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Reality As Complex Space

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One of the basic postulates of physics is that space-time is to be described using a $(3+1)$-dimensional real geometry. Within this geometry, the complex space of quantum mechanics is defined. Recently R. Mirman has shown that the geometry of space-time is determined by this relationship between the real and complex descriptions of reality. In the present paper, further justifications are given for Mirman's arguments.

1. INTRODUCTION

Why does physical reality appear to us as a $(3+1)$-dimensional real space? Mirman has investigated this question in a recent series of papers (Mirman 1984, 1988*a*,*b*). He begins with the following observation. Physical space can be thought of in two different ways. First there is the real space of normal experience, and then there is the complex space of quantum mechanics. The symmetries of space can be described on the one hand in terms of real coordinates, and on the other hand in terms of complex ones. Thus, there should be an isomorphism between the Lie algebras representing the real and the complex descriptions. Which pairs of Lie groups admit isomorphisms of their algebras? This is a classical question which has long ago been answered. Barut and Raczka (1965) provide a table of all possible isomorphisms involving real, simple Lie groups. We are only interested in the isomorphisms which relate groups acting on complex vector spaces to groups acting on real vector spaces. Here is the list of all such isomorphisms. (I retain their convention for assigning names to the groups. Note that Barut and Raczka leave out the trivial cases in two real and one complex dimension.) One has

$$NU_2^1 \approx NR_3^2 \tag{1.1}$$

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$$SU_2 \approx R_3 \tag{1.2}$$

$$SL(2, C) \approx NR_4^3 \tag{1.3}$$

$$SU_2 \otimes SU_2 \approx R_4 \tag{1.4}$$

$$NU_4^2 \approx NR_6^4 \tag{1.5}$$

$$SU_4 \approx R_6 \tag{1.6}$$

The isomorphisms (1.2) and (1.3) are of course familiar to anyone who has glanced at the beginning of any book on quantum field theory. The last three isomorphisms do not usually appear there, although (1.4) is sometimes mentioned. Also, many other groups, such as SU_3, do not appear at all. What is the meaning of this?

One way of approaching this question is to make a list of requirements, or axioms, which isolate the first three isomorphisms on the list. For example: (A) We are interested in isomorphisms of real Lie algebras relating symmetry groups of real spaces to symmetry groups of complex spaces. (B) The complex symmetry group must be simple. (C) If a symmetry group of a real space is on the list, then the corresponding symmetry group of every possible real subspace is also on the list.

In the rest of this paper I will attempt to justify these three requirements. In doing so, I will depart somewhat from the arguments of Mirman.

2. THE CORRESPONDENCE PRINCIPLE

We will be concerned with a correspondence between the idea of space in terms of real numbers and space in terms of complex numbers. Sixty years ago, when the theory of quantum mechanics was first formulated, people tried to justify the quantum mechanical (complex) description in terms of a number of very general philosophical principles. In particular, great importance was given to the so-called "correspondence principle." The idea was that although the theory of quantum mechanics is supposed to supersede the old theory of classical mechanics, nevertheless the framework of classical mechanics is still necessary in order to describe the theory of quantum mechanics. It is an unfortunate fact that most people avoided a precise description of this correspondence between the old and new theories, preferring to emphasize the revolutionary nature of the new theory.

If we are to examine sensibly the correspondence between various Lie algebras, then it is necessary to understand the correspondence principle, and this involves investigating the interpretation of quantum mechanics. Now, as Ballentine (1970) has shown, the controversy which surrounded the original formulation of the theory of quantum mechanics led essentially to two different interpretations. The "Copenhagen interpretation" of Bohr and Heisenberg is well known. The skeptics of those days—Einstein, Schrödinger, and so forth—could not accept this interpretation. But this is not to say (as was sometimes represented by the Copenhagen school) that they rejected quantum mechanics itself. On the contrary, they accepted it as a statistical theory—on a par with any other statistical theory. This is the meaning of Einstein's often misunderstood statement that quantum mechanics is "incomplete." By its very nature, the theory of statistics deals with situations where the details are unknown; if all details were *completely* known, then one would have no need to bring in statistics in the first place. Thus, in a trivial sense, all statistical descriptions of reality must be incomplete. By arguing in this way—but without being able to describe a concrete mechanism for the strange new quantum interference effects—the adherents of this early "statistical interpretation" were at a disadvantage in their dispute with the Copenhagen school, who simply maintained that the statistical calculation was everything. In the present paper I will adopt the statistical interpretation. Not only does it lend itself to a more rational understanding of physics, but also it provides a natural explanation of the reasons for the assumptions (A), (B), and (C).

Classical mechanics and quantum mechanics are two different theories which are concerned with describing two different aspects of the physical world. The goal of classical mechanics is to describe a given and fixed sequence of events. Quantum mechanics is concerned with statistics. Statistics, in turn, has to do with the results of repeatable experiments; certain conditions are taken as given and fixed—the experimental conditions—and within the framework of a given experiment, various different results are possible. For example, one possible experiment might be to measure the air temperature at 9 a.m. every morning at some particular weather station. Looking at the situation from the point of view of classical mechanics, the problem would be to explain why some precise temperature, say 19°C, was measured on a particular day, say 1/1/1980. Perhaps on the next day an accident occurred so that the thermometer tipped over, invalidating the experiment on that day. But this is irrelevant when we are concerned with describing the one particular result within the classical framework. The quantum mechanical—or statistical—view of reality is different. Here certain things are taken as given and fixed: the thermometer must be working properly, the measurement is always at 9 a.m., and so forth. If the conditions are not precisely fulfilled, then the experiment is considered invalid for that day, and the measurements are discarded. These conditions, being fixed, can be described in terms of the given "classical" world. The unknown quantity which is being measured can take on arbitrary values without invalidating the experiment. This is the "quantum"—or statistical—world. One sees then that the theory of statistics, which has been the subject of mathematical investigation for hundreds of years, requires a "correspondence principle." The fixed conditions of the experiment correspond with the known classical world. The unknown values which are being measured can be described using the "nonclassical" laws of mathematical statistics.

Of course we need more than this in order to understand quantum mechanics. The wavelike statistical effects of quantum mechanics—described in terms of complex "amplitudes"—seem to be difficult to explain, at least when thought about in the framework of the old mathematical models which were used to describe classical statistical mechanics. Elsewhere I have given arguments to support the idea that these statistical laws can arise from an underlying classical model which is discrete, rather than continuous (Hemion, 1988). A different derivation, also based on the idea of an underlying discrete model, is to be found in Landé (1965). But whatever justification one prefers to give to these statistical laws of quantum mechanics, the fact remains that the argument can be based on the old "correspondence principle" of statistics.

We are concerned here with the correspondence between a description of the world in terms of real and complex numbers. I will argue that the real description is the "classical" description, in the sense that "experimental conditions" of typical quantum mechanical experiments are best formulated in terms of real numbers. The results of the experiments can most easily be understood in terms of a statistical calculation involving complex numbers.

3. THE "REAL" WORLD OF FIXED CONDITIONS

Consider some typical "experiment" in a physics laboratory. The experiment involves measuring repeatedly some definite process. For example, one common type of experiment is to let particles travel through a bubble chamber, and the positions of the tracks they leave are then determined. The positions might be measured using a camera attached to a fixed framework, which in turn is resting firmly on the floor of the laboratory. All of these conditions are necessary in order to have a valid run of the experiment. When thinking about the experiment, the "frame of reference" within which the measurements are made is naturally related to the material objects from which the experiment is constructed. For example, the abstract idea of a "coordinate axis" is perhaps, in reality, a massive and stable metal girder in the laboratory.

Now all of the physical details of the girder are unimportant for the purpose of defining a coordinate axis. The axis is given by two points—say two fixed measurement points on the apparatus. Perhaps a careful experimenter may even decide to relate the whole experiment to four known survey points in the neighborhood. But what are these points? They are related to very stable material objects which appear to remain well fixed for long periods of time. Surely any sensible person would agree that this is the *reality* behind the abstract idea of a "frame of reference." Events within the physical world are related to some finite number of points defined in terms of physical objects.

Consider one of these reference points. Being stable, it remains well defined throughout the time the experiment is being run. Thus, the point defines a path through time (a "world-line"). Since the path is part of the *definition* of the experiment, it must be sufficiently long to allow it to be used to measure all possible runs of the experiment. What is the most direct way to relate events in the physical world to the word-lines which define the experiment? Surely the most natural answer is to take the points of intersection with the null light-cone from a given event. For simplicity we can just take the light-cone *above* the event. Now each world-line can be parametrized using the real numbers. Thus, each event in the real world is assigned a unique real number for each word-line used to define the experiment. If we happen to choose four different world-lines, then we have four spatial coordinates. But at this stage there is nothing special about the number four. Taking n world-lines gives n real coordinates. In this way we obtain—within the context of the given experiment—a Euclidean coordinate system of some finite dimension. (Note also that this idea allows a natural definition of "coordinate neighborhoods." A given experiment involves a certain local Euclidean coordinate system. But one may decide to concentrate on another aspect of the world, which then becomes another "experiment"—bringing with it another local coordinate system. A large system of local coordinate systems could be built up, allowing complicated global structures.)

Why choose the light-cone structure for defining real coordinates, and why are we allowed to parametrize the world-lines using real numbers? Some readers may object that in doing this we are implicitly bringing in many new concepts: light, electromagnetism, the Dirac theory, etc. But is this true? It may be better to argue that when talking about light-cones, one is really talking about causal structure. Independent of all experiment, one could claim that the events of the physical world must form a partially ordered set. Physical laws, even reason itself, has to do with the idea of cause and effect. If one event is part of the cause of another, then it must come before it in time. This causal structure then is the basic reason why real numbers are relevant in the study of physical space. Each world-line must be a totally ordered set.

Finally it might be interesting to compare this with Newton's point of view. In his work he proposed a new frame of reference—in contrast to the "common people," who think of themselves only with respect to the material objects which they perceive. His framework is an abstract and imperceptible framework which he called "absolute space." But this point of view is completely opposed to the framework for real space which has been described in this section. Newton's absolute space would be useless if it contained no material objects. On the other hand, the space of world-lines of material objects can be defined without reference to an absolute space. Hence this "common" space is the simple and more basic concept.

4. THE "COMPLEX" WORLD OF QUANTUM MECHANICS

The quantum mechanical world, dealing with the results of experiments, cannot be justified in terms of commonplace reasoning, as was done in the previous section for the real world. But despite this, I will argue that it is the complex world which comes first, determining the structure of real space.

Why can the statistical results of experiments be best described using the language of probability "amplitudes," states, transition probabilities, and all the rest? Perhaps some readers will be happy to accept these laws of quantum mechanics as being basic "laws of nature" which are beyond the reach of further human inquiry. In that case, or if one has some other method of understanding quantum statistics, then one can simply skip to the beginning of the next section and follow the argument from there.

However, in Hemion (1988) I attempted to justify the statistical laws of quantum mechanics in terms of ensembles of discrete, partially ordered sets. The idea was that quantum statistics must arise in just the same way as any other statistical phenomenon. The simplest way to describe this is to use the classical coin-flipping "experiment." What are the odds that if you flip a coin starting from now, the first three throws will result in heads? The theory of statistics tells us that the answer is 1 in 8. What reasoning leads to this answer? In the theory of statistics one begins by listing all possible different outcomes. The physicist may prefer to think of this as being many different "universes" or "worlds." Each of the possible outcomes is associated with a possible world. (Or, more realistically, a great ensemble of possible different worlds.) The main premise of the theory of statistics is that all of these possible worlds are equally likely. Thus, the probabilities are obtained by simple counting arguments. Now, to be quite clear on this—to avoid all confusion with one or another kind of philosophical "interpretation" of quantum mechanics—I will list here the basic assumptions of the theory of statistics, as it is taught in any faculty of mathematics.

I. Begin by defining precisely the conditions of the "experiment."

II. Make a list of all possible outcomes of the experiment.

III. Assert that all of these outcomes are equally likely.

IV. The *actual* experiment, once performed, is known, and is thus no longer relevant to the theory of statistics.

In particular, the reader should note that in order to deal with point II, the theory of statistics does not require the concept of a treelike world, splitting into countless branches. Also, point IV does not involve the concept of statistical calculations "collapsing" with each run of an experiment. Although the theory of statistics involves the consideration of all possible worlds, still, it does not deny that the underlying object is one *single* given world. The detailed properties of this actual world cannot be investigated using the theory of statistics.

What is this actual world? Why are we confronted with an "uncertainty principle" which foils our attempts to investigate it more precisely? My argument was that this is due to the fact that the actual world is discrete. I attempted—unsuccessfully—to find reasons for the perceived $(3+1)$-dimensional structure of the real world in terms of some definite geometric properties of an underlying discrete space. Certainly, if one could show that all *possible* worlds are $3+1$ dimensional, then it would follow trivially that the *actual* world is also $3+1$ dimensional. But in view of Mirman's arguments, it is clear that the converse is not necessary: although we perceive a $(3+1)$-dimensional world, this does not necessarily imply that all possible worlds—or indeed the actual world—is also $3+1$ dimensional. All we can conclude is that something near to the $(3+1)$-dimensional structure appears to be likely.

Given that the actual world is discrete, how does that fit in with the framework of the "real" world described in the previous section? The conditions of a given experiment are to be related to world-lines, which are parametrized using the *real* numbers. Now each possible world—including the actual world—is assumed to be discrete. Thus, one could easily ask, "Why not parametrize the world-lines with a discrete number system: the integers?" To answer this question, one should remember that the discreteness of a world-line can only be defined with respect to the given conditions of the experiment; that is, with respect to other discrete world-lines. That means that the discreteness must become a part of the *definition* of the experiment. An experiment which is so defined would involve "phase correlations" between its defining world-lines. Now such an experiment can certainly be imagined, but this hardly reflects the usual practical conditions in a physics laboratory. No such phase correlations are assumed, and thus we are forced to assume that the world-lines have arbitrarily fine structure. This, then, is the justification of the real parametrization.

In Hemion (1988) I only discussed the simplest possible case of a quantum experiment with no spin. The results of such an experiment are described using a single complex function: the "amplitude." Now, van der Waerden (1972) shows how (in the nonrelativistic theory) the spin of a particle can be described in terms of two complex functions, a spinor. One term of the spinor describes the statistical probability that a particle appears at a given point with spin up. The other term describes the situation for spin down. But each of these two cases can be considered alone, and the same argument as before, using the discreteness of the underlying space, explains the use of the complex function. It is usual to relate these complex functions immediately to an underlying (absolute) real space, arguing that orthogonal coordinate transformations of that space lead to unitary coordinate transformations in the complex space.

The two component spinors give a 2-dimensional complex space. This leads in the usual theory to the basic correspondence $SU(2) \approx O(3)$. But why must we restrict ourselves to just two complex dimensions? It is true that only 2-dimensional complex spaces occur in the relations (1.1)-(1.4). [The relations (1.5) and (1.6), which admit a 4-dimensional complex space, will be ruled out using requirement C.] But that would be getting ahead of ourselves. There seems no reason to make the ad hoc rule that complex space must be 2-dimensional. Just as we have done for the real case, it is better to allow experiments which—to begin with at least—could have particles with arbitrarily many components of (iso)spin. [Note that this idea might be related to the work of Gudder (1985).]

Why should the complex space be considered as being more "basic" than the real space? One way to answer this question would be to simply follow the traditional line of quantum mechanical philosophy and declare that we are not interested in describing the normal world of everyday experience; according to this view, it is necessary to restrict all attention to stylized "experiments" which are only allowed to occur in physics laboratories equipped with well-defined "observers." But surely no one is satisfied with this. The interesting question must be, Why does the everyday world seem to be so strongly governed by quantum mechanical (that is, statistical) laws? To give an example [which was also discussed by Landé (1965)], many people have life insurance policies. Their lives are thus governed by certain actuarial laws. But the *perceived* reality which each person experiences could hardly be described in any detail by a knowledge of these statistical "laws of nature"!

Now if we argue that the *actual* world behind the statistical world is to be described using some definite (not just statistical) rules, then the question must be asked, why don't we see these rules? Retreating to a simple declaration that science should only be concerned with "repeatable experiments" is no help. It seems better to argue as follows. Our perceptions of the world are always in the present. The present can be thought of—in a very general sense—as being a state of knowledge. Given this state of knowledge, the future brings us then new bits of knowledge, which, to make sense of it, we must relate to the already given knowledge. We are concerned here with geometry, so this knowledge can be reduced to the everyday idea of objects, or points, in real space. Given a certain state of real geometrical knowledge, then this can indeed be thought of as being an "experiment" in the sense of quantum mechanics. The fact that our knowledge is incomplete (perhaps because the underlying, actual world is discrete) means that many different possible worlds are compatible with that knowledge. Thus, even though we exist in a single definite world, nonetheless our perceptions are governed by statistical effects. Certain *actual* observations, which may be statistically unlikely, are attributed to "quantum fluctuations." Being unlikely, they are difficult to fit in to our normal experiences of the real geometric world. But of course such fluctuations are an essential part of any statistical model.

5. JUSTIFYING THE THREE ASSUMPTIONS

Begin with assumption A, that the geometry of physics must admit an isomorphism of real Lie algebras relating symmetry groups of real spaces to symmetry groups of complex spaces. There are two questions. (i) Why should there be an isomorphism, and (ii) why take real (rather than complex) algebras?

Consider question (i) first. The traditional argument here is that the spaces simply have "symmetries." What does this mean? Spaces are given in terms of coordinates. One then says that the choice of coordinates was arbitrary, so let us take some other choice. Since one single space is being described, there must be a symmetry. Now it is clear that this argument is based on the idea of "absolute" space. If one takes the common everyday space of true physical reality, relating the geometry to given material objects, then much of the arbitrariness—and with it the symmetries—in the description of geometry disappears. Thus, it is necessary to describe more carefully the extent to which arbitrary choices are made, both in the real and in the complex description.

Choose some fixed experiment. The results of the experiment can be related to a system of real coordinates defined in terms of some finite system of world-lines. Let, say, P be some definite result of the experiment. Then P can be taken as a point in an m-dimensional real Euclidean space. (For simplicity choose the parametrization of the world-lines in such a way that P is assigned the zero point.) Other possible results of the experiment can also be considered. These other results can be assigned real coordinates in some neighborhood of P. (Perhaps the experiment is only valid in a limited neighborhood.)

Now each of the results has some definite probability which is to be calculated using complex "amplitude" functions. The arguments which justify these complex functions [both in Landé (1965) and Hemion (1988)] are based on the idea of trying to find "smooth" functions which approximate as nearly as possible nonsmooth (discrete) measurements. This certainly implies that the complex amplitude functions must vary smoothly within the real space of possible results. Around P, then, the complex functions can be represented by a mapping $\vartheta : K \to R^m$, where $K \subset C^n$ is some subset of n-dimensional complex space and R^m is m-dimensional real space. Using the smoothness of ϑ, we can choose K to be sufficiently small that ϑ is diffeomorphic to the tangent mapping at P. It is sensible to assume that ϑ is one to one; otherwise, more than one probability would be assigned to a single experimental result—which would clearly be nonsense. The image of the mapping ϑ might be a proper subspace of R^m, but in this case we have chosen too many world-lines; some number of them can be discarded and we still are able to distinguish all possible different results. In summary, then, ϑ is assumed to be both one to one and onto.

But now we can return to the conventional arguments. We have chosen the real coordinates as being simply the path lengths along the world-lines which define the experiment. These real coordinates define a Euclidean neighborhood of P. Of course, we may equally well choose any other real coordinate system which can be obtained from the original one by some linear transformation; the choice is just as arbitrary as in the usual framework of "absolute space." Therefore, the same arguments as usual lead to the requirement that there must be an isomorphism of Lie algebras. But note that the freedom of choice involves the coordinates of the real space, defining the conditions of the experiment. This is the reason for taking real Lie algebras, rather than complex ones. The fact that the complex space only has meaning within some given and fixed experiment—that is, some *real* coordinate system—means that (within this framework at least) one cannot give a physical interpretation to the idea of isomorphisms of complex Lie algebras. (In the next section I will relate this to the Clifford algebra formalism, where the complex numbers are given a purely geometric meaning.)

The next task is to justify assumption B, which is that the complex symmetry groups should be simple. But Mirman has already provided the justification in this case. If the experimental results were to allow a decomposition into a number of independent subspaces, then it would be as if there existed various "parallel" worlds which are independent of one another.

Such worlds must be—at least within the context of a given quantum theory—invisible to us, and should therefore play no role in that theory. It may be that different kinds of phenomena can be best described using separate statistical calculations; for example, interactions involving the strong force might at first be best calculated independently of the electromagnetic "world" which we see. Thus, we start with two different ideas of the world: the "normal" world of electromagnetism, and the "strange" world of "iso-" space. If there were no points of contact between these two different worlds, then it would be best to say that the "iso-" space world simply does not exist. In fact, though, charged particles may suddenly appear or disappear, thus making themselves known in the "normal" world. Does the existence of such apparently "independent" forces of nature invalidate the principle that the complex symmetry groups should be simple? No. For the symmetry is in the statement of the experiment in the real world. The experiment, and the experimental measurements, can only be formulated in terms of what can be directly experienced; that is, within the context of electromagnetic phenomena—with respect to one single and coherent type of phenomenon. What we can directly see determines our idea of the geometry of space-time. Thus, it is only necessary to require simplicity with respect to the statistical functions related to this direct experience of the world.

Finally we must justify assumption C, which is that if a symmetry group of a real space is to be considered, then the corresponding symmetry group of every possible real subspace is also to be considered. But the justification of this assumption is simple. If an experiment is given in terms of n different world-lines, then it is perfectly possible to define another experiment by simply ignoring one of these world-lines. This new experiment must be well defined if the original experiment was. Furthermore, the statistical properties of the set of possible results can equally well be calculated in terms of complex amplitude functions.

To summarize, given that the complex amplitude functions can be interpreted using a statistical framework, then justifications for the three assumptions seem to follow easily. We have not examined the fundamental correspondence between the Poincaré group and the inhomogenenous $SL(2, C)$ [see, for example, Streater and Wightman (1964)]. But this violates each of our three assumptions, so it is difficult to justify it directly in terms of the arguments presented here. The fact that we have chosen a *fixed* point P and related that to the statistical calculations in a fixed experiment means that the symmetries leave P fixed. On the other hand, this isomorphism can be derived from the given isomorphisms of real, simple algebras by using the classical method of Wigner (1939). The usual idea is that physical theories themselves contain symmetries. But in contrast, the argument here is based on the idea that quantum theory, being a theory of statistics, can only be given meaning in the context of given fixed experiments. These fixed experiments are defined in terms of the real world, whose description is subject to symmetries.

6. THE CLIFFORD ALGEBRA FORMALISM

Up until now, we have been concerned with explaining why the geometry of the physical world is determined by the fact that space should be described in terms of real and complex numbers. Although it is often maintained that the complex formulation is the essence of quantum mechanics, nevertheless it is possible to formulate the Dirac equation in terms of a Clifford algebra (or *geometric algebra*) over the real numbers (Hestenes, 1975). The imaginary number $\sqrt{-1}$ is given a new interpretation in this theory. It is no longer to be considered as a pure number (a scalar), but rather it is represented by a Clifford number whose square is -1. In Hestenes' theory, this number represents a unit volume in space-time. Using this formalism, it is possible, at least in a formal sense, to do away with the idea of two different descriptions of the world: the real and the complex descriptions. They are replaced with a simpler unified description in terms of real numbers alone. It is an interesting exercise to see how the present arguments can be applied to the Clifford algebra formulation. The necessary mathematical framework is developed in Hestenes and Sobczyk (1984).

The basic idea is that a spinor is no longer taken to be a complex vector. Instead Hestenes defines a spinor [in the geometric algebra $\mathcal{G}(\mathcal{R}_{p,q})$ of $(p+q)$-dimensional pseudo-Euclidean space] to be an even multivector ψ with the property that for all vectors x in that space, $\psi x \psi^\dagger$ is also a vector. Since spinors are usually taken to describe the quantum mechanical probabilities, it follows that it is also necessary to examine the structure of spinors in this new theory. Now spinors are elements of Spin+(p, q), the rotor group of $\mathcal{R}_{p,q}$. Thus, a given spinor ψ generates the rotation $\psi x \psi^\dagger$. Hestenes and Sobczyk show that for spaces with Euclidean or Lorentz signature [i.e., $(1, q)$ or $(p, 1)$], every rotor can be expressed as a commuting product of simple rotors. A simple rotor S, in turn, is a product of two unit vectors

$$S = ab = a \cdot b + a \wedge b$$

with $S^\dagger S = a^2 b^2 = 1$.

If $p+q<4$, then any spinor is a simple rotor. Also, in $\mathcal{R}_{3,1}$ each spinor must be a simple rotor. This follows since either $a^2 = b^2 = 1$. or $a^2 = b^2 = -1$ is impossible for a nontrivial rotor in a Lorentz space. On the other hand, in 4-dimensional Euclidean space there do exist spinors which can be decomposed into the commuting product of two nontrivial simple rotors (just take two pairs in an orthogonal set of basis vectors for $\mathcal{R}_4$). Also, for all spaces $\mathcal{R}_{p,q}$, with $p+q>4$, there exist spinors which are nontrivial commuting products of simple rotors. To summarize, then, $3+1$ dimensions is the most that a space can have if we want to ensure that spinors are "simple"—i.e., cannot be decomposed into commuting products of simpler spinors. But the requirement that spinors should be simple can be justified on the same grounds as those used to justify assumption B above.

There is still one point to be cleared up, though. Within the Clifford algebra framework the spaces $\mathcal{R}_{p,q}$, with both p and q greater than one, have not yet been ruled out. But for this it seems best to establish a more general result—namely that there should be at most one dimension of time. In Dorling (1970) it was argued that time must be 1-dimensional, since otherwise the path lengths of timelike particle world-lines could not be extremal. Thus, physics in terms of variational principles would be impossible. Now this argument is certainly convincing. Nevertheless, the main idea of the present paper is that we should try to divest physics of as many abstract principles as possible.

I have already brought in the principle of cause and effect in order to justify the use of real numbers for describing experiments. But this principle is also sufficient to eliminate all spaces with more than one dimension of time. For if any such space is given, then it must contain $(2+2)$-dimensional space. (If space-time is assumed to be curved, then one may take here the tangent space to a given point.) Thus, we need only show that in $2+2$ space there must exist distinct points P and Q such that each lies in the future set of the other. For simplicity, take $P=(0,0,0,0)\in R^{2,2}$. Then the set of points V_P, with vanishing pseudo-Euclidean distance to P, forms a "light-cone" structure in $R^{2,2}$. The different connected regions of $R^{2,2}—V_P$ must represent points of space-time which are all in the same causal relationship to P. Ascending from P to the point $A=(0,0,0,1)$, we travel forward in a straight line through the second "time" dimension, so that surely we would be justified in saying that $P<A$. However, from the point A, one can travel one unit directly backward in the first time dimension, following a straight line to reach the point $Q=(0,0,-1,1)$. Let L_{AQ} be the straight line segment joining A and Q. Then $L_{AQ}\cap V_P=\varnothing$. Hence $P<Q$. But now we can find a similar path from P to Q, but going into the past and thus showing that $Q<P$.

Finally it is interesting to note that a spinor, which describes the electromagnetic field in the Dirac theory, is essentially 2-dimensional. But this leaves $\mathcal{R}_{3,1}$, a 4-dimensional space, with a number of "degrees of freedom" still unaccounted for. The point is examined in Hestenes (1982), where it is shown that the extra structure is just what is needed to describe the weak force. This provides a simple and unified description—within the context of the Dirac theory—of the electroweak force, on the same level of mathematical unification as was achieved by Maxwell's theory in unifying the electrical and magnetic forces.

7. THE "LAWS" OF PHYSICS

The physical world is clearly very complicated. In order to make sense of things, people try to understand physics in terms of abstract rules which are considered to apply to all objects. This idea that there must exist basic "laws of nature" is surely the cornerstone of science itself. The search for these laws has obviously led to great progress in understanding the physical world. Thus, the concept of an abstract "physical law" is a useful one. But by the same token, the development of physics has shown that no law which has yet been formulated has proven to be very satisfactory. Are we justified in looking for some final, absolute "law of nature" which will be accepted as such by all future generations? This absolute quality is given for mathematical theorems, but the history of science hardly gives encouragement to the view that the physical world will ever be understood in a similarly absolute sense.

For example, the principle of relativity and the principle of gauge invariance are accepted by almost everyone today as being basic "laws of nature." It is thought that these two laws can be easily understood and accepted as being of a basic, fundamental character. On the other hand, most people are unhappy with the currently accepted, paradox-ridden interpretations of quantum mechanics. Thus, all standard textbooks try to relate quantum mechanics to these two basic principles. But can an unbiased observer of the history of science believe that these foundations are of a truly fundamental, unshakable nature?

Take the principle of relativity. In a sense, it is an attempt to get away from Newton's "absolute space." The practical problem was to explain the negative results of the Michaelson-Morley experiment in view of Maxwell's idea of an absolute space, which he chose to call the "ether." The principle of relativity is that the physical world is invariant with respect to different choices of ether (or absolute space). This idea is now so universally accepted that it is difficult to imagine why people were reluctant to accept it at the beginning of this century. But the reality today is that we are faced *in fact* with the opposite problem. In a certain fundamental sense, the measurements of the cosmological background radiations are measurements of the velocity of the earth through the "ether." Most people are happy to say that they still believe in the principle of relativity, even though they admit that it only applies on a "local" scale of distances (whatever that is taken to mean), but surely a critical observer must be skeptical.

But what a simplification will result if we accept relativity and gauge invariance as being mere consequences of the quantum mechanical description! Given that the complex world is the more basic concept, then, as we have shown, the real geometry of the theory of relativity must follow. Thus, the theory of relativity should *not* be understood in terms of some absolute philosophical principle, involving vaguely postulated "frames of reference." Perhaps in the future, people will accept the idea that the principle of relativity is nothing more than a simple consequence of quantum mechanics.

Of course the principle of gauge invariance (not just in the Abelian gauge theories) is also a basic consequence of the quantum mechanical description. Therefore both of these principles—relativity and gauge invariance—are of a secondary nature. It may be that the actual world behind the statistical world of quantum mechanics is governed by certain definite and absolute laws. But the fact that our experience of the world is dominated by its statistical character may mean that these definite laws will remain hidden, and in fact will remain unimportant for an understanding of the physical world.

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