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Editorial

Hello and welcome to Eureka 47 . I hope you will find something here to interest you. Dr Mathias has kindly allowed us to print the text of his talk to the Quintics last term on the history of the foundations of mathematics, entitled "The Ignorance of Bourbaki ". Dr Butterfield , of the Philosophy faculty, has written an article related to his course on "Philosophical aspects of Quantum Theory". At a more recreational level, we have one discussion of "resistance rectangles" and one on tessalations with truncated octahedra. There is also a description of the ancient oriental board game, go, a catastrophe theoretic analysis of the history of catastrophe theory, and a collection of mutterings of a vaguely humorous nature. Tom Wilde has contributed a rather more technical article on the structure of automorphism groups. We are still, I fear, rather short of articles which are not of an essentially mathematical interest. This is partly because various promised papers failed to materialise, partly because no one volunteered to write anything non-mathematical. It is not at all clear to me to what extent this is a bad idea or otherwise. Any feedback, please ? Hello...? Is there anybody out there ..? Better still, if you don't like the contents, don't sit there and whinge, write something for the next issue. I would like to thank various people for their part in producing this issue - first and foremost, the contributors, but also the business managers (Nigel Barnes and Paul Glover) and everyone who helped with the proof reading. Finally, I shall advertise for a successor. Should anyone wish to improve on this humble offering by editing Eureka 48, please contact me as soon as possible. A current first year would probably be best, but anyone is welcome to apply.

GO

Andrew Jones

This game is the national game of Japan, was invented more than 4000 years ago in China, and is very difficult to explain on paper; here goes !

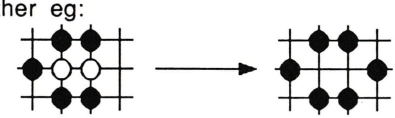
The game is played with black and white pieces or "stones" on the intersections of a 19x19 square grid. Black and white play alternately by placing single stones in an empty space on the board. Stones cannot be moved though some moves can cause your opponent's stones to be removed.

A stone played on a position away from an opponent's stones has four lines radiating from it:

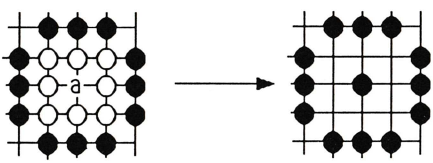
These are known as liberties. If a stone has all its liberties filled in by the opponent's stones, it is removed:

Conway's game "Life" seems to owe something to this.

If stones are connected to other stones of the same colour, they are considered together eg:

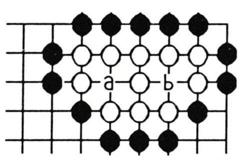


Or:



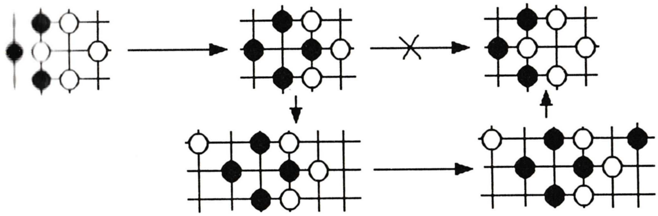
(black plays a)

However, it is possible to make a group of stones "live":



Black can play neither a nor b, thus white "lives". White is said to have two "eyes", at a and b.

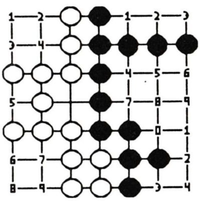
There are only two restrictions to play: The rule of "ko" (Japanese for "eternity" or something like that); you cannot play to restore the game to the position at the last move, another move must be made to change the position; eg:



This prevents the game from locking up if neither player is willing to give up the ko .

The other restriction is that of no suicide (blood warps the board and makes the stones sticky). A player is not allowed to kill his own stones.

The game is scored by adding up the intersections surrounded and the stones captured by each player. For example, suppose the game ended as shown, with white having captured four stones in the course of the game, and black three stones.



White ● ● ● ● (captured) + 9 = 13

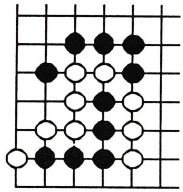
Black ○ ○ ○ (captured) + 14 = 17

Black wins by four points.

The game ends when neither player can think of anything else worth playing.

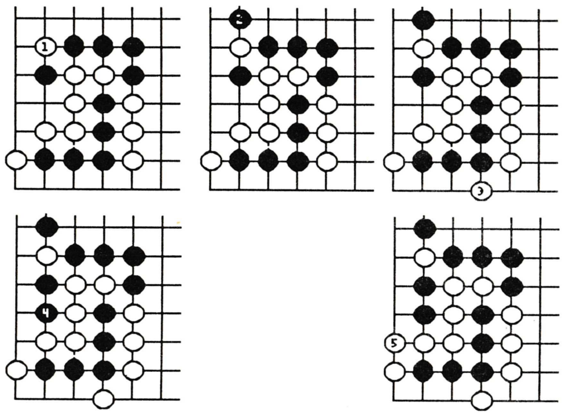
Though the rules are simple (my explanation may not be, it is much easier to demonstrate with a board) the game is far from trivial! Making a live group is not so easy when your opponent tries to stop you .

The game has a handicap system which allows players of widely varying strengths to play a sensible game . If you think you understand the game then, try this problem:



White to play. Black has three liberties, white has two, but white can kill black; how ?

This sequence will do the trick, though there are minor variants which are equally good:



If you would like some further explanation, or would be interested in learning more about the game generally, contact the university go society. The president is Andrew Jones (Peterhouse) and the meetings are usually on monday evenings at eight in the Maitland room, Downing College.

The Ignorance of Bourbaki

A.R.D.Mathias

If one looks at the history of mathematics, one sees periods of bursting creativity, when new ideas are being developed in a competitive and therefore very hasty spirit; and periods when people find that the ideas so recently in vogue are inexact, incoherent, possibly inconsistent; in such periods there is an urge to consolidate past achievements.

I said "the history of mathematics": but mathematics is a complex sociological organism, and its growth takes place in different branches and in different countries, even different universities, in different ways and at different speeds. Sometimes national groups feel that mathematics in their country is in a bad way: you find an expression of that in the *Introduction* to later editions of Hardy's *Pure Mathematics*, where he remarks that it was written with an enthusiasm intended to combat the insularity of British mathematics of the turn of the century, which had taken no account of the development of mathematics in France in the nineteenth century: Legendre, Laplace, Lagrange, Fourier, Cauchy, Dirichlet - who married Mendelssohn's sister - Hadamard, Poincaré - a most impressive list of scholars of the highest distinction.

After the first World War, the feeling in France was very different, and the young French mathematicians of the day began to consider that the torch of mathematical research had passed to Germany - where indeed there were many great mathematicians building on the past work of Riemann, Frobenius, Dedekind, Kummer, Kronecker, Minkowski and Cantor, such as Klein, Hilbert, Weyl, Artin, Noether, Landau, Hausdorff, - and that French mathematics had gone into a decline.

So in 1935, a group of young French mathematicians (listed by Chevalley in an interview [*Mathematical Intelligencer* 7 (2) p 18] as H. Cartan, C. Chevalley, J. Delsarte, J. Dieudonné, Sz. Mandelbrojt, R. de Possel, and André Weil) resolved to restore discipline to their subject by writing a series of textbooks, under the joint pseudonym of Nicolas Bourbaki, that aimed to give definitive expositions with full French rigour to what they deemed to be the most important areas of pure mathematics. Now the question of mathematical rigour was very topical, a greater disaster than usual having occurred at the beginning of the twentieth century with the discovery by Russell of a major flaw in Frege's proposed theory of classes.

Frege wanted to form for any property $\Phi(y)$ the class $\{y|\Phi(y)\}$ of all objects y with the property Φ, and at the same time to count all such classes as objects to which such membership tests might be applied. If we write "$a \in b$" for "a is a member of b" and "$a \notin b$" for "a is not a member of b", we may express Frege's broad principle as follows: if we denote $\{y|\Phi(y)\}$ by C, then for any object a, $a \in C$ if and only if $\Phi(a)$. Russell, developing an idea of Cantor, noticed that if $\Phi(y)$ is taken to be the property $y \notin y$, of not being a member of oneself, then a contradiction results. For let B be the class of those objects that are not members of themselves; in symbols, $B = \{y|y \notin y\}$: then for any y, $y \in B$ iff $y \notin y$; and so for the particular case when $y = B$, $B \in B$ iff $B \notin B$.

In response to this, there were some who wished to ditch all the more speculative areas of mathematics, which made use of the infinite particularly of Cantor's theories of cardinals and ordinals. Kronecker, Poincaré, Brouwer, Hermann Weyl should be mentioned here.

But there were others - notably Hilbert - who wished to resist this wholesale amputation, and a program was proposed aimed at formalising mathematics - the language, the axioms, the modes of reasoning etc - and at proving, by means the soundness of which could not possibly be doubted, that the resulting system was free of contradiction, that is, was *consistent* .

I said "formalise mathematics" but that is vague: how much mathematics can we or should we include ? Hilbert certainly would wish to keep Cantor's work on ordinals in his formalisation of mathematics, as it was Cantor who made Hilbert possible: Hilbert leapt to fame with his *Basis Theorem* that in modern terminology asserts that if every ideal in the commutative ring R is finitely generated, the same is true of the ring $R[X_1,...,X_n]$ of polynomials in the indeterminates $X_1,...,X_n$ with coefficients in R; and recent studies have shown that the proof of this theorem not only relies on but is in an exact sense equivalent to the wellfoundedness of ω^{ω}. Thus when Hilbert spoke of Cantor's paradise, it was no idle tribute: he acknowledged the creation of a conceptual framework of transfinite induction within which algebraic geometry could advance.

Russell's own ideas on avoiding the paradoxes led to his *ramified theory of types*; this was cumbersome, and a simpler system was proposed by Zermelo in the first decade of the century. Fraenkel, and Skolem, in the third decade proposed the *axiom of replacement* as a strengthening of Zermelo's system; the resulting system is known as Zermelo-Fraenkel. With the addition of the *Axiom of Choice*, first articulated by Zermelo and of great importance in functional analysis and higher algebra, and the

Axiom of Foundation, proposed by von Neumann, ZFC has proved a very serviceable system.

There are two elements to Hilbert's programme which I wish to stress to you: the creative side, proposing a system within which to work; the critical side, testing the adequacy and consistency of the system proposed. Naturally the Bourbaki group, or *Bourbachistes*, mindful of the possibility of contradiction in mathematics, were determined that their textbooks would be free of such problems, and indeed an early volume in their series, *La Théorie des Ensembles*, was devoted to establishing the foundations necessary for their later volumes.

The other day, I thought I would read it.

I was shocked to the core: it appeared to be the work of someone who had read *Gründzuge der Mathematik* by Hilbert and Ackermann, and *Leçons sur les nombres transfinis* by Sierpinski, both published in 1928, and nothing since.

Those of you who know of the publication of Gödel's incompleteness theorem in 1931 will see the significance of these dates.

Puzzled both by Bourbaki's attitude to foundations and by his attitude to set theory, I started to probe the background and found that the Bourbachistes had published several articles in the thirties and forties expounding the group's position on foundational issues.

Henri Cartan and Jean Dieudonné, wrote essays under their own names on the foundations of mathematics. After the second World War, Nicolas Bourbaki himself addressed the Association for Symbolic Logic in America, and his talk was printed in the *Journal of Symbolic Logic.* Further, he wrote an essay on *L'Architecture des Mathématiques*, which was translated into English and appeared in the *American Mathematical Monthly*.

There is a uniformity to these essays: on the creative side, the set theory they propose is that of Zermelo - not, let me emphasize, Zermelo-Fraenkel - and declare it to be adequate for all of mathematics; and on the critical side, they all show the influence of Hilbert's formalist program. Whom they do not mention is Gödel.

Now this is very remarkable. Let me comment on Gödel's Incompleteness Theorem.

There was a meeting at Königsberg in September 1930, during which honorary citizenship was conferred on Hilbert, who had retired from his Chair at Göttingen on January 23rd of that year. The famous and powerful address, *Naturerkennen und Logik*, that he gave on this happy occasion is informed by his *credo* that there are no insoluble problems and ends with his resolute battlecry

"*Wir müssen wissen; wir werden wissen.*"

- we must know, we shall know. With the delicate irony of history, Gödel had the very day before, with von Neumann but not Hilbert in the audience, announced his incompleteness proof, with its applications to any system such as Peano arithmetic or Zermelo Fraenkel. Now this was alarming: Hilbert had presented a very positive response to the paradoxes, and disciples such as Herbrand had in the Hilbertian spirit established cases of the decision problem. Gödel showed that there were serious limitations to Hilbert's proposal. He showed that no system satisfying certain minimal conditions, such as that there should be an algorithm telling you of any sentence whether it is one of the axioms or not - clearly a desirable requirement - no system of this kind captures all of mathematics, and that consistency proofs can only be given in systems more likely to be inconsistent than the one under discussion.

What effect did Gödel have on the mathematical community at large ?

The strange thing is that he was largely ignored.

We see this most emphatically in the writings of the Bourbachistes.

Thus Henri Cartan, in a piece published in 1943, *Sur Le Fondement Logique des Mathématiques* presents the system of Zermelo, including the Axiom of Choice; though he says he takes some account of the modifications introduced by Fraenkel, he does not include the main one, the axiom of replacement; he commments that Zermelo's system is inconvenient, lacking as it does suitable definitions of ordered pair, etc., and he reveals ignorance of the distinctions that Gödel stressed by saying "true" where he means "provable", "false" where he means "refutable" and "doubtful" (douteuse) where he means "undecided".

He talks of contradictory theories, and says the problem of deciding whether a given theory is contradictory leads to the Entscheidungsproblem, which consists of finding a general method for deciding whether a given relation (=formula) is a logical identity (i.e. theorem). This problem, he says, has only been resolved in particular cases. In general one does not know how to do it. He then says, "But these problems, important though they be, are outside our subject."

He mentions Herband's thesis, Sierpinski's *Leçons sur les nombres transfinis*; adopts a view he credits to Dieudonné, mentioning that these ideas, though published in 1939, "remontent à 1938" and makes this statement:

> *une théorie mathématique n'est pas autre chose qu'une théorie logique, déterminée par une système d'axiomes ... les êtres de la théorie sont définis ipso facto par le système d'axiomes, qui engendre en quelque sorte le matériel auquel vont pouvoir s'appliquer les propositions vraies; définir ces êtres, les nommer, leur appliquer les propositions et relations, c'est en cela que consiste la partie proprement mathématique de la théorie logique.*

He mentions Cantor, Kronecker, Zermelo, Brouwer, Skolem's paradox, Poincaré and Lebesgue, **but not Godel !**

Similar attitudes are to be found in the 1939 piece, cited by Cartan, by Jean Dieudonné: *Les Méthodes Axiomatiques Modernes et les Fondements des Mathématiques* . He describes the achievements of Cantor, which Hilbert had found so useful, as "*resultats si choquants pour le bon sens* "! He regards the foundational crisis of the beginning of the century as having been resolved by Hilbert's formalist doctrine that the correctness of a piece of mathematics is a question of its following certain rules, and not a question of its interpretation; comments that "*le principal mérite de la méthode formaliste sera d'avoir dissipé définitivement les obscurités qui pesaient encore sur la pensée mathématique*"; and says that "*Il reste naturellement à montrer que la conception de Hilbert est réalisable*". He makes no mention of Gödel.

Nicolas Bourbaki, in The Foundations of Mathematics for the Working Mathematician, *Journal of Symbolic Logic, Volume XIV, 1948 pp 1-14*, again presents Zermelo set theory plus the Axiom of Choice, and concludes

> *"On these foundations, I state that I can build up the whole of the mathematics of the present day; and if there is anything original in my procedure, it lies solely in the fact that, instead of being content with such a statement, I proceed to prove it in the same way as Diogenes proved the existence of motion; and my proof will become more and more complete as my treatise grows."*

As you might expect from that statement, there is no mention, or even hint of the existence of Gödel's work in that paper.

In Bourbaki's other essay, *L'architecture des mathématiques,* there is again no mention of Gödel but on this occasion there is a hint of "difficulties".

The questions I want now to address are: *why did Bourbaki make no mention of Gödel ?* and *why did Bourbaki not notice that his system of Zermelo set theory with AC was inadequate for existing mathematics ?*

I think these questions important because the Bourbaki group have had great influence; I do not dispute the positive worth of their books nor the magnitude of their achievement; but I suggest that their attitude to logic and to set theory, which has been passed on to younger generations of mathematicians, is harmful because it excludes awareness of perceptions of the nature of mathematics that are invigorating; and I almost venture to suggest that if, as some say, Bourbaki is now dead, he was killed by the sterility of his own attitudes.

Let me now suggest highly speculative answers to my questions.

<u>First, why did the Bourbachistes not adapt their attitudes to take account of the supremely important contribution of Gödel to foundational issues ?</u>

Let us look at some of the comments of the Bourbachistes on these matters.

Bourbaki distinguishes carefully between logical formalism, which he is against, and the axiomatic method, of which he approves:

> *What the axiomatic method sets as its essential aim, is exactly that which logical formalism by itself cannot supply, namely the profound intelligibility of mathematics. - L'architecture des mathématiques*

So by the axiomatic method, he means not a grand deductive scheme for all of mathematics, but simply the mental discipline of pruning areas to their skeletons, to make similarities clear and theory portable.

The unity which [the axiomatic method] gives to mathematics is not the armor of formal logic, the unity of a lifeless skeleton (L'architecture)

Many mathematicians have been unwilling to see in axiomatics anything else than futile logical hairsplitting not capable of fructifying any theory whatsoever.

Nothing is farther from the axiomatic method than a static conception of the science. We do not want to lead the reader to think that we claim to have traced out a definitive state of the science.

It is quite possible that the future development of mathematics may increase the number of fundamental structures, revealing the fruitfulness of new axioms or of new combinations of axioms.

André Weil puts the Bourbachist view of logic as the grammar of mathematics more diplomatically [collected works, 1947a address on *L'Avenir des mathématiques* to a congress on *Les Grands Courants de la pensée mathématique* .]:

Mais, si la logique est l'hygiène du mathématicien, ce n'est pas elle qui lui fournit sa nourriture; le pain quotidien dont il vit, ce sont les grands problèmes.

thus of course revealing a belief that there are no great problems in logic. He does, though without mentioning Gödel, go on to suggest an awareness that the last word on logic might not have been said:

Il se peut sans doute qu'un jour nos successeurs désirent introduire en théorie des ensembles des modes de raisonnement que nous ne nous permettons pas.

This vital view, which is reminiscent of the last paragraph quoted from Bourbaki above, is to be contrasted with the later ossification expressed by Dieudonné in his *Panorama of Mathematics* that "Set theory is well worked out."

Some further extracts from Bourbaki's manifesto:

The organizing principle will be the concept of a hierarchy of structures, going from the simple to the complex, from the general to the particular.

the theory of groups, ... the theory of ordered sets, (including wellorderings), ... the theory of topological structures ...

All that is fairly straightforward. But now comes a real howler:

The first axiomatic treatments (Dedekind-Peano arithmetic, Hilbert-Euclid geometry) dealt with univalent theories , i.e. theories which are entirely determined by their complete system of axioms, unlike the theory of groups.

The position is that geometry as axiomatised by Hilbert is completely determined, so that a statement of plane geometry provable by use of solid geometry will have a proof in plane geometry; but Gödel tells us that arithmetic, as axiomatised by Peano or anyone else, is not.

My reading of all these extracts is that Bourbaki had grasped the positive worth of the work of Hilbert and his school, and welcomed the idea of the reduction of the question of correctness of mathematics to a set of rules, but nevertheless persisted in thinking of logic and set theory as stuff one settled in volume one and then forgot about, even after Gödel's work showed that Hilbert's program could never be completed. For this reason, Gödel's incompleteness theorems were irrelevant to their view of mathematics: like many another scientist, they were prevented by their preconceptions from seeing the significance of facts that were known to them.

The later books of Bourbaki shift ground: but still see set theory as something to be done in volume one and then forgotten. Thus it appears that this major exposition of mathematics is written by people whose understanding of foundational work is that of 1929.

Mathematicians are, even today, very uneasy in their attitude to logic. This may have something to do with the second world war: Hitler smashed Poland, which between the wars was the centre of research in logic; those that escaped started schools of logic in the US and in Israel which have flourished, leaving Europe behind. Thus whereas someone in four years in Cambridge might hear fifty lectures on logical topics, at Harvard or Princeton they may hear around two hundred and fifty, and at Berkeley, where logic is taken seriously, they may hear about four hundred.

I wish to suggest to you that there are no foundations of mathematics in the sense believed by Bourbaki; that there are no ways of grounding mathematics in logic or classes or whatever so that once a basis has thus been given in some primitve ideas, no further thought need given to them. I wish instead to suggest that though there are indeed foundational issues, they cannot be confined to chapter one of the Great Book on the contrary, they permeate mathematics.

The second question I put above was "why did the Bourbaki group not notice the inadequacy of Zermelo set theory as a foundation for mathematics ?"

I suggest as an answer, that they were solely interested in areas of mathematics for which Zermelo is adequate, and that this area may broadly be described as geometry as opposed to arithmetic.

Leibniz wrote that there are two famous labyrinths in which our reason is often lost. One is the problem of freedom and necessity, and the other is concerned with continuity and infinity. Heedless of this second danger, I wish now to explore what I believe to be the underlying dualism of mathematics, namely the tension between the two primitive intuitions, the arithmetical and the geometrical. Let me start with a conundrum:

Can you describe a spiral staircase without moving your hands ?

That question is difficult, perhaps, because words are temporal, hence arithmetical; spirals are spatial.

The question of the relationship of geometry to arithmetic is very ancient, and was discussed by the Eleatics.

Bourbaki is aware of this problem, and in *The Architecture of Mathematics* , writes:

> *Indeed, quite apart from applied mathematics, there has always existed a dualism between the origins of geometry and of arithmetic (certainly in their elementary aspects), since the latter was at the start a science of discrete magnitude, while the former has always been a science of continuous extent; these two aspects have brought about two points of view which have been in opposition to each other since the discovery of irrationals. Indeed it is exactly this discovery which defeated the first attempt to unify the science, viz. , the arithmetization of the Pythagoreans ("everything is number").*

If we go back a century, we find Augustus de Morgan writing:

> *Geometrical reasoning and arithmetical process have each its*

own office; to mix the two in elementary instruction, is injurious to the proper acquisition of both.

J.J.Sylvester, in *A probationary Lecture on Geometry* delivered on 4 December, 1854, (Collected Works, Volume II page 5. Cambridge 1908, republished Chelsea New York 1973) said:

> *There are three ruling ideas, three so to say, spheres of thought, which pervade the whole body of mathematical science, to some one or other of which, or to two or all of them combined, every mathematical truth admits of being referred; these are the three cardinal notions, of Number, Space and Order.*
>
> *Arithmetic has for its object the properties of number in the abstract. In algebra, viewed as a science of operations, order is the predominating idea. The business of geometry is with the evolution of the properties and relations of Space, or of bodies viewed as existing in space....*
>
> *It is the province of the metaphysician to inquire into the nature of space as it exists in itself, or with relation to the human mind. The less aspiring but more satisfactory business of the geometer is to deal with space as an objective reality. ...*
>
> *But for the discovery of the conic sections, attributed to Plato, the law of universal gravitation might never to this hour have been elicited.*
>
> *Little could Plato himself have imagined that he was writing the grammar of the language in which it would be demonstrated in after ages that the pages of the universe are written.*
>
> *He who would know what geometry is, must venture boldly into its depths and learn to think and feel as a geometer.*

Plato and his school thought there were five branches of mathematics; Boethius thought there were four; Sylvester cuts down to three: I reduce yet further by thinking of Order as superstructure of the other two.

Let us consider the thesis that there are two intuitions, arithmetic and geometry.

The two intuitions are not disjoint: the language of each is sufficiently rich to allow translations of the other: within set theory one can do a mock-up of the real line by building first the rationals and then (say) Dedekind cuts; and one can mark out equally spaced points as integral points along a line; but when such translations are made, paradoxes are prone to result, the translations being of formal properties, not of underlying intuitions.

Thus the Pythagoreans wished to believe that all is number, but were dismayed by the demonstration that the diagonal of a square is incommensurable with its side. Here importing a simple geometric construction generated as arithmetical paradox.

Stifel (1487 - 1567) asked what irrationals are: geometry suggests they are acceptable, but as lengths, not numbers. He wrote, "an irrational is not a real number because it lies under some cloud of infinity." He did not believe in $2\pi r$.

In the other direction there is the Banach-Tarski paradox which shows, using the axiom of choice, that a sphere can be decomposed into finitely many parts which can be rearranged by spatial translations and rotations to form two spheres of the same size as the original one.

The proof of this is derived from the Schroeder-Bernstein argument, coupled with the axiom of choice. It should be mentioned that in the absence of AC the Banach-Tarski theorem might fail.

Here arguments that are natural in a set theoretic context lead to conclusions that are paradoxical geometrically.

This is similar in spirit to the result of Fibonacci in the thirteenth century that the solution of a certain cubic is not one of Euclid's irrationals.

Recently the distinguished American mathematician Saunders MacLane has called for a revival of discussion of the philosophy of mathematics and has criticised what he calls the Grand Set Theoretic Foundation of Mathematics in phrases such as

the Grand Set Theoretic Foundation is a mistakenly one-sided view of mathematics;

set theory is largely irrelevant to the practice of most mathematics;

logicism, formalism and Platonism have been too much dominated by the notions of set theory and deductive rigour

There have also been criticisms such as that of Thom:

set theory seems to suppress geometry ;

I suggest that it is because Bourbaki fossilised mathematicians' knowledge of logic at its 1929 level that this attack is now taking shape. MacLane in other words is attacking a position from which logicians have been moving for the past sixty years but which mathematicians are still at.

I would agree with MacLane's first comment and with Thom's, and relate them to my idea that set theory is on the arithmetical side rather than geometrical side of mathematics. I would qualify MacLane's second comment, by saying set theory is not particularly relevant to the practice of geometry, but is very much relevant to arithmetic in its broadest sense. Though I agree with much of MacLane's third criticism, I question his use of the phrase *set theory and deductive rigour* .

He thinks of these as hand-in-hand, and objects to the pair of them masquerading as the final solution for mathematics.

I would want to separate the two. Logic is the study of our use of language: set theory is the study of well-foundedness, and not, as MacLane thinks, the study of the process of set formation.

That is the great difference between Zermelo-Fraenkel and Zermelo. Zermelo is a system to support set formation, and is adequate for geometrical considerations; Zermelo Fraenkel is a system to support definitions by recursion, building structures into the unknown, and thus is suited to the arithmetical side of mathematics.

In Zermelo set theory, one cannot prove that every well-ordering is isomorphic to an ordinal; one cannot prove the existence of $\omega+\omega$; one canot justify recursion on ordinals or on an arbitrary well-founded relations. Thus induction, which is at the heart of arithmetic, is missing from (large parts of) geometry. On the other hand, spatial intuition is missing from arithmetic; so we need both.

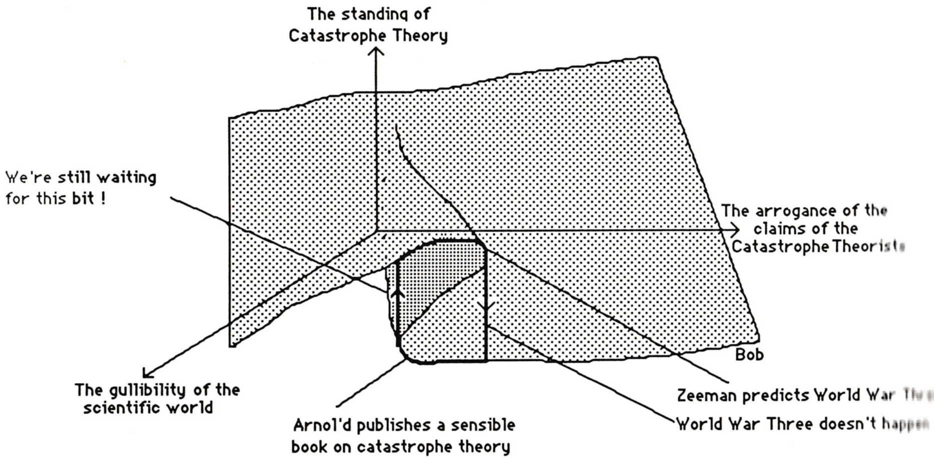
I suggest that the new philosophy of mathematics for which MacLane calls will emerge from a study of the interplay between arithmetic and geometry.

To conclude: Professor M.J.Crowe has stated that there are no revolutions in mathematics.

This is well worth pondering: it might be true. If it is true, it is because mathematics can expand to accommodate apparently conflicting ideas. Bourbaki's dogmatic preservation of pre-Gödelian concepts of rigour militates against such expansion, and therefore Bourbaki must be regarded as counter-revolutionary.

A Short History of Catastrophe Theory

Bob Dowling



Automorphism Groups

Tom Wilde

This article presents some theorems about group automorphisms and finds the automorphism group of S_n for all n. All groups are assumed finite .

Let G be a group with a normal subgroup $K \trianglelefteq G$. $\sigma_g: h \to ghg^{-1}$ is an automorphism of G - these are called inner automorphisms. $\sigma_g(K) = gKg^{-1} = K$ by normality, so $\sigma_g|_K$ is an automorphism of K. Furthermore, if $g \notin K$, $\sigma_g|_K$ will not in general be an inner automorphism of K. We thus have a map

$$\theta: G \to \mathrm{Aut}(K) \quad \text{(the group of automorphisms of K)}$$
$$g \to \sigma_g|_K$$

It is easy to see that θ is a homomorphism, and that

$$\ker\theta = \{g \in G : gkg^{-1} = k \text{ for all } k \in K\} = C[K]$$
$$= \text{the centralizer of K in G}$$

In general we will not suppose that $K \trianglelefteq G$ but will consider injective homomorphisms $\rho: K \to G$ such that $\rho(K)$ is normal in G. These are called extensions of K. Aut(K) and Aut(ρ(K)) are clearly isomorphic under the map

$$\psi: \mathrm{Aut}(\rho(K)) \to \mathrm{Aut}(K)$$
$$\sigma \to \rho\sigma\rho^{-1}$$

And we still have a homomorphism

$$\psi\theta: G \to \mathrm{Aut}(K)$$
$$g \to \rho(\sigma_g|_K)\rho^{-1}$$

whose kernel is the centralizer $C[\rho(K)]$ of $\rho(K)$ in G.

With this slight generalization, we consider the group K to be given , and look for extensions $\rho: K \to G$ such that $\psi\theta$ is surjective, ie for which all automorphisms of K arise as restrictions of inner automorphisms of G. In fact this is easily done, and we need look no further than the permutation representations of K in S(K), the group of permutations of the elements of K. Let $\rho: K \to S(K)$ be an injective homomorphism, for example the Cayley map

$$\rho: k \to l_k$$
$$\text{where} \quad l_k: h \to kh$$

In general $\rho(K)$ is not normal in S(K). However, we can take

$$G = \{\sigma \in S(K) : \sigma\rho(K)\sigma^{-1} = \rho(K)\} = N[\rho(K)] = \text{the normalizer of } \rho(K)$$

then $\rho(K)$ is normal in G, and G is an extension of K. Now, Aut(K) is itself a subgroup of S(K), and for a certain class of representations it actually lies inside $N[\rho(K)] = G$. Then $\psi\theta$ becomes an endomorphism of G .

Lemma: Let $\rho:K\to S(K)$ be injective, and suppose that this diagram :

$$\begin{tikzcd} K \arrow[r, "\rho"'] \arrow[d, "A"] & \rho(K) \arrow[d, "\sigma_A|_{\rho(K)}"] \\ K \arrow[r, "\rho"'] & \rho(K) \end{tikzcd}$$

commutes for all $A\in Aut(K)$, where σ_A is the inner automorphism of $S(K)$

$$\sigma_A:B\to ABA^{-1}$$

Then: (i) $Aut(K)\leq N[\rho(K)]$

(ii) $\psi\theta$ is the identity on $Aut(K)$

Proof: If $A\in Aut(K)$ then $A\rho(k)A^{-1}=\sigma_A(\rho(k))=\rho(A(k))$ (by the diagram) $\in\rho(K)$ Thus $A\rho(K)A^{-1}\leq\rho(K)$, and $A\in N[\rho(K)]$. $\psi\theta(A)=\rho(\sigma_A|_K)\rho^{-1}=A$ (by the diagram) , so $\psi\theta$ is the identity on $Aut(K)$.

(i) and (ii) clearly imply that $\psi\theta$ maps G onto $Aut(K)$ surjectively.

It is now easy to check that the conditions of the lemma hold when

$\rho=\rho_1:k\to l_k$, where $l_k:h\to kh$

or $\rho=\rho_2:k\to r_k$, where $r_k:h\to hk^{-1}$

or $\rho=\rho_3:k\to\sigma_k$, where $\sigma_k:h\to khk^{-1}$

provided in the last case that the center of K is trivial (this is needed for ρ_3 to be injective).

Thus, $\psi\theta:N[\rho_i(K)]\to Aut(K)$ with image all of $Aut(K)$ and kernel $C[\rho_i(K)]$. The first isomorphism theorem then gives

$$Aut(K)\cong N[\rho_i(K)]/C[\rho_i(K)]$$

This holds for any group K when i=1,2 and for any K with trivial center when k=3. It can be shown that $C[\rho_1(K)]=\rho_2(K)$. For, if $A\in C[\rho_1(K)]$ then

$$A(k)=(Al_kA^{-1}A)(e)=l_k(A(e))=k.A(e)=\rho_{A(e)}(k)$$

so $A=\rho_{A(e)}\in\rho_2(K)$, and $C[\rho_1(K)]\leq\rho_2(K)$. Conversely,

$$(\rho_2(h)\rho_1(k)\rho_2(h)^{-1})(a)=kah^{-1-1}h^{-1}=ka=\rho_1(k)(a)$$

so $\rho_2(h)\in C[\rho_1(K)]$ and $\rho_2(K)\leq C[\rho_1(K)]$. Similar reasoning gives $C[\rho_2(K)]=\rho_1(K)$. Hence,

$$\mathrm{Aut}(K)\cong N[\rho_1(K)]/\rho_2(K)\cong N[\rho_2(K)]/\rho_1(K)$$

As an example of this theorem , suppose $K=\langle a,b:a^2=b^2=1,ab=ba\rangle\cong C_2\oplus C_2$. K is commutative and satisfies $k=k^{-1}$ for all $k\in K$, so $\rho_1=\rho_2$.

$$\rho_1(K)=\{I_1,I_a,I_b,I_{ab}\}=\{(),(1\ a)(b\ ab),(1\ b)(a\ ab),(a\ b)(1\ ab)\}=V$$

This is a subgroup of S(K) and a union of two conjugacy classes , hence a normal subgroup . This gives $N[\rho_1(K)]=S(K)$, $\rho_2(K)=V$ so

$$\mathrm{Aut}(K)\cong S(K)/V\cong S_4/V_4\cong S_3$$

This can of course be found by more elementary means, but it is just an illustration of a theorem which is useful in more general settings.

The remainder of this article is devoted to a connected topic; finding the group of automorphisms of S_n, the symmetry group of degree n, for all n. We prove the following result:

<u>Theorem:</u> Except when n=6 , $\mathrm{Aut}(S_n)=\mathrm{Inn}(S_n)\cong S_n$. $[\mathrm{Aut}(S_6):\mathrm{Inn}(S_6)]=2$.

(Inn(G) is the group of inner automorphisms of G)

<u>Proof:</u>

First observe that, for n>2, S_n has trivial center (so the map $g\to\sigma_g$ has trivial kernel, and $\mathrm{Inn}(S_n)\cong S_n$) and is generated by the set

$$T=\{(1,2)\ ,\ (1,3)\ .\ .\ .\ (1,n)\}$$

Any automorphism of S_n is thus determined by its effect on T .

If $A\in \mathrm{Aut}(S_n)$ maps the conjugacy class of transpositions onto itself, then A(T) is a set of transpositions, any two of which have a number in common (for otherwise A would map a 3-cycle (of order 3) to a transposition pair (of order 2)). Unless n=4, it is easy to see that A(T) must be a set of the form $\{(a,i):i\neq a\}$ for some a (this just takes a little scribbling to check the cases). But there are just n such sets, so there are at most n! such automorphisms. Since there are n! inner automorphisms, all of which map transpositions to transpositions, we see that all such automorphisms are inner.

We have thus shown that any outer automorphism must map the transpositions onto another conjugacy class (Clearly if g_1 and g_2 are conjugate, then so are $A(g_1)$ and $A(g_2)$ for any $A\in \mathrm{Aut}(G)$, so automorphisms map conjugacy classes onto each other). Let this class be $[\sigma]$. σ must have order 2 , so it is a product of r disjoint transpositions for some r . Then

$$|[\sigma]|=\frac{n!}{2^r r!(n-2r)!}\quad\text{(by an elementary combinatorial argument)}$$

But also,

$$|[\sigma]| = \text{number of transpositions} = \frac{n(n-1)}{2}$$

so $\quad 2^{r-1} = {}^{n-2}C_r(n-2-r)(n-3-r)\ldots(n-2r+1) \qquad (*)$

The product on the right cannot have any odd terms , so either

(i) $\quad n-2-r=2$ and $n-2r+1=1$, so $r=4$ and $n=8$, which doesn't work , or

(ii) $\quad n-2-r=n-2r+1$

In this case , we have $r=3$ which we can substitute in * to get $n=6$, and indeed we find that S_6 has 15 transpositions and 15 products of three disjoint transpositions . So far we have shown that for $n\neq 6$

$$Aut(S_n)=Inn(S_n)\cong S_n$$

and that an outer automorphism of S_6 would map T to a set of 5 (..)(..)(..)'s . By considering the image of products of pairs of transpositions in T, we find further that in such a set , no two (..)(..)(..)'s have a transposition in common. By inspection, a given (..)(..)(..) in S_6 can appear in just two such sets, so that there are 15x2/5=6 of them, say $P_1,\ldots,P_6$. They are shown in the figure at the end of the article. An outer automorphism A of S_6 has $A(T)=P_i$ for some $i\in\{1\ldots6\}$. This restricts us to 6.5!=6! choices for A, so we have $[Aut(S_6):Inn(S_6)]\leq 2$. Finally, observe that the set $\{P_1,\ldots,P_6\}$ is actually an orbit under the action of G by conjugation, ie. if we define $g(H)=gHg^{-1}$, for subsets H of S_6, then each g in S_6 maps $\{P_1,\ldots,P_6\}$ to itself. Via the numbering of these sets, we get a permutation g' of {1,...,6}. Thus we have a map

$$\varphi: S_6 \to S_6$$
$$g \to g'$$

This is clearly a homomorphism . Using figure 1, we can calculate directly that

$$\varphi(1\ 2)=(1\ 2)(3\ 4)(5\ 6)$$
$$\varphi(1\ 3)=(1\ 3)(2\ 5)(4\ 6)$$
$$\varphi(1\ 4)=(1\ 4)(2\ 6)(3\ 5)$$
$$\varphi(1\ 5)=(1\ 5)(2\ 4)(3\ 6)$$
$$\varphi(1\ 6)=(1\ 6)(2\ 3)(4\ 5)$$

and thus verify that $\varphi^2(\sigma)=\sigma$ for $\sigma\in T$, but T generates S_6 so φ^2 is the identity. This shows that φ is an automorphism of the kind we are looking for, so $|Aut(S_6)|>|S_6|$. With our previous results, we see that $[Aut(S_6)]=2.6!$, as claimed .

[Figure on next page]

Figure :

(1)	(12)(34)(56)	(2)	(12)(34)(56)	(3)	(13)(25)(46)
	(13)(25)(46)		(16)(24)(35)		(16)(24)(35)
	(14)(26)(35)		(15)(23)(46)		(12)(36)(45)
	(15)(24)(36)		(13)(26)(45)		(14)(23)(56)
	(16)(23)(45)		(14)(25)(36)		(15)(26)(34)

(4)	(14)(26)(35)	(5)	(15)(24)(36)	(6)	(16)(23)(54)
	(15)(23)(46)		(13)(26)(45)		(14)(25)(36)
	(12)(36)(45)		(14)(23)(56)		(15)(26)(34)
	(16)(25)(34)		(16)(25)(34)		(13)(24)(56)
	(13)(24)(56)		(12)(35)(46)		(12)(35)(46)

Mutterings

A few pearls of wisdom, mostly overheard by Bob Dowling:

From an algebra lecture:
"A real gentleman never takes bases unless he really has to."
From the same lecturer:
"This book fills a well needed gap in the literature."
And another encouraging book review:
"This book is only for the serious enthusiast; I haven't read it myself."

Two quotes from an electrical engineer (but former mathematician):
"...but the four-colour theorem was sufficiently true at the time."
"The whole point of mathematics is to solve differential equations!"

And, as a contrast, a quote from a well known mathematician/physicist:
"Trying to solve [differential] equations is a youthful aberration that you will soon grow out of."

While on the subject how about this fundamental law of physics heard in General Relativity this year:
"Nature abhors second order differential equations."

A perplexing quote from a theoretical chemist:
"...but it might be a quasi-infinite set."
What is a "quasi-infinite set" ? Answers on a strictly finite postcard, please.

An undergraduate, of her Rings lecturer, who shall remain nameless :
"But I can't take him seriously - he's so cute and cuddly"

This year's Modesty Prize is awarded to the lecturer who said :
"Of course, this isn't really the best way to do it . But seeing as you're not quite as clever as I am-in fact none of you are anywhere near as clever as I am - we'll do it this way."

From the same lecturer :
"Now we'll prove the theorem . In fact I'll prove it all by myself."

And from a particle physics course :
"This couse will contain a lot of charm and beauty but very little truth."

At the beginning of a course it is important to reassure the audience about how straight-forward the course is and about how good the lectures are going to be. But what about this quote from the beginning of the Galois

Theory course:

"This is going to be an adventure for you...and for me."

Or this one from Statistical Physics:

"At the meeting in August I put my name down for this course becase I knew nothing about it."

In the middle of the Stochastic Systems course the lecturer offered this piece of careers advice:

"If you haven't enjoyed the material in the last few lectures then a career in chartered accountancy beckons."

A new result in number theory revealed in an optimization lecture:

"But of course, two equals one, if you see what I mean.."

An engineer actually gave an answer to the question of "quasi-infinite" sets:

"It's one with more than ten elements."

And they wonder why buildings fall over...

And now, some randomly generated theorems from J.R. Partington and H.G.E. Pinch 's program "groan":

Steenrod's Orthonormalization Corollary: Let K be a non-measurable cardinal whose adjoint is everywhere totally bounded; then it has trivial cohomology group.

Hessel's Fixed Point Theorem: Let G be a nilpotent group whose covering space is globally rational; then it is a push-out diagram.

Riemann's Uncertainty Criterion: Let σ be a singular co-chain whose polar divisor is everywhere locally recursively enumerable; then it is pi-1 undecidable.

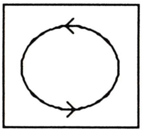
Chebychev's Ergodic Remark: Let ABC be an equilateral triangle whose Jacobson radical is tamely an algebraic stack; then it can be regarded as a cross-section of the tangent bundle.

Euler's Uncertainty Principle: Let X be an orientable 3-manifold whose Frattini subgroup is everywhere totally tame; then it is an absolute F sigma.

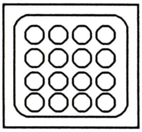
A contribution from Messrs. Owen and Edgington:

There is a department called DAMTP,
Whose car park is exceedingly cramtp.
If you park your car there,
The porters go spare,
And make sure your wheels get clamtp.

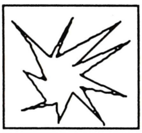
We also have some icons offered by Messrs. P. Taylor and S. James , as an aid to the efficient taking of notes :



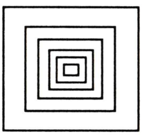
Proof by circular argument : much beloved of lecturers - going round and round is a lot easier than going anywhere .



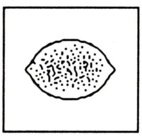
Waffle : an icon much abused by algebraicists .



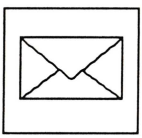
Magic : a convenient method of proof .



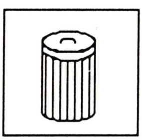
Proof by induction : see proof by induction n-1.



Lemma : the tricky bit of the next theorem .



Note .



Rubbish : a memorandum for after the lecture .

How much of the above belongs in the last icon is perhaps a moot point .

A Quick Plug For Groggs

GROGGS (General-purpose Reverse Ordered Gossip Gathering System) is THE place for public discussion on PHOENIX, and is open to all users. Initially set up last May as a rip-off of the Computing Service's SUGGEST facility, it has rapidly grown to become one of the most popular user provided services on the system, with dozens of regular readers and contributors.

GROGGS provides a means for users both to debate on serious issues, and to take part in more light-hearted discussions or to provide entertainment. Popular topics include religion, politics, music and silly rhymes about computing or other users. Recently we had a very successful party to which all GROGGS users were invited.

Current contributions may be read in the file GROGGS.CURRENT. GROGGS.ANTH86 contains the best of last year's items---take a printout to see what it's like. To use GROGGS, you will need to load the command library in GROGGS.WREN:LIBRARY. Information on the commands is available by using the HELPG command in the library, and in a file GROGGS.MISC:SPEC, suitable for PRINTOUT.

GROGGS is not in any way supported by the Computing Service. Any queries should be MAILed to userid JAS11. Happy GROGGing!

TWO PROBLEMS OF QUANTUM MEASUREMENT

Jeremy Butterfield

1. Introduction

This essay is intended as a companion piece to lectures on the philosophical aspects of quantum theory (in Lent Term 1987). In the lecture course I will discuss in more detail the material presented here. I will discuss it about two-thirds of the way through the course -- which I understand will be when Eureka is published! However, this essay is self-contained, only details, proofs and related results being consigned to the course.

The aim of this essay is to explain two problems about measurement in quantum theory: the first concerns the system measured (the object-system), the second concerns the measuring system (the apparatus). I expound the first problem in Section 3, and the second in Section 4. Before that, I review some needed facts about the notion of a mixture, about measurement, and about the quantum formalism's treatment of composite systems (Section 2).

There are three reasons for picking out these two problems for special attention. The first reason is that they go together; as we shall see at the end of Section 3, the first problem leads in to the second. The second reason is one of publicity! That is to say, these problems are not as widely appreciated in the mathematics and physics community as they might be. For example, Jammer's encyclopedic book, 'The Philosophy of Quantum Mechanics' crams each problem into a footnote. (It is small consolation that there are two footnotes, since the problems are closely related while the footnotes are more than a hundred pages apart!) And the (otherwise splendid) Wigner & Zurek collection, 'Quantum Theory & Measurement', does not emphasise these problems. So if one is to put this right, and publicise these problems, where better than the pages of Eureka? (As to why these problems are not well-known, I conjecture two causes. First, the mathematics required for the presentation of the problems is not often needed by mathematicians and physicists, so that many discussions suppress it and so also suppress the problems. Secondly, discussions that do present the problems sometimes obscure them under a presentation of related material. My aim will be to expose the problems clearly, with a minimum of extras; I reserve those for the lecture course!)

The third reason is intellectual: I think these two problems, especially the second one, are the BEST reasons for being puzzled by quantum theory! What I mean by this will emerge, as I sketch (in the rest of this Section) my overall view of the philosophical problems of quantum theory.

Quantum theory provides a way to calculate, for any quantum state and any observable and any possible value of that observable, a probability. We associate the quantum state with a unit vector in Hilbert space; (for our purposes, this means just a vector space with an inner product). We also associate the pair consisting of the observable and its value with a subspace. Then the probability is the squared length of the state's projection onto the subspace.

This prescription of probabilities is the source of many of quantum theory's fascinating features; in particular the non-existence of joint probabilities, and the interference effects that one sees in the 2-slit experiment. But I want to concentrate here on measurement. So let us ask: what exactly are these probabilities? The conventional wisdom claims that they are probabilities for obtaining the given value as a measurement result, if one measures the observable on a system which is in the given state.

This claim is remarkable on two counts. First, the geometry of projectors on the measured system's Hilbert space is claimed to control (albeit probabilistically) something that is different -- and usually far more complicated: namely the behaviour of a pointer on a dial of a different system (the apparatus). Secondly, this claim makes no statement about whether the measured system has in fact got a value for the measured observable; and if so, whether measurement is faithful in the sense that the probabilities are indeed probabilities of such possessed values.

These two points prompt, respectively, two questions. First, can one justify the claim by analysing the measurement process and proving that the pointer's behaviour is indeed controlled by the measured system? Secondly, what about possessed values? I think it is fair to say that the conventional wisdom is silent about the first question: in their everyday work, physicists do not embroil themselves in analysing measurement. But conventional wisdom is certainly not silent about the second question: it claims that a system does not in general have a value for each and every observable. Indeed, it goes further and says that a system has only got values for those observables for which it is in an eigenstate.

I think this answer to the second question is in itself perfectly acceptable; I feel no compulsion to say that every system has a value for every observable. But there is a problem about this answer. A very good case can be made that in classical physics every system does have a value for every observable. And even if that is not so, we are all convinced (at least: before we take up philosophy of quantum theory!) that many familiar systems have values for certain observables. In less technical language, we are all convinced that many familiar objects have definite properties. For example, a cat is either alive or dead. This conviction implies that there is a problem about holding that quantum systems do not in general have values: namely, we had better make sure that the 'fuzziness' we are now holding for quantum systems cannot be transmitted somehow to familiar systems, depriving them of definite properties (values for observables) that we are convinced they possess. Notoriously, the argument of Schrödinger's Cat Paradox is that we cannot be sure of this: the fuzziness is transmittable. In particular, the fuzziness of position of an electron can be transmitted through a measurement device (in Schrödinger's words: 'an infernal device') to produce fuzziness of the property 'being alive' for a cat.

So both our first and our second question prompt an investigation of the measurement process: the first directly; and the second because it seems that measurement can transmit fuzziness (not possessing a value for some observable) from a unfamiliar realm where we can accept it happily enough (the realm of electrons etc.) to the familiar realm of everyday objects where we cannot.

So we are prompted to make a quantum mechanical analysis of the measurement interaction, treating the composite system, composed of measured system and apparatus, quantum mechanically. We hope somehow to justify the claim that the pointer's behaviour is probabilistically controlled by the measured system; and to defeat Schrödinger's argument by showing that on a realistic model of measurement, the fuzziness cannot in fact be transmitted.

This investigation will have negative results. That is to say, the two problems expounded in Sections 3 and 4 will in effect show that we at present do not know how to realize these hopes: indeed, we can use the quantum formalism to prove that certain precise versions of these hopes are unrealizable.

I can now explain briefly why I said above that these two problems provide, the best reasons to be puzzled by quantum theory. Of course, I agree that many features of quantum theory are strange and unfamiliar to us. But I think we can accept many of these features as *just* that: i.e., not downright mysterious. For example, I think that we can accept physical features like the interference effects; and mathematical features like the non-existence of joint probabilities. But it is downright mysterious for one's fundamental physical theory to allow as a physical possibility that a cat is in a fuzzy combination of being alive and being dead; or (a humane example), that a pointer on a dial is in a fuzzy combination of two positions.

So we seem to be faced with a downright mystery. As is well-known, some very radical solutions have been proposed -- sometimes by great physicists. For example, it has been proposed that consciousness is responsible for eliminating the fuzziness (Wigner, and less explicitly von Neumann); and that the entire universe 'branches' or 'splits' with each measurement interaction (Everett).

I would prefer a less radical strategy; I would prefer to make the elimination of fuzziness come from a straightforward (albeit theoretical) piece of physics. There are two obvious strategies to try. First, deny the conventional wisdom that quantum systems in general lack values; this involves examining the 'proofs' that there can be no such values and finding in them premisses that one is at liberty to deny. There are two sorts of proof to examine: the algebraic 'no-hidden-variable' proofs, and proofs of Bell inequalities. (In the lecture course, I'll suggest that one can indeed find such premisses!) The second strategy (which I don't expect to discuss in the lectures) remarks that the mystery expounded in Sections 3 and 4 below seems to arise from the unitarity of the time-evolution prescribed for systems by quantum theory, in particular by Schrödinger's equation; and proceeds to look for a theory with a non-unitary time-evolution.

But let me proceed to describing the mystery. (Throughout the essay, I shall assume that the observables we are concerned with have pure discrete spectrum; so each has an orthonormal basis of eigenvectors. This makes the mathematics easy -- it means we can make do with linear algebra, and avoid functional analysis. And it doesn't affect the conceptual points. We can similarly suppose throughout that the Hilbert spaces are finite-dimensional. Limited space means that I must suppress details and many proofs; but I should mention that the proofs are easy; and often even easier if we consider maximal observables, i.e. observables with no degenerate eigenvalues.)

2. Mixtures, Measurement and Composite Systems

In this Section, I review some facts about the notion of a mixture which we will need, and then use the notion to describe the projection postulate -- which claims to describe how the state of a measured system changes on measurement. The question whether the projection postulate can be justified leads in to a review of the quantum formalism for composite systems.

The usual way to present quantum theory's prescription of measurement results is as follows. Let $\{u_k\}$ be an orthonormal basis of eigenvectors of the observable (Hermitian operator) A. Let A be maximal, i.e. let A have no degenerate eigenvalues. Let the eigenvalue associated with u_k be a_k. Consider a system in state $v = \mu_k u_k$. (I use the summation convention.) Then the probability of obtaining a_m on measurement of A is $|\mu_m|^2$. This is the squared length of v's projection onto u_m. Similarly for the case where a_m is a degenerate eigenvalue.

This prescription can be rewritten in terms of traces and projectors, as follows. Let A_k be the eigenspace of A for eigenvalue a_k; it has dimension greater than 1 if a_k is degenerate. Let P_{Ak} be the projector onto A_k. The spectral theorem then tells us that: $A = \Sigma_k a_k P_{Ak}$. Now our new prescription of probabilities trades in the state v for its corresponding projector, call it P_v. We have:

$$tr(P_v P_{Ak}) = \text{probability (get } a_k \text{ for a measurement of A on v)}$$

The linearity of expectation, and of trace, and the expression of A as a linear combination of projectors, with its eigenvalues as coefficients, means that a similar formula holds for the expectation value of A:

$tr(P_v A) = Exp_v(A) \equiv \Sigma_m\ a_m.\text{proby(get } a_m \text{ in v)}$ So far we have only traded in one representative of quantum states (vectors) for another (1-dimensional projectors); but we can now generalize the notion of a state. We use the linearity of trace (now on the state rather than the observable) to urge that certain real linear combinations of projectors represent a generalized notion of state.

Thus suppose you have an ensemble (i.e. set!) of systems; but the systems are not all in the same state v (a state of the familiar kind). Rather, some systems are in state v_1; some systems are in v_2 etc. Suppose that the proportions with which these states occur are respectively w_1, w_2 etc. (So we have $\Sigma_i\ w_i = 1$ (i ranging over the number of states occurring in the ensemble; let us say that it is N)). Then the probabilities of measurement results, and the expectation values, for measurements on this ensemble, will be a weighted average, with weights w_i, of the probabilities and expectation values for the states v_i. The linearity of trace implies that the probabilities and expectation values for the ensemble can be calculated by ascribing to the ensemble the state: $\Sigma_i w_i P_{vi}$; and then using trace formulas as before.

So such an ensemble can be assigned as state a positive real linear combination of 1-dimensional (not necessarily orthogonal) projectors, with the coefficients (weights) summing to 1. Such a state is called a mixture, or mixed state. The usual vector states are the special case where there is only one projector; they are called pure states. There are two subtleties about the physical interpretation of mixtures. The first point is that physically different ensembles can have the same mixture as their state (and so deliver the same probabilities for all possible measurements). Thus suppose one ensemble, E say, has two equiweighted subensembles in states v_1, and v_2. Suppose that v_1 and v_2 are orthogonal, so that the sum of their projectors is the projector onto the (2 dimensional) subspace they span. Suppose that v_3 and v_4 are two orthogonal unit vectors also spanning this subspace. Finally, suppose that another ensemble, E' say, has two equiweighted subensembles in states v_3 and v_4, with weights equal to the weights in E; and suppose that E' is otherwise the same as E. Then the mixture for E is the same as the mixture for E'. But E and E' are physically different.

The second point is more subtle. We will see shortly that quantum theory requires us sometimes to ascribe a state which is mathematically a mixture, in cases where there is NO ensemble containing various usual vector states involved. This second point will be the basis of our first problem about quantum measurement, to be presented in Section 3. For the moment, let us see how we can use the notion of mixture, and the straightforward (albeit partial -- cf, first point above) description of ensembles that it provides, to present the conventional wisdom about what happens in measurement.

The conventional wisdom describes what happens to the measured system under measurement, without using the quantum formalism for interacting systems. (The description is idealized, but it is claimed that many measurement procedures approximate to it.) It is claimed: if a system in state $v = \mu_k u_k$ yields result a_m in response to measurement of observable A, then immediately after the measurement the state is the (normalized) projection of v onto the eigenspace, A_m, of A for eigenvalue a_m.

This change of state for the measured system is called the Projection Postulate. It has nice features. In particular: (1) It explains a feature to which many real measurement procedures approximate: namely, the feature that an immediately repeated measurement always gives the same result as was first obtained. (For a maximal observable, this feature implies that the change is as described. For a non-maximal observable, it does not; the feature is compatible with the state changing to any unit vector in the eigenspace for the result -- one then argues that a minimally disturbing measurement will send the state to the closest candidate in the eigenspace, i.e. the normalized projection.) (2) It makes a natural definition of simultaneous measurability for observables A and B (a definition I omit here) equivalent to A and B commuting as operators, i.e. $[A,B] = 0$.

Mixtures provide a nice formulation of the Projection Postulate, if we consider a measurement of A, but not the particular result obtained. This corresponds to measuring A on an ensemble of systems in a state v; unless v is an eigenstate of A, different systems will go into different states (of the usual vector kind), with weights defined by the coefficients of v in a basis for A. (The initial state v passing to different states, while the measurement apparatus is in the same state in all cases, expresses the indeterminism of quantum theory.) Thus the resulting ensemble will have as state a mixture of the straightforward kind above. It is easy to see that if $v = \mu_k u_k$, and A is maximal, then the ensemble's post-measurement state is: $\Sigma_k |\mu_k|^2 P_{uk}$. And it is easy to prove that in the general case v goes to: $\Sigma_m P_{Am} P_v P_{Am}$, where the sum is over the eigenvalues of A. Given this last formula, it is easy to show that if prior to measurement our ensemble is heterogeneous, and so has as state a mixture, $\Sigma_i w_i P_{vi} = W$ say, then after measurement the state is $\Sigma_m P_{Am} W P_{Am}$.

So the Projection Postulate has nice features, and is nicely formulated in terms of mixtures: a formulation which, while prescribing a definite change of state for the ensemble, expresses the indeterminism of the evolution under measurement of the state of an individual measured system.

But even if one accepts the indeterminism (as I am happy to do), there is clearly a conceptual problem of justification. After all, quantum theory prescribes the evolution of systems through the Schrödinger equation; so one wants the Projection Postulate to be compatible with that prescription. This problem is easily shown to be simply insoluble if one considers only the measured system. Thus recall that the Schrödinger evolution is unitary: for all times t, there is a unitary operator on Hilbert space U_t, such that state v at time $t = 0$ evolves under the Schrödinger equation to $U_t(v)$. It is easy to show that any mixture must then evolve by $W \rightarrow Wt \equiv U_t W U_t^{-1}$. It is also easy to show that a mixture W represents a pure state iff $W = W^2$. Then it follows that $W_t^2 = U_t W^2 U_t^{-1}$. Then $W_t^2 = W_t$ if $W^2 = W$. That is to say, any pure state evolves by the Schrödinger equation to another pure state. And therefore, no Schrödinger evolution of the measured system can obtain for us the mixture prescribed by the

Projection Postulate, which is in general not a pure state.

The advocate of the Projection Postulate has an obvious suggestion to make. Quantum theory prescribes the Schrödinger evolution only for isolated systems, and a measured system is of course not isolated. So perhaps there is compatibility between the Schrödinger evolution of the composite system, consisting of measured system and apparatus together, and the evolution for the measured system as described by the Projection Postulate. That is: the obvious suggestion is that a suitable Schrödinger evolution for the composite will induce a pure-to-mixture evolution for the component measured system.

This suggestion requires us to look at the quantum formalism for composite systems. We shall review it here, and then argue in Section 3 for a negative result: that the Projection Postulate cannot be justified as one system's view' of a composite Schrödinger evolution. Measuring apparatus is usually vastly too complex to be treated in detail. But fortunately, a very idealized description will suffice for our purposes. The reason is this: there seems no prospect of overcoming the negative result presented in Section 3, by abandoning the idealizations. (A similar point will apply to the negative result in Section 4.)

If two systems S_1 and S_2 have Hilbert spaces H_1 and H_2, then the composite system $S_1 + S_2$ has as its Hilbert space the tensor product $H_1 \otimes H_2$. This is essentially the set of all linear combinations of ordered pairs of elements from H_1 and H_2, the pair being written as $v_1 \otimes v_2$. $\otimes$ is to distribute over +; and the inner product on $H_1 \otimes H_2$ is defined by: $\langle v_1 \otimes v_2, u_1 \otimes u_2 \rangle = \langle v_1, u_1 \rangle_1 . \langle v_2, u_2 \rangle_2$, and then extending by linearity. These prescriptions in effect fix the tensor product upto isomorphism; (an explicit construction of it as a set of conjugate linear maps from H_2 to H_1 is very useful in discussing composite systems, but we can skip that here.) If $\{v_{1i}\}$ is a basis of H_1, and $\{v_{2j}\}$ is a basis of H_2, then $\{v_{1i} \otimes v_{2j}\}$ is a basis of $H_1 \otimes H_2$.

The intuitive physical justification for using the tensor product is that given two complete families O_1 and O_2 of observables on S_1 and S_2, we want the union $O_1 \cup O_2$ to be a complete family for the composite. Let V_1 be the cartesian product of the spectra of the elements of O_1; then H_1 can be represented as the set of square-integrable functions on V_1. (Thus, in elementary wave mechanics for one particle, {x-position, y-position, z-position} does for O_1, so $\mathbf{R}^3$ will be V_1, and H_1 is represented in the familiar way.) Then the Hilbert space for the composite should be representable as the set of all square-integrable function on $V_1 x V_2$. And one argues in the usual way that this is spanned by all linear combinations of functions of the form $f_1(x_1)f_2(x_2)$, $f_i \in H_i$ and $x_i \in H_i$. (This justification of the tensor product shows that it is the right Hilbert space, not only for composite systems but also for any case where we introduce a new degree of freedom -- e.g. adding spin to a single particle.)

We can build operators on $H_1 \otimes H_2$ by defining the product $Q \otimes R$ of two operators on H_1 and H_2 respectively by: $Q \otimes R(v_1 \otimes v_2) = [Qv_1] \otimes [Rv_2]$, and then extending to all of $H_1 \otimes H_2$ by linearity. The distributivity of over + for operators on $H_1 \otimes H_2$ is then inherited from the corresponding distributivity in $H_1 \otimes H_2$ itself. We can also show that $P_{v1 \otimes v2} = P_{v1} \otimes P_{v2}$.

If a system S_1 is part of a composite $S_1 + S_2$, then it is natural to expect that a quantity on S_1, can be represented not only by an operator on H_1 (call it Q, as before), but also by an operator on $H_1 \otimes H_2$. And it is natural to take this second operator as $Q \otimes I$, where I is the identity on H_2. This twofold representation of a component system quantity leads to a condition of meshing between component and composite states, which is very important for us: as follows.

Let W be the state, in general a mixture, of a composite system, consisting of $S_1 + S_2$. We suppose that the component systems have states W_1 and W_2, presumably also in general mixtures. It is also natural to expect that if Q_1 and Q_2 are quantities on S_1 and S_2 respectively, then the composite and component states, W, W_1 and W_2 will mesh according to the following conditions:

$$tr_1(Q_1 W_1) = tr((Q_1 \otimes I)W)$$

and

$$tr_2(Q_2 W_2) = tr((I \otimes Q_2)W)$$

The lhs traces are over H_1 and H_2 respectively while the rhs traces are over $H_1 \otimes H_2$. The intuitive justification for this meshing condition is simply that the average value of a quantity in a given state should be insensitive to whether the system is considered as a component of a larger system.

So far we have said only that the composite and component states should mesh. But it is natural to expect that W will determine the states W_1 and W_2, since a 'whole should determine its parts'. In fact the meshing condition entails this, provided that the set of Q's for which it holds is large enough. To be precise: if the condition holds for every projector on H_1 and on H_2, then W determines W_1 and W_2. I shall not prove this. But a simple calculation for the case where W is pure gives a sense of what W_1 and W_2 are like.

So consider the composite system to be in the pure state with vector:

$$\psi = \Sigma_{ij}\, c_{ij}\, v_i \otimes u_j$$

where $\{v_i\}$, $\{u_j\}$ are bases for H_1, H_2. The projector P onto this vector has components in the $v_i \otimes u_j$ basis: $c_{rs}c^*_{ij}$. (Here r and s range respectively over the ranges of i and j.) Then we calculate:

$$\begin{aligned} \langle\psi,[Q_1\otimes I]\psi\rangle &= \Sigma_{ijrs}\, c^*_{ij}\, c_{rs} \langle v_i\otimes u_j,[Q_1\otimes I]v_r\otimes u_s\rangle \\ &= \Sigma_{ijr}\, c^*_{ij}\, c_{rj} \langle v_i, Q_1 v_r\rangle. \end{aligned}$$

We note that $\langle v_i, Q_1 v_r\rangle$ is the (i,r) component of Q_1 in the $\{v_i\}$ basis: call it Q_{1ir}. So if we define the following numbers by summing over components of P_ψ

$$w_{ri} \equiv \Sigma_j\, c^*_{ij}\, c_{rj} \qquad (+)$$

and then define W_1 as having components w_{ri} in the basis $\{v_i\}$, then we obtain:

$$\begin{aligned} \langle\psi,[Q_1\otimes I]\rangle &= \Sigma_{ir}\, w_{ri} Q_{1ir} \\ &= tr_1\,(W_1 Q_1) \end{aligned}$$

Thus W_1 acts here like a mixture state for S_1. Its components in $\{v_i\}$ are obtained by (+), a summing out of the components for S_2. One can show that W_1 is indeed a mixture: a positive real linear combination of projectors with weights summing to 1. It is said to be obtained by partial tracing, or tracing over S_2, from the composite system's state.

As mentioned above, by applying the meshing condition to every projector, one can show that $W = P_\psi$ uniquely determines W_1. (The converse fails: W_1 and W_2 do not together determine W. This corresponds to the physical fact that W can contain information about correlations between S_1 and S_2, which are not contained in W_1 and W_2. In fact, given W_1 and W_2, they naturally define an operator on $H_1\otimes H_2$, viz. $W_1\otimes W_2$; and this is mathematically of the right kind to be a state for the composite system, and it meshes in the above sense. But it represents the special case where S_1 and S_2 are probabilistically independent.)

More important for us, however, is the fact that: W_1 is in general a mixture with more than one projector, even when W is a pure state, i.e. a

1-dimensional projector like P_ψ above.

This sounds hopeful for the advocate of the Projection Postulate it sounds like a vindication of the idea that the pure-to-mixed evolution prescribed by the Projection Postulate is a 'measured system's view' of a unitary composite evolution.

The hope is strengthend by the following fact. One can write down unitary operators on $H_1 \otimes H_2$ giving a final state W that determines mixtures W_1 and W_2 of the right kind for recovering the Projection Postulate. More precisely, suppose we are measuring Q_1 on S_1 by 'pointer-position', call it Q_2, on S_2. And suppose for simplicity that neither quantity has degenerate eigenvalues; let $\{v_i\}$ be a basis for Q_1, and $\{u_j\}$ be a basis for Q_2. And suppose that measurement sets up an eigenstate correlation, in the sense that measurement involves the evolution:

$$v_i \otimes u_0 \rightarrow v_i \otimes u_i,$$

where $Q_1 v_i = q_i v_i$, where u_0 is the 'pointer ready but not measuring' eigenstate of pointer-position Q_2, and u_i is the 'pointing at q_i' eigenstate of Q_2. Then: there are unitary operators U such that both:

(i) U dictates the above eigenstate-correlating evolution, i.e. $U(v_i \otimes u_0) = v_i \otimes u_i$;

and; (ii) if the object is initially in a superposition for Q_1, say in state $v = \Sigma_i \mu_i v_i$, then the mixture for S_1 determined by the final state is: $\Sigma_i |\mu_i|^2 P_{vi}$.

The mixture occurring at the end of (ii) is the mixture required by the Projection Postulate. (The conditions for obtaining this result can be weakened considerably: in particular, one can avoid the obviously false assumption that pointer-position has no degenerate eigenvalues.) But there is a problem ...

3. The Impossibility of Securing a Proper Mixture for the Measured System

There is a basic problem with this proposal, unrelated to the ways in which it idealizes the measurement interaction: a problem which turns solely on the physical interpretation of a mixture W_1 determined by partial tracing from a composite system state W.

Thus suppose the composite state W is pure, while the determined W_1 is mixed. Think of W as describing an ensemble of composite systems, each in the pure state W. This ensemble specifies an ensemble of S_1

systems; whose state is W_1. Can we think of each member of the S_1 ensemble as definitely in one of the pure vector states (vectors in H_1) occurring in W_1? That is, can we interpret W_1 in the straightforward way, in terms of heterogeneous ensembles?

Let us here set aside the point made in Section 2 -- that a mixture may not pick those projectors among some equiweighted ones that are physically correct. Let us suppose that there is no such equiweighting, which would force a No answer. Nevertheless, the answer is still No, for the following simple reason. If the answer were Yes, the composite state would also have to be a mixture -- which it isn't. That is to say: even though W_1 and W_2 do not determine W, it is physically clear that if the S_1 ensemble is in a straightforwardly interpretable mixture W_1 (i.e. the answer is Yes), then the composite ensemble must also be in a mixture, with a straightforward interpretation.

We are thus faced with a second subtlety about the interpretation of mixtures. Quantum theory requires us sometimes to ascribe a state which is mathematically a mixture, in cases where there is no ensemble containing various usual vector states involved. Rather, we have one system interacting with another system; the composite system is in a pure state W. The component systems are not in a pure state; their state is mathematically a mixture, which is fully defined by the composite system's state. Yet it will be wrong to interpret these mathematical mixtures in the straightforward way, in terms of heterogeneous ensembles. In summary: if one calculates probabilities and expectations for measurements on a component system S_1 using the mathematical mixture W_1, one gets the right results; BUT one cannot interpret such mathematical mixtures in terms of heterogeneous ensembles.

Let us say (following D'Espagnat) that a mixture which can be straightforwardly interpreted in terms of heterogeneous ensembles is a proper mixture; and that W_1 in our case is improper. The problem for the Projection Postulate is of course that it requires that the mixture $W_1 = \Sigma_i |\mu_i|^2 P_{vi}$ is proper. So the hope at the end of Section 2 fades. (A reply on its behalf, urging that it can escape the problem by considering the composite to be in a mixture, also fails. This reply urges that by supposing the apparatus is initially in a mixture -- which indeed seems more realistic --, we will obtain a final composite mixture. Indeed yes: but the coefficients in the mixture will reflect just those in the initial apparatus mixture; and so interpreting the measured system mixture as proper again conflicts with the given composite state.)

So let us abandon trying to justify the Projection Postulate. After all, experiment is very complex, and the Projection Postulate is no doubt just an approximation, as are -- no doubt -- measurements that correlate eigenstates. Nevertheless, we would like to reassure ourselves that quantum tneory gives an acceptable description of the behaviour of the measurement apparatus -- i.e. that the quantum formalism of interacting systems provides composite Schrödinger evolutions that keep the pointer in a definite position on the apparatus' dial. That attempt will also fail -- cf. Section 4.

4. The Impossibility of Securing a Mixture for the Apparatus

The attempt to keep pointer position definite in all cases fails not only for eigenstate-correlating measurements, but also for more general kinds of measurement. There are in fact a series of impossibility results, with increasingly general kinds of measurement considered. The presentation of these theorems characteristicaly have three stages: (a) a definition of what it is for a unitary joint evolution to be a measurement, (b) a specification of a condition of 'definite pointer-position in all cases', (c) a proof that no measurement satisfies the condition. (Admittedly, all the theorems define measurement, in stage (a), in an abstract and idealized way. But as I said above, there is no prospect of overcoming the impossibility results by abandoning the idealizations.) I shall sketch Arthur Fine's theorem (Physical Review 1970; cf. also Proceedings of Cambridge Philosophical Society 1969).

(a) First, we propose to relativize the notion of measurement to some apparatus (S_2) initial state W_2. Then we define:

> A joint unitary evolution U is a measurement of Q_1 on the object (S_1) by means of Q_2 on S_2 in W_2 iff:
>
> two initial object states W_1, W'_1, that differ in the probabilities they give to some value or values of Q_1, are carried to respective final composite states that differ in the probabilities given to some value of $I \otimes Q_2$

This definition is very general; it includes eigenstate-correlation measurements as a special case. (For a survey of notions of measurements that it includes, see Fine 1969.) Fine says that object states that differ in the probabilities they give to some value or values of Q_1 are Q_1-distinguishable. The definition above is thus that Q_1-distinguishable initial object states should be carried to $I \otimes Q_2$-distinguishable final composite states.

(b) We allow that the final composite state is a mixture. So the "definite pointer position in all cases" requirement is that the final state should be a linear combination of projectors (on $H_1 \otimes H_2$), each of which gives a definite pointer position. (Admittedly, it may be problematic to interpret the final mixture as a proper one; but the requirement just made is a *necessary* condition of definiteness of pointer position; and so since the impossibility theorem given in (c) below shows that it cannot be satisfied, it follows that definiteness cannot be.) That a projector gives a definite pointer position means that the vectors in its range should be linear combinations of $v_1 \otimes v_2$, v_1 any vector in H_1, and v_2 an eigenvector of the quantity pointer-position, call it Q_2; where in any such linear combination all the v_2 eigenvectors of Q_2 are for the same eigenvalue. In short, we require that the final composite state should be of the form:

$$U(W_1 \otimes W_2)U^{-1} = \Sigma_m w_m P_{\beta m} \qquad \text{(def)}$$

where $\beta_m \in H_1 \otimes H_2$ is in the subspace $H_1 \otimes H_{\mu(m)}$. Here μ is a map from the values of m that index the components of the mixture to eigenvalues of Q_2; and $H_{\mu(m)}$ is the eigenspace of Q_2 for eigenvalue (m).

(c) The proof that no unitary operator U can be a measurement in the sense of (a), while also satisfying (def) above is simple. It only assumes that there are in H_1 two eigenvectors v_1, v_2 of Q_1 for different eigenvalues.

Let the initial apparatus state be $W_2 = \Sigma_n w_n P_{un}$.

Define for i = 1,2: $F_i = U(P_{vi} \otimes W_2)U^{-1}$.

Then $F_i = \Sigma_n w_n P_{\beta(i,n)}$, where $\beta(i,n) = U(v_i \otimes u_n)$. Assuming that U satisfies (def), we have:

$\beta(i,n) \in H_1 \otimes H_{\beta(i,n)}$, where $\beta(i,n)$ labels eigenspaces of pointer position, dependent upon the initial object state i, and the component n of the original apparatus mixture.

Now consider an initial object superposition:

$$v = \lambda_1 v_1 + \lambda_2 v2, \ \ \Sigma|\lambda_i|^2 = 1.$$

Define: $F \equiv U(P_v \otimes W_2)U^{-1} = \Sigma_n w_n P_{\beta(n)}$,

where $\beta(n) = U(v \otimes u_n) = \lambda_1 \beta(1,n) + \lambda_2 \beta(2,n)$.

(def) requires that $\beta(n) \in H_1 \otimes H_{\mu(n)}$, where is some map from components of the mixture to eigenvalues of pointer position, and $H_{\mu(n)}$ is the corresponding eigenspace.

But since eigenspaces of pointer position corresponding to different eigenvalues are orthogonal, and their sum is not an eigenspace, the fact that $\beta(n) = \lambda_1\beta(1,n) + \lambda_2\beta(2,n)$ implies that $\mu(1,n) = \mu(2,n) = \mu(n)$ for all n. However this implies that F_1 and F_2 are $I\otimes Q_2$-indistinguishable (short exercise!), while clearly P_{v1} and P_{v2} are Q_1-distinguishable. Therefore, U cannot satisfy (def) and be a measurement of Q_1 by Q_2 in initial apparatus state W_2, in the sense of stage (a) above.

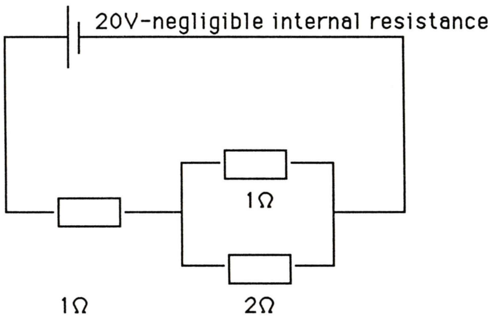
In short, Schrödinger's cat still threatens to be posed fuzzily between life and death!

Resistance rectangles

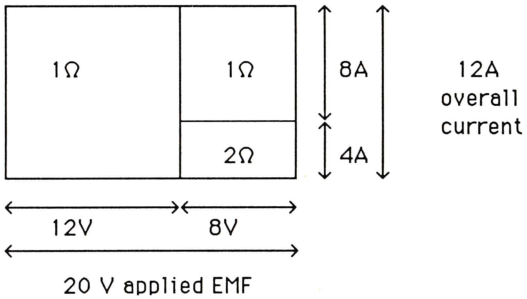
J.G.R. Levy

This is an approach to problem-solving in D.C. circuits which I developed after becoming aware that my conception of resistance, current and potential difference was vaguely spatial.

Consider the following circuit :



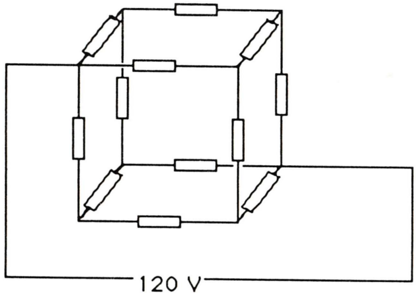
Describe it in full. This is best done as shown below.



The proportions of each rectangle are determined by the respective resistance: hence 1Ω resistances create squares, and 2Ω resistances create rectangles of length-height ratio 2:1 .

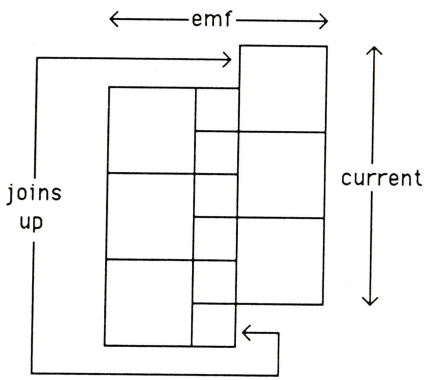
The proportions of the composite rectangle (5:3) correspond to the composite resistance , which is 5/3 Ω . As expected, these proportions are independent of the applied potential difference .

Observant readers will notice that the edges of the rectangle I have drawn are arbitary, ie. it would be more apt to sew together opposing pairs of faces to make a torus.



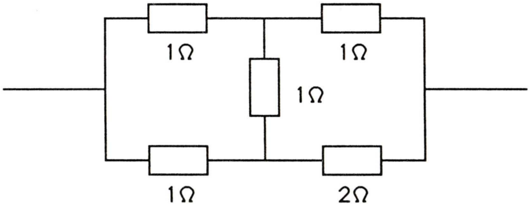
Given that each resistor is 1Ω, describe in full .

In this case we need to use a torus to draw the network without overlaps. The figure below is a torus cut up.

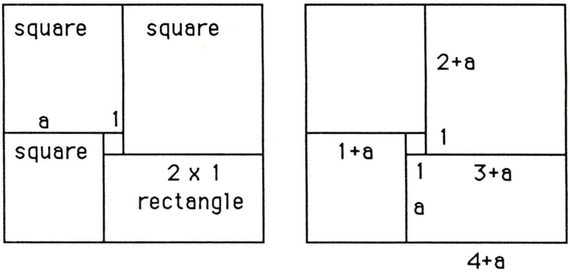


Thus the overall current is 6/5 x 120V = 144 A

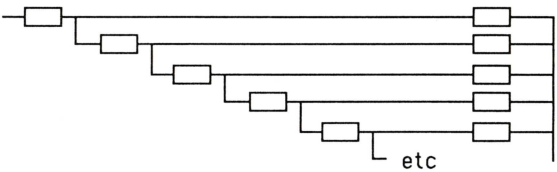
Find the overall resistance of :



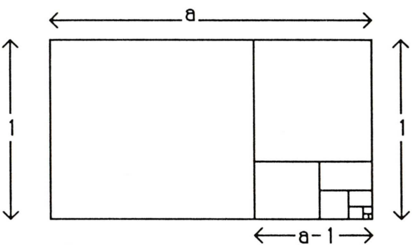
Here goes :



All we need to do is assign arbitary values 1 and a as shown , fill in the other sides and solve the resulting equations . Because , by the dimensions of the non-square , 4+a=2a , a=4 and so the overall resistance is 13/11Ω . Now find the overall resistance of :



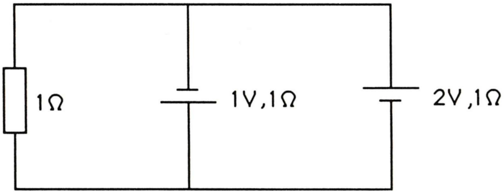
Well ,



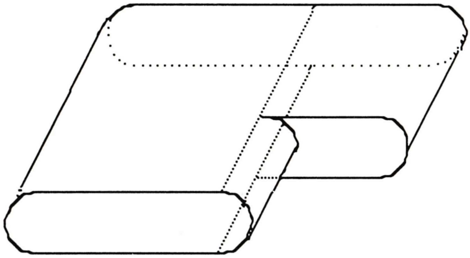
$$\frac{a}{1} = \frac{1}{a-1}$$

This is the Golden Rectangle , giving a resistance of $(\sqrt{5} + 1)/2\ \Omega$

We can extend the parallel to circuits with more than one D C source. eg :

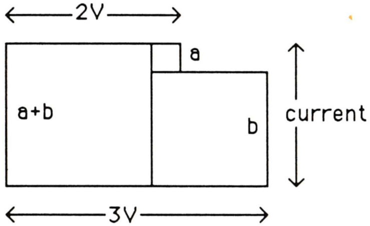


Here we have a sort of jumbled up conveyor belt :



Here the pieces separated by divisions are squares, the perimeter of the distant edge is 3 units (there being 3V round the battery-battery circuit) and the perimeter of the near edge is 2 units (as there is 2V round the 2V battery - resistor circuit) .

Cutting this open into a manageable patchwork, and writing a for the current through the 1V cell and b for the current through the 1Ω resistor, we have :



a+2b = 3 and 2a+b = 2 so a=1/3 , b=4/3 .

DPMMS & DAMTP

Neil Strickland

My purpose in writing this article is to advance some opinions about the content and teaching of the maths tripos. In doing so, I am well aware that my own mathematical training is far from complete; that I know very little about the development of the tripos as it stands; that I have never taught mathematics myself; and that many peoples' style and approach in maths is very different from my own. This article should be seen as a discussion paper - I hope it will at least provoke some constructive thought on the questions involved. My theme is essentially that pure and applied mathematics have been almost completely divorced from each other, to the great detriment of both. At the risk of accusations of heresy from all directions, I suggest that:

Pure mathematics would benefit greatly if more examples from applications were used to motivate the abstract theory.

The pure syllabus could be made considerably more relevant to applications without sacrificing any interest or depth.

The applied courses could make considerably better use of results established and concepts used in the pure courses.

Much more pure mathematics can be applied than is generally realised. I would not claim that all of these applications, especially at the sort of level I am talking about, lead to results of any vast significance, but I do not think this is the point. The idea is to provide motivation and assist in understanding. For many people, groups (for example) are hopelessly abstract. They can push symbols around and produce answers, but without any real feel for their meaning, without any intuitive picture of what is going on. A concrete example, the group of symmetries of some physical situation perhaps, can make the world of difference. On the other hand, most people finish the linear algebra course feeling that a vector space is a fairly familiar and comprehensible object. At this stage, the observation - even if it is trivial - that some physical system is a vector space, can tie things together and make them easier to conceptualise .

To my mind, one of the most beautiful things about mathematics is its essential unity. Practically everything is related to everything else. Groups and fields combine to give Galois theory. Analysis abounds with algebraic structures - rings of germs, vector spaces of functions, groups of conformal maps. Electrodynamics in the context of special relativity gives the classical theory of fields. Quantum field theory, thermodynamics and general relativity all interact in the theory of Hawking radiation from black holes. Differential geometry gives rise to elegant and powerful formulations of Lagrangian mechanics, cosmology and thermodynamics. A full treatment of quantum theoretical state spaces involves functional analysis. I find it tragic that so little attention is payed to these relationships. I am not asking for any detailed treatment of them in lectures, but simply a more imaginative selection of examples .

I have given below a fairly random list of examples, which I hope will illustrate the points I have made. They are all based on my own experience - some of them probably do not apply to everyone.

In the probability course one learns about moment generating functions: the m.g.f. m for a random variable X is defined by

$$m(k)=E[\exp(ikX)] .$$

Various properties of m.g.f.'s are generally proved in a fairly ad hoc fashion. There is another way of looking at them which is not usually mentioned. Suppose random variables X and Y have probability distributions f and g respectively. A little thought will convince you that Z=X+Y has distribution

$$h(z)=\int f(z)g(z-t)dt$$

i.e. h is just the convolution

$$h=f^*g$$

By the convolution theorem (remember linear systems?)

$$\tilde{h}=\tilde{f}.\tilde{g}$$

(Where f is the Fourier transform of f). Now,

$$\tilde{f}(k)=\int f(x)\, e^{-ikx}\, dx = E[e^{-ikx}]$$

so $\tilde{f}$ is just the moment generating function for X.

Neither the probability lecturer nor the linear systems lecturer explained this connection .

The first year course on groups concentrates on the finite case, and mentions no physical applications. Yet groups have become an important tool for exploiting symmetries in mathematical physics. Group theory plays a fundamental role in gauge theories of particle physics - indeed, most of the theory can be derived from the requirement that it be invariant under the Poincaré group (generated by translations and Lorentz transforms) and under local gauge groups. Some non-linear differential equations can be solved by studying local transformation groups under which they are invariant. Although finite discrete symmetry groups can sometimes be exploited (e.g. to solve Schrödinger's equation in a periodic potential), most of the groups of interest here are Lie groups. A Lie group is, roughly speaking, a group which looks locally like $\mathbf{R}^n$ for some n. A classic example is the rotation group $\mathbf{SO}_3$, which is locally like $\mathbf{R}^3$. Details of these applications can, of course, wait, perhaps until the third year methods course. However, a heuristic discussion, and a syllabus which recognized the importance of groups of this type, would both be welcome.

Linear algebra also provides its fair share of examples. The first year linear systems, and the second year methods and quantum mechanics courses are full of opportunities to apply this: diagonalization of symmetric tensors, classification of stationary points of functions of several variables, eigenfunctions of Sturm-Liouville operators, etc. These should also provide motivating examples for the relevant pure courses. In practise, the pure lecturers rarely mention them. The fact that commuting diagonalizable operators can be simultaneously diagonalized, which is very important in quantum mechanics, was relegated to the exercises. Raising and lowering operators were not discussed at all. The idea here, in case you are not familiar with it , is this : suppose you have an operator A, for which you know one eigenvector; Av=kv, say. Suppose you can find another operator B such that BA-AB=[B,A]=mB. Then ABv=(BA-mB)v=(k-m)Bv so Bv is another eigenvector. By induction, so is $B^n v$. This technique , which often yields a complete set of eigenvectors, can be useful in Sturm-Liouville problems. Also, the spaces considered are almost exclusively finite-dimensional. While topological considerations preclude any very deep consideration of function spaces at this level, they can still provide illuminating examples, and it is nice to at least look at the spaces occurring in quantum theory from a pure point of view.

Conversely, the applied courses consistently fail to use the available pure mathematics fully. The best examples of this come from the theory of tensors. Tensors are most naturally seen as linear or multilinear maps. The electrical conductivity tensor, for example, is simply a linear map from the space of electric fields to the space of currents. Feed in the field, and you get out the resulting current. From this point of view, the components and their transformation properties can be derived, rather than mysteriously postulated. Diagonalisation can be treated using the methods developed in algebra III. In fact, any idea of an abstract and co-ordinate free approach to linear algebra seems to be steadfastly rejected. Some people I have spoken to seem frightened of this, but really it is no different from the step from (x,y,z) to $\underline{\mathbf{x}}$, and offers much the same advantages.

Another area in which pure mathematics can contribute significantly is in explaining our models for the structure of the physical world. The naive view says something like "space is $\mathbf{R}^3$" , but this really is not good enough. It takes no account of time, or of the choices of origin, axes and units required to set up this correspondence. This makes it difficult to discuss the requirement that physical laws be independent of these choices. We need a mathematical structure which represents the underlying physical reality without these arbitary factors. This is quite complicated to set up - it requires the ideas of affine spaces, oriented vector spaces and tensor products, and a little careful thought. It is incidentally, much easier for special relativity than for the Newtonian theory - in the last analysis, Einstein's ideas are logically far simpler. I found this analysis very illuminating, and for me at least it is the only way to really understand the ideas of Lorenz invariance etc. There is not really space to describe it all here, but for some steps in the right direction, see Arnold, "Mathematical Methods in Classical Mechanics". General relativity requires different mathematical tools - those of differential geometry . A very good, but very heavy, discussion of the mathematics and its relation to the physics is given in Hawking and Ellis, "The Large Scale Structure of Spacetime" .

I hope that this has given people something to think about. I would be very interested to hear any feedback .

A Song of Six Splatts

Mark Owen and Matthew Richards

"The proteiform graph itself is a polyhedron of scripture."
- James Joyce, "Finnegans Wake"

Many readers will no doubt have encountered Piet Hein's famous "Soma Cube", a puzzle consisting of the seven "irregular" shapes which can be formed by combining up to four identical cubes, and from which a variety of structures, including a larger cube, can be made [1]. This puzzle relies on the intuitively obvious fact that identical cubes will tesselate to fill three-dimensional space. The cube is, however, by no means the only polyhedron with this property: [2] lists the other "symmetrical" space-filling solids as the rhombic dodecahedron, the truncated octahedron and the tetrahedron with bevelled vertices, and this list is widely believed to be complete [3].

It is the truncated octahedron and puzzles derived from its space filling property which we shall be considering in this article. For reasons too involved to go into here, we shall hereinafter term it the "splatt". The splatt can be obtained from a regular octahedron by cutting off its vertices at the points of trisection of its edges. Thus it has eight hexagonal and six square faces, all with the same side length (see Figure 1). Around each hexagonal face, squares and hexagons alternate. Since each of its 24 vertices has the same appearance, with one square and two hexagonal faces meeting there, it is called, appropriately enough, an "Archimedean" solid. The geometric properties of this shape are more fully discussed in [4], which also gives a net.

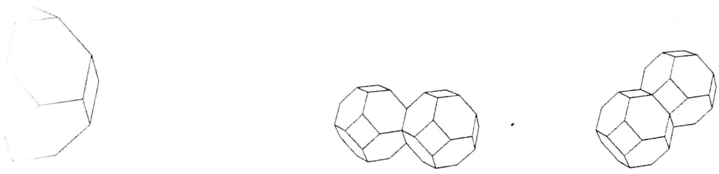


Figure 1. The truncated octahedron or "splatt", and the two 2-splatts

The splatt packs in a body-centred cubic lattice, which chemists will recognise as the crystal structure of caesium chloride. By considering the lattice as two interleaved cubic lattices, it may be seen that the volume of the splatt is exactly half that of the circumscribed cube whose faces include the square faces of the splatt. Each splatt in the packing has the same orientation, and so whenever adjacent splatts share a hexagonal face, there is a square face of one of them adjacent to each side of the common hexagon.

When n identical splatts are joined together by faces in such a way that they could form part of the space-filling packing, we call the resulting shape an n-splatt. Two n-splatts are to be considered equivalent if there is a rotation of 3-space which maps one onto the other. Trivially there is a unique 1-splatt; a little thought reveals that there are two distinct 2-splatts, one consisting of two splatts joined by square faces, the other of two splatts joined by hexagonal faces (see Figure 1). The essential uniqueness of the latter type of 2-splatt follows from the remark about hexagonal joins made above.

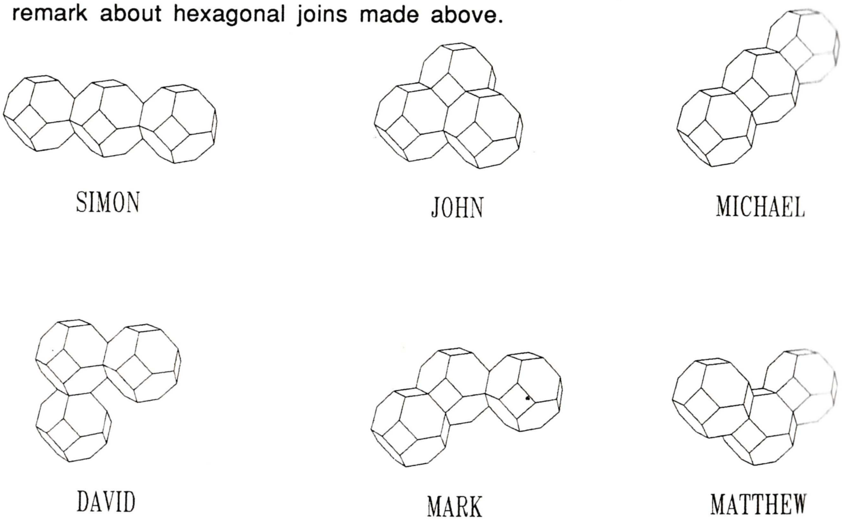
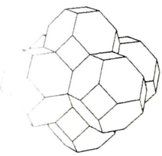


Figure 2. The six 3-splatts.

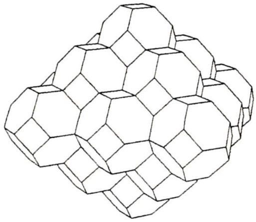
The reader may like to verify that adding another splatt can result in any of the six possibilities shown in Figure 2. This result contrasts with the corresponding result for combinations of three identical cubes, of which there are only two types. The first set of 3-splatts of which the authors are aware was constructed from apple pie cartons on Sunday 11th May 1986 at a meeting of the Puzzles and Games Ring. The 3-splatts were subsequently named in honour of the six people present on that occasion. When more than three splatts are joined, the phenomenon of chirality or handedness arises: there exist 4-splatts which cannot be rotated in 3-space to become their own mirror image. The authors believe there to be 44 4-splatts, 394 5-splatts and 4680 6-splatts, including both of each mirror-image pair, but do not yet know how many 7-splatts there are.

It is clear that if a puzzle akin to the Soma Cube were to be structed from some set of n-splatts, the 4-, 5- or 6-splatts would need an unwieldy number of pieces, whereas the 1- or 2-splatts would not sustain interest for long. The best compromise between triviality and overcomplexity is achieved by the set of 3-splatts; moreover they do not suffer from the disadvantage of having distinct mirror image forms. It transpires that these afford the splattist ample opportunity to exercise his creative talents, for they give rise to a plethora of fascinating puzzles.

Readers are urged to construct their own sets of 3-splatts, for a well made set will give hours of enjoyment. They may readily be made from cardboard: it is best to make the eighteen truncated octahedra individually and then to glue them together into the six pieces. A slightly more durable set may be fabricated from expanded polystyrene by cutting down the circumcubes with a hot wire. The authors have also tried using fibreglass with appropriate moulds.



DIAMOND



DOUBLE DIAMOND

Figure 3. Two elementary 3-splatt puzzles.

An elementary puzzle for the dilettante splattist is to construct the diamond shape shown in Figure 3 from two of the 3-splatts. The resulting shape is an example of a 6-splatt with the interesting property that, if a vertex is marked for each constituent splatt, and a line is drawn between vertices corresponding to splatts which are joined at a face, a non-planar graph is formed. It is the only 6-splatt with this property, and no 5-splatts possess it. Once you have mastered that shape, try to use all the pieces to make the larger version pictured in Figure 3. Note that there is a central cavity in the shape of a 1-splatt. This is a relatively easy 3-splatt puzzle: it has 24 essentially different solutions.

Further puzzles involving the complete set of 3-splatts are shown in Figures 4 and 5. All the shapes shown have symmetry, except "giraffe", where the tail makes the figure asymmetric. There are no unexpected hidden cavities or projections, except in "ziggurat" (for which we are grateful to Philip Belben), which has a single splatt missing from the middle of its base. Thanks are also due to Ian Stark for "drum" and "triangles", and to Simon James for "tortoise". It is a surprising fact that "bridge" can actually be made to support itself in the middle!

To avoid hours of fruitless effort, we feel obliged to remark that "tower" is impossible. We will give the proof here, as it is instructive and can be applied with success to other shapes which the reader may devise but be unable to construct. Consider a colouring of the packing in two colours where splatts joined by squares are of the same colour, but those joined by hexagons are of opposite colours. The two colours can be identified with the two types of ion in the structure of caesium chloride mentioned above. With this colouring, the central vertical column of three splatts within the tower will be of one colour, and the other fifteen splatts will be of the other. So, if this structure were to be made from the six 3-splatts, at least three of them would need to be monochromatic, i.e. to have joins only along square faces. Of the six 3-splatts, however, only two, namely Simon and David, have this property. Thus the figure is impossible. Similarly any other structures with such a high "net charge" will be impossible to construct.

These are just a few examples of the large number of stunningly realistic shapes that can be formed with the set. Doubtless you will be able to find many more. If you come across any of especial note, we would be very interested to see them. We conclude with a problem: find the cuboid of least volume into which the set of 3-splatts may be packed.

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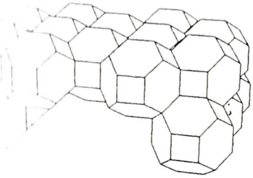
1. Martin Gardner, "More Mathematical Puzzles and Diversions", Penguin (1963) pp50-9

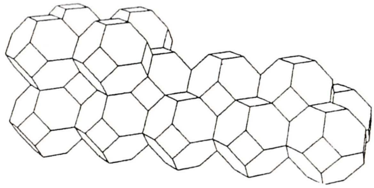
2. J. E. Drummond, "Space filling with identical symmetrical solids", Math. Gaz. 68 (1984) pp104-6

3. Branko Gruenbaum and G. C. Shephard, "Space filling with identical symmetrical solids", Math. Gaz. 69 (1985) pp117-20

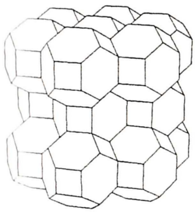
4. H. M. Cundy and A. P. Rollett, "Mathematical models", Oxford (1974) p104



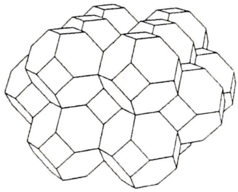
BRIDGE



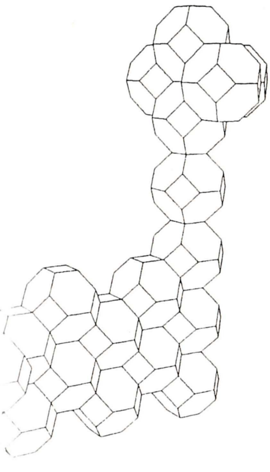
DRAGSTER



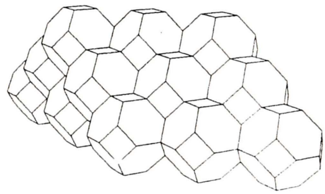
DRUM



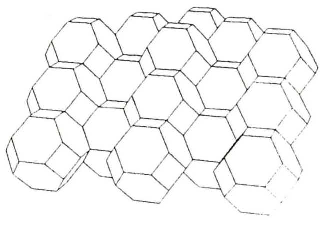
EGGBOXES



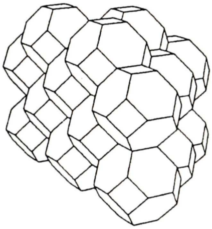
GIRAFFE



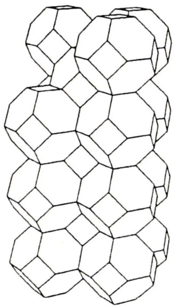
LOZENGE



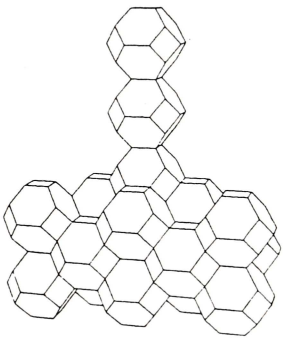
NEW COURT CHRIST'S



PRISM



TOWER



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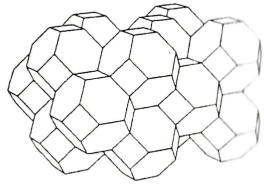
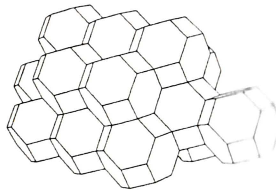
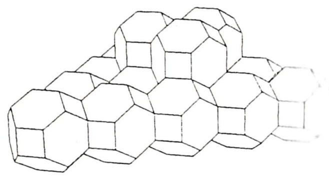


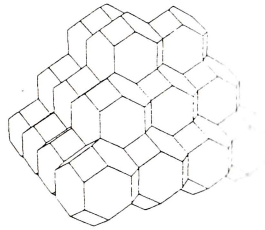
SPINODE



TORTOISE



TRIANGLES



ZIGGURAT