will present a generalization shortly). Standard reals have a definite cardinality. It is the same as the cardinality of the set of subsets of the integers and it is often denoted by $2^{\aleph_0}$ since the set of subsets of a set X is in one-to-one correspondence with the functions from X to any set with 2 elements. Is there a cardinality between $\aleph_0$ and

$2^{\aleph_0}$? This is Cantor's Continuum problem, eventually shown [1] to be unsolvable from the standard

axioms for set theory. Here we reach inscrutability.

It is in the uncountability of the continuum itself that we skirt so close to paradox. Nothing shows this better than the most general form of the Cantor diagonal argument, proving that X and 2^X must have different cardinalities for any set X . Look at the proof.

We need the following terminology. Let $\text{Card}(X)$ denote the cardinality of the set X . We say that $\text{Card}(X) < \text{Card}(Y)$ if there is an injective mapping from X to Y , but no surjective mapping from X to Y .

Theorem. If X is any set, then $\text{Card}(X) < \text{Card}(2^X)$.

Proof. X injects into 2^X via the mapping that takes each element x of X to the singleton set $\{x\}$. Thus we must prove that no map $f : X \rightarrow 2^X$ is surjective. Let f be given. Let $R = 3D\{x \in X \mid x \text{ is not a member of } f(x)\}$. If $R = 3Df(a)$ for some a in X , then x belongs to $f(a)$ exactly when x does not belong to $f(x)$. Letting x equal a , we find that a belongs to $f(a)$ exactly when a does not belong to $f(a)$. This is a contradiction, and we conclude that R is not of the form $f(a)$. Consequently the mapping f is not surjective. QED.

From this theorem there issues an ever ascending hierarchy of infinities, all indicated in the no-more-than-countable world of our speech. From this theorem there issues the *possibility* of building a model of the continuum from aggregates of points. Without it, we would be at a loss to produce the (geometrically) required uncountability. Yet all this skirts close to paradox and nothing shows that paradox more quickly than to imagine Ω , the set of all sets. Ω cannot be larger than herself. Surely $\Omega = 3D2^\Omega$. Cantor's argument goes to a contradiction. In fact, if $\Omega = 3D2^\Omega$, then this contradiction is exactly the Russell Set R of all x in Ω such that x is not a member of x .

We can conceive of the Set of all Sets, but it cannot be brought forth into eternal existence without contradiction. We can conceive of the continuum, and it can be brought forth into mathematical existence as a set of points. The expense is near-paradox and inscrutability.

Images draw forth the notion of completed infinity. Consider an unending nest of boxes:

$$B = 3D\{ \{ \{ \{ \{ \{ \dots \} \} \} \} \} \}.$$

Here the seduction is that B seems to be defined by the finite equation

$$B = 3D[B],$$

a box that sits inside itself, a small notational hall of mirrors. Everything shifts down with the addition of one more set of outer parentheses. An infinite set is always in one-to-one correspondence with a proper subset of itself. Self-reference is infinity in finite guise.

A little play of signs will produce the effect without the apparent excursion to infinity. Let us assume the existence of a realm of entities that are both operators and operands. Each can act on any of the others. If A and B are such entities then we let AB denote the action of A on B to produce another such entity AB . Now let us define an entity J by the equation

$$JX = 3D[XX].$$

Let $X = 3DJ$ and obtain

$$JJ = 3D[JJ].$$

= Is JJ the same as B ? No! JJ and its self-referential property is the analog of the continuum and its uncountability *before* we have chosen any particular model for it. Infinity, in the form of self-reference, is the necessary consequence of the assumption of a domain of operations where every operation can be the subject of every other operation. To put it bluntly, self-reference and re-entry is the inevitable consequence of a *linguistic domain*. I will not attempt here to define precisely a linguistic domain other than to note that every text and utterance in any language in the human domain can be construed as both an operator on texts and as an operand, suitable for being operated upon by texts and their speakers. The speakers themselves are not separate from the language that they speak.

The linguistic domain is analogous to the continuum. It is generated through the desire for a set of properties whose consequences are near paradoxical and eventually inscrutable. It has been the intent of this part of the essay to juxtapose these points of view - of space in the sense of the continuum, and linguistic domain in the sense of an arena of active texts. The pivot between these points of view is the unavoidable fact that everything that we say about the continuum is said in language, so that all this is enfolded into the linguistic domain that the writer and reader of this text hold in common.

Sign and space are inseparable. There can be no space without the relations of signs and there can be no signs except in some space. Need for continuity of space leads to uncountability and/or to self-reference. Need for the action of language leads to self-reference, infinity and the possibility for continuity.

3 Rotations, Reflections and Quaternions

This section is a vignette about the efficacy of the Cartesian view of Euclidean space (as triples of real numbers) in the formation of a model for rotations and reflections. With the notion of coordinates at hand, and the principal orthogonal directions i, j and k , we can form arbitrary points in three dimensional space as linear combinations $ai + bj + ck$. It is desired to have an algebra for i, j, k so that if u represents a unit vector perpendicular to a given plane P , then the function $R(v) = 3Duvu$ represents reflection in the plane P .

This principle for the representation of reflection inverts the structure of reflection as we pass from geometry to algebra. What is reflected becomes the mirror. The mirror becomes the that which is reflected. The mirror in three dimensional space is the plane M . The direction u perpendicular to this plane becomes its algebraic representative. The vector v is acted upon by the mirror M in the three dimensional space. The vector v becomes the notational mirror for u in the line space. We find

$$uvu.$$

The u is mirrored by v in the line space of the algebra.

The algebra writes itself. Vectors i and j determine the plane M with normal direction k . Therefore k is reflected to $-k$ by M , while i and j are fixed (being in the mirror). Therefore $kik = 3Di$, $kjk = 3Dj$ and $kkk = 3D - k$. Assuming (for simplicity) that the algebra is associative (that is, $x(yz) = 3D(xy)z$ for any x, y, z) and that each of i, j and k are invertible, these equations imply that $kk = 3D - 1$, $ki = 3D - ik$, $kj = 3D - jk$. Using the other planes, we conclude that $ii = 3Djj = 3Dkk = 3D - 1$ and that any distinct pair a, b of i, j, k have the property that $ab = 3D - ba$. But what vector is ij ? We want ij to again be a vector in three dimensional space. Three dimensional space admits such a product. We can take $ij = 3Dk$ and more generally, the formula $ab = 3D - a.b + axb$ where $a.b$ denotes the dot product and axb denotes the cross product. This defines Hamilton's quaternions

[2]:

$$ii = 3Djj = 3Dkk = 3Dijk = 3D - 1.$$

Note that the algebra we have constructed is actually an algebraic structure on the *four dimensional space* of points of the form

$$a + bi + cj + dk,$$

where a, b, c are real numbers. Four dimensions arise smoothly from the algebra with the necessity of $ii = 3Djj = 3Dkk = 3D - 1$. The algebra represents not only reflections, but also rotations in three dimensional space. Every rotation about an axis can be written as a composition of two reflections in planes that intersect in this axis. From this one can compute that if u is an axis in three space then rotation about this axis by an angle θ is given by the formula

$$Rot(v) = 3Dgv\bar{g}$$

where

$$g = 3D\cos(\theta/2) + u\sin(\theta/2)$$

and

$$\bar{g} = 3D\cos(\theta/2) - u\sin(\theta/2).$$

The fantastic property of the quaternion algebra is that it embodies the rules for finding the new axis and angle for a composite of two rotations, geometric properties of the composition that are hidden to more prosaic representations.

If Hamilton (in the first part of the nineteenth century) had tried to represent reflections in three dimensional space by an algebra, he would have quickly come to invent the quaternions. But at that time non-commutative algebras were unknown. Hamilton spent fifteen years searching for an algebraic structure on three dimensional space that would simultaneously generalize the three dimensional cross product and the properties of the complex numbers (an algebra on the plane whose properties in turn generalize the real numbers). The move to algebra remakes the concept of space in a subtle interaction of sign and symbol that strikes deep into the heart of geometry.

4 Networks and Discrete Space

A radical point of view would replace space by a discrete structure of relationships. As we have remarked in the previous two sections of this essay, the geometry of the Greeks held a discrete web of relationships in the

context of a concept of continuous space. That space was not coordinatized in our way, nor was it held as an infinite aggregate of points. In the second section, we have discussed how three and four dimensional spaces are tied to deep properties of representational algebra and the simple geometry of order on a line (implicating non-commutativity).

The point of view of this section is a direct descendant of Euler's work in the eighteenth century that brought forth the use of abstract graphs as holders of spatial structure. After Euler it was possible to find the classification of the Greek regular solids in the (wider) classification of the possible regular graphs on the surface of the sphere. Metric disappears into relationship under the topological constraint of Euler's formula $V - E + F = 3D2$, where V denotes the number of vertices, E the number of edges and F the number of faces for the connected graph G on the sphere.

A network itself can represent an abstract space. Embeddings of that network into a given space (such as graphs on the two dimensional sphere) correspond to global constraints on the structure of the abstract graph.

Now, however, a new theme arises, motivated by a conjunction of combinatorics and physics. Imagine labelling the edges of the network from some set of "colors". These colors can represent the basic states of a physical system, or they can be an abstract set of distinct markers for purely mathematical purposes. Once the network is labelled each vertex can be an entity with a collection of labels incident to it. Let there be given a function that associates a number (or algebra element) to each such labelled vertex. Call this number the *vertex weight* at that vertex. Let C denote a specific coloring of the network N and let $\langle N|C \rangle$ denote the product, over all the vertices of N of the values of the vertex weights. Finally let $\langle N \rangle$, the *amplitude* of the network be defined as the summation of $\langle N|C \rangle$ over all colorings of the net.

$$\langle N \rangle = 3D \sum_C \langle N|C \rangle$$

This is sometimes also called the *partition function* of the network.

Amplitudes of this sort are exactly what one computes in finding the partition function of a physical system or the quantum mechanical amplitude for a discrete process. In all these cases the network is interwoven with the algebraic structure of the vertex

weights. It is only recently that topological properties of networks in three dimensional space have come to be understood in this way [4], [5]. This has led to new information about the topology of low dimensional spaces, and new relationships between physics and topology.

A classical example of such an amplitude was discovered by Roger Penrose [6] in elucidating special colorings of 3-regular graphs in the plane. A 3-regular graph G has three edges incident to each vertex. When embedded in the plane, these edges acquire a specific cyclic order. Three colors are used. One associates to each vertex the weight

$$\epsilon_{abc}$$

where a, b, c denote the edges meeting the vertex in this cyclic order, and the epsilon is equal to 1, -1 according as the edges have distinct labels in the given or reverse cyclic order, or 0 if there is a repetition of labels. The resulting amplitude counts the number of ways to color the network with three colors so that three distinct colors are incident to each vertex. This result is a perspicuous generalization of the classical four color problem of coloring maps in the plane with four colors so that adjacent regions receive different colors.

The Penrose example generalizes to networks whose amplitudes embody geometrical properties of Euclidean three dimensional space (angles and their dependence). Geometry begins to emerge as the "statistical averages" of properties of an abstract and discrete network of relationships. Topological properties emerge in the same way. The idea of space is changing to the idea of a network with global states and a functor that associates this network and its states to the more familiar models of properties that a classical observer might see.

It is in this way that space as network, and observation as amplitudes (or normalized amplitudes) comes to make sense out of quantum mechanics. The apparent paradoxes of quantum mechanics become the simple properties of network amplitudes. Space, time and algebra become interwoven in patterns that transcend continuity, coordinates and abstract axiomatic description.

A network equipped with colors and amplitudes is not a space or a geometry. The network is a formal structure, a mathematical/linguistic structure. The partition function/amplitude provides a functor to

properties of the net that cohere into geometry and topology and physics. With formal system as *sign* and the pattern of amplitudes as *space* we reach our final pivot in the dichotomy of sign and space.

There are no paradoxes in this network picture. No signals propagate, no loops turn round with contradictions. Everything happens (in the sense that all states of the network contribute to an observation), but not everything can be observed. The net observes itself, but this cannot be fully formalized.

4.1 Remarks on Quantum Mechanics

We should remark on the basic formalism for amplitudes in quantum mechanics. The Dirac notation $\langle A|B \rangle$ [7] denotes the probability amplitude for a transition from A to B . Here A and B could be points in space (for the path of a particle), fields (for quantum field theory), or geometries on spacetime (for quantum gravity). The probability amplitude is a complex number. The actual probability of an event is the absolute square of the amplitude. If a complete set of intermediate states $C_1, C_2, \dots, C_n$ is known, then the amplitude can be expanded to a summation

$$\langle A|B \rangle = \sum_{i=1}^n \langle A|C_i \rangle \langle C_i|B \rangle.$$

This formula follows the formalism of the usual rules for probability, and it allows for the constructive and destructive interference of the amplitudes. It is the simplest case of a quantum network of the form

$$A - \text{---} * \text{---} - C - \text{---} * \text{---} - B$$

where the colors at A and B are fixed and we run through all choices of colors for the middle edge. The vertex weights at the vertices labelled $*$ are $\langle A|C \rangle$ and $\langle C|B \rangle$ respectively. A measurement at the C edge reduces the big summation to a single value. By combining this basic principle of quantum amplitudes with the abstract structure of complex networks, we can begin to obtain a quantum geometry and quantum topology for the spaces and signs of our experience.

One way to introduce spacetime into the network picture is to introduce a temporal direction into the net. Thus at each vertex one must specify labels of *before* and *after* to each edge of the net that is incident to the vertex. If there is a sufficiently coherent assignment of such local times, then a global time direction can emerge for the entire network. Networks endowed with temporal directions have the structure of morphisms in a category where each morphism points

from past to future. A category of quantum networks emerges equipped with a functor (via the algebra of the vertex weights) to morphisms of vector spaces and representations of generalized symmetry groups. Appropriate traces of these morphisms produce the amplitudes.

Quantum non-locality is built into the network picture. Any observer taking a measurement in the net has an effect on the global set of states available for summation and hence affects the possibilities of observations at all other nodes in the network. By replacing space with the network we obtain a unified view of a precursor to spacetime in which quantum mechanics is self-explanatory.

5 Distinction

In the final analysis this essay has been about the consequence of making a distinction. In order to perceive, the universe must divide itself into that which sees and that which is seen. As such it must (partially) elude itself. It is this elusive process, mirrored in the interplay of sign and space that points to the whole.

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ANPA 1994

What do you say about ANPA to a group of orthodox physicists.

- .particle physicists
- .cosmologists
- .generalists

Mike Herron
October 1994
version 2

Scenario

- Audience
 - Friendly (must be carefully selected)
 - Particle physicists at CERN/LAC
 - OR
 - Cosmologists
- Your goals
 - convey the ANPA (physics) story
- Non Goals
 - convey the complete ANPA story
- Advice
 - use Dirac as an entry point
 - Nobel prizes go to experimentalists

Orthodox Physics Model(S)

- Scale dependent not a unified approach
 - Quantum for the very small
 - Newtonian for the human scale
 - Relativistic for the very large
- Some constants put in by hand
- Diracs numerical observation(10E40)
- Time added as an afterthought
- Space time is a Platonic receptacle
- Not really one model
 - particle physicists (quarks and leptons)
 - cosmologists (big bang)

Orthodox Physics Principles

- Continuousness
- Projection (orthogonal projection)
- Structure
 - No history
 - No context
- Not Combinatorial and essential Analytic
- Information not a basic concept
- Object emphasis
- Computing as counting
- DETERMINISTIC

Orthodox Physics Status

- Last 10 years
 - experimental results of significance (top quark)
 - no new predictions
- 10 constants to be put in by hand
- Cost of experiments unbearable
- Quantum thinking "fuzzy"
- Newtonian thinking embedded
- Relativity thinking accepted
- Connectivity rejected(EPR/Bell/Aspect)

ANPA Model

- All scales..... a unified approach
- No constants put in by hand (values not accurate)
- Produces Diracs and Eddingtons numbers
- Time "precedes" space (Manthey,Etter)
- Space (3D) from the theory
- One model emerging but not yet coherent
 - COMBINATORIAL HIERARCHY

ANPA 1995

■ Advisory Council Comments

- Chew
- Suppes
- Isham

■ ANPA 1994 presentation updated.

Mike Horner
September 1995

Geoffrey Chew

■ Chew's Theory

- How can we discuss anything at all?
- Requires a large constant (10²⁴007)

■ ANPA

- Has never understood "it"
- A coherent story needed

Pat Suppes

■ Now retired from Stanford

- multiple interests

■ ANPA

- never really understood "it"
- a necessary type of community
- keep a lean organization even if generously funded

Chris Isham

■ Imperial College London

- Active in helping us

■ ANPA

- A necessary type of organization
- Does not understand the underlying concepts
- Not understood by the physics community
- What are our goals?

Advisory Council Summary

■ ANPA is

- an alternative (to standard physics)
- a necessary type of organization

■ ANPA is not understood

- Underlying concepts
- Combinatorial Hierarchy
- Goals

Mike Horner
September 1995

Today

■ 20 minute presentation

■ 40 minute discussion

- Improve the presentation

■ Improvements

- add in any missing points
- priorities/emphases
- highlight the uniqueness of ANPA(physics)
- reorder the sequence of the story

■ a new physics related version will be published

■ a non physics version will be created

ANPA Physics Status

- Significant results recently
- No constants put in by hand(?)
- Computation Experiments inexpensive
- Quantum thinking clear
- Newtonian thinking partially purged
 - Our language implies there is an "Arena in which things happen"
 - The whole of physics is about matter light and connections
 - Euclid is in our bones
- Relativity thinking (un)integrated
- Connectivity (relationships) basic

ANPA Physics Principles

- Discreteness
- Discrimination
- Process
 - History
 - Context
- Combinatorial / Synthetic / Holistic
- Information a basic concept
- Relationship emphasis
- Computing is modelling
- NON DETERMINISTIC

Questions

- Standard Physics
 - Model
 - Principles
 - Status
- ANPA Physics
 - Model
 - Principles
 - Status
- ANPA outside of Physics

Combinatorial Hierarchy

- Two Simultaneous Processes
 - An evolving discovery /discrimination process
 - A naming /labelling process
- A four level hierarchy emerges
- Discrimination changes at each level(?)
- Coupling Constants Values appear
- Hierarchy evolution terminates
 - labels evolve
 - entities to be labelled evolve more rapidly

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(World Scientific 1993)
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Concerning Physics

- The ground of Physics is to put life into equations
- Experiments are repeatable in time and space
- Classical models are deeply embedded in Quantum field theory
- String theories are (perhaps) information oriented
- A goal of physics is to make predictions verifiable by experiment
- Possible use of non dimensional ratios

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