

SOME REMARKS ON DISCRETE PHYSICS
AS AN ULTIMATE DYNAMICAL THEORY*

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ABSTRACT

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

An extension of "Some Remarks" presented at the
Seventeenth Annual International Meeting of the
ALTERNATIVE NATURAL PHILOSOPHY ASSOCIATION
Wesley House, Cambridge, England, September. 7-10, 1995

* Work supported by the Department of Energy, contract DE-AC03-76SF00515.

Our preferred descriptive phrase for discrete physics, or bit-string physics, is “a new fundamental theory”, harking back to Eddington.^[1,2] Unfortunately, it has become the fashion to call such schemes “Theories of Everything”; I have even used the phrase myself in order to establish a context, although I was careful to put it in quotation marks.^[3] This fashion has recently been criticized by 't Hooft, the leading Dutch theoretical physicist.^[4] He points out that the most that can be accomplished by the theories which physicists have in mind when they use the phrase is to provide dynamical laws for physics and physical cosmology which have no arbitrary parameters and admit of no exceptions. What actually happens and is happening in the world of experience is much richer than any modest descriptive framework of this type could possibly explain, even if it should turn out to be successful from a physicist's point of view. 't Hooft suggests that we should use instead the more accurate term “Ultimate Dynamical Theory”, which I have employed in the title of this paper. I believe that this corresponds to what Eddington had in mind when he used the title “Fundamental Theory” for his last work, and am confident that it is compatible with Fredrick Parker-Rhodes' vision of “The Inevitable Universe”.^[5,6] It is also what Steve Weinberg is talking about in his *Dreams of a Final Theory*. One of my reasons for bringing up this topic here is that these discussions by prominent physicists allow us to claim that, after 17 years in the wilderness, we can now place some of the ANPA work within the spectrum of conventional speculative physics.

't Hooft makes it quite clear in his discussion of this topic that the most clearly articulated candidate for an Ultimate Dynamical Theory, the standard model of quarks and leptons, falls short of the mark. The obvious reason for its failure is that the standard model contains 19 (or 20) parameters which have to be taken from experiment. 't Hooft has to name two numbers because of the ambiguity created by the problem of whether or not to count the parameter(s) needed to make contact with the international system of units (kilogram, meter and second). If Newton's constant of universal gravitation (N_G) is used to fix the mass scale in terms of the standard kilogram it can be called the 20th parameter, but *some*

such convention is necessary. I agree that this step should not be included in the parameter count. Once the mass scale is set, one still needs to define units for length and time. In the case of SI units, the unit of length (meter) is *defined* as the distance light travels in 1/299 792 458 seconds, but the unit of time also needs to be defined. Presumably 't Hooft follows the usual theorists' convention that this can be fixed without ambiguity by measuring the quantum of action (h) or of angular momentum ($\hbar = h/2\pi$), but he does not discuss this explicitly. For instance, one can take M_{Planck} to be the unit of mass given by $[\hbar c/G_N]^{\frac{1}{2}}$, the unit of length as $\hbar/M_{Planck}c$ and the unit of time to be $\hbar/M_{Planck}c^2$.

What are the 19 empirical constants that the standard model of quarks and leptons requires? 't Hooft specifies these as follows: 3 gauge coupling constants; 3 lepton masses m_e, m_μ, m_τ ; 6 quark masses $m_u, m_d, m_s, m_c, m_b, m_t$; 4 quark mixing angles; 1 topological angle θ_s ; 2 self-interaction parameters for the Higgs field (one gives the mass of the Higgs boson m_H and the other the strength of the Higgs to vacuum transitions). Even to determine these parameters from experiment, except for the lepton masses, is a difficult and theory-laden task. The Higgs boson has yet to be observed, and the detailed interconnection between the various parameters referring to the strong (quark) interactions keep many theoretical and experimental particle physicists employed in "testing the standard model". Many such detailed tests have been carried through to quantitatively successful conclusions, and self-consistent sets of *empirical* parameters. This large body of work is the solid basis from which dreams of a final theory take off. Nevertheless, the fact that the theory does not allow these 19 parameters to be calculated remains untouched.

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

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

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

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

Bit-string physics does much more than eliminate two parameters needed in the standard model. We share with the standard model a common description of the quantum numbers of quarks and leptons as analyzed using the weak interactions. The conventional theory has no systematic way of computing any of the 19 empirical parameters they need other than by putting self-consistent *experimental* bounds on them. But we have already computed 7 independent, basic parameters which they can only take from experiment. Further, the remaining 10 we need should be within our grasp. We endeavor to explain why in what follows.

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

Parker-Rhodes' construction also gave $\hbar c/e^2 = (2^2 - 1) + (2^3 - 1) + (2^7 - 1) = 137$ as the inverse fine structure constant. But at that stage in our history, it was all too easy to dismiss these two numerical results as coincidences, with no basic reason for identifying them with the results of physical measurements. One of my reasons for founding ANPA was to underpin these basic results with a fundamental theory which could be understood by physicists. If one accepts

subsequent work in discrete physics as a theoretical justification for accepting 137 as a first approximation to the measured value of α^{-1} , this gives us the equivalent of one of the three gauge coupling constants needed in the standard theory. We then have 16 additional numbers to compute, compared to the 19 needed in a conventional approach.

In our theory, the three gauge coupling constants in the conventional theory are replaced by $\alpha^{-1} = \hbar c/e^2$ at the mass of the electron, the Fermi constant for the weak interactions (G_F) in the dimensionless form $\hbar c/G_F m_p^2$ and the weak angle (θ_W), usually given in terms of $\sin^2 \theta_W$. Bastin suspected long ago (Ref. 8) that the 256^2 in the second Parker-Rhodes sequence was related to the inverse strength of the weak interactions. When Noyes got involved, he noted that the Fermi constant relates to experiment in a different way than the Yukawa-type coupling used to understand α . Also, it requires the specification of a mass scale. Putting these two facts together with the necessity of using m_p to set the hierarchy mass scale, one concludes that $\hbar c/G_F m_p^2 = 256^2 \sqrt{2}$, which agrees with experiment to about 7%. The equivalent of a third gauge coupling constant ($\sin^2 \theta_W$) was identified by Noyes as 1/4 from the structure of our theory of weak-electromagnetic unification. At this point we had 14 constants to go, but the lack of precision in comparison to experiment allowed our critics to feel, with some justice, that the whole scheme was still a bizarre, numerological coincidence.

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

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

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

$$G_F m_p^2 / \hbar c = [256^2 \sqrt{2}]^{-1} \times [1 - \frac{1}{3.7}] = 1.02 \text{ 758} \dots \times 10^{-5}$$

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

$$\sin^2 \theta_{\text{Weak}} = 0.25 [1 - \frac{1}{3.7}]^2 = 0.2267 \dots$$

$$\text{experiment} = 0.2259(46)]$$

These results are not as yet in complete agreement with experiment, but so far the weak parameters have not received appropriate electromagnetic corrections; symmetrically, the value for α^{-1} still requires weak interaction corrections. Another problem is to make rigorous the qualitative (unpublished) argument which shows that the value of 137 at low energy will “run” down to about 128 at the mass of the Z_0 in much the same way that the conventional theory requires. Nevertheless, we are far ahead of the conventional theory, which doesn’t have a clue as to how to compute numbers of the quality already achieved in 1962 and 1966!

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

coupling constant, as quoted below. We now have three coupling constants, two of the three lepton masses and two of the strong interaction parameters ($m_\pi, G_{\pi N N}^2$), making seven of the 17 parameters we are looking for, Comparison with experiment is remarkably good, considering all the corrections which are still needed in a self-consistent theory:

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

$$\text{experiment} = 1836.15 \ 2701(37)$$

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

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

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

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

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

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

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

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

In order for us to calculate the remaining 10 parameters, or their equivalent in our theory, we will have to develop a detailed finite particle number scattering

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

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

Consulting Clive’s new book (Ref. 1), we find that Eddington was much impressed by the way in which the two separate fields of electricity and magnetism were connected by Maxwell. One of the facts used was that the connection between electrostatic and electromagnetic units had to be a velocity which was, empirically, close to the velocity of light. This equality between the numerical results of two very different types of measurement became an *a priori* necessity once Maxwell’s theory was accepted. Further, Eddington saw clearly that in the hands of Einstein, this clue and the equivalence principle led on to a new understanding of gravitation, unifying all of classical physics using tensor calculus. His enthusiastic adoption of

Einstein's theory led to a highly successful career. Somewhere along the way, he began to think that the unification between quantum mechanics and gravitation might be brought about by understanding the combinatorics of the tensor calculus as a necessary part of the construction of physical theory. Clive does not make clear just when this happened. But he does indicate that Eddington was shocked to learn that relativistic quantum mechanics as constructed by Dirac did *not* fit into the tensor calculus scheme he had been taking as his starting point for a fundamental theory. The result was, according to Clive, the two difficult books *The Relativistic Theory of Electrons and Positrons* (RTPE) and *Fundamental Theory* (FT) which did much to erode (some might say destroy) Eddington's standing in the physics community.

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

mathematical transformation properties, could then well have had a devastating effect on Eddington's program.

Clive responds:^[20]

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

Unfortunately, we will probably never know more. Eddington left very few papers, and little correspondence survives. According to Clive, his interactions were mainly with colleagues at Cambridge, and were verbal, so the relevant historical material does not exist.

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

My final caution applies even if some spectacular triumph should come out of our studies. Fredrick (Parker-Rhodes) expressed the belief (in Ref. 5) that it was just possible that in his theory of indistinguishables, the search for basic structure might indeed have "bottomed out". Even granted that we succeed, I am dubious. We may well learn that, carried out and analysed in a certain way, we can always expect that the result of many specific experiments could have been calculated in advance. But I suspect that when our confidence gets too great (or more likely,

even before that), experience will thrust new puzzles under our noses that collapse larger or smaller pieces of the edifice we have so painfully erected. In short, I will always remain an empiricist. I agree with David Hume that there is no way we can bind the future.

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