ANGULAR MOMENTUM: AN APPROACH TO COMBINATORIAL SPACE-TIME

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I want to describe an idea which is related to other things that were suggested in the colloquium, though my approach will be quite different. The basic theme of these suggestions have been to try to get rid of the continuum and build up physical theory from discreteness.

The most obvious place in which the continuum comes into physics is the structure of space-time. But, apparently independently of this, there is also another place in which the continuum is built into present physical theory. This is in quantum theory, where there is the superposition law: if you have two states, you're supposed to be able to form any linear combination of these two states. These are complex linear combinations, so again you have a continuum coming in—namely the two-dimensional complex continuum—in a fundamental way.

My basic idea is to try and build up both space-time and quantum mechanics simultaneously—from *combinatorial* principles—but not (at least in the first instance) to try and change physical theory. In the first place it is a *reformulation*, though ultimately, perhaps, there will be some changes. Different things will suggest themselves in a reformulated theory, than in the original formulation. One scarcely wants to take *every* concept in existing theory and try to make it combinatorial: there are too many things which look continuous in existing theory. And to try to eliminate the continuum by *approximating* it by some discrete structure would be to change the theory. The idea, instead, is to concentrate only on things which, in fact, *are* discrete in existing theory and try and use them as *primary concepts*—then to build up other things using these discrete primary concepts as the basic building blocks. Continuous concepts could emerge in a limit, when we take more and more complicated systems.

The most obvious physical concept that one has to start with, where quantum mechanics says something is discrete, and which is connected with the structure of space-time in a very intimate way, is in *angular momentum*. The idea here, then, is to start with the concept of angular momentum—here one has a *discrete* spectrum—and use the rules for combining angular

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momenta together and see if in some sense one can construct the concept of *space* from this.

One of the basic ideas here springs from something which always used to worry me. Suppose you have an electron or some other spin $\frac{1}{2}\hbar$ particle. You ask it about its spin: is it spinning up or down? But how does it know which way is 'up' and which way is 'down'? And you can equally well ask the question whether it spins right or left. But whatever question you ask it about directions, the electron has only just *two* directions to choose from. Whether the alternatives are 'up' and 'down', or 'right' and 'left' depends on how things are connected with the macroscopic world.

Also you could consider a particle which has *zero* angular momentum. Quantum mechanics tells us that such a particle has to be spherically symmetrical. Therefore there isn't really any choice of direction that the particle can make (in its own rest-frame). In effect there *is* only one 'direction'. So that a thing with zero angular momentum has just one 'direction' to choose from and with spin one-half it would have *two* 'directions' to choose from. Similarly, with spin one, there would always be just three 'directions' to choose from, etc. Generally , there would be $2s+1$ 'directions' available to a spin s object.

Of course I don't mean to imply that these are just directions in space in the ordinary sense. I just mean that these are the choices available to the object as regards its state of spin. That is, however we may choose to interpret the different possibilities when viewed on a macroscopic scale, the object itself is 'aware' only that these are the different possibilities that are open to it. Thus, if the object is in an s-state, there is but one possibility open to it. If it is in a p-state there are three possibilities, etc., etc. I don't mean that these possibilities are things that from a macroscopic point of view we would necessarily think of as directions in all cases. The s-state is an example of a case where we would not!

So we oughtn't at the outset to have the concept of macroscopic space-direction built into the theory. Instead, we ought to work with just these discrete alternatives open to particles or to simple systems. Since we don't want to think of these alternatives as referring to pre-existing directions of a background space—that would be to beg the question—we must deal only with *total* angular momentum (j-value) rather than spin in a direction (m-value).

Thus, the primary concept here has to be the concept of *total angular momentum* not the concept of angular momentum in, say, the z-direction, because: which is the z-direction?

Imagine, then, a universe built up of things like that shown in fig. 1. These lines may be thought of as the world-lines of particles. We can view *time* as going in one direction, say, from the bottom of the diagram to the top. But it turns out, really, that it's irrelevant which way time is going. So I don't want to worry too much about this.

I'm going to put a number on each line. This number, the *spin-number*

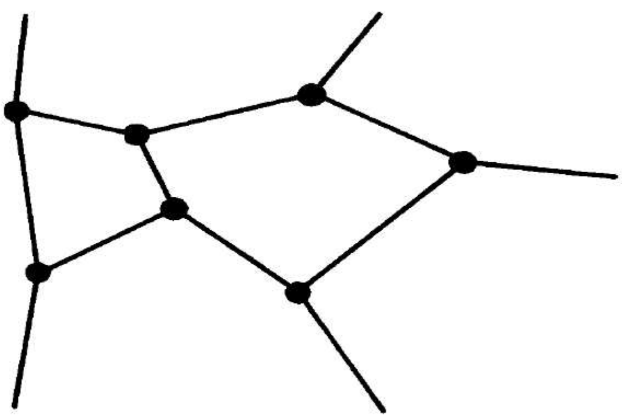

Fig. 1

will have to be an integer. It will represent twice the angular momentum, in units of $\hbar$. All the information I'm allowed to know about this picture will be just this diagram (fig. 2): the network of connections and spin numbers 3, 2, 3, . . . like that. I should say

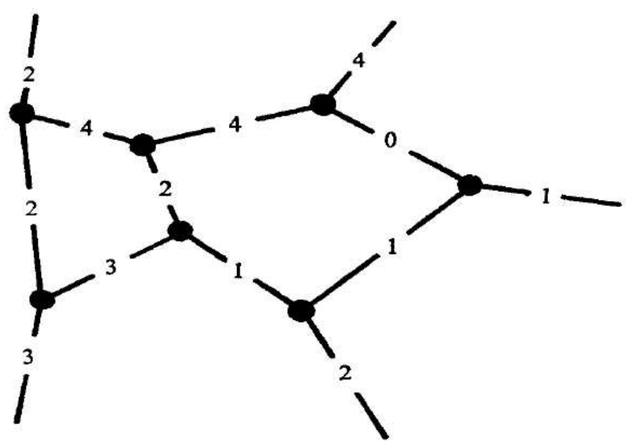

Fig. 2

that the picture I want to give here is just a model. Although it does describe a type of *idealized* situation exactly according to quantum theory, I certainly don't want to suggest that the universe 'is' this picture or anything like that. But it is not unlikely that *some* essential features of the model that I am describing could still have relevance in a more complete theory applicable to more realistic situations.

I have referred to these line segments as representing, in some way, the world-lines of particles. But I don't want to imply that these lines stand just for *elementary* particles (say). Each line could represent some compound system which separates itself from other such systems for long enough that (in some sense) it can be regarded as isolated and stationary, with a well-defined total angular momentum $n \times \frac{1}{2}\hbar$. Let us call such a system or particle an n-unit. (We allow $n = 0, 1, 2, \ldots$) For the precise model I am describing, we must also imagine that the particles or systems are not moving relative to one another. They just transfer angular momentum around, regrouping themselves into different subsystems, perhaps annihilating one another, perhaps producing new units. In the diagram (fig. 2), the 3-unit at the bottom on the left splits into a 2-unit and another 3-unit. This second 3-unit combines with a 1-unit (produced in the break up of a 2-unit into two 1-units) to make a new 2-unit, etc., etc. It is only the *topological* relationship between the different segments, together with the spin-number values, which is to have significance. The time-ordering of events will actually play no role here (except conceptually). We could, for example, read the diagram as though time increased from the left to the right, rather than from the bottom to the top, say.

Angular momentum conservation will be involved when I finally give the rules for these diagrams. These rules, though combinatorial, are actually derived from the standard quantum mechanics for angular momentum. Thus, in particular, the conservation of total angular momentum must be built into the rules.

Now, I want to indicate answers to two questions. First, what *are* these combinatorial rules and how are we to interpret them? Secondly, how does this enable us to build up a concept of space out of total angular momentum? In order not to get bogged down at this stage with too much detail, I shall defer, until later on, the complete definition of the combinatorial rules that will be used. All I shall say at this stage is that every diagram, such as fig. 2 (called a *spin-network*) will be assigned a non-negative integer which I call its *norm*. In some vague way, we are to envisage that the norm of a diagram gives us a measure of the frequency of occurrence of that particular spin-network in the history of the universe. This is not actually quite right—I shall be more precise later—but it will serve to orient our thinking. We shall be able to use these norms to calculate the probabilities of various spin values occurring in certain simple 'experiments'. These probabilities will turn out always to be rational numbers, arising from the fact that the norm is always an integer. Given any spin-network, its norm can be calculated from it in a purely combinatorial way. I shall give the rule later.

But first let me say something about the answer to the second question. How can I say anything about *directions* in space, when I only have the non-directional concept of total angular momentum? How do I get 'm-values' out of j-values, in other words?

Clearly we can't do quite this. In order to know what the 'm-value' of an n-unit is, we would require knowledge of which direction in space is the 'z-direction'. But the 'z-direction' has no physical meaning. Instead, we may ask for the 'orientation' of one of our n-units *in relation to some larger structure* belonging to the system under consideration. We need some larger structure which in fact does give us something that we may regard as a well-defined 'direction in space' and which could serve in place of the 'z-direction'. As we have seen, a structure of spin zero, being spherically symmetrical, is no good for this; spin $\frac{1}{2}\hbar$ is not much better; spin $\hbar$ only a little better; and so on. Clearly we need a system involving a fairly large total angular momentum number if we are to obtain a reasonably well-defined 'direction' against which to test the 'spin direction' of the smaller units. We may imagine that for a large total angular momentum number N, we have the potentiality, at least, to define a well-defined direction as the *spin axis* of the system. Thus, if we *define* a 'direction' in space as something associated with an N-unit with a large N value (I call this a *large unit*), then we can ask how to define *angles* between these 'directions'. And if we can decide on a good way of measuring angles, we can then ask the question whether the angles we get are consistent with an interpretation in terms of directions in a Euclidean three-dimensional space, or perhaps in some other kind of space.

How, then, are we to define an angle between two large units? Well, we do this by performing an 'experiment'. Suppose we detach a 1-unit (e.g. an electron, or any other spin $\frac{1}{2}\hbar$-particle) from a large N-unit in such a way as to leave it as an $(N-1)$-unit. We can then re-attach the 1-unit to some other large unit, say an M-unit. What does the M-unit do? Well (according to the rules we are allowed here) it can either become an $(M-1)$-unit or an $(M+1)$-unit. There will be a certain probability of one outcome and a certain probability of the other. Knowing these probability values, we shall have information as to the angle between the N-unit and the M-unit. Thus, if our two units are to be 'parallel', we would expect zero probability for the $M-1$ value and certainty for the $M+1$ value. If the two units are to be 'anti-parallel' we would expect exactly the reverse probabilities. If they are 'perpendicular', then we would expect equal probability values of $\frac{1}{2}$, for each of the two outcomes. Generally, for an angle θ between the directions of the two large units we would expect a

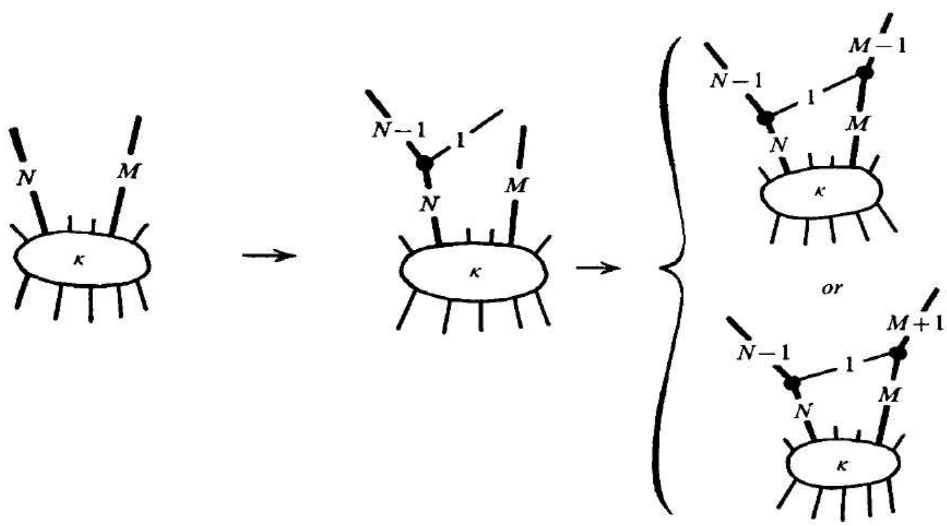

Fig. 3

probability $\frac{1}{2}-\frac{1}{2}\cos\theta$ for the M-unit to be reduced to an $(M-1)$-unit and a probability $\frac{1}{2}+\frac{1}{2}\cos\theta$ for it to be increased to an $(M+1)$-unit. Let me draw a diagram to represent this experiment (fig. 3). Here κ represents some known spin-network. By means of a precise (combinatorial) calculational procedure—which I shall describe shortly—we can calculate, from knowledge of the spin-network κ, the probability of each of the two possible final outcomes. Hence, we have a way of getting hold of the concept of Euclidean angle, starting from a purely combinatorial scheme.

As I remarked earlier, these probabilities will always turn out to be *rational* numbers. You might think, then, that I could only obtain angles with rational cosines in this way. But this would be a somewhat misleading way of viewing the situation. With a finite spin-network with finite spin-numbers, the angle can never be quite well-enough defined. I *can* work out numerical values for these 'cosines of angles' for a finite spin-network, but these 'angles' would normally not quite agree with the actual angle of Euclidean space until I go to the limit.

The view that I am expressing here is that rational probabilities are to be regarded as something which can be more primitive than ordinary real number probabilities. I don't need to call upon the full continuum of probability values in order to proceed with the theory. A rational probability $p = m/n$ might be thought of as arising because the universe has to make a choice between m alternative possibilities of one kind and n alternative possibilities of another—all of which are to be equally probable. Only in the limit, when numbers go to infinity do we expect to get the full continuum of probability values.

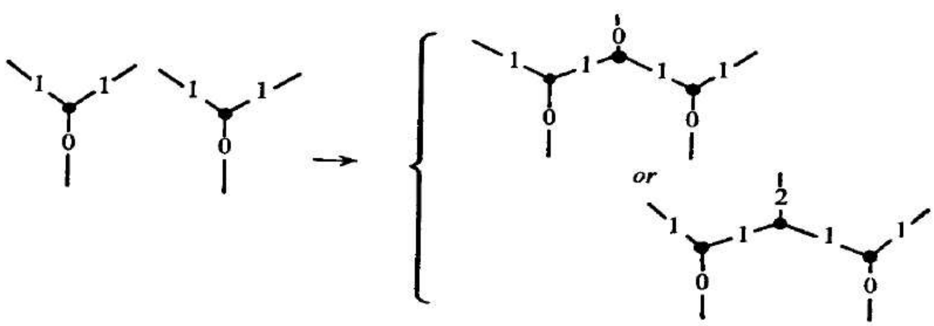

Fig. 4

As a matter of fact, it was this question of rational values for primitive probabilities arising in nature, which really started me off on this entire line of thought concerning spin-networks, etc. The idea was to find some situation in nature which one might reasonably regard as giving rise to a 'pure probability', I am not really sure whether it is fair to assume that 'pure probabilities' exist in nature, but by these I mean probabilities (necessarily quantum mechanical) whose values are determined by nature alone and not in principle influenced by our ignorance of initial conditions, etc. I suppose I might have thought of branching ratios in particle decays as a possible example. Instead, I was led to consider a situation of the following type.

Two spin zero particles each decay into pairs of spin $\frac{1}{2}\hbar$ particles. Two of the spin $\frac{1}{2}\hbar$ particles then come together, one from each pair, and combine to form a new particle (fig. 4). What is the spin of this new particle? Well, it must be either zero or $\hbar$, with respective probabilities $\frac{1}{4}$ and $\frac{3}{4}$ (assuming no orbital components contribute, etc.). Although you can see that there are objections even here to regarding this as giving a 'pure probability', at least the example served as a starting point. (This example was to some extent stimulated by Bohm's version of the Einstein-Rosen-Podolsky thought experiment, which it somewhat resembles.) The idea, then, is that any 'pure probability' (if such exists) ought to be something arising ultimately out of a choice between equally probable alternatives. All 'pure probabilities' ought therefore, to be rational numbers.

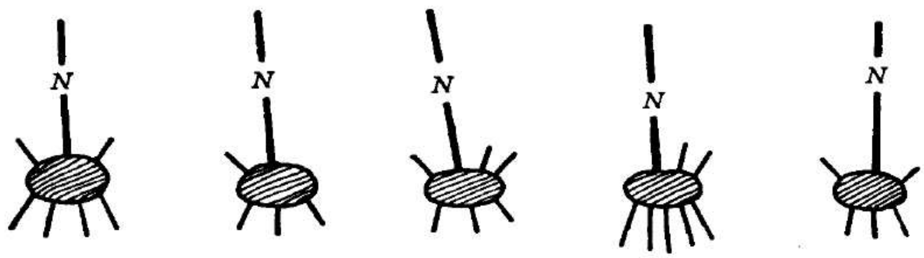

Fig. 5

But let me leave all this aside since it doesn't affect the rest of the discussion. Actually, I haven't quite finished my 'angle measuring experiment', so let me return to this.

Let us consider the following particular situation. Suppose we have a number of disconnected systems, each producing a large N-unit. There are to be absolutely no connections between them (fig. 5).

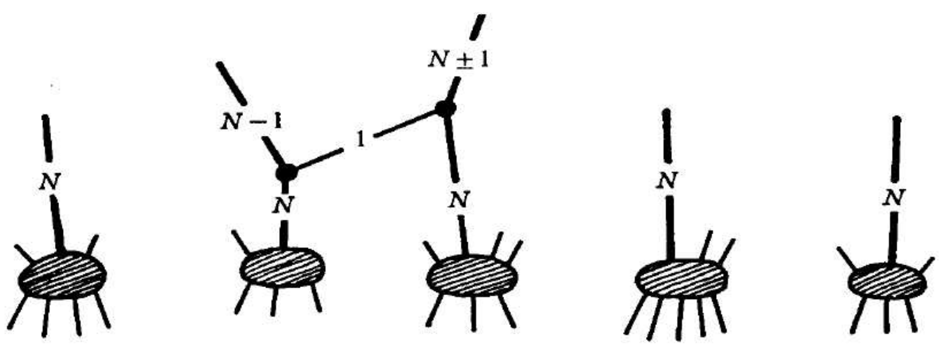

Fig. 6

Let me try to measure the 'angle' between two of them by doing one of the 'experiments' I described earlier. I detach a 1-unit from one of the N-units in such a way as to leave it as an $(N-1)$-unit. Then I reattach the 1-unit to one of the other N-units (fig. 6). According to the rules (cf. later) it will follow that the probability of the second N-unit to become an $(N \pm 1)$-unit is $\frac{1}{2}(N+1 \pm 1)/(N+1)$. These two probabilities become equal in the limit $N \to \infty$. Thus, if we are to assign an 'angle' between these units, then, for N large, this would have to be a *right-angle*. This is just using the probability blindly. I would similarly have to say, of any other pair of the N-units, that they are at right-angles. It would seem that I could put any number of N-units at right-angles to each other. In this instance I have drawn five. Does this mean that we get a five-dimensional space?—or an ∞-dimensional space?

Clearly I have not done things quite right. There are no connections between any of the N-units here, so one would like to think of the probabilities that arise out of one of these experiments as being not *just* due to the *angle* between the N-units (if they have an angle in some sense), but also due to the 'ignorance' implicit in the set-up. That is, we think of the probabilities as arising in two different ways. In the first instance, probabilities can arise in this type of experiment, if we have a *definite angle* between two spinning bodies (as we have seen). These are the genuine quantum mechanical probabilities. But, in the second instance, we may just be *uncertain* as to what the angle is between the two bodies. This lack of knowledge, concerning the history (or origins) of the two bodies, will give us a contribution to the probability value—an *ignorance* factor—which will serve to obscure the meaning of the probability in terms of angles. In the present instance, we are allowed absolutely no information concerning the interconnections between the different N-units, so the probability is

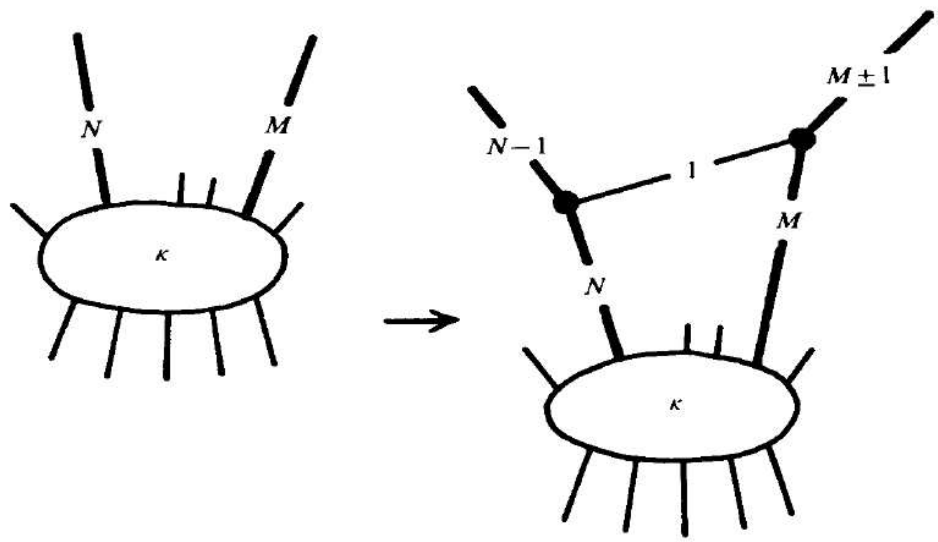

Fig. 7

not really due to 'angle' at all. In this extreme case, the probability is *entirely* 'ignorance factor'. In general, the two effects will be mixed up, so we shall need a means of separating them.

Let me change the picture a bit. I'll put in some 'known' connecting network (now denoted by κ) and have two large units coming out, as in fig. 7. I do one of these experiments, but then *repeat* the experiment. Suppose the N-unit is reduced to an $(N-1)$-unit and then to an $(N-2)$-unit. The M-unit becomes an $(M \pm 1)$-unit and then an $(M \pm 2)$-unit, or an M-unit again (fig. 8). The question is:

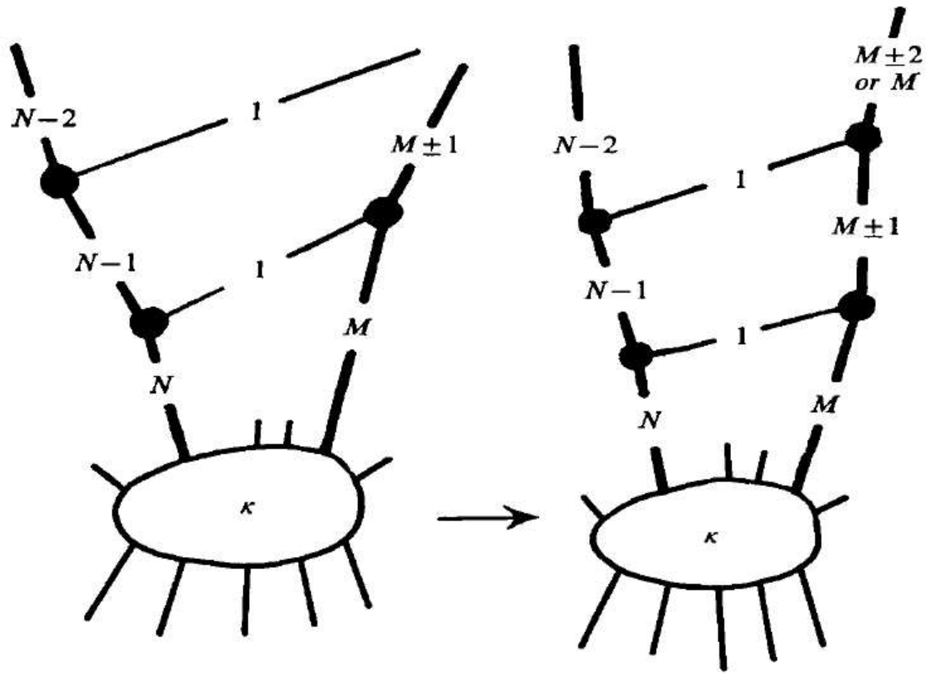

Fig. 8

is the probability of the second experiment influenced by the result of the first experiment? If this is essentially an 'ignorance' situation, where one doesn't initially know such about how the spin axes are pointing, then the *result* of the first experiment provides us with some *information* as to the relative directions of the spin axes. Therefore, the probabilities in the second experiment will be altered by the knowledge of the result of the first experiment.*

If the probabilities calculated for the second experiment are *not* substantially altered by the knowledge of the result of the first experiment, then I say that the angle between the two large units is essentially well-defined. If they *are* substantially altered by the results of the first experiment, then there is a large 'ignorance' factor involved, and the probabilities arise not just from 'angle'.

*It should be borne in mind that all these probability values are simply calculated here, from knowledge of the spin-networks involved. The 'experiments' are really theoretical constructions. However, it would be possible (in principle—with the usual reservations) to measure these probabilities experimentally, by simply repeating the experiment many times, each time reconstructing the spin-network afresh.

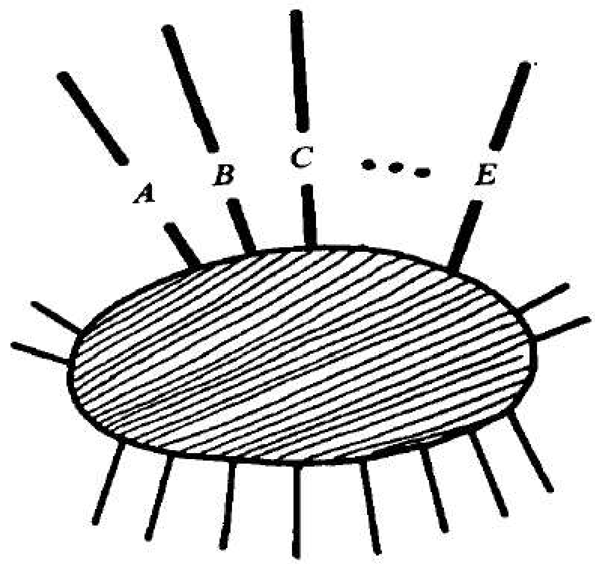

Fig. 9

Suppose, now, we have the system shown in fig. 9, which has a number of large units emerging, and suppose that it happens to be the case that the angle between any two of them is well-defined in the sense I just described. (All the numbers $A, B, \ldots$ are large compared with unity; I can do a few odd experiments which do not much change these numbers.) Then there is a theorem which can be proved to the effect that these angles are all consistent with angles between directions in Euclidean three-dimensional space.

Now, should I be in any way surprised by this result? Admittedly I should have been surprised if the method gave me any different space; but on the other hand, it is not completely clear to me that the result is something I could genuinely have inferred beforehand. Let me mention a number of curious features of the theory in this context. In the first place, suppose I set the situation up with wave functions and everything, and work according to ordinary quantum mechanical rules. I have these particles (or systems) with large angular momentum, and I finally find out that I get these angles consistent with directions in Euclidean three-dimensional space. I never, at any stage, specified that these large angular momentum systems should, in fact, correspond to bodies which do have well-defined directions (as rotation axes). There are states with large total angular momentum (e.g. $m = 0$ states) which point all over the place, not necessarily in any one direction.

I can start from some *given* Euclidean 3-space and use an ordinary Cartesian description in terms of x, y, z. I can use particles (or systems) with large total angular momentum, but which do *not* happen to give well-defined directions in the original space. Then I work out the 'angles' between them and find that these angle do not correspond to anything I can see as angles in the original description, but they are nevertheless consistent with the angle between directions in *some* abstract Euclidean three-dimensional space. I therefore take the view that the Euclidean three-dimensional space that I get out of all this, using probabilities, etc. is the *real space*, and that the original space, with its x, y, z's that I wrote down, is an irrelevant convenience, like co-ordinates in general relativity, where one writes down any co-ordinates which don't necessarily mean anything. The central idea is that *the system defines the geometry*. If you like, you can use the conventional description to fit the thing into the 'ordinary space-time' to begin with, but then the geometry you get out is not necessarily the one you put into it. So I don't know whether I should be surprised or not by the fact that I actually get the right geometry in the end.

There is a second aspect of this work that I think I regarded as slightly surprising at first. This is the fact that although no complex numbers are ever introduced into the scheme, we can still build up the full *three*-dimensional array of directions, rather than, say, a two-dimensional subset. To represent all possible directions as states of spin of a spin $\frac{1}{2}\hbar$ particle, we need to take *complex* linear combinations (in the conventional formalism). Here we only use rational numbers—and complex numbers cannot be approximated by rational numbers alone! Again, the answer seems to be that the space I end up with is not really the 'same' space as the (x, y, z)-space that I could start with—even though both are Euclidean 3-spaces.

One might ask whether corresponding rules might be invented which lead to other dimensional schemes. I don't in fact see *a priori* why one shouldn't be able to invent rules, similar to the ones I use, for spaces of other dimensionality. But I'm not quite sure how one would do this. Also, it's not obvious that the whole scheme for getting the space out in the end would still work. The rules I use are derived from the irreducible representations of SO(3). These have some rather unique features.

Now, from what I've said so far, you might wonder whether you would just scatter the numbers on the network at random. Actually, you can if you like, but unless you are a bit careful the resulting spin-network will have zero norm. And if the norm is zero, then the situation represented by the spin-network a not realizable (i.e. zero probability) according to the rules of quantum mechanics.

There are, in fact, two simple necessary requirements which must be satisfied at each vertex of a spin-network, for its norm to be non-zero. Notice first that all the spin-networks that I have explicitly drawn have the property that precisely *three* edges (i.e. units) come together at each vertex. (This isn't one of the 'requirements' I am referring to. It's just that I don't know how one would handle more general types of vertex within the scheme.) Suppose we have a vertex at which an a-unit, a b-unit and a c-unit come together (fig. 10).

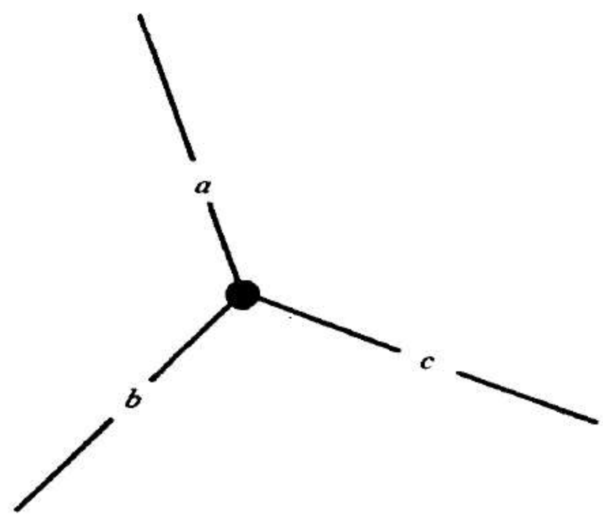

Fig. 10

Then for a spin-network containing the vertex to have a non-zero norm, it is necessary that the triangle inequality hold:

$$a + b + c \geq 2\max(a, b, c) \quad ;$$

and furthermore that there be conservation of fermion number (mod 2):

$$a + b + c \quad \textit{is even.}$$

These are, of course, properties that one would want to hold in real physical processes, with the interpretations that I have given to the spin-networks.

But even if these requirements hold at every vertex, the spin-network may still have zero norm. For example, each of the two types of spin-network shown in fig. 11 has zero norm, where $n \neq 0$ in the first case and $n \neq m$ the second. In each case, the shaded portion represents some spin-network with no other free ends. In fact, the

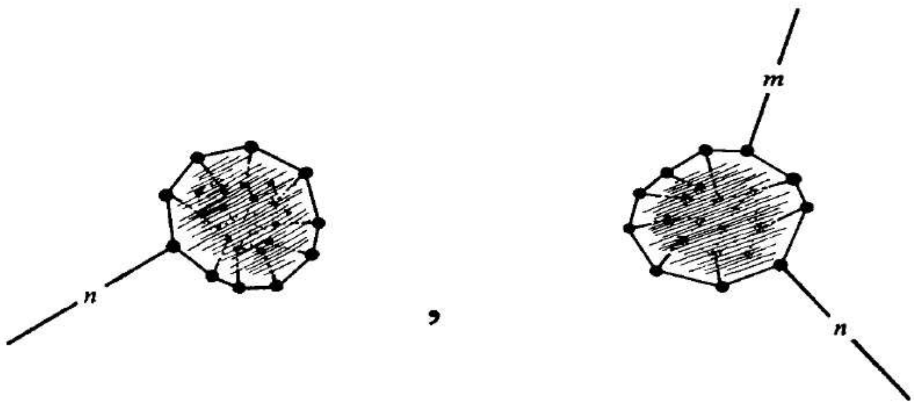

Fig. 11

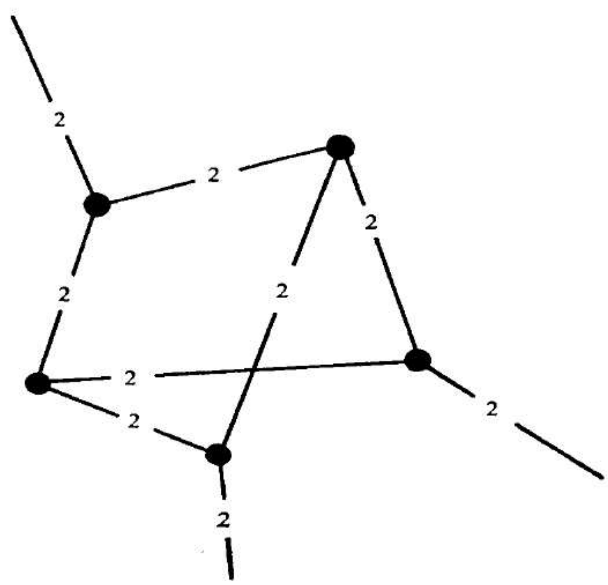

Fig. 12

first is effectively a special case of the second, with $m = 0$. This is because any 0-unit can be omitted from a spin-network (if we also suitably delete the relevant vertices) without changing the norm. We may interpret the vanishing of the norm whenever $n \neq m$ in the second case as an expression of *conservation of total angular momentum.*

In addition to these cases, there are many particular spin-networks which turn out to have zero norm. One example is shown in fig. 12. But so far I have only been giving particular cases. Let us now pass to the general rule.

I shall give the definition of the norm in terms of a closely related concept, namely, what I shall call the *value* of a closed oriented spin-network. I call a spin-network *closed* if it has no free ends (e.g. analogous to a disconnected vacuum process). A spin-network which is not closed will not be assigned a value. The definition of *orientation* for a spin-network is a little difficult to give concisely. Any spin-network can be assigned two alternative orientations. Fixing the orientation of a closed spin-network will serve to define the *sign* of its value (which can be positive or negative). Roughly speaking the orientation assigns a cyclic order to the three units attached to each vertex—but if we reverse the cyclic order at any *even* number of vertices this is to leave the orientation unchanged. The orientation will change, on the other hand, if the cyclic order is reversed at an *odd* number of vertices.

I shall adopt the convention, when drawing spin-networks, that the orientation is to be fixed by the way that the spin-network is depicted on the plane. At each vertex we specify '*counter-clockwise*' as the cyclic order for the three units attached to the vertex. This defines the spin-network's orientation. The diagrams in fig. 13 illustrate an example of a closed spin-network with its two possible orientations.

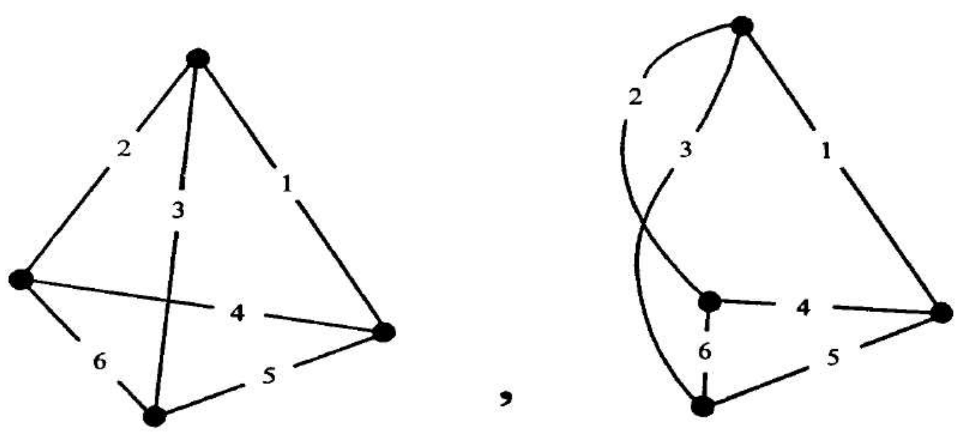

Fig. 13

It will also be convenient to use the representation of a spin-network as a drawing on a plane, in order to keep track of signs properly when defining the value. This may have the effect of making the definition *seem* less 'combinatorial' than it really is. Of course, the definition could be reformulated without the use of such a drawing if desired. Consider, then, a closed spin-network α depicted as a

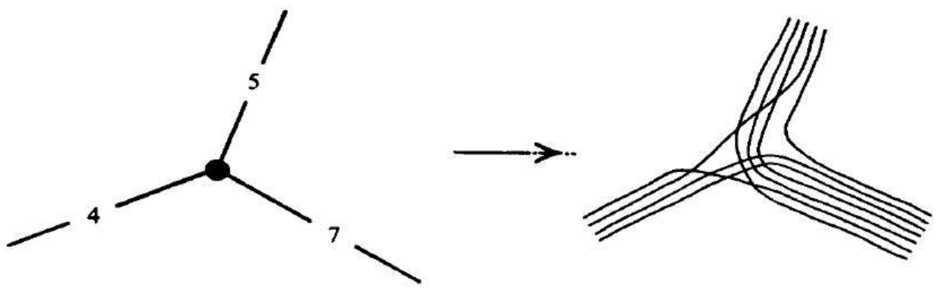

Fig. 14

drawing on a plane. Now, imagine each n-unit to be replaced in the drawing by n parallel strands. At each vertex, the strand ends must be connected together in pairs, but no two strands associated with the same n-unit are to be connected together. Let us call such a connection scheme a *vertex connection.* One such vertex connection is illustrated in fig. 14, while fig. 15 shows a non-allowable connection,

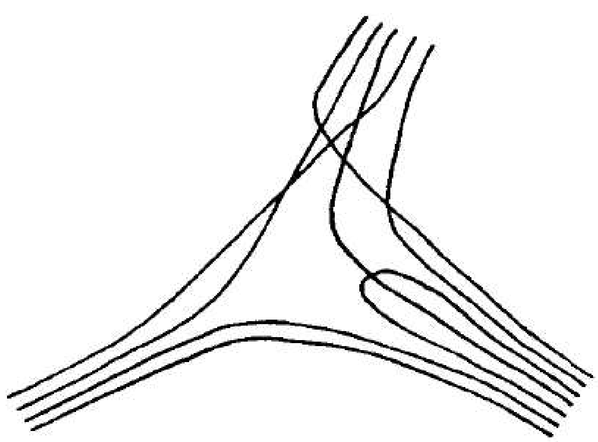

Fig. 15

since two strands of the 7-unit are connected to one another. The *sign* of a vertex connection is defined most simply as $(-1)^x$ where x is the number of intersection points between different strands at the vertex, as drawn on the plane. (These intersection points must be counted correctly if more than two strands cross at a point, or if two strands touch: and ignored if a strand crosses itself. It is simplest on the other hand, just to avoid such features by drawing the strands in general position and not allowing any strand connection to cross itself.) The sign of a vertex connection, in fact, does not depend on the details of how it is drawn, but only on the pairing off of the strands. The allowable vertex connection depicted above has -1 as its sign, since there are thirteen crossing points.

When the vertex connections have been completed at every vertex of a closed spin-network, then we shall have a number of *closed loops*, with no open-ended strands remaining. Consider, now, every possible way of allowably completing the vertex connection for the spin-network α. We form the expression

$$\text{value of } \alpha = \frac{\sum \pm(-2)^c}{\prod n!}$$

where the summation extends over all possible completed allowable connection schemes, where the '$\pm$' stands for the product of the signs of all the vertex connections, where c is the number of closed loops resulting from the vertex connections and where the product in the denominator ranges over all the units of the spin-network, n being the spin-number of the unit. The value of any closed spin-network always turns out to be an integer.

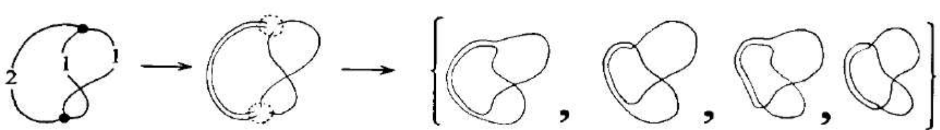

Fig. 16

$$\begin{aligned} \rightarrow \text{ value } &= \frac{1}{2!\,1!\,1!}\{+(-2)-(-2)^2-(-2)^2+(-2)\} \\ &= -6. \end{aligned}$$

Let us consider a simple example, given in fig. 16. Note that the 'accidental' intersection, arising from the crossing of the two 1-units in the first drawing of the spin-network, does not contribute to the sign of the terms in the sum. Only the intersections at the vertex connections count.

The definition of the value of a closed oriented spin-network that I have just given is perhaps the simplest to state, but it is by no means the most useful to use in actual calculations. When the spin-networks become even slightly more complicated than the simple one evaluated above, the detailed calculations can become very unwieldy. A more useful procedure is to employ certain reduction formulae which can be used to express complicated networks in terms of simpler ones.† For this purpose, it will be necessary to introduce a slight variation of the spin-network theme; I shall consider the related concept of a *strand-network*.

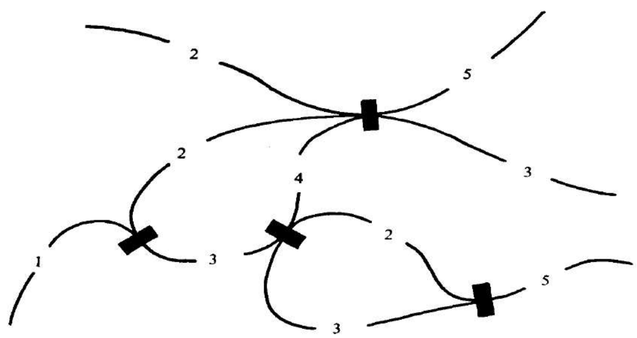

Fig. 17

A strand-network is a series of connections relating objects (which I shall still refer to as n-units) an example of which is depicted in fig. 17. The units are 'tied together' at various places, as indicated by the thick bar. Any spin-network can be translated into strand-network terms, by replacing each vertex according to the scheme shown in fig. 18. I thus introduce three more ('virtual') units at each vertex. A strand-network is *closed* if it has no free ends. Any closed (oriented) strand-network will have a *value* which is an integer (positive, negative or zero). This value will be chosen to agree with that defined for a spin-network, in the case of closed strand-networks obtained by means of the above replacement. Generally, to obtain the value of s closed strand-network β we employ the same formula as before:

$$\text{value of } \beta = \frac{\sum \pm(-2)^c}{\prod n!}$$

†Diagrams closely related to spin-networks were introduced by Ord-Smith and Edmonds for the graphical treatment of quantum mechanical angular momentum. (See reference (l) for a detailed discussion.)

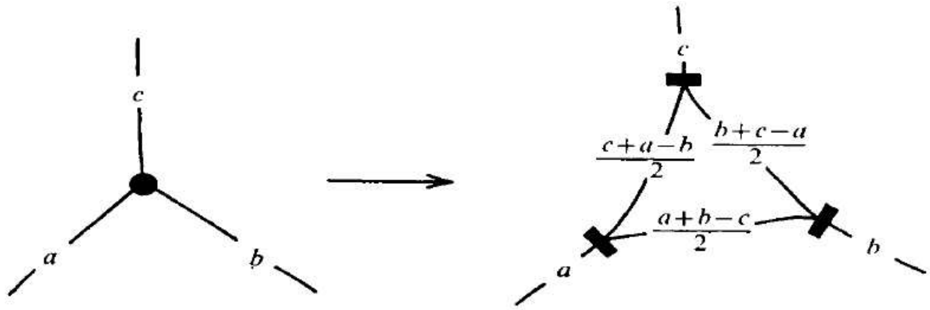

Fig. 18

where now the '±' sign refers to the product of all the signs of all the permutations involved in each strand connection at which the strands come together. For example, one possible connection scheme for

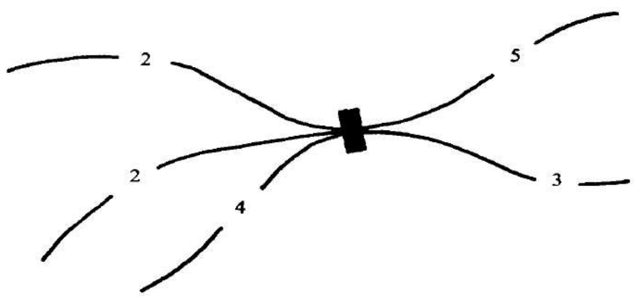

Fig. 19

'strand vertex' of fig. 19, would be that shown in fig. 20. This connection scheme would contribute a minus sign, since an odd permutation is involved. (There are nine crossing points—this is essentially

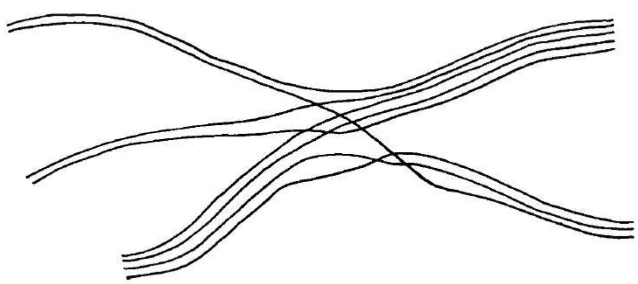

Fig. 20

the 'Aitken diagram' method of determining the sign of a permutation.) Notice that for a connection scheme to be possible at all, we require that the total of the spin-numbers entering at one side must equal the total of the spin-numbers leaving at the other. This *one* requirement now takes the place of the 'triangle inequality' and 'fermion conservation' that we had earlier.

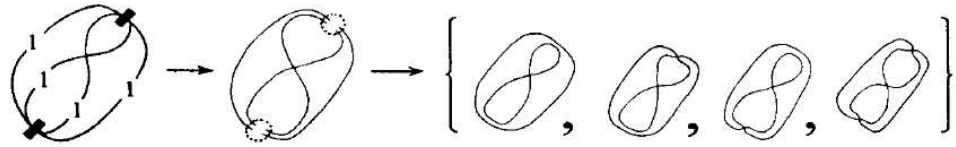

Fig. 21

$$\therefore \text{ value } = \frac{1}{1!\,1!\,1!\,1!}\{+(-2)^2-(-2)-(-2)+(-2)\}$$
$$= 2.$$

Let us evaluate the simple closed strand-network of fig. 21 as an example. Again there is an 'accidental' intersection depicted (where two 1-units cross) which does not contribute to the sign of the terms in the sum.

Let me list a number of relations and reduction formulae which are useful in evaluating strand-networks (fig. 22). (I am not going to prove anything here, but most of the relations are not hard to verify.) These relations may be substituted into any closed strand-network and a valid relation between values is obtained. Finally, let me make the remark that the value is *multiplicative*, that is to say, the value of the union of two *disjoint* strand-networks or spin-networks is equal to the product of their individual values.

I now come to the definition of the *norm* of a spin-network. A strand-network will likewise have a norm. This is simply obtained by drawing *two* copies of the spin-network (or strand-network), joining together the corresponding free end units to make a closed network, and then taking the modulus of the value of this resulting closed network. As an example, the norm of the spin-network consisting of a single vertex is found in fig. 23. An even simpler example, depicted in fig. 24, is the norm of a single isolated n-unit.

Finally, let me describe how the norm may be used in the calculation of *probabilities* for spin-numbers, in the type of 'experiment' that we have been considering. (Again I shall give no proofs.) Suppose we start with a spin-network α, with an a-unit and a b-unit among its

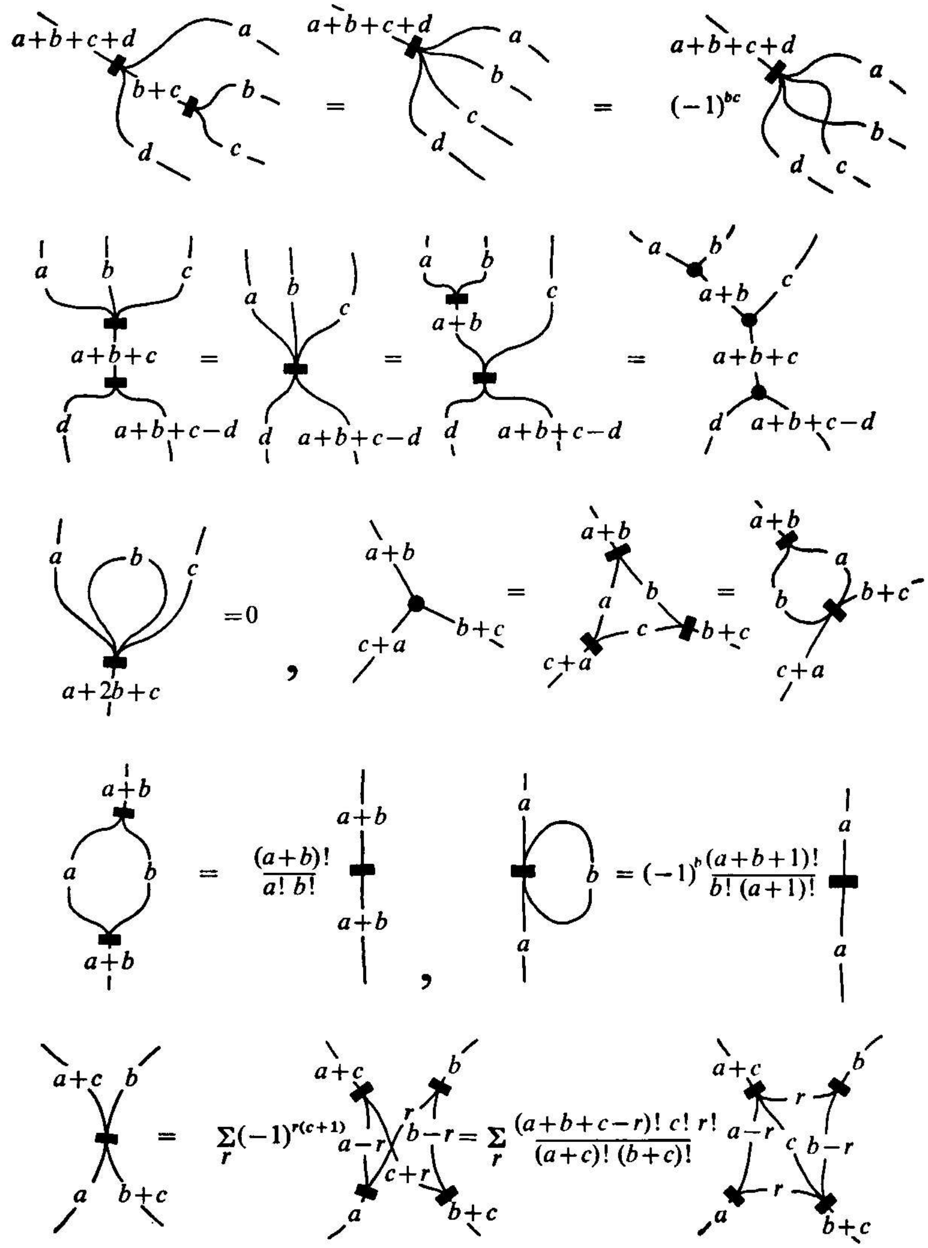
a+b+c+d
b+c
$(-1)^{bc}$
a+b+c
a+b+c−d
=0
a+2b+c
a+b
b+c
c+a
$\frac{(a+b)!}{a!\,b!}$
$= (-1)^b \frac{(a+b+1)!}{b!\,(a+1)!}$
a+c
$\sum_r (-1)^{r(c+1)}$
a−r
b−r
c+r
$= \sum_r \frac{(a+b+c-r)!\,c!\,r!}{(a+c)!\,(b+c)!}$

Fig. 22

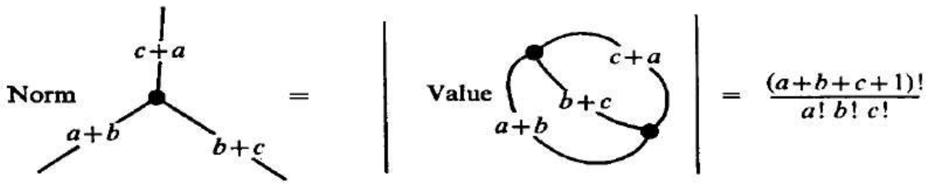

Fig. 23

free ends (fig. 25). Suppose the a-unit and the b-unit come together to form an x-unit, the resulting spin-network being denoted by β (see fig. 26). We wish to know (given α) what are the various probabilities for the different possible values of the spin-number x. Let γ denote the spin-network representing the coming together of the a-unit and

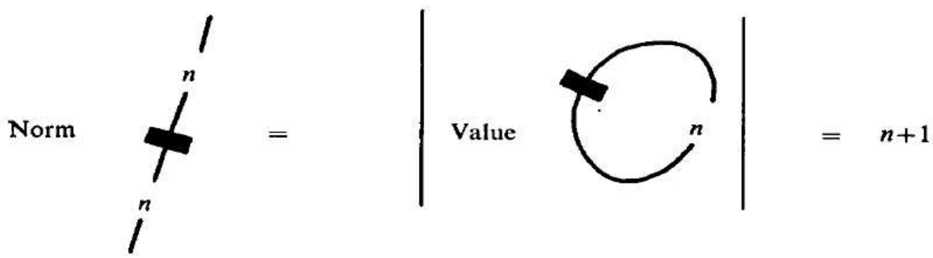

Fig. 24

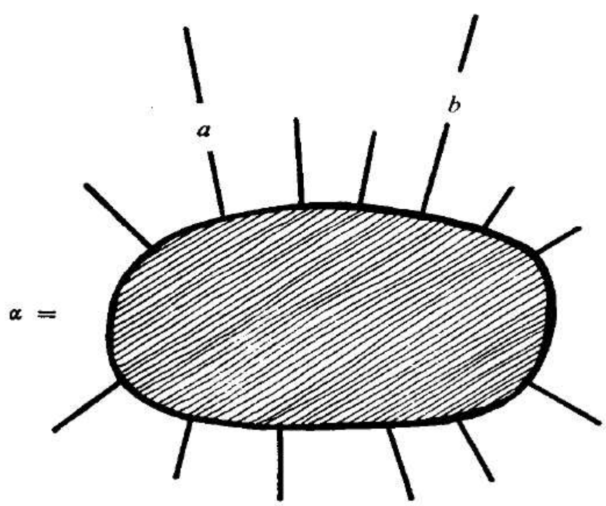

Fig. 25

the b-unit to form the x-unit. Let ξ denote the spin-network consisting of the x-unit alone. These are illustrated in fig. 27. Then the required probability for the resulting spin-number to be x is

$$\text{probability} = \frac{\text{norm}\beta\,\text{norm}\xi}{\text{norm}\alpha\,\text{norm}\gamma}\,.$$

Using the explicit expressions for norm γ and norm ξ that were just given as examples (using a slightly different notation), we can rewrite this as

$$\text{probability} = \frac{\text{norm}\beta\,(x+1)\,\{\frac{1}{2}(a+b+x)+1\}!}{\text{norm}\alpha\,\{\frac{1}{2}(a+b-x)\}!\,\{\frac{1}{2}(b+x-a)\}!\,\{\frac{1}{2}(x+a-b)\}!}$$

From the combinatorial nature of the definition of norm, it is clear that these probabilities must all be rational numbers. And with the interpretation of ‘angle’ that I have given, the three-dimensional Euclidean nature of the ‘directions in space’ that are obtained, is a *consequence* of these combinatorial probabilities.

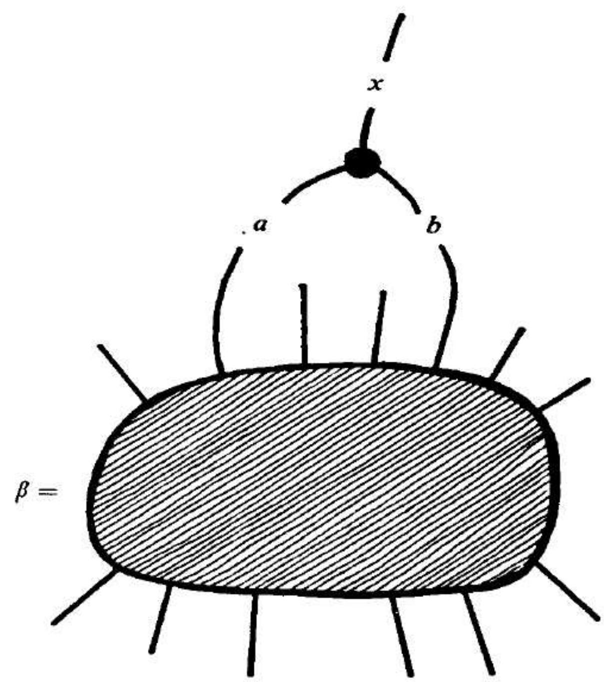

Fig. 26

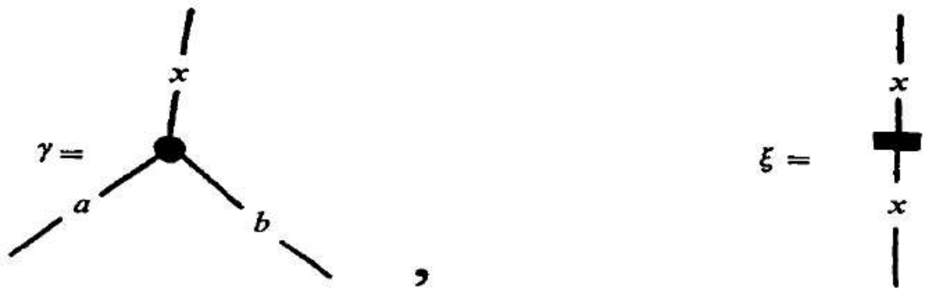

Fig. 27

I should emphasize again that the space that I get out in the end is the one *defined* by the system itself and is not really the same space as the one that might have been introduced at the start if a conventional formalism had been used. Thus, although undoubtedly the reason that we end up with directions in a Euclidean three-dimensional space is intimately *related* to the fact that we start with representations of the rotation group SO(3), the precise logical connections are not at all clear to me. When I come to consider the generalization of all this to a relativistic scheme in a moment, this question will again present itself. I shall also need to consider the spatial locations of objects, not just their orientations. My model works with objects and the interrelations between objects. An object is thus 'located', either directionally or positionally in terms of its relations with other objects. One doesn't really need a space to begin with. The notion of space comes out as a *convenience* at the end.

Essentially, I have so far been using a non-relativistic scheme. The angular momentum is not relativistic angular momentum. From a four-dimensional viewpoint, the directions I get are those orthogonal to a given timelike direction, i.e. directions in three-dimensional space. All the particles are going along in this same timelike direction. Perhaps they can knock each other a little bit, but they are not really moving very much. They just transfer angular momentum backwards and forwards. All the particles are, strictly speaking in the same place, not moving relative to one another. Consequently, one does not have any problem of mixing between orbital and spin angular momentum. Once I allow orbital contributions to come in, then I must drastically change the scheme, since now not only is the question of 'direction' and 'angle' involved, but so also is 'position' and 'distance'. Thus, if one thinks of *real* particles moving relative to each other, then there is the problem not only of doing things relativistically, but also of bringing in actual displacements between particles. Consider two particles in relative motion. Suppose they come together and combine to form a system with a well-defined total spin. Then to obtain the spin of the combined system, we cannot just add up the individual spins because we have to bring in the orbital component. There is a mixture of the actual displacements in space with the angular momentum concept. So I spent a long time thinking how one should combine rotations and displacements together into an appropriate relativistic scheme. Eventually I was led to consider a certain algebra for space-time which treats linear displacements on the same footing as it treats rotations. Thus, linear momentum is treated on a similar footing to angular momentum.

Now you might raise the objection that linear momentum has a continuous spectrum, while it is only for angular momentum that one has discreteness. This is a problem of some significance to us. My answer to it is roughly the following: each particle has its own discrete spectrum for its angular momentum. When two particles are considered together as a unit, then again there is a discrete spectrum for the combined system. The way these 'spins' add up implicitly brings in the relative motion between the two particles. So the momentum is brought in through the back door, in a sense, for one could be always talking in terms of 'bound systems'.

I consider momentum states as being linear combinations of angular momentum states. There is indeed a problem to see how this continuous momentum should be built up from something discrete, but, in principle, there is nothing against it. In effect, the idea is that the momentum should be brought in indirectly. I would propose that, in a sense, there should *not* be a well-defined distinction between momentum and angular momentum—except in the limit. Individual particles and simple systems would not really 'know' what momentum is. Like the idea of 'direction' that I considered earlier, it would be only in the limit of large systems that the concept of momentum really attains a well-defined meaning. Smaller systems might retain a combined concept of momentum and angular momentum, but these things would only sort themselves out properly in the limit.

The algebra I have used to treat linear displacements and rotations together, or linear and angular momentum together, I call the algebra of *twistors*. I have used the term 'twistor' to denote a 'spinor' for the six-dimensional $(+\,+\,-\,-\,-\,-)$ pseudo-orthogonal group O(2,4). The twistor group is the $(+\,+\,-\,-)$ pseudo-unitary group SU(2,2), which is locally isomorphic with O(2,4). In turn, O(2,4) is locally isomorphic with the fifteenparameter (local) conformal group of space-time. Under a conformal transformation of space-time, the twistors will transform (linearly) according to a representation of the group SU(2,2).

The basic twistor is a four-complex-dimensional object. We can thus describe it by means of four complex components Z^α:

$$(Z^\alpha) = (Z^0,\, Z^1,\, Z^2,\, Z^3)\ .$$

The complex conjugate of the twistor Z^α is an object $\bar{Z}_\alpha$ whose components

$$(\bar{Z}_\alpha) = (\bar{Z}_0,\, \bar{Z}_1,\, \bar{Z}_2,\, \bar{Z}_3)$$

are given (according to a convenient co-ordinate system) by

$$\bar{Z}_0 = \overline{(Z^2)}, \quad \bar{Z}_1 = \overline{(Z^3)}, \quad \bar{Z}_2 = \overline{(Z^0)}, \quad \bar{Z}_3 = \overline{(Z^1)}.$$

This implies that the Hermitian form $Z^\alpha \bar{Z}_\alpha$ (summation convention assumed) has signature $(++--)$, which is required in order to give SU(2,2). (I have already described these objects(2) and their geometrical significance in Minkowski space-time, and a later paper(3) goes into some further developments, including some of the quite surprising aspects of the theory which arise when one starts to describe physical fields in twistor terms.)

When $Z^\alpha \bar{Z}_\alpha = 0$ I call Z^α a *null* twistor. A null twistor Z^α has a very direct geometrical interpretation in space-time terms. In fact, Z^α defines a *null straight line*, which we can think of as the world line of a zero rest-mass particle. (An important aspect of twistor theory is that *zero rest-mass* is to be regarded as more fundamental than a finite rest-mass. Finite mass particles are viewed as composite systems, the mass arising from interactions.) The twistor λZ^α, for any non-zero complex number λ, defines the same null line as does Z^α. But we can distinguish Z^α from λZ^α by assigning to the particle a 4-momentum (in its direction of motion) and, in addition, a sort of 'polarization' direction (both constant along the world line of the particle). When Z^α is replaced by rZ^α (r real) the momentum is multiplied by r^2. When Z^α is replaced by $e^{i\theta}Z^\alpha$ (θ real) the 'polarization' plane is rotated through an angle 2θ. If Y^α is another null twistor, the condition for the null lines represented by Y^α and Z^α to *meet* is

$$Y^\alpha \bar{Z}_\alpha = 0,$$

i.e. this is the condition for the two particles to 'collide'. (To be strictly accurate we have to include the possibility that they may 'meet at infinity'.

In addition, some of the null twistors describe 'null lines at infinity' rather than actual null lines.)

The *non-null* twistors are divided into two classes according as $Z^\alpha\,\bar{Z}_\alpha$, is positive or negative. If $Z^\alpha\,\bar{Z}_\alpha > 0$, I call Z^α right-handed; if $Z^\alpha\,\bar{Z}_\alpha < 0$, left-handed. In Minkowski space-time, one can give an interpretation of a non-null twistor in terms of a twisting system of null lines. The helicity of the twist is defined by the sign of $Z^\alpha\,\bar{Z}_\alpha$. In more physical terms, the twistor Z^α (up the phase) describes the momentum and angular momentum structure of a zero rest-mass particle.‡ We can make the interpretation that $Z^\alpha\,\bar{Z}_\alpha$, is (twice) the *intrinsic spin* of the particle, measured in suitable units, with a *sign* defining the helicity. If $Z^\alpha\,\bar{Z}_\alpha \neq 0$, then it is not possible actually to localize the particle as a null straight line. Only if $Z^\alpha\,\bar{Z}_\alpha = 0$ do we get a uniquely defined null line which we can think of as the world line of the particle; otherwise the particle to some extent spreads itself throughout space.

The twistor co-ordinates Z^0, Z^1, Z^2, Z^3, together with their complex conjugates $\bar{Z}_0$, $\bar{Z}_1$, $\bar{Z}_2$, $\bar{Z}_3$, can be used in place of the usual x, y, z, t and their canonical conjugates p_x, p_y, p_z, E. In fact, anything that can be written in normal Minkowski space terms can be rewritten in terms of twistors. However, in principle, the twistor expressions for even quite simple physical processes may turn out to be very complicated. But in fact it emerges that the basic elementary processes that one requires, can actually be expressed very simply if one goes about it in the right way. Analytic (holomorphic) functions in the Z^α variables play a key role. So does contour integration.

We can regard the Z^α as quantum operators under suitable circumstances. Then $\bar{Z}_\alpha$, can be regarded as the canonical conjugate of Z^α. (I shall go into the reasons for this a little more later.) We have commutation rules

$$Z^\alpha\,\bar{Z}_\beta - \bar{Z}_\beta\,Z^\alpha = \delta^\alpha{}_\beta\,\hbar$$

‡Using a convenient co-ordinate system, we can relate the momentum P_α and the angular momentum $M^{ab}(= -M^{ba})$ to the twistor variables Z^α, $\bar{Z}_\alpha$ a by:

$$P_0 + P_1 = 2^{\frac{1}{2}}\,Z^0\,\bar{Z}_2, \quad P_0 - P_1 = 2^{\frac{1}{2}}\,Z^0\,\bar{Z}_3, \quad P_2 + i\,P_3 = 2^{\frac{1}{2}}\,Z^1\,\bar{Z}_2$$
$$P_2 - i\,P_3 = 2^{\frac{1}{2}}\,Z^0\,\bar{Z}_3, \quad M^{23} + i\,M_{01} = Z^1\,\bar{Z}_1 - Z^0\,\bar{Z}_0, \quad M^{23} - i\,M_{01} = Z^3\,\bar{Z}_3 - Z^2\,\bar{Z}_2$$
$$M^{13} + M^{03} + i\,M_{02} + i\,M_{12} = 2\,Z^2\,\bar{Z}_2, \quad M^{13} - M^{03} + i\,M_{02} - i\,M_{12} = 2\,Z^3\,\bar{Z}_2$$
$$M^{13} + M^{03} - i\,M_{02} - i\,M_{12} = 2\,Z^1\,\bar{Z}_0, \quad M^{13} - M^{03} - i\,M_{02} + i\,M_{12} = 2\,Z^0\,\bar{Z}_1$$

Then, since Z^α and $\bar{Z}_\alpha$ do not commute, we must re-interpret the expression for the *spin-helicity* $\frac{1}{2}n$ as the *symmetrized* quantity,

$$\frac{1}{4}(Z^\alpha \bar{Z}_\alpha - \bar{Z}_\alpha Z^\alpha) = \frac{1}{2}n\hbar$$

Only zero rest-mass states can be eigenstates of this operator. The eigenvalues of n are $\ldots -2, -1, 0, 1, 2, \ldots$ The operators for the ten components of momentum and angular momentum (together with those for the five extra components arising from the conformal invariance of zero rest-mass fields) are

$$Z^\alpha \bar{Z}_\beta - \frac{1}{4}\delta^\alpha{}_\beta Z^\gamma \bar{Z}_\gamma$$

in twistor notation. The usual commutation rules for momentum and angular momentum are then a consequence of the twistor commutation rules.

One idea would now be to use this fact and simply let the twistors take the place of the *two-component spinors* that lay 'behind the scenes' in my previous non-relativistic approach, and then to attempt to build a concept of a four-dimensional space-time from whatever graphical algebra arises from the twistor rules. I have not attempted to do quite this, as yet, since I am not sure that it is exactly the right thing to do. There are certain other aspects of twistor theory which should really be taken into account first.

Let me mention one particular point. It is a rather remarkable one. If the twistor approach is going to have any fundamental significance in physical theory, then it ought, in principle at least, to be possible to carry the formalism over and apply it to a *curved* space-time, rather then just a flat space-time. These objects, as originally defined, are very much tied up with the Minkowski flat space-time concept. How can we carry them over into a curved space-time? Actually, a twistor for which $Z^\alpha \bar{Z}_\alpha = 0$ carries over very well. Its interpretation is now simply as a *null geodesic* (i.e. world line of a freely moving massless particle) with a momentum (pointing along the world line) and a 'polarization' direction (both covariantly constant along the world line). On the other hand, it does not seem to be possible to interpret a *non-null* twistor, in a general curved space-time, in precise classical space-time terms. Nevertheless it turns out to be convenient to *postulate* the existence of these non-null twistors—as objects with no classical realization in curved space-time terms. (In a sense, twistors are more appropriate to the treatment of *quantized* gravitation[§] than of classical general relativity.)

[§]Since this lecture was delivered, there have been some developments in twistor theory

Let us concentrate attention, for the moment, on the *null* twistors only so that we can consider purely geometrical questions. We are interested in properties of null geodesics which refer to each geodesic as a *whole* and not to the neighbourhood of some point on a geodesic. Consider, for example, the condition of orthogonality between twistors. We have seen that in flat space-time, the condition of orthogonality $Y^\alpha \bar{Z}_\alpha = 0$ between two twistors Y^α, Z^α corresponds to the *meeting* of the corresponding null lines. In curved space-time this is not really satisfactory, because although I can tell whether two null geodesics are going to meet if I look in the neighbourhood of the intersection point, if I look somewhere else, I can't see whether or not they will meet, because the curvature may have bent them away from each other. So, in fact, the orthogonality property is not something which is preserved, as an invariant concept, when one turns to curved space-time. On the other hand, certain things *are* preserved; and, somewhat surprisingly, they correspond to assigning a *symplectic* structure to the twistor space.

This symplectic structure is expressed (in appropriate co-ordinates) as the invariance of the 2-form

$$dZ^\alpha \wedge d\bar{Z}_\alpha$$

(using Cartan notation). Strictly speaking, this requires the postulated non-null twistors, in addition to the null ones. The null twistors only form a seven-real-dimensional manifold, whereas a symplectic manifold must be even-dimensional. The null twistor manifold must be embedded in the eight-real-dimensional manifold consisting of *all* twistors. The structure of the null twistor manifold is that induced by the embedding in this eight-dimensional symplectic manifold. In addition to the symplectic structure (and closely related to it), the expressions

$$Z^\alpha \bar{Z}_\alpha, \quad Z^\alpha d\bar{Z}_\alpha, \quad Z^\alpha \frac{\partial}{\partial Z^\alpha}$$

---

of relevance to this discussion. It seems to be possible to express quantized gravitational theory in twistor form. By means of $3k$-dimensional contour integrals in spaces of many twistor variables, one can apparently calculate scattering amplitudes for processes involving gravitons, photons and other particles. Diagrams arise which can be used to replace the spin-networks of the formalism described here. It is not impossible that the calculations can be reduced to a set of comparatively simple combinatorial rules, but it is unclear, as yet, whether this is so. The work is still very much at a preliminary stage of development and many queries remain unanswered.

are also invariant. All these invariant quantities can be interpreted, to some extent, in terms of the geometrical properties of null geodesics. But it will not be worthwhile for me to go into all this here.

The invariance of the symplectic structure of the twistor space for curved space-time can be re-expressed as the invariance of the *Poisson brackets*

$$[\psi, \chi] = i \frac{\partial \psi}{\partial Z^\alpha} \frac{\partial \chi}{\partial \bar{Z}_\alpha} - i \frac{\partial \chi}{\partial Z^\alpha} \frac{\partial \psi}{\partial \bar{Z}_\alpha} \quad .$$

This strongly suggests that in the passage to quantum theory, $\bar{Z}_\alpha$ should be regarded as the conjugate variable to Z^α. Thus, we are led to the commutation rules I mentioned earlier, relating quantum operators Z^α and $\bar{Z}_\alpha$. These commutation rules in turn give us the commutation relations for momentum and angular momentum, as I indicated before. This suggests that there may possibly be some deep connection between these commutation rules (or perhaps some slight modifications of them) and the curvature of space-time.

The picture that one gets is that in some sense the curvature of space-time is to do with canonical transformations between the twistor variables Z^α, $\bar{Z}_\alpha$. Suppose we start in some region of space-time where things are essentially flat. Then we can interpret Z^α and $\bar{Z}_\alpha$ in a straightforward way in terms of geometry and angular momentum, etc. Suppose we then pass through a region of curvature to another region where things are again essentially flat. We then find that our interpretations have undergone a 'shift' corresponding to a canonical transformation between Z^α and $\bar{Z}_\alpha$. In effect the 'twistor position' (i.e. Z^α) and 'twistor momentum' (i.e. $\bar{Z}_\alpha$) have got mixed up. Somehow it is this mixing up of the 'twistor position' and 'twistor momentum' which corresponds to what we see as space-time curvature.

Going back to my original combinatorial approach for building space up from angular momentum, we can ask now whether such a combinatorial scheme could be applied to the twistors. Might it be that, instead of ending up with a flat space, we could end up with a curved space-time? Even if I start with the commutation rules appropriate just to the Poincaré group, or perhaps the conformal group, it is obvious that I must end up with essentially the same space that I 'start' with? One has to define the things with which one builds up geometry (e.g. points, angle, etc.), in *terms* of the physical objects under consideration. It is not at all clear to me that the geometry that is built up in one region will not be 'shifted' with respect to the geometry built up in some other region. Is it then not possible that a space-time possessing curvature might be the result? That is really the final point I wanted to make.

REFERENCES

(l) Yutsis, A. P., Levinson, I. B. and Vanagas, V. V. *Mathematical apparatus of the theory of angular momentum* (Jerusalem: I.P.S.T., 1962).
(2) Penrose, R. *J. Math. Phys.* (1967) **8**, 345.
(3) Penrose, R. *Int. Jl Theor. Phys.* (1968) **1**, 61.