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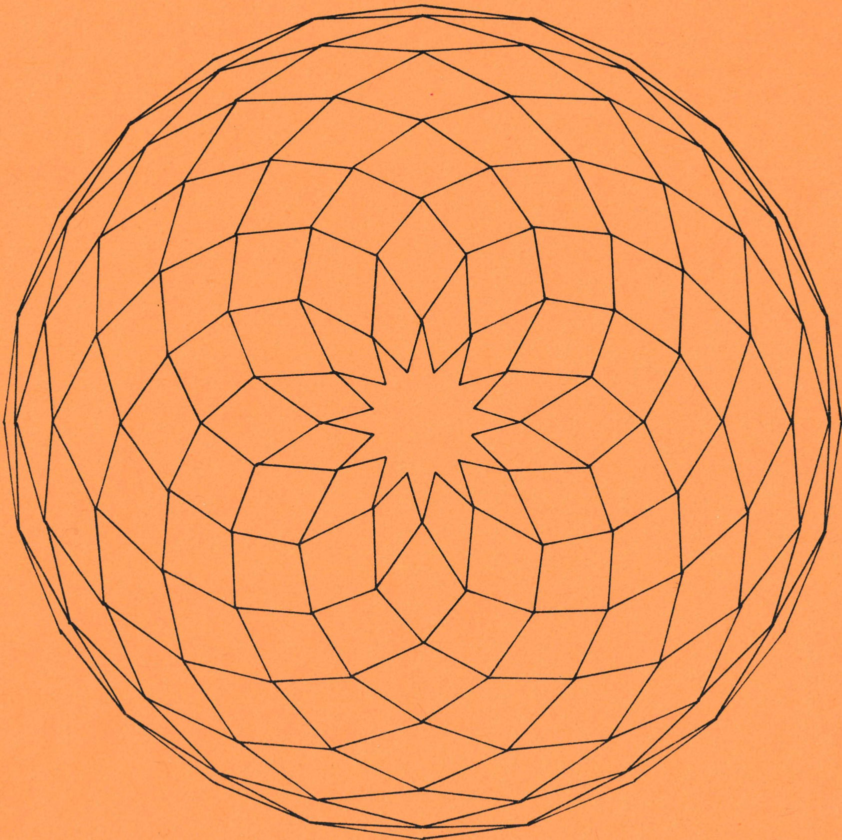
EUREKA



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Editorial

EUREKA apologies for its nonappearance last year. The new editors hope that this edition has been worth the wait. In this issue there are articles which are intended to give the interested undergraduate a whiff of what has been going on at the research front. We have one regret though: we received almost no articles from undergraduates. Surely some of you have done or discovered something (mathematically) interesting ? Please let us know - we are available all year round at St.Johns.

The Archimedeans 1980/1

Unfortunately, last year's publication hiatus means that the programmes of Paul Verschueren and Mark King were born to blush unseen and waste their sweetness upon an air destered on "Eureka". However, this year, under the Presidency of David Wood, the Society has seen considerable growth - in Eureka, QARCH, recruiting and elsewhere. Social events included the usual punt-joust (which was ordained by our forefathers to drown the piety of the Committee), and also the innovation of a Mathematicians' Balloon Debate. This brought the first visit of the Invariants for 2½ years, but once again they chickened out of the Problems Drive.

Our evening programme began with Stuart Parsonson (of sixth-form texbook fame) and included Roger Penrose, Clive Kilmister, Philip Higgins and Frank Watson. Professor Fröhlich came almost at the drop of a hat. Unfortunately the most provoking lectures were given to the smallest audiences. The lunchtime programme - for some years now established in Trinity Lecture-Room Theatre - was beset by cancellations due to illness. It was intended to go outside the closed circle of our own lecturers (but included Dr. Maxwell and past presidents Peter Landshoff and Arthur Norman) to touch on local industry and education, and bring in sabbatical visitors: this year Walter Brady and Frank Harary.

The structure of the Society has been shaken up: expanding departments caused an explosion in the size of the Committee, and it was felt that a serious review (the first for twelve years) was required. Discussion and negotiations probing the very foundations of the Society led to the adoption of a new Constitution at a 4½-hour AGM (the first three hours of which were twice quorate). Aside from rationalising the rules of membership (including a guarantee of three copies of Eureka to Life Members), The most dramatic provision is that the Committee (like a supernova) has shed an expanding advisory shell, consisting of agents and (ex officio) members of the Faculty Board and past officers, and now possesses an executive core of officers, who have lost their power to co-opt. Hopefully this will make the Society run more smoothly in future.

The programmes of the College Societies have had their usual stars, but are suffering badly from poor publicity. There has been diversity into history of maths, economics and astronomy, and in particular the TMS (who once again lost their cricket match with the Adams Society) expanded their programme by inviting several outside speakers.

Priorities this year are publicity, social events, general decorum and the efficient progress of the Society.

Trinity College

Paul Taylor
President

A Sense of History

It is commonly believed (e.g. Croft, H.T., "Eureka" 27 (1964) that Archimedes perished in the sack of Syracuse by Marcellus in -211. In fact recent evidence (Greenlees, J.P.C., "Archmidedeans Minutes Books" (1.11.80)) shows that in fact he escaped to Alexandria in a hot-air balloon, and thence to a place in the fens of Brittannica called Grantabrigium, where he founded a secret order which was opened in the late thirties. Upon this creed is based the Doctrine of the Direct Apostolic Succession of the Presidents of the Archimedeans. We are now looking for ideas for marking the 22nd centenary in 1989, and would also welcome any information leading to the return of our old minutes books, some of which were once left in the Statistics Library.

Nearer to the present day, our next Triennial Dinner will be held in February 1982; the details have yet to be decided, but it would be nice to have a few past members present. Also, if there is enough support from past and present members a Society photograph might be taken on that day. Anyone interested should write to me for details.

St. John's College

Chris Budd
Vice President

The Mathematical Association

Going down from Cambridge into my first teaching job was a major shock to the system. The other mathematics teachers were not my generation and had little to offer by way of stimulating mathematical conversation - or even the advice I desperately needed as a novice teacher. The department could be described as 'static' rather than 'dynamic'.

Fortunately as a schoolboy I had come across the Mathematical Gazette, where I had found interesting articles on mathematics that I could actually read for myself. Then, browsing in Foyles, I came across the mathematical Association's reports on the teaching of geometry .. arithmetic .. algebra .. - just what I needed! So I wrote off and joined. This gave me a contact with the meetings of the London Branch (there are some 20 branches around the country), where the talks varied from the down-to-earth (how do you teach ratio?) to the amusing (mathematics in the fairground) and the reudite (non-linear differential equations), and where I could talk to teachers from other schools. I started to go to the annual conferences, at which I heard, and even met, some of the "household names" in mathematics and teaching - and also (just as important) could keep abreast of new books and equipment. Then it snowballed; I began to get put on the groups writing the Association's reports, and ultimately found myself editing the Gazette - which had sparked off my interest in the first place.

For anyone going into teaching at school or college level, joining the Mathematical Association is a "must". It is a way not only of keeping in touch with developments in both mathematics and its teaching, but also of getting involved in these for oneself. The Association also has a good sprinkling of members in universities and a few in industry and commerce. You can get all the particulars by writing to Mr. Jim Gray (the Executive Secretary), the Mathematical Association, 259 London Road, Leicester LE2 3BE; there is a special low subscription rate for members so long as they are under 25 or students. If you want any more information, contact me at the Institute of Education, Shaftesbury Road, Cambridge (Tel. 69631).

Douglas Quadling

Problems Drive 1980

Questions

1. Find all real solutions of:

   (i) $\sin(60^\circ+x) - \sin(60^\circ-x) = \sin x$;

   (ii) $\cos^2 x - \frac{3}{4} = \cos(x+30^\circ)\cos(x-30^\circ)$;

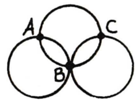
   (iii) $1 = \cos 4x - \tan 43x \sin 4x$.

2. Prove that if a,b,c are integers such that a+b+c is even then there is an integer n such that

   $ab+n$, $ac+n$, $bc+n$ are all perfect squares.

   Be brief!

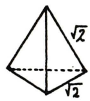
3. Three towns A,B,C are connected by seven paths as shown in the diagram.

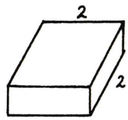


   A group of Archimedeans starting at A decides to go on a ramble round these paths in such a way that they travel along each of the seven paths exactly once.

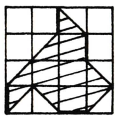
   In how many ways can this be done?

4. How many regular tetrahedra with sides of length $\sqrt{2}$ can be fitted into a box with inside dimensions of $1\times2\times2$?





5. Dissect this shape into three congruent (allowing reflections) pieces:



6. What is the largest real number x for which there exist four different numbers in the interval [0,1] (i.e. $0\le x_i\le 1$) such that for any three of them which are not all equal

   $$|x_0+x_1-2x_2| \ge x \quad .$$

7. The following conversation was overheard at a maths. lecture where all mathematicians were either Pure or Applied:

   A: I am Pure and so is C .

   B: C is not Pure.

   C: B is Pure or A is Applied.

   Given that Pure mathematicians always tell the truth and Applied mathematicians always lie, what were A,B, and C; or is it impossible to tell? All three are mathematicians.

8. This 4×4 grid used to have a letter written in each square. The 8 "words" spelt across and down were:

SATT SSAT STAS STSA

ATST ATAS TAST TATA .

What "word was spelt down the diagonal from top left to bottom right?

9. If α is a root of the cubic equation:

$$x^3 - x^2 + x + 1 = 0 ,$$

then of what cubic equation with integer coefficients is α^2 a root?

10. What is the next number in each of these sequences?

(i) 134, 93, 41, 11, 8, 3 ;

(ii) 1, 3, 25, 253, 3121 ;

(iii) 00000, 30103, 47712, 60206, 69897 ;

(iv) 2, 6, 20, 70, 252 .

11. Give an example of a cubic polynomial $P(x) = ax^3+bx^2+cx+d$ $(a \neq 0)$ with integer coefficients such that $P(0)$, $P(1)$, $P(2)$, $P(3)$ and $P(4)$ are all perfect squares of integers.

12. Solve in integers x & y, $(x+y)^2-(x+2)^2-(y+1)^2 = 1$.

Is The Sun A Clock?

By D O Gough

In 1843 a German amateur astronomer, Heinrich Schwabe, pointed out that the number of dark spots on the sun varies in an apparently regular way, with a period of about 10 years. This triggered a concerted effort amongst professional astronomers to establish whether the claim was true. It was led by Rudolf Wolf, of the Zürich Observatory, who assembled historical evidence and instituted an international programme of controlled sunspot observations. He concluded in 1856 that a cycle had indeed been present at least since the beginning of the eighteenth century, and that the period was 11 years. Subsequent historical investigations have revealed that prior to 1640 recorded observations were too sparsely scattered to be of statistical value, and for the seventy years after that date there were hardly any sunspots at all. This epoch of an almost unblemished sun, which is now known as the Maunder minimum, more or less coincided with the reign of Louis XIV of France, who was named, for apparently unrelated reasons: le Roi Soleil.

Wolf's programme of observations has continued, and we now know that the sunspots result from strong magnetic fields that emerge through the surface of the sun. We know also that the cycle is not strictly periodic, the distribution of the intervals between successive sunspot maxima being approximately Gaussian with a standard deviation of 1.8 years.

Naturally there has been considerable interest in trying to understand the sunspot cycle. At first this was simply for its own sake, which of course is ample justification for any academic, but very recently it has been realised that sunspots modify the light output from the sun to a degree that may be sufficient to influence the terrestrial climate, and the problem has taken on a new order of importance. There are two types of theory for the cycle, and the issue to which I address myself here is whether one can use the observations to choose between them. But before doing so I should point out an important aspect of the structure of the sun which is essential for the appreciation of the theories: the sun is a sphere of fluid which in its outer half by volume is in a state of turbulent agitation, whereas in the inner half it is relatively quiescent. The density of the sun increases quite substantially with depth, and only about 3 per cent of the mass is contained in the outer turbulent region.

Theories of the first type are based on the hypothesis that the entire sun partakes in some kind of torsional oscillation. The solar interior is electrically highly conducting, which means that any magnetic field is essentially tied to the fluid. As the sun twists nonuniformly inside, it stretches the magnetic field which provides a restoring force to reverse the direction of the motion. We do not know the strength of the magnetic field inside the sun, but if one assumes that beneath the turbulent zone it is everywhere about the same as in the sunspots the oscillation period turns out to be about 22 years*. This is the principal observational justification for these theories.

Theories of the second type presume that the important dynamics is essentially confined to the outer turbulent region. The sun is said to possess a turbulent dynamo. The details of how the oscillation is produced are too complicated to discuss here, and depend on subtle properties of the fluid motion. There is no argument as simple as that given above to predict the oscillation period, and the theories have to be tuned to give the correct result. Nevertheless most solar physicists believe that these theories provide the better approximation to the truth, merely because they can apparently explain many other aspects of the solar cycle that have not been explained by the first type of theory. In practice this is a common reason for believing scientific theories, and one must be wary of the fallacious argument upon which such belief is based. The number of predictions of a theory is a dangerous measure of its reliability; for the more widely a theory is believed, the more thoroughly it is investigated, and consequently the number of its predictions is almost bound to be greater. It is only where two theories make contradictory predictions that there can be some hope of choosing between them.

There is a fundamental difference between the theories of the solar cycle that might lead to a crucial test. Theories of the first type presume that the solar interior undergoes a periodic oscillation. Sunspots are produced after stretched magnetic field has somehow risen through the outer turbulent region to the solar surface. The turbulence imparts a randomness on the rise time which explains why the sunspot cycle is not periodic. Nevertheless the sunspot

* The solar magnetic cycle is an oscillation with period 22 years in which the polarity of the field reverses. Sunspots are a rectified signal, and so vary with half the period.

cycle is closely associated with the interior clock, and variations in its phase are simply the variations in the rise time of the field, which can be measured by the fluctuations in the intervals between successive sunspot maxima or minima. The inertia of the outer region is so low that the turbulence has only a very slight influence on the dynamics of the interior, and the clock keeps almost perfect time. On the other hand, dynamo theories rely on the turbulence itself to produce the oscillation, and it is impossible for phase to be maintained indefinitely. Each oscillation is produced by a process with a characteristic timescale which on average is 11 years, but which suffers random variations causing the sun to lose its memory.

With a sufficiently long data record it would be a simple matter to distinguish between the two classes of behaviour. But with only the 25 cycles that have been recorded since the beginning of the eighteenth century there is not enough information to draw an unambiguous conclusion. The issue must be left to judgement, and this I leave to the reader.

For a model of the clock, suppose that the time of the occurrence of the n^{th} sunspot maximum (or minimum) is $t_n = nT + \tau_n$, where T is constant and the τ_n are independent random variables with zero mean and standard deviation τ. The period of the n^{th} cycle is $P_n = T + \tau_n - \tau_{n-1}$, and the mean period of N successive cycles in $\overline{P}_N = T + N^{-1}(\tau_N - \tau_0)$. For a model of the dynamo, assume that the interval between two successive cycles is $P_n = \Psi + \psi_n$, where Ψ is constant and the ψ_n are also random variables with zero mean, and with standard deviation ψ. This is really an extreme representation of the turbulent dynamo, because it assumes that the sun has no memory of the previous cycles at all. In this case

$$t_n = n\Psi + \sum_{m=1}^{n} \psi_m, \quad \overline{P}_N = \Psi + N^{-1} \sum_{m=1}^{N} \psi_m .$$

It is the object of the investigation to compute a measure of the phase deviation of the cycle from a perfect clock. Of course we do not know which clock to choose, but for simplicity let us choose the clock that ticks at times $T_n = n\overline{P}_N + \varepsilon$, where ε is constant. (More sophisticated statistical estimates of the best clock to choose yield similar results.) The variance σ_Φ^2 of the phase difference $\Phi_n = t_n - T_n$ can thus be defined by

$$\sigma_\Phi^2 = (N+1)^{-1} \sum_{n=0}^{N} \Phi_n^2 - \left[(N+1)^{-1} \sum_{n=0}^{N} \Phi_n\right]^2 ,$$

and the variance of the cycle periods is

$$\sigma_P^2 = N^{-1} \sum_{n=1}^{N} (P_n - \bar{P}_N)^2 .$$

Both these quantities can be evaluated from the sunspot record, using the actual times of either sunspot maxima or sunspot minima for t_n, and $\bar{P}_N = N^{-1}(t_N - t_0)$. Moreover, it is quite straightforward to calculate the expectations of σ_Φ^2 and σ_P^2 for the two models. The results are proportional to τ^2 or ψ^2 and are independent of ε. Their ratios are

$$R^2 \equiv \frac{E(\sigma_\Phi^2)}{E(\sigma_P^2)} = \frac{N(5N-1)}{6(N+1)^2} \quad \text{and} \quad \frac{N}{6}$$

for the clock and dynamo models, irrespective of the probability distributions of τ_n and ψ_n. Notice that as $N \to \infty$ the measure R of the phase wandering is bounded for the clock model but increases as $N^{\frac{1}{2}}$ for the dynamo model, as one would expect.

In Figure 1 is plotted the two theoretical ratios R^2 as a function of N, together with values from the sunspot record. Twenty four consecutive cycles were divided into q contiguous groups to yield values of σ_Φ^2/σ_P^2 for $N = 24q^{-1}$. The crosses represent the averages of the q values for sunspot maxima, and the circles represent sunspot minima; the vertical lines indicate the standard deve-ations amongst the q values. Neither model fits the data perfectly and one is tempted to conjecture that the real sun lies somewhere between the two, having a memory of finite duration. If that is the case, how long is the memory?

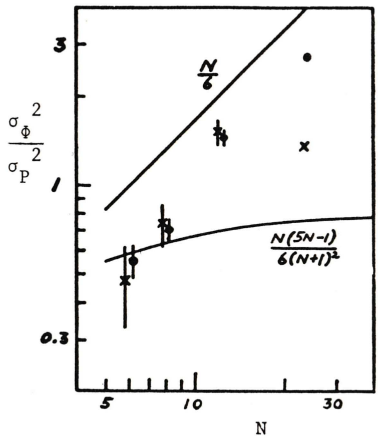


Figure 1

A step towards answering that question has been made recently by Jim Barnes, Chief of the time-keeping division of the US National Bureau of Standards. He studied a computer simulation of a rectified 22 year oscillator that is randomly perturbed, adjusting the frequency response to the perturbation until it produced fluctuations in the period with the same variance as that of the sunspot data. In Figure 2 is shown the actual sunspot record since 1650 and a portion of the numerical simulation. Evidently, the portion of the randomly driven computer model was not chosen randomly, but it does mimic the data remarkably well. The variance in the peak heights after 1700 is essentially the same as it is for the sun, and there is even a Maunder minimum. According to Barnes and his collaborators (1980), the model has a bandwidth of (500 years)$^{-1}$, and an engineering rule-of-thumb is that several inverse bandwidths of data are required to establish whether or not phase in maintained. The model also predicts that intervals of about 50 years of continuous low sunspot activity occur on average once every 500 years.

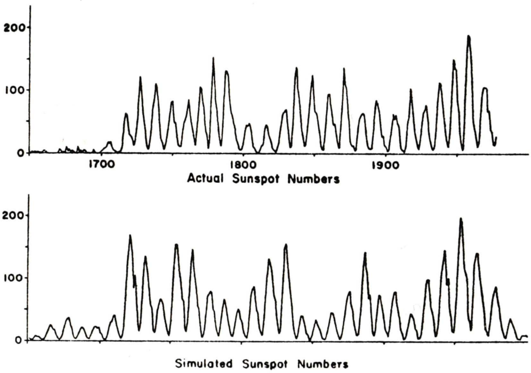


Fig. 2. Actual and simulated annual, mean sunspot numbers

The physical correspondence between this model and the actual sun has not been made clear. The model is merely a mathematical invention, with no obvious physical content implied by its inventor, and for this reason many solar physicists have considered it to have no value. This is an error. Its value is that it demonstrates that to differentiate with sunspot statistics between the artificial model and any physical theory that has demonstrably no relation to it would require a data base considerably longer than the one we have at present.

References

Barnes, J.A., Sargent III, H.H. and Tryon, P.V., 1980, <u>Geochim. Cosmochim. Acta.</u>, Suppl., <u>13</u>, pp 159-163.

Schwabe, S.H., 1843, <u>Astron. Nachrichten</u>, <u>20</u>, 283.

Wolf, R., 1856, <u>Astron. Mitt. Zürich</u>, <u>1</u>, 8.

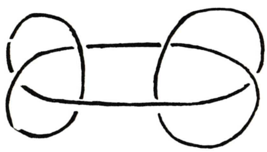
Summer Fun

By G N Old

Picture the scene. An idyllic summer's day. The smell of honeysuckle. The bees droning lazily in the background. The gardener, his stout, tanned forearms showing below his rolled-up sleeves, is about to water the garden. Suddenly he notices, to his considerable annoyance, a knot, near the tap end of the hose-pipe. Being a fellow of some considerable practicality he thinks - Is there any knot I can tie in <u>my</u> end of the hosepipe, which, when I shake it down to the tap end, will leave the whole thing unknotted?

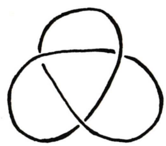
Human intuition seems divided on this point. Some think you obviously can't, some that you obviously can. Some don't know, but think a fried of theirs did it once. It turns out this is a relatively easy question in knot theory. Avid readers of Eureka [2] will know a knot is a simple closed (differentiable) curve in 3-dimensional space $\mathbb{R}^3$, both being oriented. In other words a knot is what you think it is, except that the string has no ends. Two knots are considered the same, if you can 'shake' one into the other. The question above concerns the question of composing two knots - composition being defined by first tying one knot in the string, then the other.

e.g. <u>Reef knot</u>

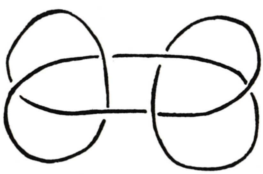


A+B

A=

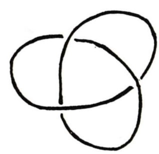


<u>Granny knot</u>

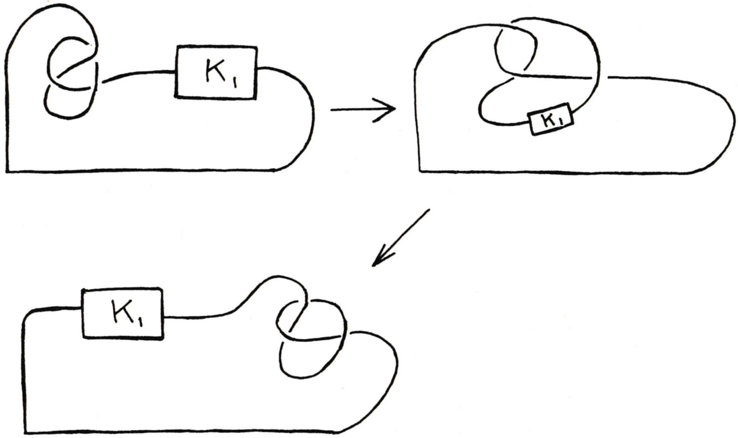


A+A

B=

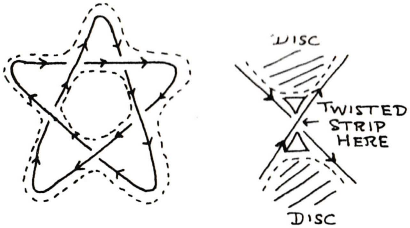


It is obvious that this composition is commutative (hence the use of +) if you think of one of the knots being made very small and slipping it through the other one.



This operation '+' thus makes knots into a commutative semigroup, with identity (the so-called 'unknot' made by a standard loop). It turns out, we can have an 'obvious' notion of 'prime knot' (i.e. one which does not admit a decomposition $p = A+B$ unless A or B is the unknot] and we have a unique prime factorisation theorem. The hosepipe question amounts to asking: 'Do any elements have inverses in this group?'

We can answer this question by introducing a 'Seifert surface' for a knot, which is defined to be an orientable (i.e. 2-sided) surface, whose boundary is the knot. It turns out every knot has a Seifert surface. An easy way to see this is to put an arrow on the string of the knot, then draw 'Siefert circles' which are obtained by starting at a point on the knot and following it round, until you reach a crossing, which you 'short circuit', according to the direction of the arrow.



Attach discs, one for each Seifert circle and connect the discs by twisted strips at each crossover point

(The above example is easy to visualise, but sometimes the circles are nested, and you will have to think of the discs as stacked on top of each other). For a full(er) exposition see R.H.Fox [1].

We define the genus of a knot $g(k)$ to be the smallest genus of a Seifert surface for the knot. Then a knot has genus 0 <=> it is the unknot.

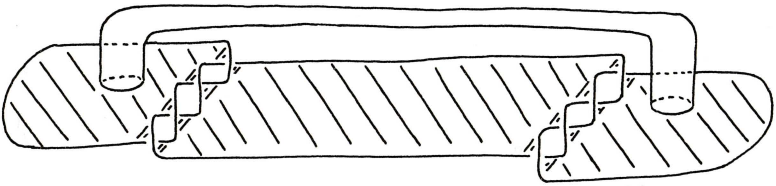
Theorem. $g(k_1+k_2) = g(k_1) + g(k_2)$.

This answers the gardener's question, since k_1+k_2 is not knotted iff $g(k_1+k_2) = 0$; iff $g(k_1) = g(k_2) = 0$ i.e. iff k_1 and k_2 are both unknotted. So no nontrivial knot has an inverse.

We can sketch a proof of the Theorem as follows. By 'adding' the surfaces for k_1 and k_2 we see

$$g(k_1+k_2) \le g(k_1)+g(k_2) .$$

Conversely, take a surface for k_1+k_2 , T say. The trouble is, we could have chosen a silly surface for k_1+k_2 ; e.g. we could have an extraneous tube from one side to the other, and this is a Seifert surface for the knot shown.



However, it turns out that this is the worst that can happen and by 'cutting' these tubes (which decreases the genus of the Seifert surface) we get a Seifert surface which is the sum of the Seifert surfaces for k_1 and k_2 respectively.

We get $g(k_1+k_2) \ge g(k_1)+g(k_2)$, which proves the theorem.

[1] R.H.Fox A Quick Trip thro'knot theory: Topology of 3-Manifolds & Related Topics. Ed. M.K. Foot.
[2] W.B.R.Lickorish. Eureka 40 1979.
[For definition of genus see, for example, Maunder on Alg.Topology.]

Central Force O-bits Under Different Power Laws

By A Tan

The subject of planetary motion has attracted astronomers, mathematicians and physicists for centuries. It is well known that central force orbits under the inverse square law of gravitation are conic sections. The usual procedure to show this is to eliminate the time between the equations of motion and solving the resulting differential equation. The procedure of integration is often complicated when the attractive force is not inverse square in nature. An alternative method of study is to prescribe the orbit and to determine the force law. For example, MacMillan [1] has shown that for a circular orbit with the attracting centre on the circumference, the force law varies as the inverse fifth power of the distance from the attracting centre. Other orbits are also given by MacMillan [1] as exercises.

In this article, we intend to find out the force laws for the general curve in plane polar coordinates (r,θ)

$$r^n = a^n \cos n\theta, \tag{1}$$

where n is a rational number. Following the method of MacMillan [1], we can show that the force law for this general curve is

$$f(r) \propto \frac{1}{r^m}, \tag{2}$$

with

$$m = 2n + 3. \tag{3}$$

Equation (1) represents a family of curves, some of which are listed in Table I. The corresponding inverse power of the variation of the force is also given in the table, together with examples of such forces in nature. The orbital curves are plotted in Figure 1. The numbers correspond to the inverse power of the radial distance.

The curve with $n = -2$ is a rectangular hyperbola whose asymptotes are the lines with slopes ± 1. The curve with $n = -\frac{3}{2}$ is also a hyperbola but the asymptotes are lines making angles of $\pm \frac{\pi}{3}$ with the x-axis. It corresponds to the attraction of an infinite surface distribution of matter (or charge). $n = 1$ is the case of an infinite line distribution. In Fig. 1, this line passes through the origin and is perpendicular to the plane of the paper. $n = -\frac{1}{2}$ is an example of the inverse square law, to which we shall return later. The case $n = 0$ belongs to the spirals, which have been dealt with at length by Lamb [2]. The cases $n = 1$ and $n = 2$ can be found in MacMillan [1] and also in Lamb [2]. The latter case has a physical example in the weak Vander Waals' forces between two neutral atoms or molecules (cf. Born [3]).

The cardioid presents a case for which the force varies as the inverse fourth power of the distance. It may be recalled that, in Einstein's General Theory of Relativity, the gravitational field may be thought to be inverse square plus a small inverse fourth power term. The inverse square term, of course, gives the Newtonian orbit, while the inverse fourth power term is responsible for the perihelic precession (cf. Eddington [4]). Geometrically, we might think of the perihelic precession as the result of the perturbation of a weak cardioid on the dominant elliptic orbit.

The list of curves is, by no means, exhaustive. There are curves higher up and lower down in the ladder and also in between the entries. But a few interesting observations can be made from Table I and Figure 1. It is easy to see that curves higher than the parabolic pass through the attracting centre. Hence, the formation of the family of planets and satellites is not possible, in such worlds, only collisions are. The curves on the other side of the scale are hyperbolic in nature, with similar consequences (only scattering is possible, in such worlds).

Even in the familiar world of the inverse square law of force, all trajectories are not stable orbits. The parabolic and hyperbolic trajectories, for example, are unbounded and represent scattering. Only elliptical orbits and the limiting case of the circular orbit can constitute stable planetary orbits. The case of the inverse square comprises a broader category of curves, except for the case of the inverse cube law (cf. Lamb [2]). For now, the entire family of conics can be candidates for legitimate trajectories. We can show this by taking in Equation (1), the polar equation of a conic

$$r(1 + e\cos\theta) = 2a. \tag{4}$$

Table I

Inverse Power of r (m)	Order of the Curve (n)	The Curve	Example in Nature
-1	-2	Hyperbola	
0	$-\frac{3}{2}$	Hyperbola	Infinite Surface Distribution
1	-1	Straight Line	Infinite Line Distribution
2	$-\frac{1}{2}$	Parabola	Point Distribution
3	0	Spirals	Electric Dipole
4	$\frac{1}{2}$	Cardioid	Electric Quad-rupole
5	1	Circle	Electric Octupole
7	2	Lemniscate	Van der Waal's forces

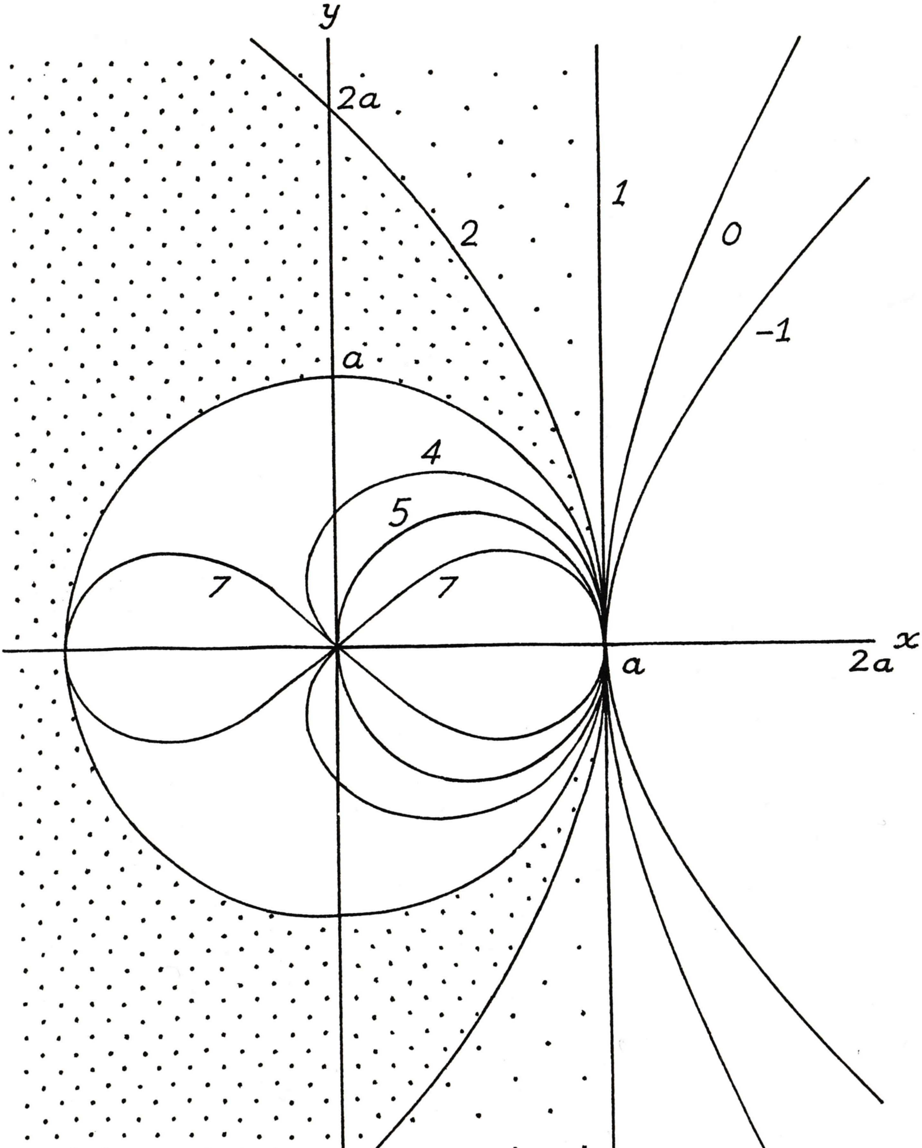


Fig 1

Here e is the eccentricity of the conic. The dotted areas of Fig. 1 show the domain of the inverse square force trajectories. The lightly dotted area is where hyperbolic trajectories ($e>1$) lie. The densely dotted area, on the other hand, is the domain of the elliptic orbits ($e<1$). It is here that the stable planetary orbits can be found. This includes the circle ($e = 0$) but excludes the parabola ($e = 1$).

Hence, in most cases, the formation of a solar system as we know, is not feasible. Life seems possible only in the inverse square case. And little wonder, mother nature chose the force law to vary as the inverse square of the distance.

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Joint Spectra

By G R Allan

The notion of an eigenvalue will be familiar to all readers of Eureka. Thus, if E is a complex, finite-dimensional vector space and if $T : E \to E$ is a linear mapping, then the complex number λ is an eigenvalue of T if $(\lambda - T)x = 0$, for some non-zero vector x in E. The eigenvalues of T are the roots of $\det(\lambda - T) = 0$; in particular, the fundamental theorem of algebra tells us that T has at least one eigenvalue (which was why we chose a <u>complex</u> vector space).

Also familiar, to some readers, will be the fact that, if we try to generalize to infinite-dimensional spaces, eigenvalues need no longer exist. This is so even if we restrict attention to continuous linear mappings on a separable Hilbert space. The easist example is to take E as ℓ_2 (all complex sequences $(z_n)_{n\geq 1}$ with $\Sigma|z_n|^2<\infty$) and the unilateral shift operator $S : \ell_2 \to \ell_2$, which takes $(z_1, z_2, z_3, \ldots)$ to $(0, z_1, z_2, \ldots)$. Another example is the indefinite integration operator $V : L^2[0,1] \to L^2[0,1]$ defined by $(Vf)(x) = \int_0^x f(t)dt$ $(f \in L^2[0,1]$, $0 \leq x \leq 1)$. (Don't worry about $L^2[0,1]$ if it is unfamiliar - just take <u>continuous</u> functions f on $[0,1]$; but that will not be a Hilbert space.) Unlike S, the operator V is a <u>compact</u> linear operator, making the example even more distressing. However, those readers familiar with the problem will, no doubt, also be aware of the remedy. Our first attempt at generalizing the notion of eigenvalue was too simple-minded. If E is any complex Banach space (this means a complex vector space in which a reasonable notion of length or <u>norm</u>, $\|x\|$, of elements x of E has been defined) and we write $L(E)$ for the algebra of all continuous linear mappings $T : E \to E$, we say that T is <u>non-singular</u> if there is S in $L(E)$ with $ST = TS = I$; in the contrary case T is <u>singular</u>. We then say that the <u>spectrum</u> $\sigma(T)$ is the set $\{\lambda \in \mathbb{C} : \lambda - T$ is singular$\}$. In the case of finite-dimensional E and $T \in L(E)$ then $\sigma(T)$ is still just the set of eigenvalues. In the general case, however, that is no longer true (essentially because for E infinite-dimensional, T may be injective without being surjective); indeed $\sigma(T)$ is always a non-empty, compact subset of $\mathbb{C}$. (For example, for the operators S, V given above, $\sigma(S)$ is the closed unit disc, while $\sigma(V) = \{0\}$. In general, given any compact non-empty $K \subset \mathbb{C}$, there is a Banach space E and some T in $L(E)$ with $\sigma(T) = K$) .

Reverting to the finite-dimensional case for a moment, there is a notion of joint (or simultaneous) eigenvalue for two commuting linear operators: thus for T_1, T_2 in $L(E)$ with $T_1T_2 = T_2T_1$, say that (λ_1, λ_2) in $\mathbb{C}^2$ is a joint eigenvalue of (T_1, T_2) if there is some non-zero x in E such that $(\lambda_1 - T_1)x = (\lambda_2 - T_2)x = 0$. (Exercise for reader: if $T \in L(E)$ and we put $T_1 = T$, $T_2 = T^2$ then the joint eigenvalues are just (λ, λ^2) as λ runs through the eigenvalues of T.) The appropriate generalization to joint spectrum (with E , in general, infinite-dimensional) of commuting continuous linear mappings is not so easy; the trouble is that there are several reasonable possibilities which give different answers in general.

One, rather sophisticated, approach, due to J.L.Taylor ([1]) involves the following idea. Given a complex Banach space E and commuting T_1, T_2 in $L(E)$, we define a sequence of linear mappings:

$$0 \to E \xrightarrow{\alpha} E \oplus E \xrightarrow{\beta} E \to 0 ,$$

where $\alpha(x) = (T_1x, T_2x)$ $(x \in E)$ and $\beta(x_1, x_2) = T_2x_1 - T_1x_2)$ $((x_1, x_2) \in E \oplus E)$. The fact that T_1, T_2 commute ensures that $\beta\alpha = 0$. The pair (T_1, T_2) is called non-singular if and only if the above sequence is exact (i.e. α is injective, β surjective and $\operatorname{im} \alpha = \ker \beta$) ; otherwise (T_1, T_2) is singular. The joint spectrum $\sigma(T_1, T_2)$ then consists of all ordered pairs (λ_1, λ_2) of complex numbers such that $(\lambda_1 - T_1, \lambda_2 - T_2)$ is singular.
We observe that the case of a single operator T could be put in corresponding form, using just the sequence

$$0 \to E \xrightarrow{T} E \to 0 .$$

The extension to n commuting operators $(n>2)$ is now more or less routine, given some familiarity with elementary homological algebra (the Koszul complex).

This definition, of joint spectrum for commuting operators, is a good one, although technically rather complicated to handle. Other notions, easier to understand, depend on choosing a particular closed commutative subalgebra of $L(E)$ containing $\{T_1, \ldots, T_n\}$ and defining $\sigma_A(T_1,\ldots,T_n)$ to be the set of n-tuples $(\lambda_1,\ldots,\lambda_n)$, of complex numbers, such that the set $\{\lambda_1-T_1,\ldots,\lambda_n-T_n\}$ generates a proper ideal of A. In the case $n = 1$, if A is any maximal commutative subalgebra of $L(E)$ containing T, then $\sigma_A(T) = \sigma(T)$. However, for $n \geq 2$, no such simple relationship persists and it seems that Taylor's definition of $\sigma(T_1,\ldots,T_n)$ is not equivalent to $\sigma_A(T_1,\ldots,T_n)$ for any choice of A.

One important property possessed by all these definitions of joint spectrum (it represents a good deal of the point of the definitions) is that, for any complex-analytic function $f(z_1,\ldots,z_n)$, defined on a neighbourhood of the joint spectrum in $\mathbb{C}^n$, there is a sensible definition of $f(T_1,\ldots,T_n)$ in $L(E)$ (via a lot of hard work); in particular the correspondence $f \mapsto f(T_1,\ldots,T_n)$ is a homomorphism. This process (known as 'analytic functional calculus') provides a means for using analytic function theory in the study of operators and of commutative Banach algebras. There have been some elegant and deep results in this area. Much remains to be done.

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Hydrodynamics With Random Geometry

By G K Batchelor

The dramatic growth of the oil-extraction industry in Britain during the past ten years has been accompanied by developments in several of the relevant areas of technology. One of them is concerned with the way in which oil may be made to flow through porous rock deep underground. The popular image of an oil well as a deep bore-hole dipping into a pool of pure oil which gushes up the well spontaneously is unfortunately an over-simplification. A petroleum reservoir is a geological system in which the oil is held in the minute crevices of a porous rock, such as sandstone, in a layer usually much wider than it is deep. The boundaries of the reservoir are layers of impermeable rock, and there may be neighbouring regions of porous rock in which the pores are filled with 'natural gas' or with water. Forcing the oil to move to one of the wells which have been drilled and then getting it to the surface, preferably without too much water, is difficult. With all the resources of present-day technology, and favourable geological conditions, only about half the oil in an underground reservoir may be recovered. (Fifty years ago, the recovery factor was much less, from 10 to 15 percent.) There is thus a high premium on improvements in our understanding of how the oil, water and gas flow through the porous rock and in the techniques for controlling this complex flow system.

There is of course an early stage in the operation of an oil well when, provided perforations have been made in the casing of the bore-hole at the places where the well passes through oil-bearing strata, the oil does flow naturally up the well under the action of the high pressures normally occurring in underground reservoirs. But this phase of so-called 'primary recovery' does not last very long, because the extraction of some of the oil relieves the pressure in the reservoir and the flow decreases to uneconomic levels.

The controlling engineers must then get behind the oil and push, thereby restoring the pressure and forcing it up the well in the phase of secondary recovery. They can do this by drilling new injection wells at the edge (in the projection on a horizontal plane) of the reservoir and pumping water down these wells and into the oil-bearing strata of rock. The effect of surface tension at the interface between the oil and the water provides an additional push in the right direction, because water 'wets' the rock surface more than oil and the water is sucked into the porous rock as it is into blotting paper or soil. Another possible course of action is to pump gas down an injection well to the top of the reservoir so as to increase the pressure in the oil. The locations of the initial exploration wells, the production wells and the later injection wells are chosen mostly on the basis of data from seismic surveys, made by generating compression waves from a point on the earth's surface and observing the waves reflected from the interfaces between different rock strata. The interpretation of the seismic data is usually rather uncertain, especially in the early stages when few wells have been drilled and there is little confirmatory evidence from bore-holes; and mistakes in the location of wells are very costly.

Flushing oil out of the rock with water or blowing it out with gas likewise gives diminishing returns, because pockets of oil remain behind in the rock, held there in part by the effect of surface tension at oil-water interfaces with high curvature. In the tertiary recovery phase various sophisticated techniques for dislodging these oil pockets from the rock crevices may be tried. One method is to pump an aqueous concentrated solution of some surface-active agent down the injection wells. The molecules of this surface-active agent are adsorbed at oil-water interfaces, and reduce the surface tension, in the way that detergents decrease the surface tension of grease-water interfaces in a kitchen sink; the interface between the oil and the water can then be deformed more easily, and the oil and water tend to move more as a single fluid.

Underlying all these engineering operations are some fascinating hydrodynamic problems of fluid flow within a region with randomly-shaped boundaries. The pores of the rock form a system of connected tortuous channels of very small linear dimensions (of the order of 0.1 mm) through which the oil or water or gas flows. Now although the details of the flow, such as the shape of the streamlines, depend on the precise form of the channel boundaries, and therefore are beyond the reach of calculation, we can infer from the smallness of the channel width that the acceleration of each fluid element is negligible by comparison with the frictional force per unit mass. This is 'creeping' flow (or low-Reynolds-number flow), in which the resultant force on each element of fluid is zero. In the case of flow of a single fluid, there are two forces acting on each element, one associated with the applied pressure gradient and one associated with the tangential surface forces due to fluid friction, and they must be equal and opposite. The fluid friction forces depend linearly on the velocity of the fluid, and so we can formulate the statistical relation

$$\langle \nabla p \rangle = -\mu \langle \underset{\sim}{u} \rangle / k ,$$

where the angle brackets denote an average over a volume large enough to contain many pores of the rock, p denotes pressure, $\underset{\sim}{u}$ the vector velocity and μ the fluid viscosity, and k is an important statistical parameter of the porous medium, called the permeability. This law was first established by Darcy, in the middle of the 19th century, in the course of work on the percolation of water through soil. In the case of a porous medium which consists of approximately spherical sand grains pressed together, like sandstone rock and also many soils, it is preferable to put $k = \alpha d^2$, where d is the average grain size and α is a dimensionless constant dependent on the (dimensionless) distribution of grain sizes and on the statistics of the arrangement of the grains.

Laboratory experiments on the percolation of oil or water through a statistically uniform porous medium consisting of a packed bed of hard little spheres have provided some information about α. As one might expect, α depends primarily on the porosity (the fraction of the total volume that is occupied by fluid), and only weakly on the other statistical parameters describing the arrangement of the spheres. This is a useful result, because the porosity is one of the few statistical properties of the rock geometry which can be inferred from measurements made <u>in situ</u> by instruments lowered down a bore-hole (measurement of the dielectric constant of the liquid-filled rock, for instance, gives a good estimate of porosity).

Computer simulations of randomly packed beds of spheres have also been devised recently, and have revealed a rather surprising dependence of the statistical parameters, including the porosity, on the way in which the bed is formed, even for the simple case of a bed of uniform spheres. It appears that if the bed is formed 'gravitationally' by adding one sphere after another to one face of the bed, as when balls are thrown into a tub, the porosity is 0.40 and the sphere arrangement is not isotropic. If on the other hand the bed is formed (on the computer) by choosing the location of sphere centres randomly and then adjusting these position co-ordinates judiciously to avoid any overlap of spheres, an isotropic arrangement with a porosity of as low as 0.363 may be obtained. Both these values lie between the porosities for a simple cubic regular array of spheres (0.476) and for a regular tetrahedronal array (0.26). In a real porous rock the sand grains are not uniform in size and the porosities are consequently rather smaller, around 0.2, because small grains can be packed into the spaces left by large ones.

It is difficult to observe the geometrical properties of a three-dimensional sample of a porous medium, and an alternative procedure which is feasible for some materials is to make measurements on the exposed plane face obtained by slicing carefully through a piece of the medium. What statistical properties of the porous medium, assumed to be isotropic, can be determined from this plane section? Quite a lot, fortunately, because the porosity, the area of the solid surface in unit volume, and the mean curvature of the solid surface are all equal to their two-dimensional counter-parts measured on a plane section. On the other hand, the vital property of the existence of connected paths through the interstices does not seem to be determined by a plane section.

An inevitable consequence of movement of fluid through a porous medium with random structure is that fluid elements which initially are neighbours gradually wander apart through taking different paths through the medium. This 'geometrical diffusion' is mathematically interesting but causes problems for the oil engineer since the interface between oil and injected water becomes more diffuse as it moves through the rock and turns into a broad region occupied partly by oil and partly by water. Determination of the diffusivity (which has different components in the directions parallel and perpendicular to the average flow velocity) in terms of the statistical parameters characterizing the geometry of the porous medium is an unsolved problem, although some progress has been made with very simple models of a porous medium, such as a set of straight randomly oriented tubes between junctions. Random variations of the permeability of the porous medium, on a scale much larger than the pore size, likewise cause dispersion in the moving fluid.

Geometrical diffusion is not the only process which prevents the moving interface between the oil and the water from remaining sharp and plane. There is a general tendency, referred to loosely as 'fingering', for the less viscous liquid (water) to run ahead of the more viscous liquid (oil) in a series of parallel fingers when the two liquids are being pushed through a porous medium by an applied pressure gradient. These fingers grow spontaneously, and one may show mathematically (as students were invited to do, in Part III of the Mathematical Tripos in 1958 and again in 1963) that an initially plane interface between oil and water is unstable, in the sense that the amplitude of a sinusoidal wavy disturbance of the interface will grow exponentially, when the interface is moving through the porous medium in the direction of the more viscous fluid. Thus the use of water, which is the only liquid available at the well-head in large quantities, to flush oil out the rock reservoir cannot be completely successful because the water will rush ahead and leave oil behind in regions between the water fingers. Sometimes a suitable polymer, with flexible long chainlike molecules, is dissolved in the injected water in order to increase the viscosity of the water and so to diminish the rate at which the fingers develop.

Surface tension effects also cause the interface between oil and water to become increasingly distorted as it moves through the rock and so to hinder the process of flushing oil out of the reservoir. The linear dimensions of a crevice in the rock determine the local curvature of an oil-water interface, and large pressure variations due to surface tension can arise from variations in the size of crevices. No results concerning the statistical properties of an interface with surface tension in a porous medium are yet available, either for a stationary interface or for one in motion, and there is need for theoretical analysis. Laboratory investigations suggest that oil blobs easily become trapped in crevices of certain shape, essentially because surface tension resists the large increase in surface area of the blob that would be needed for the blob to pass through a constriction, and that after oil-bearing rock has been flushed with water for some time a significant fraction of the pore space is still filled with small oil blobs which cannot be dislodged. If the surface tension at the interface could be reduced, by the addition of a suitable solute to the water pumped down an injection well, these trapped blobs of oil would move more freely, but it is obviously impossible to reach all the blobs with the injected material.

It is said that North Sea oil will be exhausted before the end of this century. The more precise statement is that about half of it will have been extracted by then and that we do not know how to get at the other half. Improvement of the yield is even more important than discovering new oil fields, and presents a challenge to applied mathematicians no less than to engineers.

Almost All Games Are First Person Games

By D Singmaster

ABSTRACT

Fremlin (1) and other authors have observed that most games are first person games. The object of this note is to show that almost all games are first person games in the following sense. Let $N(n)$ be the number of games of length $\leqq n$ and let $N_1(n)$ be the number of first person games of length $\leqq n$. Then $\lim_{n\to\infty} N_1(n)/N(n) = 1$.

The games we are considering are variously called combinatorial, termination or positional games. They are two person games which are finite in extent. The following concepts and definitions are based on Scott (2). Following Scott, we consider a game as described by its finite game tree, with redundant branches and rearrangements ignored. That is, we identify a position with the set of its succeeding positions. We identify a game with its initial position and any position can be considered as the beginning of a game, so we consider the terms "game" and "position" as synonymous. The only difficulty in our identification occurs when we are dealing with terminal positions. Terminal positions can be either, a win or a loss for the last player, or a tie. We let W, L, T denote these three kinds of terminal positions.

DEFINITION 1.

An admissible terminal set S is any subset of {W, L, T}, except $S = \phi$ and $S = \{T\}$.

Thus there are six admissible terminal sets. The case $S = \phi$ is excluded since our games must end and the case $S = \{T\}$ is excluded since this means that every game ends in a tie, which is trivial.

DEFINITION 2.

Let S be an admissible terminal set. A game of type S of length 0 is an element of S. For $n > 0$, a game of type S of length $\leqq n$ is a game of length 0 or a nonempty subset of

$$\{G \mid G \text{ is a game of type } S \text{ of length } \leqq n-1\}.$$

The term "game" will mean a game of some type. The length $L(G)$ of a game G can be defined by $L(G) = n$ if G is a game of length $\leqq n$ and G is not a game of length $\leqq n-1$. Then $L(G) = 1 + \max_{g \varepsilon G} L(g)$ for games of length > 0. $L(G)$ is the maximal number of moves to get from G to a terminal position.

Each game is classifiable as first person, second person or tie by the next definition. These categories are symbolized by 1, 2, t.

DEFINITION 3.

The value $V(G)$ of a game of length 0 is given by:

$$V(W) = 2; \quad V(L) = 1; \quad V(T) = t.$$

For $n > 0$, the value of a game of length n is given by:

$$V(G) = 1 \text{ if } \exists g \varepsilon G,\ V(g) = 2;$$

$$V(G) = 2 \text{ if } \forall g \varepsilon G,\ V(g) = 1;$$

$$V(G) = t \text{ if } \forall g \varepsilon G,\ V(g) = 1 \text{ or } t \text{ and } \exists g \varepsilon G,\ V(g) = t.$$

Note that if $T \notin S$ then there are no tie games.

DEFINITION 4.

Let $N_1(n)$, $N_2(n)$, $N_t(n)$ be the numbers of first person, second person and tie games of length $\leqq n$. Let $N(n)$ be the total number of games of length $\leqq n$.

PROPOSITION 5. Let $|A|$ denote the cardinality of a set A. Then

$$N_1(0) = |\{L\} \cap S|; \quad N_2(0) = |\{W\} \cap S|; \quad N_t(0) = |\{T\} \cap S|;$$

$$N(0) = |S|;$$

$$N_2(n+1) = 2^{N_1(n)} - 1 + |\{W\} \cap S|;$$

$$N_t(n+1) = 2^{N_1(n)+N_t(n)} - 2^{N_1(n)} + |\{T\} \cap S|;$$

$$N(n+1) = 2^{N(n)} - 1 + |S|;$$

$$\begin{aligned} N_1(n+1) &= N(n+1) - N_2(n+1) - N_t(n+1) \\ &= 2^{N(n)} - 2^{N_1(n)+N_t(n)} + |S| - |\{W,T\} \cap S|. \end{aligned}$$

THEOREM 6.

For any admissible terminal set S, $\lim_{n\to\infty} N_1(n)/N(n) = 1$.

Proof. Clearly $N_1(n)$ and $N_2(n)$ go to infinity as n does and so does $N_t(n)$ if $T \in S$. Now, from Proposition 5,

$$\begin{aligned} N_1(n+1)/N(n+1) &\sim 1 - 2^{N_1(n)+N_t(n)}/2^{N(n)} \\ &= 1 - 1/2^{N_2(n)} \to 1. \end{aligned}$$

Below we give the first few values of $N_1(n)$, $N_2(n)$, $N_t(n)$ and $N(n)$ for each type S.

	n	$N_1(n)$	$N_2(n)$	$N_t(n)$	$N(n)$
S = {W}					
	0	0	1	0	1
	1	1	1	0	2
	2	2	2	0	4
	3	12	4	0	16
	4	61440	4096	0	65536

	n	$N_1(n)$	$N_2(n)$	$N_t(n)$	$N(n)$
S = {L}					
	0	1	0	0	1
	1	1	1	0	2
	2	3	1	0	4
	3	9	7	0	16
	4	65025	511	0	65536
S = {W,L}					
	0	1	1	0	2
	1	3	2	0	5
	2	25	8	0	33
	3	$2^{33}-2^{25}+1$	2^{25}	0	$2^{33}+1$
S = {W,T}					
	0	0	1	1	2
	1	2	1	2	5
	2	16	4	13	33
	3	$2^{33}-2^{29}$	2^{16}	$2^{29}-2^{16}+1$	$2^{33}+1$
S = {L,T}					
	0	1	0	1	2
	1	1	1	3	5
	2	17	1	15	33
	3	$2^{33}-2^{32}+1$	$2^{17}-1$	$2^{32}-2^{17}+1$	$2^{33}+1$
S = {W,L,T}					
	0	1	1	1	3
	1	5	2	3	10
	2	769	32	225	1026

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Impure Mathematics

Once upon a time (1/T) pretty little Polly Nomial was strolling across a field of vectors when whe came to the boundary of a singularly large matrix.

Now Polly was convergent, and her mother had made it an absolute condition that she must never enter such an array without her brackets on. Polly, however, who had changed her variables this morning and was feeling particularly badly behaved, ignored this condition on the basis that it was insufficient and made her way amongst the complex elements.

Rows and columns closed in on her from all sides. Tangents approached her suface. She became tensor and tensor. Quite suddenly two branches of an hyperbola touched her at a single point. She oscillated violently and lost all sense of directrix and went completely divergent. As she reached a turning point, she tripped over a square root that was protruding from the erf and plunged over a steep gradient. When she rounded off once more she found herself inverted, apparently alone in a non-euclidean space.

She was being watched, however. That smooth operator, Curly Pi, was lurking inner product. As his eyes devoured her curvilinear coordinates, a singular expression crossed his face. He wondered, was she still convergent? He decided to integrate improperly at once.

Hearing a common fraction behind her, Polly rotated and saw Curly Pi approaching with his power series fully extrapolated. She could see at once by his degenerate conic and dissipative terms that he was bent on no good.

- Arcsinh! - she gasped.

- Ho, ho - he said - What a symmetric little asymptote you have. I can see that your angles are very secsy.

- Oh, sir - she protested - keep away from me. I haven't got my brackets on!

- Calm yourself, my dear - said our suave operator - your fears are purely imaginary!

I,I - she thought - perhaps he's not normal but homologous?

- What order are you? - the brute demanded.

- Seventeen - replied Polly.

Curly leered - I suppose you've never been operated on?

- Of course not - Polly replied quite properly - I'm absolutely convergent.

- Come, come - said Curly - Let's off to a decimal place I know and I'll take you to the limit.
- Never! - gasped Polly.
- Abscissa! - he swore, using the vilest oath that he knew. His patience was gone. Coshing her over the coefficient with a log until she was powerless, Curly removed her discontinuities.
He stared at her significant places, and began smoothing her points of inflection. Poor Polly. The algorithmic method was now her only hope. She felt his hand tending to her asymptotic limit. Her convergence would soon be gone forever.
There was no mercy, for Curly was a Heavyside operator. Curly's radius squared itself; Polly's loci quivered. He integrated by parts. He integrated by partial fractions and then performed Runge-Kutta on her. The complex beast even went all the way round her and did a contour integration. Curly went on operating until he had satisfied all her hypotheses, then he exponentiated and became completely orthogonal.
When Polly got home that night, her mother noticed that she was no longer piece wise continuous, but had been truncated in several places. But it was too late to differentiate now. As the months went by, Polly's denominator increased monotonically. Finally she went to L'Hospital and generated a small pathological function which left surds all over the place and drove Polly to subtraction.
The moral of our sad story is this: If you want to keep your expressions convergent, never allow them a single degree of freedom.......

On How To Simplify Mathematics

By P Freyd

There's a member of the American Maths. Society who publishes an abstract every month or so which purports to show the inconsistency of ZFC, that is, Zermelo-Frankel set theory contradicts the axiom of choice. The pattern is always the same: he states a "theorem" in ZF and then constructs a counter example in ZFC. What makes all of this unsatisfactory is that his "theorem" always has a plain counter example staring you in the face and it never needs the axiom of choice.

I propose to fill this much needed gap in the literature. I bumped into this particular "proof" when I was an undergraduate and it took me and two of my professors the better part of a day to find my error. Since then the quickest person I've found has been P.T. Johnstone who took about ten seconds. But I warn you - I have seen strong mathematicians reduced to babbling believers in the inconsistency of their own profession. (As you will note, you do not need to know Zermelo-Frankel, just naive set theory. ZF may be regarded as a particular axiomization of naive set theory. The trouble is that Gödel proved that the inconsistency of ZFC implies the inconsistency of ZF which in turn implies that our basic notion of set theory is inconsistent.)

"Theorem"

<u>The circle group</u>, $\mathbb{R}/\mathbb{Z}$, <u>has just two automorphisms</u>.

"Proof"

It clearly suffices to show that all automorphisms are continuous. Let G be any compact group and suppose θ is a discontinuous automorphism. Let G_2 be the compact group obtained by keeping the same underlying set and group structure as G_1 but topologized so that $C \subset G_2$ is closed iff $\theta(C)$ is closed in G_1. We need only show

"Lemma"

<u>A group has at most one compact topology</u>.

"Proof"

Given G_1 and G_2 as described above, let G_{12} be the topological group obtained by keeping the same group structure but topologized so that $C \subset G_{12}$ is closed iff C is closed in both G_1 and G_2. Clearly G_{12} is T_1 (points are closed) and any T_1 group is T_2 (Hausdorf). The identity map yields continuous maps $G_i \longrightarrow G_{12}$ for $i = 1,2$ and a continuous one-to-one map from a compact to a Hausdorf space is a homeomorphism. Q.E.D.

With the axiom of choice we may decompose $\mathbb{R}/Z$ (as for any divisible abelian group) as the direct sum of its torsion subgroup, T, and a torsion-free divisible subgroup: use the divisibility of T to obtain a retraction $T \longrightarrow \mathbb{R}/Z \longrightarrow T$; then $\mathbb{R}/Z \simeq T \oplus F$ where F is the kernel of $\mathbb{R}/Z \longrightarrow T$. Any torsion-free divisible abelian group may be regarded as a vector space over the rationals. We may choose a basis and it clearly must be of the order of the continuum (the torsion subgroup is countable). Any permutation of the basis yields an automorphism of F, hence of $\mathbb{R}/Z$. The automorphism group in ZFC has $2^{2^{\aleph_0}}$ elements.

This "proof" is satisfactory in that one really needs some sort of axiom of choice to construct a third automorphism. Given any homomorphism $f:\mathbb{R}/Z \longrightarrow \mathbb{R}/Z$ consider the map $g:\mathbb{R} \longrightarrow \mathbb{R}/Z \xrightarrow{f} \mathbb{R}/Z \longrightarrow \mathbb{C}$ from the reals to the complex numbers, where $\mathbb{R} \longrightarrow \mathbb{R}/Z$ is the canonical covering and $\mathbb{R}/Z \longrightarrow \mathbb{C}$ is the canonical embedding (it sends x to $e^{2\pi ix}$). The axiom of measurability, known to be consistent with ZF, says that any function from $\mathbb{R}$ to $\mathbb{C}$ is measurable (it makes sense to integrate it). For any $n \in Z$ define $h(x) = g(x)e^{2\pi inx}$. Because $h(x+y) = h(x)h(y)$ we have $\int_0^1 h(x+y)dx = h(y)\int_0^1 h(x)dx$. But h is periodic and we have

$$\int_0^1 h(x+y)dx=\int_y^{1+y} h(x)dx=\int_y^1 h(x)dx+\int_1^{1+y} h(x)dx=\int_y^1 h(x)dx+\int_0^y h(x)dx=\int_0^1 h(x)dx.$$

From $h(y)\int_0^1 h(x)dx = \int_0^1 h(x)dx$ we conclude that either $h \equiv 1$ or $\int_0^1 h(x)dx = 0$. Hence, for any $n \in Z$ either $g(x) = e^{-2\pi inx}$ all x or $\int_0^1 g(x)e^{2\pi inx}dx = 0$. But the fundamental theorem of Fourier series says that $\int_0^1 |g|^2dx = \sum\left|\int_0^1 g(x)e^{2\pi inx}dx\right|^2$. Since $|g| \equiv 1$ we conclude that for some $n \in Z$, $g(x) = e^{-2\pi inx}$ all x and that the original $f:\mathbb{R}/Z \longrightarrow \mathbb{R}/Z$ is just multiplication by -n. It is consistent with ZF to assume that the group of $\mathbb{R}/Z$ has two elements.

With a little work we may replace the circle group with the field of complex numbers: the axiom of unreasonability implies that the Galois group of $\mathbb{C}$ has two elements; the axiom of choice gives it $2^{2^{\aleph_0}}$ elements.

Changing Variables

By C N Corfield

<u>Problem</u>: You have a partial derivative $(\frac{\partial A}{\partial B})_C$ (the derivative of A with respect to B holding C fixed) and want to change variables from B,C to X,Y say.

Here is a technique that does this and more besides in a straightforward way. It uses Jacobians to do the work, and we write

$$\frac{\partial(A,B)}{\partial(X,Y)} \equiv \begin{vmatrix} (\frac{\partial A}{\partial X})_Y & (\frac{\partial B}{\partial X})_Y \\ (\frac{\partial A}{\partial Y})_X & (\frac{\partial B}{\partial Y})_X \end{vmatrix} \equiv \begin{vmatrix} A_X & B_X \\ A_Y & B_Y \end{vmatrix}$$

It has the following properties:

(i) $$\frac{\partial(A,B)}{\partial(X,Y)} \cdot \frac{\partial(X,Y)}{\partial(A,B)} = 1 \quad \text{or} \quad \frac{\partial(A,B)}{\partial(X,Y)} = 1 \Big/ \frac{\partial(X,Y)}{\partial(A,B)}$$

(ii) $$\frac{\partial(A,B)}{\partial(X,Y)} \cdot \frac{\partial(X,Y)}{\partial(x,y)} \cdot \frac{\partial(x,y)}{\partial(A,B)} = 1$$

(iii) $$(\frac{\partial A}{\partial B})_C = \frac{\partial(A,C)}{\partial(B,C)}$$

It is property **(iii)** that is crucial to the whole game, for example we see that the result $(\frac{\partial A}{\partial B})_C (\frac{\partial B}{\partial C})_A (\frac{\partial C}{\partial A})_B = -1$ is a consequence of (ii) and (iii) with suitable X,Y,x,y. In thermodynamics the Maxwell relations may be derived easily using (ii) and (iii): We start from the relation $dU = TdS - pdV \equiv (\frac{\partial U}{\partial S})_V dS + (\frac{\partial U}{\partial V})_S dV$ then $\frac{\partial^2 U}{\partial S \partial V} = \frac{\partial^2 U}{\partial V \partial S} \Rightarrow (\frac{\partial T}{\partial V})_S = -(\frac{\partial p}{\partial S})_V$ and we now use (iii) and **(ii)** to write

(i) $$\left(\frac{\partial T}{\partial V}\right)_S = \frac{\partial(T,S)}{\partial(V,S)} = \frac{\partial(T,S)}{\partial(x,y)} \bigg/ \frac{\partial(V,S)}{\partial(x,y)}$$

(ii) $$-\left(\frac{\partial p}{\partial S}\right)_V = \frac{-\partial(p,V)}{\partial(S,V)} = -\frac{\partial(p,V)}{\partial(x,y)} \bigg/ \frac{\partial(S,V)}{\partial(x,y)} = \frac{\partial(p,V)}{\partial(x,y)} \bigg/ \frac{\partial(V,S)}{\partial(x,y)}$$

hence $\frac{\partial(T,S)}{\partial(x,y)} = \frac{\partial(p,V)}{\partial(x,y)}$. The possible variables are any two from the set $\{T,S,p,V\}$ and by letting $x,y \in \{T,S,p,V\}$ we obtain the Maxwell relations

$$M1 : \left(\frac{\partial S}{\partial p}\right)_T = -\left(\frac{\partial V}{\partial T}\right)_p$$

$$M2 : \left(\frac{\partial S}{\partial V}\right)_T = \left(\frac{\partial p}{\partial T}\right)_V$$

$$M3 : \left(\frac{\partial S}{\partial V}\right)_p = \left(\frac{\partial p}{\partial T}\right)_S$$

$$M4 : \left(\frac{\partial S}{\partial p}\right)_V = -\left(\frac{\partial V}{\partial T}\right)_S$$

For example M1 comes from setting $(x,y) = (T,p) \Rightarrow \frac{\partial(T,S)}{\partial(T,p)} = \frac{\partial(p,V)}{\partial(T,p)}$

$$\Rightarrow \begin{vmatrix} T_T & S_T \\ T_p & S_p \end{vmatrix} = \begin{vmatrix} p_T & V_T \\ p_p & V_p \end{vmatrix} \Rightarrow \begin{vmatrix} 1 & S_T \\ 0 & S_p \end{vmatrix} = \begin{vmatrix} 0 & V_T \\ 1 & V_p \end{vmatrix} \Rightarrow \left(\frac{\partial S}{\partial p}\right)_T = -\left(\frac{\partial V}{\partial T}\right)_p$$

As a last example of changing variables suppose you want $\left(\frac{\partial S}{\partial T}\right)_p$ in terms of T and V (rather than T and p in its present form) then using (iii)

$$\left(\frac{\partial S}{\partial T}\right)_p = \frac{\partial(S,p)}{\partial(T,p)} = \frac{\partial(S,p)}{\partial(T,V)} \bigg/ \frac{\partial(T,p)}{\partial(T,V)} = \frac{\begin{vmatrix} S_T & p_T \\ S_V & p_V \end{vmatrix}}{\begin{vmatrix} T_T & p_T \\ T_V & p_V \end{vmatrix}} = \frac{S_T p_V - S_V p_T}{\begin{vmatrix} 1 & p_T \\ 0 & p_V \end{vmatrix}} =$$

$$= \frac{S_T p_V - S_V p_T}{p_V} = S_T - \frac{p_T S_V}{p_V} = \left(\frac{\partial S}{\partial T}\right)_V - \frac{\left(\frac{\partial p}{\partial T}\right)_V \left(\frac{\partial S}{\partial V}\right)_T}{\left(\frac{\partial p}{\partial V}\right)_T}$$

$$= \left(\frac{\partial S}{\partial T}\right)_V - \left(\frac{\partial p}{\partial T}\right)_V^2 \bigg/ \left(\frac{\partial p}{\partial V}\right)_T \quad \text{on using M2.}$$

The Hunting Of J_4

By J H Conway

The aim of this article is to describe the construction of J_4 which was completed last year by 5 people - Simon Norton, Richard Parker, David Benson, John Conway, and Jon Thackray. But first I'd better tell you what J_4 is!

A simple group is one with no homomorphic image other than itself and the trivial group. There is a sense in which every finite group is made by sticking simple ones together, so the simple groups are the building blocks of group theory. Apart from the cyclic groups of prime order, which are often considered too simple even to deserve the name, simple groups are non-abelian and often very complicated indeed! The smallest is the group A_5 of all even permutations of 5 letters (A_n is simple for $n \geq 5$).

Around 1900 Burnside raised the problem of classifying simple groups, and showed that the smallest possible orders were 60,168,360,504,660,1092, with a unique simple group in each case. (These are the groups $PSL_2(q)$ of all maps $x \to \frac{ax+b}{cx+d}$ over the finite field of order q with ad-bc a square in that field, for q = 4 or 5, 7, 8, 9, 11, 13 respectively. $PSL_2(q)$ is simple if $q \geq 4$) Burnside pointed out that with the 5 notorious exceptions that had been discovered by Emil Mathieu in the 1860s, the groups fell into infinite families (like A_n and $PSL_2(q)$).

More families were found in our century, but opinion slowly hardened into the general belief that Mathieu's groups would be the only exceptional ones. I still remember the shock when in the 1960s Zvonimir Janko found a new simple group J_1 of order 175560, which at that time we thought was a very large number indeed!

Later discoveries came thick and fast, with some new infinite families, and the exceptions increasing from Mathieu's original 5 to the present total of 26 'sporadic' groups. Often, as with elementary particles in physics, a group would be 'predicted' by one mathematician, to be constructed later by another. In 1979 two such predictions were outstanding, the MONSTER, of order 8080 17424 79451 28758 86459 90496 17107 57005 75436 80000 00000, and Janko's fourth group, J_4, of order 86775 57104 60775 62880.

Let's close this introduction by describing the progress that's been made with the problem of proving that the simple groups we know really are the only ones there are. Powerful methods that reopened this theory were developed by Brauer, Suzuki, and Feit and Thompson, and continued by a host of group theorists each working on some corner of the problem, with most of the final touches being applied by Aschbacher, who has recently announced that the job has been completed. There's still a lot to be done, because when papers are measured in pounds (7·2 pounds being the record!) it's natural that there will be mistakes, and in fact the subject is notorious for them. But it really does seem that at long last this 100-year problem is essentially solved.

<u>First thoughts on J_4</u>.

Some years ago a few of us at Cambridge had worked out the character table of J_4, which is a table of numbers that is astonishingly informative about the group. For example it tells us what sizes of matrices can be used to represent group elements, exactly how many elements commute with any given one, and so on. So we knew for example that the orders of elements in J_4 were 1-8,10,11,12,14,15,16,20-24,28-31,33,35,37,40,42,43,44, and 66. The table told us that the group could be expressed as a group of 1333 by 1333 matrices with complex entries, and Norton and I had tried to construct it this way, but given up. (We have a lot of local expertise in this line - my own largest group is a group of 24 by 24 matrices, and two of the more recent sporadic groups were built here as 133 and 248-dimensional groups.) But this experience stood us in good stead, as it forced us to learn a lot about J_4 and its subgroups.

What reawakened our interest was the discovery that J_4 could probably be represented by 112-dimensional matrices over the field of order 2, and the fact that Richard Parker had invented an amazingly easy family of computer techniques for just such cases. His ideas had already polished off several outstanding problems in this field, and the building of J_4 looked like just another job for his 'meataxe'. So with high hopes we set off again around the middle of 1979.

It seemed likely that J_4 contained the known group $U_3(11)$, and in an abstract way we knew how $U_3(11)$ was represented by 112-dimensional matrices. If we could only compute such matrices explicitly, then Norton had an idea for a 'formula' in them which could be used to find an additional generator for J_4. On the other hand, it's easy to build $U_3(11)$ as a group of 1332 by 1332 permutation matrices, and this 1332-space should have a 1221 dimensional subspace preserved by all elements of $U_3(11)$.

I mention all these numbers just to give you some idea of the power of Parker's computer programmes. Just for fun I've worked out that there are more than $2^{100,000}$ - in fact exactly

$$\frac{(2^{1332}-2^0)(2^{1332}-2^1)(2^{1332}-2^2)\ \dots\ (2^{1332}-2^{1220})}{(2^{1221}-2^0)(2^{1221}-2^1)(2^{1221}-2^2)\ \dots\ (2^{1221}-2^{1220})}$$

1221-dimensional subspaces of a 1332-space. Nevertheless, our idea was to find this very particular one, and then express $U_3(11)$ as a group of matrices acting on the quotient 111-dimensional space.

We could then apply a little fudge to turn these 111-dimensional matrices into 112-dimensional ones, get a new element by Norton's formula, and J_4 would be built!

A Californian Interlude and its effects.

It wasn't all that easy. However, the first upset was a very enjoyable one. In August 1979 there was a group-theoretical conference at Santa Cruz, in California, which all but one of us attended. So for several weeks everything stopped while we basked in the wonderful Californian sun, and to some extent also in the interest of our group-theoretical colleagues in the J_4 project. I was struck down for a week by a particularly virulent form of flu, so it wasn't quite as enjoyable for me as for the others, but in general we all had a wonderful time.

When we got back to Cambridge, it was to hear that $U_3(11)$ had been safely constructed and fudged up to 112 dimensions, but that Norton's formula had failed to work. This was a bit of a pity, because he was the only one who understood it, and because we suspected (correctly!) that he would take as long to get home from California has he had to get there. (On his arrival in Santa Cruz, which is in northern California, he boasted proudly that he had come from the north, via Los Angeles!) Fortunately, it was only a day or so after his eventual return that he spotted the mistake in his formula, and the corrected version worked first time. We now had four matrices A,B,C,D which we felt confident should generate J_4 - but how could we possibly prove it?

The group game.

We proudly printed out our generating matrices, but only when you've seen a 112 by 112 matrix of 0s and 1s do you realise just how uninformative it is. We soon decided it was a waste of paper to print out more matrices, and instead we started playing the group game.

This is rather like 'Mastermind', and even more like its Cambridge precursor, which was called 'Moo', or 'Bulls and Cows'. One player ("the machine") conceals a group, generated by named elements, and challenges the other player ("you") to say what group it is. "You" may ask "the machine" for the name of the product of any two elements already named, and should try to choose these questions so as to identify the group structure in as few of these 'goes' as possible. You score a Bull when you know the concealed group exactly, and a Cow when the relations you have found so far define a finite group, but perhaps the machine's group is a proper homomorphic image of it.

There is a variation in which you cheat by looking at the multiplication table of the 'concealed' group, and work out just what are the best questions to ask. In this form we say that you buy a group for N pence if you can find N questions that would score a bull for it, and that you have cornered (or corralled) it for N pence when you have N questions giving you a cow for that group. Remember that you are told that the given letters generate the group, but not which elements are which, or even which is the identity!

Both games are quite good fun even for small groups. There's no easy way for me to tell you how it feels to play the concealed game, so I suggest you play it! But with the help of several friends I can bring you the answers for small groups in the open game. The cyclic groups of orders 1,2,3,4,5,6,7 can be bought for 0,2,2,3,3,3,3 pence respectively, and the only other thing you can do for 3 pence is corner Q_8, for which IJ=K, JK=I, KI=J is a presentation. These don't buy Q_8, because they also hold in the fourgroup V_4. Another question, say "is JI = K or some other letter" is needed, and we say that 3p merely gets you $Q_8 \to V_4$.

The groups that can be bought or cornered for 4p are $C_8, C_9, C_{10}, C_{11}, C_{12}, C_{14}, V_4, S_3, Q_8$, and $2A_4 \to A_4$, $2S_3 \to S_3$, $3C_8 \to 3C_4$. I don't know what's the most you can get for 5p - I can buy the cyclic group of order 31, the non-abelian group of order 21, and can corner $2A_5 \to A_5$, although in the letter case it needs two more questions to get the structure exactly. (The cornering relations are AC = B, BD = C, CE = D, DA = E, EB = A.) The order f(N) of the largest N-penny group grows enormously rapidly, and is what I call a supercomputable function of N. If g(N) is any computable function, such as

$$N,\ N^2,\ 2^N,\ N!,\ N^N, \text{ or even } N^{N^{N^{\cdot^{\cdot^{\cdot^{N}}}}}} \text{ (N times)}$$

thenwe have f(N) > g(N) for all but finitely many values of N. The largest group I know how to buy for 10p has order

$(2^{127} - 1).127 = 2\ 16078\ 30299\ 47959\ 24299\ 24287\ 57191\ 72814\ 27329$

and is bought by the questions (with answers)

AA = B, BB = C, CC = D, DD = E, EE = F, FF = G, GG = A, AH = I, HA = J, HJ = I. (It's not hard to see how this works!)

Playing the group game with J_4.

The game David Benson and I played against the computer for several months was a mixture of the open and concealed group games. Because we knew so much about the group, we thought we could think up good questions to ask, but first we had to find our way around, since the original generators A,B,C,D were not much better than random ones from our point of view.

How does the group game player winkle out information in such a situation? By a sequence such as

$$A_1A_1 = A_2,\ A_2A_1 = A_3,\ A_3A_1 = A_4,\ \dots,\ A_nA_1 = A_1$$

he can deduce both that A_1 has order n and that A_n is the name of the identity element. Then for instance he can find some elements of order 2 (involutions), by finding orders of random elements, and computing X^m whenever X has even order 2m.

Now the centraliser of an involution (which is the subgroup of all elements commuting with that involution) is a very important concept in finite group theory. When we had the matrix for some randomly found involution, how could we find those for other elements that commuted with it? (The chance that a randomly chosen group element of J_4 commutes with a given involution is less than 1 in 10^9.) It turns out that there is a very easy way: if T is one involution, compute the order of TU for random other involutions U. If TU has even order 2m, then T and U generate a dihedral group of order 4m, in which $(TU)^m$ is a central element, and so commutes with both T and U.

In this way, we found elements that did in fact generate the two types of involution centralisers in J_4, although it took many weeks to verify this. Since the centralisers were known groups for which we could write down generators and relations, we spent a long time twisting our generators around until we had them in the nicest possible forms, and eventually were able to verify that they had exactly the right structures. Some other known subgroups were located and verified in a similar way.

The weight of evidence at this point that our group really was J_4 was astounding. We had for example found elements of all the permitted orders (many times) but no others, and verified that our group had lots of properties known or suspected of J_4, but no properties that could not hold for J_4. The most likely alternative to the group's being J_4 was that Janko had found a proof that his group didn't exist, and organised a great conspiracy with our computer laboratory to arrange that the computer would always give a plausible answer to any of our questions. This seems unlikely, but hoaxes have happened, and it was even less likely that our answers came out right by accident!

Completing the proof.

A few days into 1980 we were spurred on by getting a curiously worded note from our American colleague Bob Griess to the effect that he had constructed a finite group that sounded suspiciously like the Monster. At first we were disappointed to think that we had lost some kind of race, but on reflection it's nice to think that (if the group-classifiers are right) we are the last people to construct a simple group!

Our original plan was to complete the proof by showing that a certain vector, which we knew to be fixed by a subgroup of known size, had exactly the desired number 173067389 of images under our group. This idea was later carried through by Benson, and in principle gives a verification using only about 10^5 questions in the pure group game. But the first proof to be finished used another method, which involved showing that a 6216 dimensional space acted upon by the group has a 4995-dimensional invariant subspace. A specially large version of Parker's meataxe programme was written and run by Thackray, and finished its last 20-minute run very early in the morning of February 20, 1980. Rather later that day we celebrated by offering champagne to everyone who turned up in the departmental coffee-lounge, and we still celebrate it as J_4 s birthday even though we later found a bug in Thackray's programme which made us rerun it (fortunately with the same result!).

A little problem for Eureka readers.

If you tackle this problem, which is small enough for you to do the calculations by hand, you'll get a very good idea of the sort of problems we faced. Consider vectors (written as 3x3 arrays for convenience) consisting of nine coordinates from the field of order 4. (Whose elements are $0,1,w,\bar{w}$, with $w^3 = 1$, $w^2 = \bar{w}$, $x+x = 0$, and the sum of any two of $1,w,\bar{w}$ being the other one.)

Then consider the operations that replace

$$\begin{array}{ccc} a & b & c \\ d & e & f \\ g & h & i \end{array} \quad \text{by} \quad \begin{array}{ccc} g & d & a \\ h & e & b \\ i & f & c \end{array} \;, \quad \begin{array}{ccc} wc & a & b \\ f & d & e \\ \bar{w}i & g & h \end{array} \;, \quad \text{and} \quad \begin{array}{ccc} g+x & b+y & i+x \\ f+y & e & d+y \\ a+x & h+y & c+x \end{array} \quad \begin{pmatrix} x = a+c+g+i \\ y = b+d+f+h \end{pmatrix}$$

which I call C E and H because they are a Clockwise turn, Eastward shift (with multipliers) and an H-shaped permutation (with additions).

We suspect, but haven't proved, that these generate a group which, if we work modulo scalar multiplication by w, is Janko's third group J_3, of order 50,232,960. Can you prove this?

It suffices to show that it is not possible to find a sequence of these operations that will take you from

$$\begin{array}{ccc} 0 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 0 \end{array} \quad \text{to} \quad \begin{array}{ccc} 0 & 0 & 0 \\ 1 & 1 & 1 \\ 0 & 0 & 0 \end{array} \;.$$

(We know that every vector with an odd number of non-zero coordinates can be taken to one of these, and that all non-zero vectors with evenly many non-zero coordinates are equivalent.)

It's fun playing with these operations, and surely someone must come up with the easy proof we have missed! Write to me when you do, and we'll publish it. Good luck!

Problems Drive

Answers

1. (i) All real x.

   (ii) No real x .

   (iii) $x = n(4^{\circ})$, n integer. Note that $x = n(90^{\circ})$ does not work for odd n and coincides with the given solution for n even.

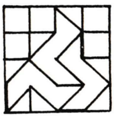
2. By example: $4n = a^2+b^2+c^2-2ab-2ac-2bc$

   or $n = \frac{1}{4}/a+b-c)^2 - ab$ (etc.) .

3. 288 .

4. 5 .

5.



6. $\frac{1}{4}$ (i.e. $(0,\frac{1}{4},\frac{3}{4},1)$).

7. A Applied

   B Applied

   C Pure .

8. SAAT, e.g.

STAS	STSA	SSAT	STSA
TATA	TAST	TATA	SATT
SSAT	ATAS	STAS	ATAS
ATST	SATT	ATST	TAST .

9. $n(x^3+x^2+3x-1) = 0$.

10. (i) 2 (Euclid algorithm).

    (ii) 46651 $(= 6^6 - 5)$.

    (iii) 77815 (= $10^5 \log_{10}6$, to the nearest integer).

    (iv) 924 $\left(= \frac{12!}{6!\ 6!}\right)$.

11. e.g.

$$\begin{aligned}
6x(x-1)(x-2) &= 6x^3-18x^2+12x \\
4x(x-1)(x-2)+25 &= 4x^3-12x^2+8x+25 \\
25+4(x-1)(x-2)(x-3) &= 4x^3-24x^2+44x+1 \\
-6(x-2)(x-3)(x-4) &= -6x^3+54x^2-156x+144 \\
25-4(x-2)(x-3)(x-4) &= -4x^3+36x^2-104x-121 \\
25-4(x-1)(x-2)(x-3) &= -4x^3+24x^2-44x+49 \\
25\pm 8x(x-2)(x-4) &= \pm 8x^3\mp 48x^2\pm 64x+25 \ .
\end{aligned}$$

12. (x,y) = (6,3)

    (2,7)

    (0,-3)

    (-4,1)