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ABSTRACT

In a preprint submitted to this journal, I.J. Good evaluates Parker-Rhodes' first order calculation of m_p/m_e and our second order calculation of the fine structure constant as if they were pieces of numerology, ignoring the facts that they are derived from a theory, are clearly stated to be first and second approximations, respectively, and that the correction terms are estimated. Admittedly the theory is non-standard, but the criteria for comparison with experiment are normal. A considered attack either on the foundations of the theory or on ambiguities in the calculations (we trust there are no mistakes) would be welcomed as constructive criticism. To call the results of our calculations "numerology" and evaluate them on that basis is more cavalier than academic; carrying out the evaluation using criteria chosen by the author which he himself characterizes as "subjective" is patently unfair.

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In a recent unpublished manuscript by Good¹^{#1} the author discusses several papers by our colleagues and by us²⁻⁴. We enjoyed reading his comments and agree with much of the sentiment that seems to have motivated the work. We certainly appreciate the fact that Good has given attention to our work. However, we must object to the analysis of both our work and that of our deceased colleague Parker-Rhodes which Good presents.

We feel that it is an injustice to these works to criticize a result as “numerology” just because one (admittedly) doesn’t understand the proposed explanation.^{#2} Indeed since Good could at most have seen the preprint of our paper “On the Fine Structure of Hydrogen” (Ref. 3) or earlier versions circulated for criticism^{#3}, and does not cite the relevant background work referenced in our preprint³ — in particular the detailed mathematical and methodological study of the ordering operator calculus⁵ and the examination of how this calculus applied to elementary particle physics and cosmology compares to and contrasts with other approaches to relativistic quantum mechanics⁶ — it is clear that he did not read the necessary material before formulating his attack.

Part of this is our responsibility. The key work on the mathematical and methodological foundations (Ref.5) was formulated by DMcG over a period of years without academic support while he was running a commercial enterprise.

#1 Good, I.J. (1988) Physical Numerology. Technical Report Number 88-26, Dept. of Statistics, Virginia Tech., Blacksburg, VA 24601, December 30. According to the author, this report contains “...three items that should appear in 1989 in the column of Comments, Conjectures and Conclusions in the *Journal of Statistical Computation and Simulation*. We are indebted to the author for sending us this preprint. Since this composite report uses continuous pagination in the lower right hand corner, we will hereinafter cite specific quotations by G #, where # denotes this pagination.

#2 G 27: “I have not yet understood their theory...”; G 35: “Elaborate arguments were provided by the authors of items (ii) and (viii) but I have not yet understood them so in this paper I shall treat them as pure numerology.”

#3 His referencing does not make clear which he had in mind.

Consequently his priorities and resources differ from those of academic professionals. Since this work is only now beginning to be submitted to and to appear in formal publications, it would be surprising if many points did not require iterative clarification. This is hardly a novel situation. Kuhn⁷ makes it clear how difficult the task of getting new ideas introduced into the practice of normal science can be, particularly when the climate of opinion is not recognized as “pre-revolutionary”. We must try harder.

Nonetheless, to have our work publicly criticized and labeled in such an offhand and prejudiced manner prior to publication, as Good proposes to do in a preprint submitted to this journal, demands a detailed response. We use the consecutive pagination of the preprint (Ref. 1) in what follows.

On page 3, Good states “... much bad numerology, mostly unpublished, [is] worth less than the backs of the envelopes upon which it is written.” The implication of this statement is that any unpublished result (even one sufficiently striking to engage his attention) is “worth less”. Since the only work he cites as “unpublished” [rather than indicating that it has been submitted for publication and citing the appropriate preprint] is ours, it would be hard for us not to take this statement personally.

On the same page, Good mentions the conditions under which the correctness of a “numerological formula” might be questioned. Our result³ and that of Parker-Rhodes in our context⁶ are clearly stated to be approximations. In the case of the Parker-Rhodes calculation of m_p/m_e , this is a first order approximation within our theory; we discuss⁶ why the value of 137 (rather than the empirical value of $1/\alpha$) has to be used in this calculation, and anticipate corrections arising from weak-electromagnetic unification, which we estimate. Our calculation of the fine

structure constant and derivation of the Sommerfeld formula are second order within our theory, and will need corrections of order $(1/137)^3$ and $(m_e/m_p)^2$. We find it strange that Good feels the need to single out these results “because they are exceedingly good” in comparison to (a) the empirical values and (b) any other extant theory.

We do not pardon Plato’s vagueness.^{#4} It is precisely this kind of sloppiness that has hampered science for so long and which we and so many others struggle to overcome. Yet Good seems to want to carry this vagueness forward.^{#5} What precise meaning can be given to phrases like “partly correct”, “some reasonable theory”, “possibly unknown”, “might explain”? Such phrases might occur in a campaign speech or legal contract, but we find them out of place in a mathematical argument.^{#6}

We do not condone plucking an explanation “out from Plato’s universe”. Explanations which follow a calculation are without value for us; we are constructive in our approach to both physics and mathematics. Everything must be finitely computable from first principles. We realize only too well that this is not “standard science”. Indeed so much “plucking” has taken place in the last eighty years that physics is a mess of disjointed and shored-up theories and numerological results. Even laboratory (particle) physics is largely a buckshot approach which does not (can not) build on the results of the past. The transition from laboratory to

#4 G 3: “I think an appropriate definition of correctness is that the formula have a good explanation, in a platonic sense, that is, the explanation might be based on a theory that is not yet known but ‘exists’ in the universe of possible reasonable ideas. (Pardon Plato’s vagueness.)”

#5 G 4: “A formula might be *partly correct* in the sense that some reasonable theory (possibly unknown) might explain why it is a good approximation.”

#6 In a business context, Good’s treatment of Parker-Rhodes’ result as pure numerology would be construed as libelous.

theory is so lacking in operationality that it might as well be mediated by a Wall Street ticker tape of numbers. More seriously, given *any* theoretical structure that correlates phenomena in a digital way (i.e. reached in real time as serial and/or clumped counts in detectors), our methodology allows us to dispose of such theories or upgrade them in short order.

We find it intriguing that Good would propose to analyze/evaluate the value of a piece of physical numerology (which according to him is *a priori* “worth less”) via a procedure which is admittedly not objective.^{#7} Question (h) on page 5 is even more curious.^{#8} Why should a totally new formula come along as a set of similar formulae? This contradicts the very nature of fundamental advances in science and mathematics.

Good then introduces measures such as “n.c.s.d.” of his own making and, we suspect, to support his particular judgments. Never mind that the number of significant decimal digits is the accepted standard for the comparison of theoretical to empirical values within the physics literature. Had we used Good’s measure in writing we would have invoked additional criticism.

On page 7 Good introduces his own measure of complexity to link (e) and (f) while admitting that it leads to difficulties with an ordinary disjunctive proposition.^{#9} Yet such propositions abound in current physical theory. Clearly the

#7 G 4: “(e) How complex or simple is it? Here, and in (f), subjectivity can hardly be avoided. (f) What, in some sense, was its prior probability of being correct (without allowing for the experimental results)? (g) If it is consistent with the latest experimental results, is it likely to remain consistent with future experiments? This question can be attacked by Bayes’s theorem if an answer to (f) can be accepted.”

#8 G 5: “(h) Is it a part of a set of similar formulae, in other words, does it satisfy, in a numerological sense, the desideratum of ‘consilience [sic] of induction’? (Whewell 1847/1967, Vol.2, pp. 77-78.”

#9 G 7: “This definition, which links headings (e) and (f), leads to some difficulties when applied to arbitrary propositions (for example $H \vee K$ is less simple but more probable than H),...”

measure is useful only if the number is computed without a propositional basis. This measure could be applied to a numerological result which is devoid of theory. As we have already indicated, this is not the case for our results, as is explained in detail in Refs. 5 and 6. Hence using this criterion in evaluating our results is a glaring error. Further, Good's argument that the value of a number depends on its complexity is circular.

Good's pre-prior probabilities have no relevance to our work or to that of Parker-Rhodes. Our numbers were hardly chosen at random nor were our formulae.

We do not see how one can estimate standard errors when a computation and all that goes into it is purely theoretical. Calculation of the propagation of errors can be appropriate when empirical input of "known" error is combined with a calculation based on a theory. Theoretical calculations such as ours which are part of a scheme of successive approximations, can and should allow an *estimate* of how big the correction terms are. We have provided preliminary discussions of these corrections for both calculations, and are preparing a more detailed discussion. Good's use of pre-prior probabilities on page 9 to evaluate our result *assumes* that it is a piece of numerology — a number chosen at random from a context. This assumption is *false*. Our comparison with experimental results follows standard practice in physics.

Good argues that the structure of physics and mathematics favors composite integers over primes.^{#10} Both number theory and group theory give critical significance to primes. How can Good assume *a priori* that this is not the case for our theory? Indeed he goes on to admit that his procedure does demand "a large

#10 G 11: "For example, we would tend to favor composite integers over primes because many formulae in physics, and in mathematics, consist of the products (and quotients) of various quantities."

reliance on subjective judgment”.^{#11} In our opinion, his reliance on subjective judgments is so great that he would have demonstrated more scholarly integrity had he written down his subjective final judgments out-of-hand rather than obscuring his opinions with pseudo-statistical arguments.

That Good allows a few operations such as “square rooting” (for example on page 11) is peculiar, especially since “square rooting” is not a generally valid procedure within our theory. This is obvious since our theory is discrete, finite and constructive. As has been known since the time of Pythagoras, these requirements restrict the validity of “square rooting” to special cases. We can, of course, find algebraic factors (in general unequal) in the appropriate contexts.

The arbitrariness of Good’s criteria is transparent in his “good ranking” of the number 120.^{#12} Why not justify the subjective judgment on the basis that 120 is 10×12 and also $100 + 20$ (10, 12, 20 and 120 are all nice numbers)? Why not use the reason that it is a factor in some large number $p \times 120$ where p is prime? The same kind of nonsense occurs at the top of page 21. In making his admittedly small sample survey, we wonder what precautions he took to exclude bias due to socio-economic, educational and cultural factors, including his own attitude toward status-quo science. Clearly the ranking would be different if made by a Cabalist, a classical physicist, or a Pythagorean. What significance would a fundamentalist give to 666, or a communist to 1917?

Skipping ahead to pp 25-27, the fact that Good’s piece of numerology does so well compared to QED is a comment on Good’s methods of evaluation, and in our opinion has no relevance to the value of QED.

^{#11} G 11: “For these and other reasons a large reliance on subjective judgment is necessary.”

^{#12} G 14:“(v) The number 120 deserves a good ranking because it is both a factorial and a triangulation of a nice number, 16.”

In the discussion of our calculation starting on page 27^{#13} there are a number of gross errors which we insist should be corrected prior to publication, or if publication has already occurred, be corrected by errata. We are completely puzzled as to why Good thinks 136α (or for that matter 137α) should be treated as a “fundamental constant”. We never said anything like this in any of *our* writings. This confusion is worse compounded when he compares our result with the 136 in *his* formula (1) (page 20) which has nothing to do with us; neither of us had seen this formula prior to reading Good’s preprint.

Good implies by his exposition of our formula that we have engaged in what he calls “one plus exaggeration”. Nothing could be further from the truth. While something like the formula he gives occurs in our work ($1 - \epsilon$), this express occurs as a factor to a polynomial constraint on the system being modeled and is used in solving for the second-order approximation to α . Furthermore, the form of the factors has theoretical meaning and is derived from that meaning. We do not compare our computed value to the empirical value using Good’s straw man of “one plus exaggeration”.

A clue as to where the muddle Good creates might have started goes back to earlier attempts by Eddington to deduce the value of 136 for $1/\alpha$ which later were modified to give 137. *This has nothing to do with our work.* Good is grossly unfair to his readers when he makes this connection. The idea which some of our

#13 G 27: “Another example of a nice-looking piece of numerology, which can look even better by the one-plus exaggeration, is due to McGovern [sic] and Noyes (1988). They pointed out that

$$137\alpha = 1 - 1/(30 \times 127) = 0.99973753 \quad (6)$$

within experimental error, the experimental value being $0.99973756 \pm 0.00000011$. This treats 137α rather than 136α as a ‘fundamental constant’ and therefore (6) and (1) are somewhat in conflict.”

collaborators took from Eddington's work was that it might be the case that some of the numbers which occur in physical theories have more to do with the way they are *computed* and the way these computations are *connected to experiment* than naked empiricism is likely to accept. We agree with Bastin⁸ that "His [Eddington's] attempt to deduce these values was wrong, but the logic of his motivation is difficult to fault."

Good's error in treating the first order calculations of $1/\alpha$ and m_p/m_e as "numerological" has no excuse. He cites our 1979 reference^[2] where the 137 is *calculated*, a preliminary attempt is made to justify the physical interpretation by a lowest order calculation of the discrete structure found in the hydrogen atom, and a preliminary attempt is made to justify the Parker-Rhodes m_p/m_e calculation in our context. We defy him to find any reference to 136, or for that matter to Eddington's arguments, in that work — which he is supposed to have read.

A lot has happened in our theory after 1979. A summary of these structural, cosmological and elementary particle consequences is given in the table which accompanies this article.⁹ This information was also available to Good, if he read our preprint³. This background relates to the fine structure constant calculation in several ways. Note that we have derived both the Lorentz transformations and the commutation relations of quantum mechanics from our fundamental principles. Note that we have also identified the quantum numbers of the standard model for quarks and leptons in our bit string representation of the combinatorial hierarchy.^{#14} We have shown that these conventional quantum numbers are conserved in our elementary (3-momentum conserving) events. In the simplest interpretation of

#14 We return to the question of whether the combinatorial hierarchy [G 28] "... can be assumed to be a correct theoretical foundation for particle physics." below.

our theory congruent with conventional practice in elementary particle physics, the first three levels of the combinatorial hierarchy are: two chiral neutrinos with the appropriate quantum (3 states), electrons, positrons, gamma rays and the coulomb interaction (7 states), the up and down quarks and anti-quarks and their associated gluons in a color octet with color confinement (127 states) which combine in the usual (valence model) way to form neutrons, protons and the π, ρ, ω mesons. The probability of a coulomb interaction, as 1 of the 137 possibilities, follows from this interpretation of our theory. We wonder if Good is technically competent to compare our detailed model with the standard model in a reasonable way. If not, he has a certain temerity in criticizing our results.

We suspect that by now the reader may be tired of following our demonstration of bias in the paper to which we are responding by following it page by page. Unfortunately Good makes a number of glaring errors in the subsequent pages. We collect these in related clumps in order to save journal space and the readers' time.

A particularly obnoxious and denigrating assumption that Good makes explicitly in at least two places^{#15} is that the formulae in question were "created" in order to explain known experimental results. How Good can make such bald statements without an historical investigation, or even noting this obvious scholarly lacuna, boggles the mind. In these two instances one of us (HPN), who created neither formula, knows a great deal about the history.

#15 G 27: "I have not yet understood their theory (which no doubt was constructed after (6) was noticed)..."

G 38: "The explanation would have been far more convincing if he had produced it without first knowing the observed value. (The same statement applies to Eddington's concocted explanations.)"

Work on the Sommerfeld formula was started by HPN because we had in hand both a relativistic quantum mechanics with *relativistic* Bohr-Sommerfeld quantization and a first order treatment of the Bohr atom, as is attested in the *Proceedings of the 9th Annual International Meeting of the Alternative Natural Philosophy Association*¹⁰ #16. The expectation was that even if the derivation of the Sommerfeld formula succeeded, it would still contain the first approximation for the fine structure constant; subsequent calculation of *all* α^2 QED effects would still have to be computed and the correction obtained from a self-consistent modification of the Parker-Rhodes formula for the electron mass. When DMcG called HPN up to report that DMcG had (a) derived the Sommerfeld formula and (b) that the line of reasoning which enabled him to do so had lead at the same time to a second order calculation of $1/\alpha$ DMcG did not have in hand the empirical value, and wanted HPN to check the numerical evaluation of the formula and make the comparison. In particular, DMcG was unaware that in 1986 the accepted value was 137.03604(11) and that the value 137.035963(15) was proposed but still under discussion. We note these two values in our longer paper (Ref. 6), but only subsequently received the new (1988) particle data booklet, which quotes 137.0359895(61)¹¹. They go on to note that this value, according to current theory, is not a constant but depends on the invariant four-momentum Q at which the measurements are made. The value quoted is given for Q at the mass of the electron, which is the appropriate value to compare with our calculation; the value they quote at the mass of the W is approximately 128. We wonder how Good would include this more complicated

#16 “Query 5: Can you by using the relativistic discrete theory obtain the Bohr-Sommerfeld fine structure splitting for the hydrogen spectrum, and by using — instead — the spin degree of freedom, show that this is consistent with the Dirac calculation of the same quantity?” Quoted from Ref. 9, p 134.

physical background in a “numerological analysis”.

HPN started hearing about *indistinguishables* from Parker-Rhodes in 1973 and was also in England in touch with that research group in 1978 shortly after P-R had completed the m_p/m_e calculation and submitted it to *Nature*. Clearly there was at least a five year period between the start of the general theory and this specific calculation. Good’s remark on page 38 that “Presumably Parker-Rhodes constructed his obscure explanation of (viii) after noticing...” cannot possibly be historically correct. That Good presents his own “reconstruction” of the way the calculation went, without a tissue of evidence and in obvious conflict with the many published discussions of the calculation, is simply outrageous.

Parker-Rhodes gave Bastin and HPN permission to report the m_p/m_e result at the 1978 Tutzing conference. This report led, ultimately, to one of the references Good cites (Ref. 2). Naturally we were much concerned as to how Parker-Rhodes had arrived at the formula; we found no evidence that it had been cooked up to meet the empirical value. In fact our discussion in Ref. 2, which Good presumably has read, makes it clear that we had considerable difficulty fitting it into our own framework and at that time could only defend it by heuristic arguments. This in no way detracts from the legitimacy of the calculation *within the The Theory of Indistinguishables*¹².

Subsequent history is also relevant. Parker-Rhodes admits freely that the use of 137 in his formula rather than the empirical value of $1/\alpha$ is hard to understand; this used to be true for us as well. What concerned us more was that we could not decide whether the “three degrees of freedom” he uses refer to three-space or to the first three levels of the combinatorial hierarchy, or both. Now, thanks to

McGoveran’s Theorem^{#17}, it is clear that our theory has to lead to a constructed 3-space; hence this aspect of the Parker-Rhodes calculation has been explained. The fine structure calculation³ makes it clear^{6,8} that in a, necessarily spherically symmetric, “self-energy” calculation, we must use the 137 rather than the empirical value. We would appreciate any criticism of these arguments by Good. We would also like to know why he uses the “empirical” value for m_p/m_e of 1836.15270(10) in his table on page 40 without discussing the fact that it has moved by well over a standard deviation from the 1986 value of 1836.15152(70), and in 1986 was relegated to a footnote. We quote both values in our longer paper⁶, and note that the change is of the order of the weak-electromagnetic unification corrections which we can anticipate. The value quoted in the 1988 tabulation is 1836.152 701(37). These values, unlike $1/\alpha$, are not supposed to “run” with energy at the level of accuracy which is relevant here, according to current theory. Again we would like to hear how Good addresses the question of comparison between our theory and the conventional approach, and whether the same methodology is appropriate in comparison between theory and the “empirical” value in the two cases. This is a question of *physics*, and has nothing to do with “numerology”.

Good’s cavalier treatment of the combinatorial hierarchy on page 27 also calls for a few historical comments.^{#18} The hierarchy was discovered by Parker-Rhodes in 1961 in a search for a simple model that would yield hierarchical structure, and not for the specific numerical values which emerged. The fact that the construction stops when the gravitational coupling constant has been generated was initially

#17 Ref. 5, p 59: “Theorem 13. The upper bound on the global d -dimensionality of a d -space of cardinality N with a discrete, finite and homogeneous distance function is 3 for sufficiently large N .”

#18 For more detail, see Ref. 9, p.117.

thought by Parker-Rhodes to be a failure of the scheme, whereas this result is in fact one of the first clues to physical interpretation. That the last two numbers $(137, 1.7 \times 10^{38})$ correspond, respectively, to the maximum number of charged particle pairs that can be *counted* within their own compton wavelength using QED and to the maximum number of gravitating baryons of protonic mass which can be *counted* within a compton wavelength using Newtonian gravitation was pointed out by HPN in 1973 using an argument originally due to Dyson¹³. Good assumes that just because the sequence $2^2 - 1$, $2^3 - 1$, $2^7 - 1$, $2^{127} - 1$ occurs as an unsolved problem in number theory (whether or not the next number in this sequence of Mersenne primes is itself prime or not) means that it cannot have any physical significance. Such an assumption flies in the face of an enormous amount of physical experience with mathematical structures that found great utility in physics long after their mathematical discovery.

The deeper issue is whether prior knowledge of an experimental “fact” rules out serious consideration of a calculation that provides numerical correlates of that fact. Contrary to Good’s implication on page 38, this happens to be standard practice in physics. Whatever philosophers of science, who rarely are practicing physicists, may say, the sequence of experimental work is rarely, if ever, a theoretical calculation followed by an experimental measurement of a previously unknown number. A relevant instance is the post World War II history of high energy particle physics, which for decades was driven by experimental technology rather than by theoretical insight. Successive group theoretical symmetry schemes (SU_2 , SU_3 , SU_6 , ...) were introduced in order to be “broken” in various *ad hoc* ways and then discarded as more data became available. We assert categorically, as our discussion of methodology^{5,6} elucidates in more detail, that the construction of

physical theories is an iterative process which *starts* from a known empirical situation, constructs a self-consistent representational model that contains no empirical parameters or intuitive empirical ideas, and then uses explicitly stated rules of correspondence to see if the model is adequate to the task. If not, the mistake can lie anywhere in the chain, and the process starts over from this new situation, using whatever insights have been gained in the process.

If Good chooses to call our work “numerology” on the grounds that we know something about the numbers we are trying to calculate, he must in all honesty attack contemporary laboratory and theoretical practice on the same grounds. They are more vulnerable than we are on this score. We are, however, afraid that he will hesitate to make an analysis of “established” theories in the same tone and with the same lack of attention to normal scholarly standards as he used in his paper.

REFERENCES

1. Good, I.J (1988). Physical Numerology. Technical Report Number 88-26, Dept. of Statistics, Virginia Tech., Blacksburg, VA 24601, December 30.
2. Bastin, Ted; Noyes, H.P., Amson, J.; and Kilmister, C.W. (1979). On the physical interpretation and mathematical structure of the combinatorial hierarchy. *International J. of Theor. Phys.* **18**, 445-488.[G 31].
3. McGoveran, D.O. and Noyes, H.P. (1988 November). On the fine structure spectrum of hydrogen. SLAC-PUB-4730, submitted to *Physical Review Letters*. [We have extended here the incomplete referencing given on G 32.]
4. Parker-Rhodes, A.F. (1981). *The Theory of Indistinguishables*. Dordrecht: Reidel.
5. McGoveran, D.O. (1988). Foundations of a Discrete Physics. *Discrete and Combinatorial Physics*, 37-104. ANPA WEST, 25 Buena Vista Way, Mill Valley, CA 94141.
6. Noyes, H.P. and McGoveran, D.O. (1989). An essay on discrete foundations for physics. *Physics Essays*, **2**, No. 1. Also available as SLAC-PUB-4528(rev.), Oct. 5, 1988.
7. Kuhn, T.S. (1962). *The Structure of Scientific Revolutions*. Univ. of Chicago Press.
8. T.Bastin. (1988). Combinatorial Physics. *Discrete and Combinatorial Physics*, 138-140. ANPA WEST, 25 Buena Vista Way, Mill Valley, CA 94941.
9. Noyes, H.P.(1989). Where we are. *Proceedings of the 10th Annual International Meeting of the Alternative Natural Philosophy Association*. F. Abdullah, City University, Northumberland Square, London. SLAC-PUB-4836 (Jan. 1989).
10. Noyes, H.P. (1988). DISCRETE PHYSICS: Practice, Representation and Rules of Correspondence. *Discrete and Combinatorial Physics*. ANPA WEST, 25 Buena Vista Way, Mill Valley, CA 94141. pp 105-137.
11. M.Aguillar-Benitez, et.al. (1988). *Particle Properties Data Book*. Amsterdam: North Holland, p.2. Note also the footnote, p.4.
12. Parker-Rhodes, A.F. (1981). *The Theory of Indistinguishables*. Dordrecht: Reidel.
13. Dyson, F.J.(1952). *Phys. Rev.* **51**, 631.

Summary of **WHERE WE ARE**

General structural results

- 3+1 asymptotic space-time
- transport (exponentiation) operator
- combinatorial construction of π
- limiting velocity
- supraluminal synchronization and correlation *without* supraluminal signaling
- discrete events
- 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

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: $[2^{127}]^2 m_p = 4.84 \times 10^{52} \text{ gm}$
- fireball time: $[2^{127}]^2 \hbar / m_p c^2 = 3.5 \text{ million years}$
- 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??]

Unified theory of elementary particles

- quantum numbers of the standard model for quarks and leptons
- gravitation: $\hbar c / G m_p^2 = 2^{127} + 136 = 1.70147... \times 10^{38}$ [$1.6937(10) \times 10^{38}$]
- weak-electromagnetic unification:
 $G_F m_p^2 = 1 / [256^2 \sqrt{2}] = 1.07896 \times 10^{-5}$ [$1.02684(2) \times 10^{-5}$];
 $\sin^2 \theta_{Weak} = 0.25$ [0.229(4)]
- the quark-lepton generation structure
- generations weakly coupled with rapidly diminishing strength
- color confinement — quark and gluon masses not directly observable
- $m_{u,d}(0) = \frac{1}{3} m_p$
- 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: $\frac{1}{\alpha} = \frac{137}{1 - \frac{1}{30 \times 127}} = 137.0359674... [137.035963(15)]$
- $m_p / m_e = \frac{137\pi}{\frac{3}{14} \left(1 + \frac{2}{7} + \frac{4}{49}\right) \frac{4}{5}} = 1836.151497... [1836.152701(100)]$
- $m_\pi \leq 274 m_e$: [$m_{\pi^\pm} = 273.13 m_e$, $m_{\pi^0} = 264.10 m_e$]