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On CH and OOC*

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ABSTRACT

We add our comments to the recent discussion between Ted Bastin and David McGoveran on the relationship between the *combinatorial hierarchy* (CH) and the *ordering operator calculus* (OOC).

1. INTRODUCTION

In March, Ted prepared a working note for Clive entitled "A note on priorities in the construction of physical space", reproduced here as Appendix I. Clive thought this text clear enough to send to David. David's comments, dated April 12 (Appendix II) were completed about the same time Ted sent me the original paper and asked for my comments. Since this issue concerns all of us, I am circulating my response to the larger group. Hopefully we can have a round or two of written discussion prior to ANPA 12. Some of Ted's comments on a June 8 version of this technical note (TN) are included as Appendix IV.

In his covering letter to me, dated April 11, Ted says my efforts will be more valuable if they are "reasonably self-contained in the sense of proceeding from principles rather than going into a lot of detail with reference back to a body of established work." I thought both David and I had made our approach clear in DP[1] and that David had provided a still more thorough discussion in FDP[2]. It is *essential* to understand both our modeling methodology and our principles; I quote some critical points below.

In the first draft of this paper I asked:

*Is there a corresponding statement on the principles and methodology used in the CH (***combinatorial hierarchy***) research on which both Ted and Clive agree?* If so, I would dearly love to be referred to it.

Ted and Clive have agreed to respond to the challenge.

1.1. Principles

For us, our *epistemological framework* is the current practice of laboratory physics and physical cosmology. Our *representational framework* is the OOC (**ordering operator calculus**), and the CH developed in the context of the OOC. Our *procedural framework* establishes rules of correspondence, such as the counter paradigm and the identification of quantum numbers, which relate these mathematical results back to the E frame. The whole scheme is iterated in any sequence, including reversals at any step, until we are satisfied for the moment, or prepared to modify or to abandon it.

"The modeling methodology presupposes that the community adopting it commits itself, individually and collectively, to:

(1) Agreement of cooperative communications:

(a) commonly defined terms as fundamental;

(b) fundamental vs derived terms;

(c) agreement of pertinence;

(2) Agreement of intent.

(3) Agreement on observations.

(4) Agreement of explicit assumptions.

(5) The razor:

(a) agreement of minimal generality;

(b) agreement of elegance;

(c) agreement of parsimony." (DP, p.85; see also FDP pp 70-71.)

"[the] five principles [are]:

Principle 1: The theory possesses the property of strict finiteness.

Principle 2: The theory possesses the property of discreteness.

Principle 3: The theory possesses the property of finite computability.

Principle 4: The theory possesses the property of absolute nonuniqueness.

Principle 5: The formalism used in the theory is strictly constructive." (DP, p.85; see also FDP, pp 4-5.)

Unless there is agreement both on the methodology and on these principles, we had better iron out the differences before proceeding.

1.2. "Formal systems", R-frames and E-frames

I start with Ted's remark in Appendix IV:

"...but formal systems (or E-frames) however useful they may be in clarifying one's thought — still leave open the questions: what is this f.s. for? and does it do what is intended?"

To begin with, what Ted calls a formal system (f.s.) is, in our methodology, called an *R-frame*, i.e a *representational framework*. For this *limited piece* of the ERP iterative structure, we do contend that the whole thing is defined throughout, and that is all that one may ask of it. This is, of course the standard Russell-Whitehead contention that Mathematics (formal systems) are *tautological*; self-consistency is all that we *can* require of them. Of course this limited view of Mathematics came a cropper when Gödel proved that all such systems rich enough to contain arithmetic yield numerically consistent equalities whose truth when read as propositions cannot be *proved* within the system. We avoid this trap by requiring any f.s. we use to be both finite and discrete. We can always name *in advance* a largest integer which will not be exceeded in any computation we undertake. If we find it either necessary or desirable to exceed this preset limit in any explicit or implicit calculation we perform, all previous arguments must be re-examined before we can proceed. In other words, we must start a new investigation.

Of course this is the beginning, not the end of the story. We agree with Ted that we must ask the questions: what is this R-frame for? and does this R-frame do what is intended? But these questions have to be asked, not as formal questions of self-consistency, but in the iterative context of the ERP modeling methodology. This explicitly contains *informal* elements in the E-frame— *epistemological framework*. Ted's criticism misses the point because he has incorrectly assumed that our E-frame is a f.s. It is not. For the task at hand, we take the E-frame to be the current practice of laboratory physics, including all experimental techniques, mathematical and computational procedures, etc. which lead to direct or statistical numerical comparisons. We are quite explicit in stating that this does *not* commit us to currently fashionable *interpretations* of these laboratory results or mathematical manipulations. Ours is a strict version of Bridgman's "operationalism". Note, however, that in contrast with Bridgman himself, we do not believe that strict application of these criteria will succeed in banishing *metaphysics* from the practice of physics, let alone from its interpretation. Our limited metaphysics is the assertion that if we can agree on an iterative ERP methodology whose objective is modeling these numerical results (or some subset of them), we do not *have* to take a stand on broader issues drawn from the traditional conflicts between *ontology* and *epistemology.* Of course this does not prevent us from taking such stands; doing so would take us into a context that goes beyond what we have called *discrete physics.*

1.3. "COMPLETENESS", MLT PHYSICS, NEW DIMENSIONS

Going back to an earlier comment of Ted's in the same paragraph, there is nothing in our methodology that "confer[s] incorrigibility". The intent is, rather, to guarantee agreement as to where to *bound* the extent of the investigation. This in no way amounts to a "loyalty oath" that prohibits other questions being raised in other contexts — or asking the question of whether the framework agreed on by the participating community is too narrow. It is simply an agreement to be explicit about where the boundary is currently placed. Our *core consensus* can be expected to be quite limited. The obvious advantage is that within the core we can give considerable precision to our results.

With this in mind, I turn to some comments in Ted's next-to-last paragraph:

"... I fix on certain amazing properties of the world and see how one can understand them. ... it does not seem to me to be essential at all to cover the whole of what physics is taken to cover."

That we can cover "the whole of what physics is taken to cover" has indeed become my objective. But I would sharply distinguish my understanding of this phrase from Einstein's demand for *completeness* on both methodological and metaphysical grounds. In so far as I understand Einstein's metaphysics, I believe that he held physics to be co-extensive with "reality". I make no such claim. My objective is not to discover "universal truth". I do not even expect our methodology to lead to a "complete and incorrigible" description of laboratory physics. My goals are much more modest.

Current physics can be characterized as MLT physics in the sense that any measurement can be expressed in terms of a numerical coefficient of a dimensional product $M^a L^b T^c$ where a, b, c can be positive or negative finite integers, rational fractions, or zero. This statement *must* include some estimate of the uncertainty of the result expressed in the same dimensional units. The numerical intercomparisons of these results and comparisons between them and theoretical calculations constitute what I take to be "the whole of what physics is taken to cover"; clearly this must include experientially repeatable understanding of the operations which lead to the numbers.

It is reasonably easy to articulate criteria which would force most practicing physicists to concede that it might be useful to assume that there are more than three independent dimensional standards. Historical examples abound. In recent times candidates for a fourth dimensional concept such as baryon number in addition to mass, weak in addition to electromagnetic charge, a "fifth force", ... have surfaced, but did not long survive. One way of stating my methodological objective is to make our scheme so tight — and so acceptable to conventional physicists — that if a new candidate for a fourth dimensional concept emerges we will be able to provide stringent tests of its acceptability. My speculative position is that four such new dimensional concepts will emerge in a correlated way that can be understood using the CH labeling of level 2.

N.B. *What follows is very incomplete, and is provided only give you some of* *the flavor of what I hope to whip into shape for ANPA 12 at the technical level.*

2. FROM BIT-STRINGS TO VECTOR COORDINATES WITH HALF-INTEGER COMPONENTS

2.1. General Remarks

During the past few months I have loaded my disk with various fragmentary drafts of papers on the construction of coordinates, vectors and wave functions from bit-strings and have yet to hit on a fully satisfactory strategy of presentation. I hope eventually to go from the connection between the general concept of attribute distance and the fact that discrimination between bit-strings can be used to provide a direct relative measure of this distance to map bit-strings onto finite vectors with integer and half-integer coefficients in a flat 2-space. Here I will start with familiar facts about bit-strings and simply show how to map them onto the "space-quantization"of elementary quantum mechanics, i.e. the rules for the addition of angular momenta and the geometrical interpretation in terms of finite angles and angular steps. Contrary to Ted's view that "angles" are a conceptually late aspect of the construction, I find this discrete approach to angles a good starting point. It can, in principle, be made fully algebraic, and should be put in more logical order than I have achieved in order to satisfy demands of parsimony and elegance. When it comes to "rules of correspondence", the continuum limit (which I suspect Ted has in mind when he talks about"angles") is firmly rejected, and the "quantization of angular momentum" boils down to the specification of the finite angular resolution (in appropriate units) that we must spell out in designing and interpreting any laboratory experiment that involves "angles" and enters the quantum domain. Since we are using discrimination as basic, the basic *invariant* for rotations has to be the common string length, or some algebraic construction based on it. Conservation laws follow immediately.

With this treatment of "rotational invariance" under our belts, we arrive at a more satisfactory treatment of "Lorentz invariance" than was possible when the transformation changed the string length (DP, pp 91-93). We find that we can, as in the treatment of rotations, think of the transformation (now in 1+1 space) as a change in the number of "1"s in a string of fixed length, corresponding to a rational fraction transformation of velocities. The restriction of finite angular resolution translates into the minimum velocity resolution (or mass, or mass resolution, or momentum, or momentum resolution) in appropriate units, as we might suspect from the Minkowski mapping onto Euclidean space. Our discrete context is preserved.

In future work I intend to use string concatenation to get the integer coordinates in the first place. The introduction of unit vectors, allows us to map the *4-event* definition $\mathbf{a} \oplus \mathbf{b} \oplus \mathbf{c} \oplus \mathbf{d} = \mathbf{0}$ onto quaternions with integer and half-integer coefficients, and recover the advantages of Greider's convenient formalism[3] for relativistic conservation laws. For me, at least, it is important to develop a technically sound and adequate description of 4-vectors whose magnitudes are restricted to integers and half-integers. When I succeed, this will reassure me that I have the apparatus in hand to represent both the quantum numbers of the standard model of quarks and leptons and discrete rotations and boosts, together with their space-time conservation laws, in such a way that we can model laboratory experience in elementary particle physics. Up to this point nothing prevents us from constructing finite n-dimensional spaces based on discrimination. The concept of attribute distance articulated using bit strings does not, in itself, lead on to the full theory.

In Chapter 3 I quote relevant pieces of a recent paper[4] that adumbers the connection between dimensionality and hierarchy closure with which Ted is (properly) so concerned. It is here that the 3+1 space restriction (McGoveran's Theorem) enters, and can be discussed. The discussion, I believe, should start with Clive's contention at ANPA 2 that in addition to *discrimination* we need a second operation which he then called *generation.*

2.2. Bit-Strings

We specify a *bit-string*

$$\mathbf{a}(S) = (..., b^a_s, ...)_S \tag{2.1}$$

by its S ordered elements

$$b^a_s \in 0,1; \quad s \in 1,2,....S; \quad 0,1,...,S \in \ \textit{ordinal integers} \tag{2.2}$$

and its norm by

$$|\mathbf{a}(S)| = \Sigma^S_{s=1} b^a_s = a(S) \tag{2.3}$$

This is the usual Hamming measure for bit-strings. Define the *null string* by $\mathbf{0}(S)$, $b^0_s = 0$ for all s and the *anti-null* string by $\mathbf{1}(S)$, $b^1_s = 1$ for all s.

Define *discrimination* (XOR) by

$$\mathbf{a} \oplus \mathbf{b} = (..., b^{a\oplus b}_i, ...)_S = \mathbf{b} \oplus \mathbf{a}; \ b^{a\oplus b}_i = (b^a_i - b^b_i)^2 \tag{2.4}$$

from which it follows that

$$\mathbf{a} \oplus \mathbf{a} = \mathbf{0}; \ \mathbf{a} \oplus \mathbf{0} = \mathbf{a} \tag{2.5}$$

Define $\bar{\mathbf{a}}(S)$ by

$$\bar{\mathbf{a}} := \mathbf{a} \oplus \mathbf{1}; \ \textit{hence} \ \mathbf{a} \oplus \bar{\mathbf{a}} \oplus \mathbf{1} = \mathbf{0} \tag{2.6}$$

Since discrimination is only defined for bit-strings of the same length S, we can often omit reference to it, as we have done above. However, when the norm *and* the anti-null string are involved we need to know the string length. In particular

$$|\mathbf{1}(S)| = S; \; |\bar{\mathbf{a}}(S)| = S - a(S) \tag{2.7}$$

For two strings $\mathbf{a}(S_a), \mathbf{b}(S_b)$ we define *concatenation* ($\|$) by

$$\mathbf{a}(S_a)\|\mathbf{b}(S_b) = (....b_i....)_{S_a}\|(....b_j^b...)_{S_b}$$

$$= (......, b_k^{a\|b}, ...)_{S_a+S_b} \tag{2.8}$$

$$b_k^{a\|b} = b_i^a, \; i \in 1, 2, ..., S_a; \; b_k^{a\|b} = b_j^b, \; j \in 1, 2, ..., S_b, k = S_a + j$$

Hence

$$a + b := |\mathbf{a}\|\mathbf{b}| = |\mathbf{b}\|\mathbf{a}| \tag{2.9}$$

but note that in general $\mathbf{a}\|\mathbf{b} \neq \mathbf{b}\|\mathbf{a}$.

2.3. Definition of Bit-String Coordinates

To map bit-strings onto integer and half-integer coordinates first note that the Hamming measure $a := \Sigma_{s=1}^{S} b_s^a$ takes the null string as the "reference ensemble" in McGoveran's definition of *attribute distance**. We restore the symmetry between the symbols "0" and "1" by using for our measure the signed coordinate

$$-\frac{S}{2} \leq q_a := a - \frac{S}{2} \leq +\frac{S}{2} \tag{2.10}$$

There are $2S$ such integrally spaced coordinates for S even and $2S + 1$ for S odd. These integer *or* half-integer coordinates can be related to the usual angular

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$\star$ .., define **attribute distance** for a specific attribute generated by an ordering operator O_i, as the measure dependent solely on the number of distinguishable states s between two ensembles of labels which O_i may generate ..." FDP, p. 28, et. seq.

momentum "space quantization" of elementary quantum mechanics by defining

$$J(S)cos\ \theta_a := q_a;\ J^2(S) := \frac{S}{2}(\frac{S}{2}+1) \tag{2.11}$$

Then integer steps correspond to "rotations" leaving the string length and hence J^2 invariant. Alternatively we can define

$$\tau(S)\ cosh\ \xi_a := q_a;\ \tau^2(S) := \frac{S}{2}(\frac{S}{2}+1) \tag{2.12}$$

with $\beta_a := tanh\ \xi_a := \frac{2a}{S} - 1$ and "Lorentz transformations" which leave $\tau^2(S)$ invariant. Extending these definitions to 3+1 dimensions for 4-events as defined above, we find that we can map the content strings (space-time) onto the C4 Clifford algebra (quaternions) in Greider's[3] formulation of non-interacting relativistic quantum mechanics for particles and fields. This fact can be used to establish the "Poincaré invariance" of our representations in the context of our integer restrictions that make all 4-vector components signed integers or half-integers. Applied to our finite label space, this mapping also can be used to establish the conservation of fermion number, weak hypercharge and baryon number across the intervals connecting two scattering events.

My approach from the start has been that we can accept the ordinal integers up to some number specified in advance (including the usual extensions to negative integers and finite rational fractions consistent with this boundary) and the bit-strings of standard computer practice without having to construct them from first principles. Part of the contretemps we are in may arise from a hidden assumption on the part of the CH protagonists that they need to construct the integers along with everything else. They clearly now *do* feel it necessary to construct *discrimination* whereas I am content to define it — the approach adopted in the original (1966) paper on the CH. Which of their (so far as I know, unstated) principles compel them to start so far back escapes me. If we have to, I am confident that what we need for physics can be constructed just as well from the OOC as from the new foundational approach to the CH, particularly since, as David points out in Appendix II, the OOC provides a richer mathematical background.

That one might need concepts more primitive than bit-strings in dealing with some questions of statistics which have arisen in using the OOC to construct wave functions is shown by David in a draft paper entitled "Elucidation of Total Attribute Distance and the Finite Exponentiation Operator" included as Appendix III. I believe I have avoided the issue by constructing finite and discrete integer and half-integer coordinates with the appropriate Lorentz and (non-commutative) rotational properties directly from bit-strings. This construction can be extended to finite Foch spaces and in the process provide an explication of why our "vacuum fluctuations" lead only to small "self-energy" corrections (see below, or a later paper.)

I will be reluctant to go deeper into the question of where integers, bit-strings and discrimination come from until I can be shown that some critical point in elementary particle physics or physical cosmology requires the subtlety. I grant that such subtleties can be expected to become important when we go beyond modeling particle physics into biology, consciousness, etc.

2.4. Construction of 2-Vectors

I now take steps to make the interpretation of bit-strings as "vectors" more compelliong by using the attribute distance between them to define relative angles. Conveniently $n_{ab} := |\mathbf{a} \oplus \mathbf{b}|$ will serve to specify this magnitude for us, given no information about the strings other than their (integer) magnitudes $a > 0, b > 0$. From the fact, proved long ago, that discrimination necessarily implies the triangle inequalities

$$|a-b| \leq n_{ab} \leq a+b \leq S$$

$$|b-n_{ab}| \leq a \leq b+n_{ab} \leq S \tag{2.13}$$

$$|n_{ab} - a| \leq b \leq n_{ab} + a \leq S$$

we can construct a discrete "angle" θ_{ab} given by

$$ab\ cos\ \theta_{ab} := \frac{1}{2}[n_{ab}^2 - (a^2 + b^2)];\ n_{ab} \geq \frac{S}{2}$$

$$ab\ cos\ \theta_{ab} := \frac{1}{2}[a^2 + b^2 - n_{ab}^2];\ n_{ab} \leq \frac{S}{2} \tag{2.14}$$

This definition of "angle" immediately implies that

$$cos\ \theta_{aa} = +1;\ cos\ \theta_{a\bar{a}} = -1 \tag{2.15}$$

Introducing negative numbers in this way allows us to replace the positive Hamming measure by a *symmetric coordinate*

$$q_a(S) := a - \frac{S}{2};\ hence\ q_{\bar{a}}(S) = -q_a(S) \tag{2.16}$$

This restores part of the symmetry between the choice of the symbols "0" and "1" which is lost in the elementary definition of discrimination. Consider two choices $(0, 1);\ (1', 0')$ and

$$0 \oplus 0 = 0 = 1 \oplus 1;\ 0 \oplus 1 = 1 = 1 \oplus 0$$

$$1' \oplus 1' = 1' = 0' \oplus 0';\ 1' \oplus 0' = 0' = 0' \oplus 1'$$

Clearly either will do, if used consistently, but dropping the primes produces an immediate paradox if "0" and "1" are used with their usual connotations. To do the full job of making the theory *independent* of the choice of symbols [which a long time ago I called *Amson invariance* in honor of his *bi-orobourous* paper] takes a lot more work than introducing negative integers and half-integers in the way we have done in this paper. The step is justified by **Principle 4** — absolute non-uniqueness in this context implies that the *choice* between symbols cannot matter. We hope to return to this question later.

In order to give the string $\mathbf{a} \oplus \mathbf{b}$ a *sense* and relate the signs of our coordinates $q_a(S), q_b(S)$ to this arbitrary convention (which allows a rule of corresponence connecting the formal system to *laboratory* directions), we take the sign of $\mathbf{a} \oplus \mathbf{b}$ positive if $a > b$ and negative if $a < b$. We do this by mapping bit-strings **a** and **b** onto vectors with integer (and eventually half-integer) coefficients which we call $\vec{a}$ and $\vec{b}$. We defining the inner and outer products

$$\vec{a} \cdot \vec{b} := ab \; cos \; \theta_{ab} := \frac{1}{2}[a^2 + b^2 - n_{ab}^2]; \;\; \vec{a} \wedge \vec{b} := ab \; sin \; \theta_{ab} = -\vec{b} \wedge \vec{a}$$

$$(\vec{a} \wedge \vec{b}) \cdot (\vec{a} \wedge \vec{b}) = n_{ab}^2 \tag{2.17}$$

where the unit vectors $\hat{a}, \hat{b}$ are defined by

$$\hat{a} \cdot \hat{a} = 1 = \hat{b} \cdot \hat{b}; \;\; \hat{a} \cdot \hat{b} = cos \; \theta_{ab}; \;\; \hat{a} \wedge \hat{b} = sin \; \theta_{ab}; \;\; sin^2\theta_{ab} + cos^2\theta_{ab} = 1 \tag{2.18}$$

Note that there are *two* arbitrary signs in this mapping. n_{ab}, as already noted, is taken as positive if $a > b$, which corresponds to taking b as the reference if a has *positive attribute distance* as usually defined, but is obviously an arbitrary convention. Simililarly, the choice of sign for $\vec{a} \wedge \vec{b}$, when this is interpreted in 3-space as normal to the plane defined by $\vec{a}$ and $\vec{b}$, depends on a left-handed or a right-handed convention. All of these conventions must eventually become part of our *rules of correspondence.* Note for now that this introduces a 4-fold *discrete* ambiguity, which can be exploited to make consistent quadrant assignments to all angles $\theta_{ab} \; mod(2\pi)$.

In addition to the inner and outer products, which we have now defined in terms of the integer norms a, b, n_{ab} for any three strings subject to the constraint

$$\mathbf{a} \oplus \mathbf{b} \oplus \mathbf{n_{ab}} = \mathbf{0} \tag{2.19}$$

we must now define *vector addition*, $\vec{a} + \vec{b}$, in order to justify our mapping. We intend to do this by the constraints

$$(\vec{a}+\vec{b})\cdot(\vec{a}+\vec{b}) = a^2 + b^2 - n_{ab}^2 = a^2 + b^2 + 2ab\ cos\ \theta_{ab} = a^2 + b^2 + 2\vec{a}\cdot\vec{b}$$

$$(\vec{a}+\vec{b})\wedge(\vec{a}+\vec{b}) = 0 \tag{2.20}$$

The final step before we map onto quaternions with integer and half integer coefficients is to introduce strings corresponding to unit vectors. We have done enough work on this to be sure that a useful choice will be to take

$$\mathbf{e}_x = (1010);\ \ \mathbf{e}_y = (1001);\ \ \mathbf{e}_z = (1100);\ \ \mathbf{e}_0 = (1111) \tag{2.21}$$

and relate these to integer vectors by noting that thanks to Eq. 2.9 we can define

$$2\mathbf{e}_\mu := \mathbf{e}_\mu \| \mathbf{e}_\mu;\ \ (n+1)\mathbf{e}_\mu := n\mathbf{e}_\mu \| \mathbf{e}_\mu \tag{2.22}$$

Since a picture is supposed to be worth a thousand words, the minimal two-space and 3-space spanned by this choice is supplied as the last page of this technical note.

REFERENCES

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3. K.R.Greider, *Found. of Phys.*, **14**, 467-506 (1984).
4. H.P.Noyes, "Discrete Gravity", to appear in the collection of papers to be distributed at the conference *Physical Interpretations of Relativity Theory. II.*, to be held at Imperial College 2-8 September, 1990, and SLAC-PUB-5218 (July, 1990).

3. CONSTRUCTING LABEL SPACE AND SPACE-TIME

Following exerpts from "DISCRETE GRAVITY"[4] are supposed to indicate where I hope to go.

CONSTRUCTING A BIT-STRING UNIVERSE

The quantum theory of gravitation and elementary particles which we are in the process of constructing comes from interweaving several different lines of research, the earliest of which started with Bastin and Kilmister in the 50's and led to the discovery of the *combinatorial hierarchy* — i.e. the terminated sequence $3, 10, 137, 2^{127} + 136$ — by Parker-Rhodes in 1961. This discovery was reported by Bastin (3), and further developed by Bastin, et.al.(4). The most fundamental recent development, which has also shed new light on the work of Stein, Gefwert, Manthey and Etter, is McGoveran's (5) *ordering operator calculus.* Some physical consequences have been published by Noyes and McGoveran (6), and the theory is undergoing rapid development.

The common thread which unites this work is the representation of the fundamental entities by *bit-strings*:

$$\mathbf{a}(S) = (..., b^a_s, ...)_S;\ b^a_s \in 0,1;\ s \in 1,2,....S;\ 0,1,...,S \in \ \textit{ordinal integers} \tag{3.1}$$

which can combine by *discrimination* (XOR) symbolized by "$\oplus$":

$$\mathbf{a} \oplus \mathbf{b} = (..., b^{a\oplus b}_i, ...)_S = \mathbf{b} \oplus \mathbf{a};\ b^{a\oplus b}_i = (b^a_i - b^b_i)^2 \tag{3.2}$$

or *concatenation* symbolized by "||":

$$\mathbf{a}(S_a)||\mathbf{b}(S_b) = (....b_i....)_{S_a}||(....b_j^b...)_{S_b} = (......, b_k^{a||b}, ...)_{S_a+S_b}$$

$$b_k^{a||b} = b_i^a,\ i \in 1,2,...,S_a;\ b_k^{a||b} = b_j^b,\ j \in 1,2,...,S_b, k = S_a + j \tag{3.3}$$

Disagreement as to the proper foundations for the theory stem from different assumptions about how the symbols "0" and "1" are to be generated or constructed in the first place, how the two operations themselves are generated or constructed, and how they are to be interleaved to generate strings of sufficient complexity to model physical cosmology and elementary particle physics. These differences will be actively discussed next week at the twelfth annual international meeting of the **Alternative Natural Philosophy Association** (*ANPA 12*) to be held at the Department of History and Philosophy of Science, Free School Lane, Cambridge, 14-17 September 1990. Anyone here who is interested is cordially invited to attend.

We will ignore these foundational differences here and take as our model the class of algorithms called *program universe* (see 5, pp 87-88). These pick two arbitrary strings from a universe containing strings of length S, discriminate them, and if the result is not the null string ($b_s^0 = 0$ for all s) adjoin it to the universe; else we concatenate an arbitrary bit, separately chosen for each string, to the growing end of each string. If we think of this bit-string universe as a block of strings of length S and height H, the second operation (called *TICK*) amounts to adjoining an arbitrary column (Bernoulli sequence) and hence $S \to S+1$. The first operation (called *PICK*) generates a string from the extant content and adds it as a new horizontal row ($H \to H+1$). I am still amazed that this simple algorithm can be used to construct the rich structures given in our summary Table!

COMBINATORIAL HIERARCHY LABELS

Finite sets of non-null bit-strings which *close* under discrimination are called *discriminately closed subsets* (*dcss*). For example, two *discriminately independent* bits-strings (i.e. $\mathbf{a} \oplus \mathbf{b} \neq \mathbf{0}$) generate 3 dcss: $\{\mathbf{a}\}$, $\{\mathbf{b}\}$, $\{\mathbf{a}, \mathbf{b}, \mathbf{a} \oplus \mathbf{b}\}$. The three member set closes under discrimination because any two members discriminate to the third. Similarly 3 discriminately independent bit-strings generate 7 dcss:

$$\{\mathbf{a}\},\ \{\mathbf{b}\},\ \{\mathbf{c}\}$$

$$\{\mathbf{a}, \mathbf{b}, \mathbf{a} \oplus \mathbf{b}\};\ \{\mathbf{b}, \mathbf{c}, \mathbf{b} \oplus \mathbf{c}\};\ \{\mathbf{c}, \mathbf{a}, \mathbf{c} \oplus \mathbf{a}\} \tag{3.4}$$

$$\{\mathbf{a},\ \mathbf{b},\ \mathbf{c},\ \mathbf{a} \oplus \mathbf{b},\ \mathbf{b} \oplus \mathbf{c},\ \mathbf{c} \oplus \mathbf{a},\ \mathbf{a} \oplus \mathbf{b} \oplus \mathbf{c}\}$$

Clearly, given j non-null discriminately independent strings one can form $2^j - 1$

dcss. If one starts with two discriminately independent bit-strings of length 2 [(01), (10) *or* (01), (11) *or* (11), (10)] and forms the three dcss, these can be mapped by three non-singular $\mathbf{2} \times \mathbf{2}$ matrices which are discriminately independent to provide three basis elements for a new level. This mapping can be repeated using $\mathbf{4} \times \mathbf{4}$ matrices with $7 = 2^3 - 1 < 16$ non-singular and discriminately independent exemplars, and once again using $\mathbf{16} \times \mathbf{16}$ matrices because $127 = 2^7 - 1 < 256$; however the mapping cannot be carried further because $\mathbf{256} \times \mathbf{256}$ matrices have only 256^2 discriminately independent exemplars and $256^2 << 2^{127} - 1$. This is still the simplest way to explain how the combinatorial hierarchy can be generated and why it terminates.

At ANPA 2 (1980) Kilmister (7) proposed a specific scheme for generating the combinatorial hierarchy (CH) which did not necessarily rely on bit-strings. Soon after, Noyes and Kilmister recognized that any generation scheme could go on generating bit-strings beyond those needed for the CH construction. This suggested that the early part of the string could represent a *label* corresponding to the quantum numbers of the elementary particles — which could be closed off once the labels were long enough to represent the 4 levels of the CH — concatenated with a *content* string which would represent a space-time expanding out to an event horizon given by the string length at any particular stage in the construction. In order to explore this situation Noyes and Manthey created *program universe* as described above. When the label strings have reached length 16, they can be organized into three orthogonal dimensions corresponding to the first 3 levels of the CH containing 3, 7 and 127 strings of length 16. These strings can be used to represent the fermion number, weak isospin and baryon number of the three generations of the standard model of quarks and leptons, and the confined color charges (see Noyes, 6). The next step in the construction closes with $2^{127} - 1$ strings of length 256 making a cumulative total of $N = 2^{127} + 136 \simeq 1.7 \times 10^{38}$ states available to us.

QUANTIZED SPACE-TIME

Once we have constructed the label-content concatenation, we can interpret the situations where PICK leads to a non-null string (i.e. $\mathbf{c} = \mathbf{a} \oplus \mathbf{b}$, or equivalently $\mathbf{a} \oplus \mathbf{b} \oplus \mathbf{c} = \mathbf{0}$) as the production (eg by pair annihilation or bremstrahlung) or absorption of a single label which either initiates or terminates a propagation of the label that continues for (or ends after) some finite number of TICKs. This is a discrete model for a Feynman vertex. The completed process combining two such vertices models a 4-leg diagram $\mathbf{a} \oplus \mathbf{b} \oplus \mathbf{c} \oplus \mathbf{d} = \mathbf{0}$ which we call a 4-event.

The choice of this criterion is not arbitrary. McGoveran (5, Theorem 13) has shown that any discrete space of D "homogeneous and isotropic" dimensions synchronized by a universal ordering operator can have no more than *three* indefinitely continuable dimensions; three separate out and the others "compactify" after a surprisingly small number of constructive operations. The proof rests on the fact that if we consider D independently generated Bernoulli sequences (i.e. arbitrary sequences of the symbols 0, 1), Feller (8) has shown that the probability that after n synchronized trials all will have accumulated the same number of "1" 's is less than $n^{-\frac{1}{2}(D-1)}$. It can be shown that the requirement that $D+1$ strings of length n discriminate to the null string is equivalent to Feller's condition. Consequently the probability of continued sequences of events involving D labels vanishes like $n^{-\frac{3}{2}}$ for $D = 4$, and increasingly rapidly for higher numbers. Applying McGoveran's Theorem to the label space allows us to understand why there are only three asymptotically conserved quantum numbers. We have mentioned fermion number, weak isospin and baryon number in making connection between the first three levels of the hierarchy and the standard model. Once we have made this identification, the colored quarks and gluons *have* to be confined independent of any "dynamical mechanism".

To map bit-strings onto integer and half-integer coordinates first note that the Hamming measure $a := \Sigma_{s=1}^{S} b_s^a$ takes the null string as the "reference ensemble" in McGoveran's definition of *attribute distance* (see 5). We restore the symmetry between the symbols "0" and "1" by using for our measure the signed coordinate $q_a(S)$ defined by

$$-\frac{S}{2} \leq q_a := a - \frac{S}{2} \leq +\frac{S}{2} \tag{3.5}$$

There are $2S$ such integrally spaced coordinates for S even and $2S+1$ for S odd. These integer *or* half-integer coordinates can be related to the usual angular momentum "space quantization" of elementary quantum mechanics by defining

$$J(S)cos\ \theta_a := q_a;\ J^2(S) := \frac{S}{2}(\frac{S}{2}+1) \tag{3.6}$$

Then integer steps correspond to "rotations" leave the string length and hence J^2 invariant. Alternatively we can define

$$\tau(S)cosh\ \beta_a := q_a;\ \tau^2(S) := \frac{S}{2}(\frac{S}{2}+1) \tag{3.7}$$

with $\beta_a = \frac{2a}{S} - 1$ and "Lorentz transformations" which leave $\tau^2(S)$ invariant. Extending these definitions to 3+1 dimensions for 4-events as defined above, we find that we can map the content strings (space-time) onto the C4 Clifford algebra (quaternions) in Greider's (9) formulation of non-interacting relativistic quantum mechanics for particles and fields. This fact can be used to establish the "Poincaré invariance" of our representations in the context of our integer restrictions that make all 4-vector components signed integers or half-integers. Applied to our finite label space, this mapping also can be used to establish the conservation of fermion number, weak hypercharge and baryon number across the intervals connecting two scattering events.

GRAVITATIONAL STABILIZATION OF THE PROTON

In order to connect our dimensional constants to quantized particle physics, we assume that N states of mass m are bound together by Newtonian gravitation to form the largest possible mass allowed within their common Compton wavelength $\hbar/mc$. Adapting an argument given by Dyson (10) for quantum electrodynamics to gravitation (Noyes, 11) we take $NGm^2/r = NGm^2/(\hbar/mc) = mc^2$. Trying to add one more particle will create a free particle of energy mc^2 in addition to this "Laplacian black hole"; in other words, this small black hole is indubitably unstable against Hawking radiation once we try to go from N to $N+1$. Hence the largest possible mass for an *elementary system* is indeed the Planck mass. If we take this maximum number N to be the terminating cardinal of the CH, $m = (\hbar c/G)^{\frac{1}{2}}/(2^{127}+136)$ and we find that m is equal to the proton mass to an accuracy of about 1%. The unit of *particle* mass for our theory can be taken to be the proton mass. (A correction we will not have time to discuss here brings Newton's constant G computed from m_p, c and $\hbar$ into agreement with experiment, as noted in the Table.) Our interpretation of this calculation is that the mass of the proton is due to its gravitational self-energy, necessarily finite in our theory. For us, as for Wheeler (2), both black holes and the Hawking radiation are basic; the two approaches are closer than one might think at first glance.

QUANTUM GEONS

Looking at our interpretation of the labels (6) in more detail, we see that electromagnetism enters only after we have constructed the third level of the CH; this is where we have the first opportunity to interpret the cumulative cardinal 137 as a first calculation of $\hbar c/e^2$. As was discovered by Parker-Rhodes (12) and afterwards argued by us (Bastin, et. al., 4) once we have accepted the proton mass (now gravitationally generated) as specifying our local unit of mass, we can calculate the electron mass as due to its electromagnetic self-energy and obtain the surprisingly accurate result given in the Table. This calculation is reviewed below. Before our construction reaches level 3, we have only the 3+7=10 states of the first two levels of the CH. These cannot as yet refer to electromagnetism. For massless content strings, We interpret these ten labels as two chiral neutrinos, two chiral photons, five chiral gravitons, and the ubiquitous "interaction" represented by the anti-null string $b_s^1 = 1$ for all s. This string couples *strings* of any composition into a possible metric relationship. We interpret this string as an early version of the "Newtonian" interaction which ties all identifiable objects together. For massless content strings it will have a "coupling constant" of 1/10, which will become weaker and weaker as more and more degrees of freedom are constructed until the closure of the hierarchy labels allows us to interpret it as "Newtonian gravitation".

Because we start out with massless states, one would think that only two chiral gravitons are allowed. But thanks to the "gravitational" self-interaction, we can form massive objects ("quantum geons") and hence macroscopic orbits relative to which all five states of the chiral gravitons with spin 2 can be defined. Note that this is basically the same argument we used to correctly calculate the precession of the perihelion of Mercury in the paper presented at the first conference in this series (Noyes, 13). As our construction proceeds, we will get one of these "quantum geons" with relative probability 1/10 compared to the probability of getting "visible matter" of 1/127. Therefore our candidate for "dark matter" should be 12.7 times as prevalent as visible matter, which is consistent with current observations.

COSMOLOGICAL CONSEQUENCES

Thinking about this construction, we realize that there will be N^2 initial scattering events which conserve baryon number, providing our universe with this number of baryons, and hence about 1% of the closure mass. Our 256^2 initial and 256^2 final states would, in the absence of further information, be equally divided between baryons and anti-baryons, i.e. on the average contain an equal number of zero's and one's, leading to baryon number zero for the universe. However, the asymmetry inherent in our construction stemming from the special role played by the null string in discrimination and the CH requires us to start the labels with a one, rather than a zero. This asymmetry will persist throughout the statistical "averaging" which follows. In our theory strings with an odd number of "1" 's correspond to fermions; we expect $1/256^4$ baryons per photon in our universe, which is about right (see Table).

Our time steps are of length h/m_pc^2 once the universe is dilute enough so that we can make a linear local connection between space and time, and recognize electromagnetic processes as improbable by about one part in 137 compared to "first law motion". It takes at least N^2 events (TICKs) after the label strings have closed to construct content strings (space-time) which has these properties and the gravitational scale for stabilizing m_p at the value which freezes the time-step. Using a linear time scale (i.e. backward extrapolation from this stage of the construction), this marks a transition between and "optically thick" and an "optically thin" universe. We call this backward extrapolation to the start of the content string — label string boundary "fireball time". Using the linear scale gives us 3.5 million years. This is consistent with our other numbers and the currently observed $2.7^o K$ cosmic background radiation.

Having established our gravitational-cosmological framework, the constructive enterprise can now address more local questions about particle masses and coupling constants. After protons, the other easily recognized stable mass value is the electron mass, so our next step is to calculate their ratio.

THE PROTON-ELECTRON MASS RATIO

An elementary starting point for the calculation of the electron-proton mass ratio is the assumption that, just as we have seen that the proton mass can be generated gravitationally, the electron mass can be generated electromagnetically. Although we could talk about this as the self-energy of the electron due to its interaction with vacuum fluctuations — whose only constituents we can recognize at this point in the construction are proton-antiproton pairs, the coulomb interaction and/or gamma rays — it is simpler to calculate the mass of the electron as generated by its charge by taking some appropriate finite statistical average over its electrostatic self-energy

$$m_e c^2 =< e^2/r > \tag{3.8}$$

Our unit of length for a spherically symmetric system is the proton Compton radius $h/2m_p c$. The system has spherical symmetry and the calculation occurs before we have enough information about other quantum numbers to add any additional degrees of freedom. Consequently we cannot use the corrected (or empirical) fine structure constant, but must use the combinatorial hierarchy value 137 to define our unit if charge, i.e. $e^2 = \hbar c/137$. Since the fluctuations involve both charged (eg proton-antiproton pairs) and neutral (eg γ-rays) particles, the charge fluctuations are independent of the space-fluctuations, but must conserve charge, i.e. $e \rightarrow xe + (1-x)e$ where x is some statistical variable; the contribution of the fluctuations outside of the range $0 \leq x \leq 1$ must cancel by symmetry. Hence

$$< e^2 >= \frac{\hbar c}{137} < x(1-x) > \tag{3.9}$$

[Here follows the Parker-Rhodes calculation.]

This completes our gravitational-electromagnetic unification at the level of the static (Newtonian and Coulomb) interactions exemplified experimentally by the two stable particles with masses m_p and m_e whose masses we have calculated relative to the Planck scale.

WEAK-ELECTROMAGNETIC UNIFICATION

Our connection between quantum numbers and space-time requires that $G_F m_p^2 \approx [\sqrt{2}(265)^2]^{-1}$, which is good to better than 7%, and McGoveran's correction (see Table) brings this reasonably close to the empirical value, as does his correction of our original estimate $sin^2\theta_{Weak} = 0.25$. The definitions of coupling constants and our bit string representation of the quantum numbers require, at this level of accuracy, that

$$M_Z^2 = M_W^2/cos^2\theta_{Weak} \tag{3.10}$$

We have seen above how the electromagnetic interaction of the electron with the vacuum fluctuations dominated by proton-antiproton pairs explains m_p/m_e in terms of a statistically calculable geometrical factor. But since the electron also couples to the vacuum fluctuations of the $W-\bar{W}$ and $Z-\bar{Z}$ via the massless neutrino in the same geometrical fashion, self-consistency requires that the calculation using the Fermi interaction rather than α must lead to the same electron mass. Chasing this through, we find that

$$M_W^2 = \frac{\pi\alpha}{G_F\sqrt{2}sin^2\theta_{Weak}} \tag{3.11}$$

Note that we achieve a good first approximation ("tree level" in the conventional jargon) to weak-electromagnetic unification *without* invoking gauge bosons. In fact, if a negative prediction counts as a prediction, I will stick my neck out and assert that the Higgs boson will not appear during the next decade in any non-controversial form.

SEWGUT

The research goal of many contemporary elementary particle physicists is to find, establish or create a strong, electromagnetic, weak, gravitational unified theory (SEWGUT). For many theorists, the gravitational aspect of a research program aimed at this goal ("quantum gravity") is both the most challenging technically and the most difficult conceptually. Thanks to the CH and the *ordering operator calculus* we have been able to pick up the stick by that end, and construct a *particle* theory in agreement with experiment to first order in $e^2/\hbar c$ for electromagnetic effects, in $G_{Fermi}m_p^2/\hbar c$ for weak effects, in $sin^2\theta_{Weak}$ for weak-electromagnetic unification, and in $G_{Newton}m_p^2/\hbar c$ for gravitational effects. We have also shown that the gross cosmological consequences of our theory are at least roughly in accord with current observational facts as conventionally interpreted. This closes off our theory at the other end of the gravitational scale. What remains is to connect all this up with the strong interactions (*quantum chromodynamics* or QCD) self-consistently.

The three axes in our label space, which we have chosen to name fermion number, weak isospin and baryon number relate to the first three levels of the combinatorial hierarchy and provide precisely the quantum numbers needed for describing the first generation of the standard model of quarks and leptons, as has been known for some time (6). Other conserved quantum numbers such as electric charge, lepton number, or weak hypercharge correspond to rotations and renamings in the 3-dimensional label space. Color confinement occurs naturally, thanks to McGoveran's Theorem, since the three axes mentioned exhaust the absolutely conserved quantum numbers. This is our version of "compactification". Our original bit-string representation of the first three levels of the combinatorial hierarchy used up $2+4+8=14$ of the sixteen slots available. Unfortunately we did not see in time that these provide a natural way to close off this structure with three generations, so we did not "predict" the width of the Z_0 before it was measured. But this clue has led to new results.

With this much solidly established, we can, tentatively, follow up our clue about the second and third generations by suggesting that the muon mass $m_\mu = 3\times 7\times 10\ m_e \approx 210\ m_e$, and (less clearly) that the τ-lepton mass $m_\tau \approx 21 m_\mu$. The first prediction can be checked by chasing through the consequences in $\pi-\mu$ and $\pi-e$ decay lifetimes, the Goldberger-Trieman relation, and all that. In principle these are now all calculable, finite and if they don't come out approximately right will give us a lot of headaches. Should this happen the discrepancies could be serious enough to cause me to abandon the whole scheme — as would a failure to get a good approximation for the Lamb shift to the next order in α.

[*Here follows the handy-dandy formula and the strong interaction calculation of the pion mass.*]

We have a second way to calculate the mass of the pion, which goes back to our version (11) of Dyson's argument (10) applied to electromagnetism rather than gravitation. Consider an assemblage of N_e charged particle pairs each of mass m in a volume whose average radius is the pair-creation radius $\hbar/2mc$ and whose electrostatic energy is

$$N_e\frac{e^2}{r} = N_e\frac{e^2}{(\hbar/2mc)} = N_e\frac{e^2}{\hbar c}(2mc^2) \approx \frac{N_e}{137}(2mc^2) \qquad (3.12)$$

Thus when the number of pairs exceeds 137, we have enough energy to create another pair. Dyson used this fact to argue that the QED renormalized perturbation series with $e^2 \to -e^2$ begins to diverge beyond 137 terms, and hence that the series is not uniformly convergent. I prefer to interpret the result as saying that we cannot *count* more than 137 charged particle pairs within their own Compton radius. If we take the smallest known stable mass — i.e. the electron mass m_e — for m we have an explanation for the termination of growth of the system. At the level of analysis we are invoking 2×137 particles, half electrons and half positrons, within their individual Compton radius are indistinguishable from a neutral pion with $m_{\pi^0} < 274 m_e$. The system is electrostatically bound. Of course this assemblage is unstable against 2γ decay, but if we add an electron plus an anti-neutrino (or a positron and a neutrino) to the assemblage the lifetime becomes much longer and the sum of the masses of the constituents comes close to $m_{\pi^\pm}$. For recent corrections due to McGoveran which bring these original estimates for m_{π^0} and $m_{\pi^\pm}$ into agreement with experiment, see the Table. We also note that this gives us a start toward understanding why the range of nuclear forces is half the classical electron radius $e^2/m_e c^2$, and the dimensional memnonic

$$e^2/m_e c^2 \ (nuclear) = \alpha(\hbar/mc) \ (QED) = \alpha^2 (me^2/\hbar^2) \ (atomic) \tag{3.13}$$

which I learned from Joe Weinberg in 1947.

Invoking our original S-matrix argument appropriately rewritten for massless constituents, this gives us $7m_\pi = m_N$, which is clearly consistent both with our calculation of the pion as 137 electron-positron pairs *and* with our calculation of $G^2_{\pi N \bar{N}} = 14$. The theory is starting to meet self-consistency checks.

The next step is to note that we are now in a position both to calculate the nucleon mass from a relativistic version of the Chew-Low bootstrap *and* from a constituent quark model starting from massless quarks, using a version of finite particle number relativistic scattering theory which I have been developing along another line of enquiry. This should give us some clues as to the relationship between current and constituent quark masses and the pressing problem of modeling "hadronization" in a simple way. If the weak interaction sector involving $\pi - \mu - e$ works out all right, we can then bring in the strange quark and the weak K-decays to sort out the states, and go on from there to u, d, s strong interaction dynamics. Charm and beauty should follow in due course. Then on to the top!

We conclude with the conjecture that following through the implications of our construction will lead to a theory which — at least to first order in $e^2/\hbar c, G_{Fermi} m_p^2/\hbar c$ and $G_{Newton} m_p^2/\hbar c$— that provides a self-consistent unification of strong, electromagnetic, weak *and* gravitational interactions (SEWGUT).

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Table of Results, June, 1990

General structural results

- 3+1 asymptotic space-time
- combinatorial free particle Dirac wave functions
- supraluminal synchronization and correlation *without* supraluminal signaling
- discrete Lorentz transformations for event-based coordinates
- relativistic Bohr-Sommerfeld quantization
- non-commutativity between position and velocity
- conservation laws for Yukawa vertices and 4- events
- crossing symmetry, CPT, spin and statistics

Gravitation and Cosmology

- the equivalence principle
- electromagnetic and gravitational unification
- the three traditional tests of general relativity
- event horizon
- zero-velocity frame for the cosmic background radiation
- mass of the visible universe: $(2^{127})^2 m_p = 4.84 \times 10^{52}\ gm$
- fireball time: $(2^{127})^2 \hbar / m_p c^2 = 3.5$ million years
- critical density: of $\Omega_{Vis} = \rho/\rho_c = 0.01175$ $[0.005 \leq \Omega_{Vis} \leq 0.02]$
- dark matter = 12.7 times visible matter [10??]
- baryons per photon $= 1/256^4 = 2.328... \times 10^{-10}$ $[2 \times 10^{-10}?]$

Unified theory of elementary particles

- quantum numbers of the standard model for quarks and leptons
with confined quarks and exactly 3 weakly coupled generations
- gravitation: $\hbar c/Gm_p^2 = [2^{127} + 136] \times [1 - \frac{1}{3 \cdot 7 \cdot 10}] =$
  $1.70147...[1 - \frac{1}{3 \cdot 7 \cdot 10}] \times 10^{38} = 1.6934... \times 10^{38}$ $[1.6937(10) \times 10^{38}]$
- weak-electromagnetic unification:
  $G_F m_p^2/\hbar c = (1 - \frac{1}{3 \cdot 7})/256^2\sqrt{2} = 1.02\ 758... \times 10^{-5}$ $[1.02\ 684(2) \times 10^{-5}]$;
  $sin^2\theta_{Weak} = 0.25(1 - \frac{1}{3 \cdot 7})^2 = 0.2267...$ $[0.229(4)]$
  $M_W^2 = \pi\alpha/\sqrt{2} G_F sin^2\theta_W = (37.3\ Gev/c^2 sin\ \theta_W)^2$; $M_Z cos\ \theta_W = M_W$
- the hydrogen atom: $(E/\mu c^2)^2[1 + (1/137 N_B)^2] = 1$
- the Sommerfeld formula: $(E/\mu c^2)^2[1 + a^2/(n + \sqrt{j^2 - a^2})^2] = 1$
- the fine structure constant: $\frac{1}{\alpha} = \frac{137}{1 - \frac{1}{30 \times 127}} = 137.0359\ 674...$ $[137.0359\ 895(61)]$
- $m_p/m_e = \frac{137\pi}{\frac{3}{14}\left(1 + \frac{2}{7} + \frac{4}{49}\right)\frac{4}{5}} = 1836.15\ 1497...$ $[1836.15\ 2701(37)]$
- $m_\pi^{\pm}/m_e = 275[1 - \frac{2}{2 \cdot 3 \cdot 7 \cdot 7}] = 273.1292...$ $[273.12\ 63(76)]$
- $m_{\pi^0}/m_e = 274[1 - \frac{3}{2 \cdot 3 \cdot 7 \cdot 2}] = 264.2\ 1428..$ $[264.1\ 160(76)]$
- $(G_{\pi N}^2 m_{\pi^0})^2 = (2m_p)^2 - m_{\pi^0}^2 = (13.86811 m_{\pi^0})^2$
  [( )] = empirical value (error) or range

Appendix I
A NOTE ON PRIORITIES IN THE CONSTRUCTION OF PHYSICAL SPACE

Ted Bastin

March 1990

The picture provided by David reminds me a bit of the Summae of Thomas Aquinas. David has simplified his enormous task by making a separation of the material into two. Thomas has the systematic natural theology, which is the domain of reason, and the revealed truth which has to be incorporated in the natural theology but which we can neither justify nor disprove. David's operator calculus is the former, and the hierarchy together with some of program-universe is the latter. There are some puzzling correspondences between the two, and these are my present topic.

I take David's account of dimensionality to get me going because this provides the strangest of these correspondences. I think we need to think out clearly whether (rejecting the revealed truth explanation) the appearance of dimensionality in both types of argument is an accident or coincidence, and, if not, which is the more fundamental appearance of it, and which the consequence. If we reject coincidence, it has to be one way round. David starts by requiring that a correct representation of dimensionality should "use a metric criterion which does not in any way distinguish one dimension from another." He says that "in a continuum theory we would call this the property of "homogeneity and isotropy," though in fact this analogy short-circuits several vital arguments: it is the first statement which is seminal. Historically it was so, for it was *precisely* this argument which started off our whole enterprise. Clive and I (Concept of order I) asked 'what *was* physical dimensionality?' and concluded that it must be defined as a property within a formal structure in which the mathematical relationships would all remain unchanged as regards truth if the dimensions were interchanged in any possible way. This property we called 'similarity of position.' A weaker condition was called 'simultaneity' in recognition of the fact that if the mathematics permitted us to give any preferential order, then this order could, and would, be used to define temporal relationships. If there were no such order definable then things would be simultaneous. The 'theory-language' embodying this requirement was seen as the simplest level of a hierarchical structure in which we conjectured that the scale-constants would play a part. We had no idea what; though we were clear we had to look elsewhere than Eddington's way of relating levels and calculating the constants. When Frederick solved these problems we found that the 'similarity of position' property was indeed possessed by the level with three dcss however one chose the generators.

For a very long time I was unable to imagine why we had the dimension structure appearing in the succession of levels as well as the 3-level. I felt sure that the former was the primary case even though that did not fit with any interpretation of the quantum numbers. I only now see that I was still trapped in a classical way of thinking about dimension structure even after all that history, and it took David's presentation at ANPA three (or was it four) years ago to awaken me from my dogmatic nightmares. What I got from him was that it required a recursive structure to define the metrical aspect of dimension even after the combinatorial condition of similarity of position had been provided. The reason is very fundamental, and goes like this: at the basis of the recursive structure of the hierarchy is the idea that we can always collapse our description back down through the levels by grouping sets of elements together and treating them as single elements. Now it must not matter for any assignable mathematical reason which element we finish up at. To put it another way, if there were a substructure in the basis grouping from which we could erect our hierarchy then this would be available to use as a single unit in its own right. This requirement links the recursion with the similarity of position ("isotropy"). It also shows that the recursion collapse must be the combinatorial germ of metrical thinking.

(Scarrott uses a similar argument to show that any concept of *information* capable of introducing meaning - which Shannon/Weaver doesn't - must be recursively structured.)

Now David's current discussion of dimension structure using Feller's result is quite different from all this. Indeed he may repudiate all I say about his thinking. Nevertheless I think both that the connexion which I have been describing is very deep and I got it from David.

I interpolate the comment here that the unification of two meanings for 'dimension' is urgently needed to explain how quantum-number structure can come to have any correspondence with classical ideas of fields, spin and so on. Pierre, in discussion in the autumn was inclined to regard this as a fortunate accident, but I now think we can do better than that. I also point out that in Clive's recent reformulation of the hierarchy structure we are compelled to be flexible about levels; for example the entities in the background have no level defined. This relativism is necessary for my earlier argument, and it was partly the rigidity implied by older understanding of the hierarchy levels that held me up.

David has been a bit hard to pin down about the place he sees the hierarchy occupying in his operator calculus. I think he would like to see it as an *example* of his general and universal scheme. There are a variety of difficulties in that way of thinking (which is why recourse to a revelational role for the hierarchy is tempting.) The Parker-Rhodes cut-off has a certain resemblance to the McGoveran-Feller theorem in that both depend upon rejecting statistically unlikely circumstances. There the resemblance ends. In particular the Feller result requires limiting arguments which it is difficult to give a combinatorial meaning to. I believe Stapp pointed this out, and Pierre and David argued briefly that an alternative combinatorial account could be provided. Unfortunately I cannot remember which document I found that in. By contrast, the Parker-Rhodes cut-off occupies an integral place in Pierre's by now very impressive account of particles derived from scattering and the coupling constants, in the second order approximation.

I hope I have by now said enough to exhibit both the sharpness and the importance of the conflict between the two methods. We have to face up to it. I put forward the following solution for consideration. We take the combinatorial hierarchy account of the origin of dimension structure as the primary one. Then we imagine David asking the question: - is there a statistical treatment of the same problem using something we could plausibly regard as exhibiting an equivalent cut-off, but with a meaning for dimension more like the conventional one? But what is the conventional one? we immediately ask. Here there is scope for invention since there is no classical account of dimensionality which does not depend upon imagined bodily experience. David uses this flexibility in the following way: he adopts the Feller result and the 'isotropy' and then *deduces* the shape that what he calls metric points must take in order to fit in with what he has adopted. The result is his representation of metric space.

This derivation of metric points would have the right form to give Pierre's conservation theorem, though it would now be obvious that it was quite unjustified to drag in the idea of anything being conserved. We notice that we can now use the dimensions as labels for quantum numbers in the restricted sense that they are independent and can be recognized as independent experimentally. Thus we can now say that a motion requires one, two or three labels to specify it, but we can't attach angles. However this is a great step. At this stage we can also introduce the relationship of three-and four-vectors. The four-vector has nothing to do with extending a three-vector by adding another place. (It certainly has nothing to do with a change to a relativistic point of view. I think we are automatically working in a relativistic frame if we accept Pierre's present views on the photon, which seem very satisfactory to me.) The relationship between the vectors is a level change from requiring two strings at level one to having one at level two. By my old argument we get spin into the system by making this change. All these changes have become possible because David has essentially redefined 'dimension.'

Now there is another aspect to the puzzle. David speaks of the metric points as being synchronized, thus referring to his second order correction of the fine-structure constant where the number of ways of performing the synchronization gave the correction. Here the number of metric points is indefinite, and it looks as though he is using the same argument as I suggested in defining bound and free states in a note that I wrote not long ago. There I followed Clive in making a distinction between a state in hierarchy construction where one has a completed level and that where one is in the stage of constructing a level. In the latter state one can go on forever. (Clive found it puzzling that one *could* jump into a new level at the first opportunity but never *had* to.) The indefiniteness is equivalent to mapping a geometry onto the dimension structure, and I think we really had now bridged the gap between the combinatorial and the geometrical with David's synchronization as the linking concept.

I go back to Clive's letter of 6/11/89 on conservation at 3- and 4- vertices. He observes that the former cannot conserve both energy and momentum whereas the former [latter ?] must, saying that Pierre would think this too well known to need saying. We might use this as the break-into point for the metrical space by requiring that in going from the combinatorial dimensionality to the metrical one in David's form we -as it were- compensate for the change by treating the combinatorial inexactness (need to impose synchronization) by metrical inexactness which means variable momentum and or energy and or experimental association of angles with counts. (All these things come together and we can't yet describe them separately.)

At this point I ought to start reinterpreting all this in terms of Hamming distances and David's representation of metric points on indefinite strings, but I am going to make a break for first reactions. My whole argument depends on the absolute need to reconcile David's dimensionality theorem with the hierarchy (which is what he mainly uses.) The way I do this is strange and it is crucial that it be right. There are various advantages which come by the way -some more obvious than others. To my mind, the most important is that we have started the job of saying what the quantum numbers are, instead of using the word 'spin' (in particular) and by default saying 'everyone knows what spin means.'

Appendix II: David McGoveran to Ted Bastin, April 12, 1990

I find the paper very interesting and do have a few comments. Some may not get into this response, but I'll complete them as time permits.

Your comparison of my thinking to Thomas Aquinas' systematic natural theology and revealed truth has potential. While I would agree that the ordering operator calculus (OOC hereinafter) might be akin to the former (at least in intent), I would say that laboratory physics is closer to the "revealed truth". For me, the combination of the hierarchy (CH hereinafter) together with Program Universe (or bit string physics more generally—and PU hereinafter) is to be a representation of laboratory physics (LP hereinafter) in terms of OOC. One of the reasons that I accelerated the development of OOC and then applied it to LP was that I did not find the foundations for either CH or PU compelling: the former is not mathematically rich enough for my taste and the latter was too loosely expressed.

OOC may not be acceptable to some, but it is my best effort to provide a rich and rigorous mathematical system which could then be used to combine all these various ideas with which we have played around, and detect and eliminate any contradictions or inconsistencies. If I have been elusive about the relationship between CH and OOC, I apologize. I thought it was clear. OOC can be used to express CH and its results, just as can the various other branches of mathematics which Clive has used to provide various "foundations" of CH. It is a formal system that happens to be context sensitive, and so leads to a different interpretation than other systems.

Please remember that "Foundations" was clearly split into two parts: the mathematical part and the application to LP. CH and PU only occur in the second part. They are not intimately bound to OOC and may have bearing on applications of OOC to other fields (though I think this unlikely). Certainly, I have not used concepts from CH or PU in my other attempts to apply OOC such as linguistics or computer science. For me, attempting to establish a priority between CH versus OOC is like trying to establish a priority between a tool (hammer) and the work (wood): they are both required to achieve anything.

If it is difficult to be precise about CH in terms of OOC it is because OOC makes it clear that the specific evolution of CH is missing: there are many ordering operators which can fill the bill—we only know their general characteristics. OOC deals with the detail of such evolution as well as both the general features and the statistical character. The former is the most important from my point of view. PU proposes no specific algorithm but a class of algorithms. This imprecision makes it impossible to satisfy certain key questions about our model of LP. Having been convinced by you, Clive, Pierre, and John that CH is a fine exoskeleton, I desired the putting in of flesh. CH, as in my fine structure and other constants computations, enters in an essential way as constraints on some of the ordering operators.

I am puzzled that you say that I start by requiring that a correct representation of dimensionality should "use a metric criterion which does not in any way distinguish one dimension from another." The Theorem introduces this notion only by way of constructing a discrete version of an n–dimensional d–space which is "homogeneous" and "isotropic". Prior to this theorem, in Foundations was introduced a definition of d–space and dimensionality which need not be either homogeneous or isotropic. The entire construction relies on these definitions.

I agree that the analogy (though it is more than this) to "homogeneity" and "isotropy" short–circuits several vital arguments. The prescription of synchronization (which occurs in my derivations of the Lorentz transformations, the fine structure, and 3–dimensionality) is far too brief and should have been used to provide formal definitions of discrete versions of these concepts. Once again, time, Oh for more time.

I was not aware that you had previously connected order with temporal indistinguishability or 'simultaneity'. The connection between distinguishability and ordering is a pervasive part of OOC.

I will not repudiate all you are saying, I like most of it.

I would like to understand how CH being an example from OOC (not quite how I see it) leads to difficulties and why recourse to a revelation role (what does this mean? As in Aquinas revealed truth?) for CH is tempting. Can you comment more fully?

I do not see that the cut–off to CH is statistical. For me it is clearly a matter of being unable to preserve certain (highly desirable for a number of reasons) mathematical properties beyond a certain level of complexity. The CH algorithm described in the ANPA 10 paper reveals this best and most intuitively for mathematical physicists.

Now regarding the difficulty of giving finite combinatorial meaning to Feller's Theorem vis–a–vis statistically unlikely circumstances. While I cannot avoid the statistical character of the proof, I can remove the problem of combinatorial interpretation. This problem arises because of the way Feller invokes convergence and difference theorems and therefore limit theorems. The asymptotic continuation of the combinatorial terms of the series seems to be essential. However, one need not resort to this method to see the validity of the theorem.

In particular, suppose that a $3 + n$ space has been generated up to some finite extent. Because of the probabilities involved, the most dense constructible 1–dimensional d–subspace will have a denser sequence of metric points than every constructible 2–dimensional d–subspace, and the most dense 2–dimensional d–subspace denser than every 3–dimensional d–subspace. However, this situation reverses at 4–dimensions so that the most dense $4 + n$–dimensional d–subspaces are now ordered as less dense than every 5+n-dimensional d-subspaces (where n is an element of 0, 1, 2, ...)! This means that every $4 + n$–dimensional d–subspace is separable into a number of isotropic and homogenous 1, 2, and 3–dimensional d–subspaces, but NOT into isotropic and homogenous 1, 2, 3 and 4–dimensional d–subspaces.

Again, there might be some (and indeed perhaps a large number) of "exceptional" generators of homogeneous and isotropic m–dimensional d–subspaces with $n > 3$. The algorithm for this generator would be deterministic. However, it is my claim that no such deterministic algorithm can be correct for other reasons as explained regarding "arbitrariness" and the very definition of ordering operator in Foundations: the complexity of the algorithm for an ordering operator is such that it cannot be given a full interpretation within the generated system.

For PU, the generators of our d–space, therefore, are of such complexity that the "next" metric mark cannot be represented in terms of all those generated so far. This precludes the possibility that the generation of the space is deterministic in the way required: namely that we can predict deterministically from the d–space generated so far and the distribution of metric marks where/when the next metric mark will be generated. Every c–dimensional d–space with $n > 3$ is not algorithmically extensible within the system. It is therefore subject only to statistical characterization. I realize this is not a formal argument and hope to make it formal in my next major effort: Foundations II.

Not long ago I questioned Pierre's reference to "McGoveran's Theorem" regarding there being only three conserved unique quantum numbers (which I take to mean that only three quantum units or parameters are possible for global descriptions and what you mean by Pierre's conservation theorem). I subsequently convinced myself that it was OK, with the fourth number being only a locally usable number. If this fourth number is color, we have "color confinement" and "asymptotic freedom". Conservation is not the issue here. (Indeed I insist that nothing ever gets "conserved" but that similar structures are recursively generated so that a "conserved property" is found to have the same "value" over some causal trajectory—see ANPA 11 paper.)

The argument is simple. PU generates strings with arbitrary quantum numbers (QNs hereinafter) selected from all those allowed. We can imagine a generation which orders the sets of strings with QNs of each type: a set of strings ordered by spin QN, another by angular momentum, etc. We now synchronize the generators so that a d–space is constructed with a diagonal of n strings, one with each of these QNs and therefore n–dimensions. Feller's Theorem now applies.

I agree that synchronization is the bridge between combinatorics and geometry—at least that is why and how I have used it.

Appendix III.
ELUCIDATION OF TOTAL ATTRIBUTE DISTANCE AND THE FINITE EXPONENTIAL OPERATOR

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A number of papers by myself and (both jointly and independently) by Pierre Noyes have made use of the concepts of total attribute distance, the finite exponentiation operator, and the transport operator. Each of these depend on a method of counting that is unfamiliar to most readers. Inasmuch as the papers have generally been couched in terms of bit strings and the original explanation of these concepts was not elucidated in terms of bit string exemplars, the reader has been left with an undue burden of interpreting a difficult and new conceptual basis for counting as presented in the ordering operator calculus. In some cases, our efforts have contained errors which created a further muddle.

It is my intent in this note to correct the problem, an obligation which I have too long and unintentionally avoided. I wish to point out from the beginning that the original reference was never intended to explain the issues which have risen from applications of the ordering operator calculus. Instead, a "Foundations II" paper has been long planned and was intended to treat the concepts of (1) "non–Euclidean" d–spaces—the definition of a metric introduced in Foundations is positive definite, an unnecessary and generally untrue restriction in non–Euclidean spaces; (2) a mathematical mechanics for dealing with the interactions of ordering operators—this is essential for a thorough understanding of my derivation of the fine structure constant and related problems; and (3) a more general exposition of the relationship between a model of a system and its representation.

Within the current context, this last item is most important. In general, there are always two ways to express context information which will preserve the statistics of a given model. This first is the most difficult part of the ordering operator calculus to keep in mind—the interpretation of the symbols is context sensitive. A context sensitive mathematics has been generally abhorred in both the mathematical and the scientific communities: I hope to show that it has its uses. The second way of expressing context information is to invest a separate symbol for each specific context, under the assumption that the semantics is separable from the syntax. The ordering operator calculus allows this only under the conditions set out by the Separability Lemma.

A significant aspect of the ordering operator calculus is the modeling methodology. A major concern of mine in developing the calculus was to be able to build two arbitrary and not necessarily complete representations of a given system in a common language and then to express the degree to which information preserving transformations will be of unequal representational power—one of the systems will not be rich enough to express all the concepts expressible in the other. This leads to the concept of hidden information.

Understanding hidden information is a good thing: it shows how new information can arise between the interaction of two systems. This is especially true in terms of the statistics of the interaction, as we shall see below. In fact, the combinatorial hierarchy itself is best understood in these terms from my point-of-view—there is a model of an underlying generation scheme characterized by the sequence 3, 7, 127, ... and there is the model of a vector space by which this generation is to be represented. The interaction between the two gives certain statistics and provides a "stop rule" for the combinatorial hierarchy.

Now to the subject at hand. I will not try to relate this treatment to physics—I leave that for my colleagues. Nonetheless I will speak in terms of bit strings with the hope that the topic will be clearer and that translation into useful physics will be easier.

First consider the population of binary bit strings of size N composed of the symbols 1 and #. Each 1 in these strings will represent the occurrence of one distinct event or object which belongs to a certain equivalence class of such events or objects (I will use event forthwith to conserve space and typing). Each # represents a non-event. This is different from saying that # represents a non-occurrence. Each # conveys no information whatsoever about the event—it only holds a place where such an event might have occurred, but did not as far as we know. Thus the "event" is unknown, and we cannot say that it is the complement of a 1 type event (1-event for short). The # just pads each string of k 1's to size N, their position being unimportant. The 1-type equivalence class will be said to contain P (or Q) distinct events—this is called the number of increments I in Foundations.

Suppose that each 1-event is labeled to designate its distinctness in the equivalence class. A common way to do this is to use ordinal labels and create the bit string so that the ordinal labels are in ascending order positionally right-to-left.

When the bit strings are created by sampling from the equivalence class without replacement, we can drop the labels and simply use the ordinal position as an implied label. Then we need some place holder for positions, that are never filled unless the sampling is exhaustive or the size of the sample k is less than P . If, however, we allow for sampling with replacement, the labels cannot be dropped if we are to distinguish strings.

Suppose we ask how many strings can be generated by sampling the 1-events with replacement subject to the constraint that the resultant strings each contain k 1's. Since the # symbols do not convey information with which we are interested for the moment, they can be ignored—dropped from the string altogether: what matters to us is the size of the sample k and the size of the equivalence class.

$$(1_4\#1_3 1_2 1_1\#) = (1_4 1_3 1_2 1_1) \ . \tag{1}$$

Then, for a bit string containing k 1's, the number of bit strings possible is just

$R(N,k,P)$:

$$R(N,k,P) = P^k \ . \tag{2}$$

Now suppose that we have reasons to believe that those #'s are important. There are two statistics that can be given immediately: the number of permutations and the number of combinations.

Consider the number of combinations $C(N,k)$ of k 1's in a string of size N. $C(N,k)$ is just the number of distinguishable strings where the occurrence of #'s matters, but the 1–events are not themselves labeled. The idea there is that it is the context of a 1–event in relation to the #'s and other 1–events that serves to identify it as a specific member of the equivalence class. Thus, if 1' is an artificial label to distinguish it from 1 strictly for purposes of illustration, then:

$$(1\#1') = (1'\#1) \neq (\#1'1) = (\#11') \tag{3}$$

where $\neq$ means "is not identical to". The point here is that a 1–event in a specific context is unique and a distinct member of the equivalence class. So permutating 1–events only serves to change their identity—it does not generate a string distinguishable from the original string.

Under this interpretation, the number of distinguishable strings is just $C(N,k)$:

$$C(N,k) = \frac{N!}{k!(N-k)!} \ . \tag{4}$$

Now consider the number of arrangements of all strings of size N with k 1's were we able to distinguish a 1–event in spite of its distinctness being defined by context, i.e. the distinction between 1 and 1' is know to us. Then:

$$(1\#1') \neq (1'\#1) \neq (\#1'1) \neq (\#11') \tag{5}$$

for counting purposes only. Under this interpretation, the total number of string (both distinguishable and indistinguishable) is just $P(N,K)$:

$$P(N,k) = \frac{N!}{(N-k)!} \ . \tag{6}$$

Now, regardless of the number of #'s in a string, the frequency probability of distinguishable strings for fixed N and k is just:

$$\frac{C(N,k)}{P(N,k)} = \frac{1}{k!} \ . \tag{7}$$

Notice that dependence on N vanishes.

Now back to our value for $R(N,k,P)$. Let P be bounded from above by the maximum number of distinct 1's to be found in the construction of the sample space of strings given by $P(N,k)$. We can now ask the key question. On the average and for fixed N, k, and P, what is the number of strings in the population of strings constructed by sampling with replacement which are distinguishable in the sense given by $C(N,k)$? This number is obviously:

$$R(N,k,P) * \left[\frac{C(N,k)}{P(N,k)}\right] = \frac{P^k}{k!} \ . \tag{8}$$

Three points of direction:

(1) In constructing the transport operator, the increment I is replaced by an operator $e\ d/dp$ since it is, in the general case, dependent on the particular parametrization of the coordinate x^i. The summation of terms like (8) for all values of k from 0 up to some K leads to the finite exponential. This corresponds to constructing a network of discrete Feynman paths where each real node is represented by a 1 and each "imaginary" node is represented by a #. It is a 1–dimensional discrete Feynman kernel. Note that the #'s are essential to the statistics. [Aside: The transport operator was constructed in non–Euclidean d–space, as were the Lorentz transformations—a fact I failed to make explicit in Foundations.]

(2) When constructing the Dirac, it is essential to note that the P (right turns strings) and Q (left turns strings) are constructed independently. They are allowed to mesh because of the #'s in each string which preserve a global context or ordering. If the number of distinguishable P and distinguishable Q strings is suitably normalized to the population of all possible strings (this will be constrained by the physics), then the joint probability of the P and Q strings being distinguishable is found by multiplying the independent probabilities.

(3) A representation of ordering operators which I have been using for some time is that of the generator or walk of a directed graph. Any particular directed graph can be represented by an $N \times N$ transition matrix: all nodes are given ordinal labels. There is then one row and one column in the matrix for each node and a 1 in a cell represents a connection from the row node to the column node. In this context, it is interesting to note that $P(N, k \leq N)$ is the number of sub–matrices with exactly one 1 in each of the N rows and exactly one 1 in each of the k columns. This completes the mapping from bit–strings to ordering operators and simultaneously shows that the permutations correspond to a special orthogonal decomposition of all possible ordering operators.

Appendix IV. From Ted to Pierre, June 21, 1990 (excerpts).

Footnotes by HPN 5 July

It is very relevant that Clive now starts from what is involved in labelling, and as you know, there has been much criticism of the labels in PU because they depend on particular mechanical devices. I appreciate the argument that it was better to have something to use (PU) rather than to wait indefinitely to get everything right. I also know that you feel a bit let down because Clive was involved in your extension of CH and now wants to replace it. He just does make a distinction between getting the mathematics free from errors and the more difficult task of getting a comprehensible intellectual structure.

The matter of labels really brings up the whole quantum-mechanical epistemology issue, and therefore runs very deep. Who does the labeling? I believe that the main virtue of CH is that it puts this question to rest. I always saw the potential for that, but could only present it by a lot of talk. However I think that with Clive's work it is now much more explicit. Support for the PU scheme of labels is drawn from David's definition of "label"; I like that very much but think it applies to computer models and leaves the actual operational connection with physics totally vague.⋆ I mean that I cannot see anything that we do in conventional measurement that corresponds to it.† Of course you jump in at this point and say that most of the thinking of some years has been directed one way or another to establish the operation connection of the theory through scattering and counting. I understand that and thoroughly agree with the trend. However that leaves us in a funny position, for what is supposed to be the application of the more general Ordering Operator Calculus? It would seem that it has to be via CH, which you and David don't want.

If it is formal, I can work with it, but it always has been presented as having an immediate physical interpretation, and try as I will I cannot avoid the feeling that I have to be on the watch to see that the new type of physical interpretation is

⋆ HPN is confused by this discussion of "labeling". Neither David nor I have any foundational discussions from Clive subsequent to *Proc. ANPA 10*, and that doesn't mention labeling. David's definition of 'label' occurs in FDP where he introduces the concept of ordering operator: "The output which results from using the ordering operators in either of these first two cases is two indistinguishable but sequence ordered object descriptions which we will call **labels** for short. [Footnote: We suggest the use of ***tags*** where the term labels would be otherwise confusing as, for example, in Noyes [Joensuu, '85] where the label refers to a particular kind of label in our sense of the term.]" — FDP, p.9. Are you referring to the first sense? or the second? or something else? Available and explicit references are badly needed here.

† HPN claims that any assignment of mass, charge and other quantum numbers to "the conceptual carrier of quantum numbers between two discrete events" in laboratory physics has a well specified connection with the bit-string labels used in DP and constructed using PU. This includes a class of "measurements" with many unique experiential exemplars which HPN would call "conventional measurements".

not being eked out with traditional constructions — circular arguments resulting. Now I am not grumbling at this long and arduous iteration and reiteration. Is it not what you advocate in your modelling principles?[‡]

In your introduction to "CH and OOC" you say that it is essential to understand "both our modelling methodology and our principles" and add that you thought you had made both these quite clear. You don't consider the possibility that the better one understands, the more one is persuaded that some aspects of the thinking are wrong. You really come quite near to claiming that your modelling principles confer incorrigibility. (Wittgenstein used to say that it was impossible that anyone who understood him should not agree with him; so you are in good company.) The questions I am raising earlier are genuine perplexities about the physical interpretation of mathematical structures (in this case OOC and CH) and how these interpretations are related, and if the effect of your modelling methodology is to suppress that enquiry then I don't find the methodology very helpful. David is very good at presenting work in a formal manner with sets of definitions, and I am very bad, but formal systems (or E-frames)[§] however useful they may be in clarifying one's thought — still leave open the questions: what is this f.s. for? and does it do what is intended? I don't think it makes sense to say "the whole thing is defined throughout, and that is all that one may ask of it." (Of course I do not believe there can be an ultimate distinction between formal expression and figurative expression (analytic and synthetic, or whatever you call it: I suspect that you would say that of your E-frames — that there is corrigibility but that it is just a question of how it is secured).

All of this may explain to you better why my reactions to the corpus of existing work is very variable — ranging from seeing some parts as profoundly right to treating others with a detached caution because I am not sure that they are not derived from more basic work which may contain muddles of the kind I have been exhibiting fear of. I think this variable reaction is what you find unsatisfactory, but also think on rereading the letter which you propose to circulate that I expressed myself a bit brutally.

I need to work at length on the material which you have sent, and will only make one or two simple comments. I know very well that in different ways you and David have got far beyond what I have succeeded in understanding and I feel very inadequate at being so slow. I understand that I have failed to appreciate your point of view on the representation of angles, and what you say seems potentially

‡ I agree completely that it is! — HPN.

§ Ted exhibits a fundamental misunderstanding of the ERP iterative modelling methodology here. The R-frame is a formal system in his sense, whereas the E-frame (in DP, the practice of laboratory physics) contains informal elements. See main text, Sec 1.2.

very satisfying. You say "I am confident that what we need for physics can be constructed just as well from the OOC as from the new foundational approach to the CH," and this seems a happy augury I agree with, though might wish to put it the other way round. You also say "They clearly now do feel it necessary to construct discrimination whereas I am content to define it." I don't think that it is so much discrimination that is at issue as labels. (Though the two are deductively connected.)¶ I would put the stress on "understanding what is involved in labeling" as compared with "defining label". It is a free country; you can define what you like. You can define 'unicorn'. Our quarrel with contemporary physicists is that they are content to define things like Planck's constant whereas we say one needs to understand the necessity for such things.*

As a very general remark anticipating my effort to provide the kind of background you ask for, I would describe my approach as essentially agnostic in the sense that I fix on certain amazing properties of the world and see how one can understand them. If successful calculations follow then so much the better and it is not likely that anyone will pay much notice until they do. It is also essential that one see how one connects with what other people call measurement in a convincing way, and that is a very stringent requirement. However, it does not seem to me to be essential at all to cover the whole of what physics is normally taken to cover. I regard the demand that one do that (and even that current physics does it — though it assumes it does) as an erroneous dogma. it is related to Einstein's demand for 'completeness', and though it would take too long to analyse that idea

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¶ HPN agrees that the important issue is labels rather than discrimination, but needs guidance as to Ted's and Clive's current position on this point, as already noted in my first footnote. My implied reference for the phrase you quote is Clive's paper in *Proc. ANPA 10* — "What do the bits in the Combinatorial Hierarchy mean?", pp 53-71 — which does not contain the word "label", so far as I have been able to ascertain. On p. 54 he lists six results the paper is about starting with "(i) The importance of the discrimination operation;".

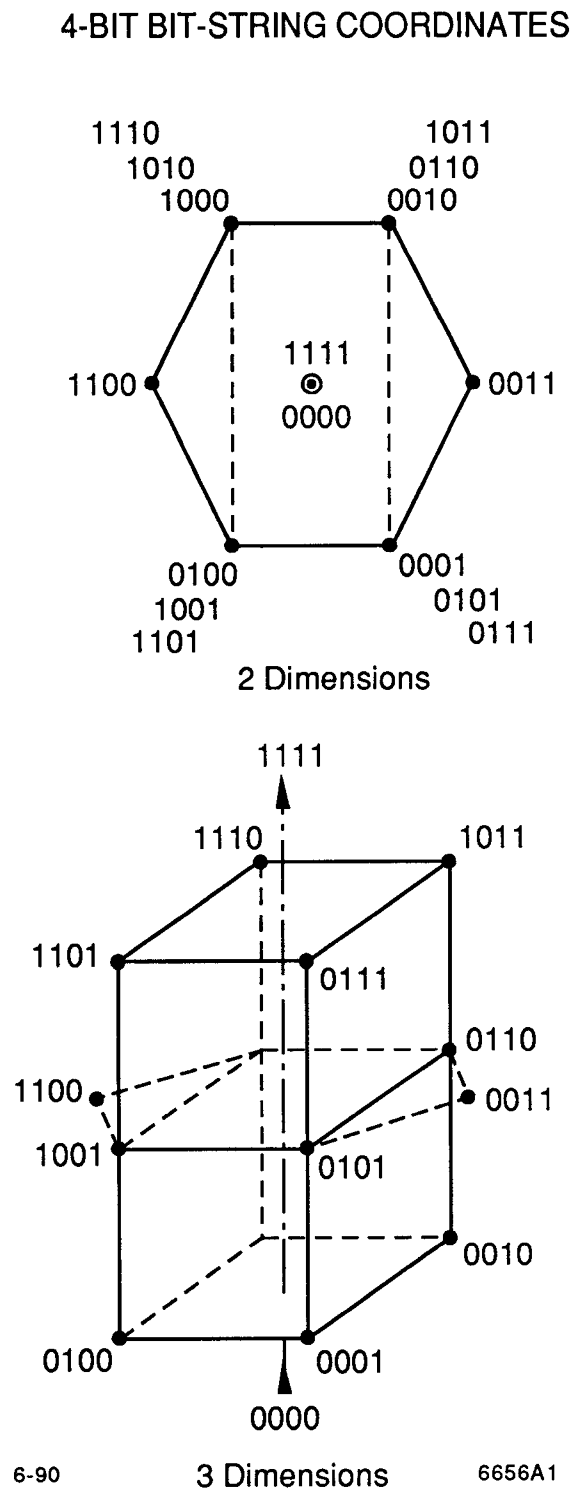
* Once again Ted's confusion between definition in a formal system (R-frame) and "operational" definition in practice (E-frame) muddles the issue. The best of contemporary physicists *do* understand this distinction and clearly separate the self-consistency of a mathematical model from its application to physics, and the experimental procedures by which this application is tested. I agree that they are often content to believe that they are studying the "real world", and are unwilling to examine how many of their results are already implied by their methodology. But this is more of a metaphysical than a methodological quarrel so far as I am concerned. *Unicorn* is still a useful term in the context of "unicorn's horn" as part of the *materia medica* of various Eastern traditions. It raises a problem for conservationists because (contrary to formal definition) two-horned rhinos are acceptable as a source. It is the power of the phallic symbolism (which occurred in another way in the medieval tradition) which needs to be understood when one faces the practical problem of stopping black market rhino hunting. *Practice* and historical context (E-frame concepts) can be more important than formal definition. I claim we need precision in distinguishing the two.

in detail, and though quantum physicists claim to have superceded it, a rump of it seems to me to remain in their thinking and practice.** It's something about the flat objectivity of the world independently of how we know about it. Thus I think that if you were to suggest to an average physicist that it didn't automatically mean anything to insist that either the world was created in a big bang or else that some form of continuous creation took place, because statements about the past must always be seen in the context of how we think we got that knowledge, then I think he would be as skeptical as his predecessor a century ago would have been if you told him that it did not automatically mean anything to assert that one event must precede or else antecede another.***

I have the feeling that your insistence that we must make a bridge into the common language of physics and your elaboration of MM (modeling methodology) owes something to the itch to satisfy physicists on this score of completeness.

** My discussion of this point is in the main text, Sec.1.3.

*** Without a statistical survey, I can't vouch for the "average" physicist. But many physicists I know *do* appreciate the point you are making. Their reactions are various.


4-BIT BIT-STRING COORDINATES
1110
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1000
1011
0110
0010
1100
1111
0000
0011
0100
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2 Dimensions
1111
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3 Dimensions
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Fig. 1