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*an Inaugural Lecture delivered
at the University College of Swansea
on 16 October 1973*

by

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M.A., D.Phil.

Department of Pure Mathematics

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ON THE TOSS OF A COIN

I

A long time ago, I went out and bought the score of Beethoven's 'Hammerklavier' Sonata, partly because it is the one human creation which impresses the 'Black Cloud' of Fred Hoyle's story. Just looking at the score gave me no real idea of what the piece sounded like, and after a long time at the piano, I was very little better off. When eventually I heard Geza Anda's magnificent recording of the work, I realised that here indeed is a sonata greater than Moonlight, Pathétique and Appassionata all rolled into one. It was there in the score but I couldn't read it.

I am in much the same position when I try to read a paper on algebraic topology, one of the most productive branches of mathematics in recent years. It is not just that I do not have the technical know-how to do even a mechanical reading of the 'notes', the terms and symbols of the language of topology. I am tone-deaf to its themes and harmonies—I have no geometric intuition.

My own branch of mathematics is probability theory. In this subject, I can read some of the notes and I can 'hear from the score' in the easy bits.

Mathematics, like music, is a language of extraordinary sophistication. My task tonight is that of interpreter. In this, I am at a disadvantage compared with a pianist or conductor.

I could write up on the board the score of a Bach fugue. I could point out entries of subjects and countersubjects, strettis, augmentation and all that sort of thing. But a Bach fugue is more than its 'academics'—or it is nothing but a trivial mathematical exercise. What makes a great fugue is precisely what transcends analysis.

Unfortunately, in mathematics, all that one *can* do is to point out the 'academics,' the definitions and theorems.

Whether you hear what transcends them depends upon you.

The analogy with music may help explain what makes mathematics research worthwhile for us ordinary mortals. There must have been a time when, in listening to a piece of music, you have felt with some excitement, "Yes, I know what must happen next." A minor new idea in mathematics arises in the same way, not as the result of some mechanical calculation, but as an insight, an intuition, a sense of rightness. The trouble is that someone else—in your 'musical' case, the composer—probably thought of it ages before.

I talked only about the appearance of *minor* new ideas in mathematics because that is all I know anything about. It is all very well to know what Beethoven will do next at odd moments in the 14th String Quartet. It is a very different thing to write that quartet. There is a lot of mathematics which approaches that level of genius.

Not for one moment would I claim that mathematics has produced greater geniuses than Bach or Beethoven. But since, to this day, 'culture' tends to mean the 'arts,' and often just the visual arts and literature, I feel bound to stress an important sense in which mathematics, like the rest of science, very literally leaves 'arts' standing. At about the time that Beethoven wrote the 'Eroica' Symphony, Gauss produced one of the greatest-ever individual pieces of pure mathematics, his 'Disquisitiones Arithmeticae' of 1801. The Eroica, superb though it may be, stays the same. What Gauss wrote has been improved and extended beyond anything he dreamt of. Mathematics, the whole of it, is one single symphonic work.

The scale of this work is terrifying. The culmination of a very advanced undergraduate course in algebraic number theory will be Dirichlet's Class-Number Formula of 1837, and even then, the student will not know much of that 1801 work of Gauss. Students of probability theory think that they are doing something advanced and modern if they go through the proof of the famous 'normal curve' theorem for coin-tossing—of which more

later. That theorem was discovered in 1732 and Gauss wrote extensively on 'normal curves and least-squares approximation.' The important fact to bear in mind is that mathematics has advanced more in the last fifty years than in all its previous history. (After all, the number of full-time mathematicians alive today is more than the sum total of all mathematicians who lived before 1900.) Work done before 1965 is often referred to as 'classical' in the literature.

That it is impossible to teach students much of what is going on today, that some of my colleagues in the department write papers of which I do not understand even a single sentence: these things are, in a sense, frustrating. Yet I would not wish it otherwise. When, about twelve years ago, I was thinking of giving up mathematics research to do something more 'relevant,' I asked a very great mathematician to state one reason why I should not. In the climate of 1973, it is worth recalling his reply: "You will never find anything that's harder."

II

The dictum "Before we begin discussion of this topic, let us be perfectly clear about our terms" used to be all the rage. Those of you who remember Professor Joad know that he would have begun this lecture with the statement "It all depends what you mean by 'probability'." He would then have endeavoured to give a precise definition of the probability that this real coin will fall heads if I toss it.

The approach by means of definition is direct but dead-end. **PRECISE DEFINITION OF REAL-WORLD PROBABILITY IS IMPOSSIBLE.**

There is a very profound sense in which the attempt to define concepts can be misguided. Take 'time' for example. Consider the following Time Paradox. A man aged 30 goes on a journey through space which takes him

10 years. He therefore returns to Earth aged 40. Yet, if his journey is at suitably high speed, he will find on his return that the Earth has passed through 10,000 years of its history and that his great-great-grandchildren are long since dead. Nearly everyone believes this to be a correct deduction from Einstein's Theory of Relativity and a true statement about the real world. What is beyond doubt is that several related, and equally bewildering, time-paradoxes predicted by Einstein have been confirmed experimentally. A most ingenious demonstration that time can be cheated was shown on a 'Burke Special' earlier this year. Time does march on—only Dr Who, Captain Kirk and their colleagues can go backwards in time—but the march of time is not the simple, uniformly paced affair we once believed.

BY SEEKING TO DEFINE TIME, WE COMPLETELY UNDERRATE THE LOGIC OF TIME.

If we seek to define probability, we find that we underrate its logic too. Probability is not the simple, straightforward affair we might wish it to be.

Problem 1. A point is chosen at random on the surface of the world. What is the probability that the chosen point is in Wales?

Never mind what the problem means! The answer is obvious. The required probability is the proportion of the world's surface which is Wales, that is, the area of Wales divided by the area of the world. That's simple and straightforward enough.

Enter two Polish magicians, Banach and Tarski, bearing between them one hollow spherical globe. They divide the globe into four congruent pieces A, B, C and D. Banach takes pieces A and B and Tarski pieces C and D. Without any sleight of hand, without any stretching or change of shape of the pieces, Banach fits together pieces A and B so as to form a *complete* globe *absolutely identical*—in surface area, in thickness, in every respect—to *the original globe*. Tarski does the same with pieces C and D.

Exit Banach and Tarski bearing *two* hollow spherical globes each absolutely identical to the single globe which they brought on.

I should not need to tell you that this trick can not be done in practice. However, Banach and Tarski showed that in theory it can be done without breaking a single law of mathematics. True, they exit the stage with *twice* the volume with which they entered. Yet, they did *not* contravene the law of conservation of volume. I warned you that the subject's logic is subtle.

What is the 'volume' of piece A? Clearly, it is *quarter* of the volume of the original globe and *half* the volume of the *identical* globe with which Banach walked off. Now, not even in advanced mathematics is it true that $\frac{1}{4} = \frac{1}{2}$. The assumption that piece A has a 'volume' leads to a contradiction. The concept of the 'volume of A' is meaningless: A is a *non-measurable* set. Banach and Tarski did not break the law of conservation of volume. They operated outside its jurisdiction.

Problem 2. A point is chosen at random on the surface of the original Banach-Tarski globe. What is the probability that the chosen point belongs to piece A?

Arguments strictly analogous to those we have just used show that Problem 2 is meaningless. The 'probability' asked for is a meaningless concept.

I included the Banach-Tarski Paradox just for fun and as a *mild* hint that things are not as straightforward as they seem. Let us now return to what is currently our main theme.

We have a basic intuitive idea that the probability that our coin will fall heads has something to do with the proportion of heads which would occur in a long series of tosses. You must accept for now that this excellent intuitive idea can not be made into a precise definition. Any so-called 'definition' based on the idea can be torn to pieces in umpteen ways. I have already said: precise definition of real-world probability is impossible.

How then are we to proceed? If we can not define probability, how can we construct a precise, rigorous probability theory?

Having recognised that a direct 'definition' approach to religious philosophy is futile, the Zen school of Buddhism introduced a remarkable form of transcendental meditation. A novice monk is given a 'koan,' or apparently unsolvable problem to ponder over. The abbot provides occasional encouragement in the form of a crack on the head with a stick. One koan is as follows: "There is a live goose in a bottle. How do you get the goose out without hurting the goose or breaking the bottle?" Fortunately, this is one of the 'worked examples' in my book of koans so I can tell you that the answer is: "There, it's out!" Of course, topologists will solve the koan immediately. They will simply assume that the bottle is a Klein bottle, for, as Professor Foulkes reminded us, the inside of a Klein bottle is the outside.

Our intuitive concept of probability is bottled up by the logical difficulty of providing definitions. It is vital that we free it. The mathematician's solution is the same as the Zen Buddhist's: "Abracadabra, there, the concept's free from definitions—let's study it." And it turns out that the outside of the bottle, which is what mathematicians study, is the inside, which is where our intuitive ideas seem confined.

We start with an intuitive idea which can not be made into a definition. But this intuitive idea suggests, or *motivates*, some mathematics which is free from the constraints of definition. Then this mathematics tells us that our intuitive idea was almost right all along. Did I say 'almost right'? Yes, 'almost' is the key word of probability theory, the 'abracadabra' which frees the subject.

Now let's do it all properly.

III

The basic mathematical theory of probability is one of the supreme achievements of lateral thinking. At each stage, it is essential to keep very clearly in mind whether we are '*inside* the bottle,' with plenty of intuition and motivation but confined and confused by logical difficulties, or '*outside* the bottle,' free to play an abstract mathematical game but wondering if it has any significance. Remember that it is the extreme subtlety of the abstract game—and that alone—which can free probability by causing the constraining logical difficulties to evaporate. Do not dismiss the game because its first stages seem childishly simple.

Motivation. Our intuitive idea is that if our coin were to be tossed infinitely often, then the proportion of heads obtained should converge to—that is, get closer and closer to—some limiting value h . It would be nice if we could use this idea to *define* the 'probability of heads' as that hypothetical limiting value h . Then h would represent the *long-run proportion of heads*. Let us speak of '*proportion probability*.' Clearly, h would be a number lying between 0 and 1: $0 < h < 1$. We would like to define the long-run proportion, t , of tails analogously. Since each toss results in either heads or tails, we should have

$$h + t = 1.$$

For a 'fair' coin, we should have $h = t = \frac{1}{2}$.

Our intuitive idea, which, unfortunately, can not be made into a precise definition, suggests the following schematic picture for the experiment of tossing our coin once:

Outcome :	H	T
Probability P :	h	t
$0 < h < 1,$	$h + t = 1$	

We have used the symbols : 'H' for 'heads' and 'T' for 'tails.'

Abstract Theory. To the mathematician, the schematic picture IS the experiment of tossing the coin once. Nothing outside the picture matters, the picture says it all. In particular, 'abstract probability' is just a rule P which assigns a number to an 'outcome' : we write

$$P(H) = h, \quad P(T) = t.$$

The abstract probability, $P(H)$, of heads is *JUST* a number between 0 and 1. It has no other significance. It does NOT mean long-run proportion of heads or anything like that. How could it when there is nothing about long-run proportion of heads in the picture ? When studying the picture, the mathematician is blind to everything outside it and even to the picture's motivation. His single-mindedness spares him confusion.

However, his picture of tossing the coin *once* does not tell the mathematician how to construct a picture of tossing the coin *twice*. To do that, he has to return to his intuitive idea for motivation.

Motivation. For the experiment of tossing the coin twice, there are four possible outcomes : HH, HT, TH and TT where, for example, 'HT' denotes 'heads on first toss, tails on second.' How should we assign 'proportion' probabilities to these four outcomes ? Let us concentrate on outcome HT. If the experiment of tossing the coin twice is repeated infinitely often, in what proportion of the repetitions should we get the outcome HT ? Well, we should get 'heads on the first toss' in a proportion h of the repetitions. Since the coin's behaviour

on any toss is *independent* of its behaviour on other tosses, we should get 'tails on the second toss' in a proportion t of those repetitions which produce 'heads on the first toss.' Thus HT should occur in a proportion 'h times t' of repetitions :

$$P(HT) = h \times t = ht.$$

Similarly,

$$P(HH) = h \times h = h^2.$$

What we are seeing is a particular case of the *Multiplication Rule* :

INDEPENDENCE IS DESCRIBED BY MULTIPLICATION OF PROBABILITIES.

Our argument leads to the following schematic picture :

Outcome :	HH	HT	TH	TT
Probability P :	h^2	ht	th	t^2
$0 < h < 1,$	$h + t = 1$			

Abstract Theory. For the mathematician, this schematic picture *is* the experiment of tossing the coin twice. As far as the mathematician is concerned, h is still *just* a number between 0 and 1 with no significance in terms of long-run proportion of heads.

As a matter of fact, I have omitted one important statement from our pictures. I now incorporate this as the basic *Addition Rule* :

P is additive

What does this mean ? It means that if, for example, I wish to calculate the abstract probability of 'heads on the first toss' from the two-toss picture then I do so as

follows : I *add* together the abstract probabilities of all those outcomes which imply 'heads on the first toss.' There are two such outcomes : HH and HT. Hence the Addition Rule gives for the probability of 'heads on the first toss' the value

$$P(HH) + P(HT) = h^2 + ht =$$

$$h(h + t) = h \times 1 = h,$$

just as it should be. In short, the two-toss picture *projects* correctly to give two one-toss pictures, one for the first toss and one for the second.

It is clear how to go on. Here is the complete picture for the three-toss experiment :

Outcome :	HHH	HHT	HTH	HTT	THH	THT	TTH	TTT
Probability P :	h^3	h^2t	h^2t	ht^2	h^2t	ht^2	ht^2	t^3
	$0 < h < 1,$		$h + t = 1$					
	P is additive							

Precisely three outcomes, HHT, HTH and THH, correspond to the event 'two heads in all.' Each of these outcomes has probability h^2t . Hence the abstract probability of getting exactly 2 heads in 3 tosses is $3h^2t$ —by the Addition Rule.

The way to construct a picture of the n -toss experiment is clear. Our pictures are consistent or PROJECTIVE in that each level projects correctly onto more elementary levels.

Our motivation has led to an abstract theory which is one small part of the subject of *combinatorics*, the "Mathematics of Patterns and Arrangements" of the title of Professor Foulkes' inaugural lecture. I would remind you that the fact that certain problems reduce to mere counting *in principle* does not prevent their defeating all the skills of the mathematician. There are plenty of

important finitistic problems about coin-tossing which no-one can touch at the present time.

The central result of the early combinatorial theory of coin-tossing is the famous 'normal curve' theorem proved by de Moivre in 1732 by using a remarkable formula obtained by Stirling two years earlier.

I am not going to bother about the details of the 'normal curve' theorem now. One of the most useful consequences of the theorem is that *for a FIXED very large number n of tosses, there is just over 99% abstract probability that the proportion of heads obtained will lie between the values a_n and b_n , where*

$$a_n = h - 2.6\sqrt{(ht/n)},$$

$$b_n = h + 2.6\sqrt{(ht/n)}.$$

Numerical examples for the case of a 'fair' coin with $h = t = \frac{1}{2}$:

n	10,000	1,000,000	100,000,000	. . .
a_n	0.487	0.4987	0.49987	. . .
b_n	0.513	0.5013	0.50013	. . .

Thus, for example, if a fair coin is tossed 10,000 times, then we may be '99% certain' that the proportion of heads obtained will lie between 0.487 and 0.513, that is, that the *number* of heads obtained will lie between 4,870 and 5,130.

The numerical examples make it plausible that the 'long-run proportion of heads' idea is somehow implied by the Addition and Multiplication Rules of the abstract theory. However, we have to jump 177 years of mathematics and into a different world to see precisely how.

The normal curve theorem is a good example of mathematics as a powerful means of calculation. Now,

and with a vengeance, mathematics takes on its other rôle as a language of great subtlety.

If we are to investigate the 'long-run proportion of heads' idea in the mathematical theory, then we have no option : *we must construct a model for the experiment of tossing our coin INFINITELY often.* Gone now are simple schematic pictures. The picture :

Outcome : HHHH... . . . TTTT...

Probability : O . . . O

is correct but worthless. The only intuition which can help us now is the rare intuition of the French mathematicians Borel and Lebesgue. In his inaugural lecture entitled "Concepts of Infinity", Professor Weston described how, by 1906, Borel and Lebesgue had completed their profound analysis of the concept of length and had provided us with the basic grammar of MEASURE THEORY. Length, area, volume, probability : they are all measures.

What we have is a sequence of schematic pictures : 1-toss, 2-toss, 3-toss and so on. This system of pictures is PROJECTIVE in the sense I explained : each level projects correctly onto more elementary levels. Such a projective system of pictures seems to demand the existence of a PROJECTIVE LIMIT, that is, a picture of an infinite experiment which projects correctly onto every finite level. It is amazing that there do exist projective systems *without* projective limits. However, the fundamental 1932 Daniell-Kolmogorov theorem guarantees the existence of a projective limit under conditions of extreme generality.

In particular, there exists an essentially unique model for the experiment of tossing our coin infinitely often, this model being formulated in the language of measure theory. The model is quite sophisticated. The Addition Rule needs to be put into a more subtle and sharper form—as Professor Weston explained. Those non-measurable

sets (remember Banach-Tarski !) appear in the model. But it can all be done and Borel did it for coin-tossing in 1909.

And now we can slot in the keystone of the whole structure. Borel considered the set of those outcomes of the infinite experiment for which the proportion of heads does converge to h . He showed that this set is measurable and that its abstract probability is exactly one. If a statement is true with abstract probability one, we say that it is 'almost certain'—but we read 'almost certain' with the stress on the 'certain' : 'almost *certain*'. If we are almost certain that a statement is true, then we are not just 99% certain, we are 100% certain, but we are *not* CERTAIN. Verbal discussion is becoming rather silly but remember that the mathematics is precise.

Here then is the statement of Borel's 1909 Strong Law of Large Numbers : IN ANY INFINITE SEQUENCE OF TOSSES OF OUR MATHEMATICAL COIN, IT IS ALMOST CERTAIN THAT THE LONG-RUN PROPORTION OF HEADS WILL EXIST AND WILL EQUAL h .

At no time in the mathematical theory did we define h as the long-run proportion of heads. Remember how insistent I was that h was *just* a number between 0 and 1 and that we should never look outside our pictures. We were single-minded but we were not as blind as we appeared. *We have constructed a consistent and powerful formal calculus in which our intuitive idea of probability as long-run proportion is allowed the maximal range of validity permitted by logic.* Our intuitive idea has been freed and it is the 'almost' which freed it. Delete the word 'almost' from my carefully chosen statement of the Strong Law and you have a self-contradictory statement. To this fact we can trace the reason why definition in Joad's sense must underrate the subject's logic. The logic of 'almost certainty' is far more subtle than that of certainty.

We have constructed a formal calculus. We have *not* solved the problem of defining real-world probability—that problem is unsolvable. However, the prodigious

success of applied probability and statistics is evidence enough that the calculus *is* relevant to the real world. I am *not* going to spell out tonight what I consider to be the basic phrase-book for interpreting the probability calculus, though I do have a very definite phrase-book in mind. I emphasise 'basic phrase-book' because a complete abstract-to-real dictionary is both unattainable and unnecessary. We probabilists have not concerned ourselves too deeply with the controversies over interpretation which so perplex philosophers and statisticians. To salve our consciences, we watch quantum theorists make, and experimenters confirm, the most wondrous predictions, in spite of—and IMMUNE FROM—the furious controversy over things like the Uncertainty Principle. The shrewdest strategy is : interpret as little as you can get away with. We have seen the benefits of single-mindedness already tonight. It is the apparent narrowness of view of mathematics, the refusal to make any compromise on questions of logic, which unearthed the *richness* of structure of time and probability and quantum mechanics. What makes a great fugue great is, in part, the harsh discipline of the fugal form.

IV

So, by 1909, the theory of coin-tossing had begun—for, make no mistake, the Strong Law was the beginning, not the end, of the mathematical theory. Borel and Lebesgue had provided some of the basic rules of counterpoint. What was needed was a theme of extraordinary harmonic richness.

In 1828, the botanist, Robert Brown, had observed that particles on the surface of a liquid perform a continual swarming motion. *Brownian motion* is the concept which was to raise probability theory to a different order of things from the Strong Law.

In 1905, Einstein had used an intuitive theory of

Brownian motion to calculate the important Avogadro's Constant of physics. In 1923, Norbert Wiener presented the first rigorous theory of Brownian motion in a paper of fundamental importance both to probability theory and to quantum mechanics. Brownian motion was already beginning to look impressive.

It was always intuitively clear that Brownian motion and an infinite coin-tossing or die-throwing game are effectively the same concept. However, it was not until 1951 that this intuitive idea was properly formulated as what we now call the Invariance Principle, the ultimate form of the 'normal curve' theorem.

The Invariance Principle made it possible to read off theorems about coin-tossing directly from corresponding results about Brownian motion. By 1963, Strassen had used this technique to specify how an abstract coin must almost certainly behave with a preposterous precision amounting almost to a contradiction of our intuitive ideas of randomness. The Strong Law became relegated to the nursery.

But WHY probe ever more deeply into the probability calculus ?

We have seen mathematics as a powerful means of calculation. We have seen that as a language for discussing logical subtleties in the philosophy of probability, mathematics is incomparably superior to English. Now we shall see a yet more brilliant facility of the mathematical language : its absolute mastery of analogy, its ability to turn analogy into identity.

If Joad *had* succeeded in producing a philosophy of coin-tossing, then all he would have produced would be a philosophy of coin-tossing. The mathematical theory of coin-tossing is not just about tossing coins. Nor is the probability calculus restricted to solving problems in probability and statistics. *As the calculus grows in depth, so it gains in versatility.*

I would be perfectly correct if I told you that what we had been studying in our pictures of coin-tossing was *heat flow*. Last year, the Invariance Principle and the immensely powerful Itô calculus of Brownian motion

solved the basic problem in the differential equation theory of heat flow, something which the gigantic theory of differential equations had completely failed to do.

But I could equally well say that we had been studying number theory. This year, that preposterously precise Strassen theorem on coin-tossing was used to demolish the famous number-theoretic problem of metric approximation by continued fractions.

And so on, and so on. But, as those of you who know "Don Giovanni" will appreciate, a "Catalogue Aria" of triumphs is of less benefit to the listener than an indication of the techniques by which such conquests are achieved in individual cases. Let me therefore explain in more detail a specific result from one of the most spectacular applications of Brownian motion theory.

But what is Brownian motion? How may we picture three-dimensional Brownian motion of a particle? For the moment, the particle is considered to be infinitely small, so that at a given time, it occupies only one point of space.

Die simulation of Brownian motion. Pick a very large number N and throw a fair die $6N$ times every second. After each throw, move the particle a distance $1/\sqrt{N}$ up, down, north, south, east or west according as the die lands 1, 2, 3, 4, 5 or 6. Our particle therefore makes very small and extremely rapid jumps in space.

Brownian motion is the limiting case when N tends to infinity. The Brownian particle moves continuously, but with infinite speed, completely changing its direction of motion at every instant. Its path crosses itself infinitely, indeed *uncountably*, often during every time interval. While the track in space of any sane particle is clearly of dimension *one*, the path of the Brownian particle is of dimension *two*—well, actually, in a certain precise sense, the dimension of the path exceeds two by an infinitesimally small amount. Many of these things are easily understood in terms of half-dimensional time—but I won't go into that now.

Let us make some concession to sanity by allowing our

'particle' to be of definite size, but let the particle perform the same bizarre Brownian motion, the orientation of the particle remaining fixed. In time t , the particle will sweep out a volume $V(t)$. It is a theorem that, as t tends to infinity, the *average rate of sweeping out volume*, $V(t)/t$, will almost certainly converge to a certain constant C . And what is C ? It is precisely the *electrostatic capacity* of the particle. This neat formula for capacity is no coincidence. The classical Dirichlet and Neumann problems of potential theory are very easily solved by the theory of Brownian motion. *Every concept in potential theory of whatever depth—regular point for Dirichlet problems, thin set, polar set, fine topology, Martin topology, balayage, you name it!—every such concept has a simple and illuminating PROBABILISTIC interpretation.*

I wish that I had time to tell you about new applications of the probability calculus. I would like to have been able to communicate to you the way in which new ideas infuse new life and excitement into mathematics and to convey to you something of the immense vitality of this oldest, but youngest, of subjects.

I hope that I have at least said enough for you to appreciate a general pattern. The abstract language of length developed by Borel and Lebesgue proves to be the ideal language for probability theory and, let's not forget, for practical statistics too, unless you want a statistical theory riddled with contradictions. The apparently-absurdly-abstract modern probability theory provides a new and better language for potential theory and for heat flow.

These lucky pure mathematicians, who do mathematics just for fun, have the gift of prophecy. The universe is sufficiently rich in structure to accommodate their every silly abstract game, their every wild imagining. If you don't believe me, ask the quantum theorists. Or listen to what Einstein wrote when he produced that revolutionary new postulate of general relativity which shattered old ideas about space, time and gravitation. He wrote, "Thus it is that, LONG AGO, mathematicians had solved

the formal problems to which we are led by the general postulate of relativity." Gauss and Riemann had already developed the exact strange geometry which Einstein needed. Nearly every page of the history of science tells the same story : *that instinct, that intuition, that ability to 'hear from the score' of pure mathematicians has always proved entirely trustworthy.*



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