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Wilberforce Road
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Published by The Archimedeans, the mathematics student society of the University of Cambridge

Thanks to the Betty & Gordon Moore Library, Cambridge

# EUREKA

## THE ARCHIMEDEANS' JOURNAL

NUMBER 36

OCTOBER 1973 PRICE 30p

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## ERRATA TO EUREKA 35

We are grateful to Mr. P. H. A. Green, who points out that the answer to the problem on page 25 is not 65, as given, but 25. (Consider triangles (15, 20, 25)(25, 24, 7)).

We are grateful to Mr. T. C. Smyth and Mr K. Nilsen, who show that the 'Alphametric' has four solutions (A, P = 0, 6 or 6, 0; L, Y, E = 3, 5, 7 or 5, 7, 3).

## FOOTNOTE TO "WELL-FOUNDED GAMES"

Let a position in Nim have k piles with $n_1, \dots, n_k$ members. For each j, express $n_j$ in binary form as $\Sigma_i d_{ji} 2^i$, where each $d_{ji}$ is either 1 or 0. Now the position is a P-position for positive Nim iff $\Sigma_j d_{ji}$ is even for each $i \geq 0$.

A position is a P-position for negative Nim iff either there is a pile with more than one counter and it is a P-position for positive Nim or every pile has 0 or 1 counter and it is an N-position for positive Nim.

The 'Marienbad' game started with piles of 1, 3, 5 and 7 counters, which is a P-position for either game.

# EUREKA

Journal of the ARCHIMEDEANS

(Cambridge University Mathematical Society)

**Number 36** **1973 October**

Editor: David J. Miller

Sub-Editor: Colin Vout

Business Manager: Martin Mellish

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# Recent results of Pfister about Sums of Squares

by **Prof. J. W. S. Cassels**

One would hardly have expected something essentially new to turn up in a topic so old and well-cultivated as that of representation by sums of squares. Recently, however, there have been several exciting new developments, mainly due to A. Pfister. Many of them generalize to general quadratic forms and there are important consequences for quadratic form theory but here we shall consider only sums of squares. Also I shall be able to explain only one facet of Pfister's work.

Let k be any field of characteristic other than 2. For integral $n > 0$ denote by $G_n = G_n(k)$ the set of non-zero elements of k which are sums of n squares of elements of k. If $a \in G_n$, say

$$a = b_1^2 + \ldots + b_n^2 = 0,$$

then clearly $a^{-1} \in G_n$, since

$$a^{-1} = (b_1/a)^2 + \ldots + (b_n/a)^2.$$

It is well-known that $G_2$ is a group under multiplication. This follows from the identity

$$(x_1^2 + x_2^2)(y_1^2 + y_2^2) = z_1^2 + z_2^2$$

where

$$z_1 = x_1x_2 - y_1y_2 \qquad z_2 = x_1y_2 + x_2y_1$$

familiar from the multiplication of complex numbers. There are similar identities

$$(x_1^2 + \ldots + x_n^2)(y_1^2 + \ldots + y_n^2) = z_1^2 + \ldots + z_n^2 \tag{1}$$

for $n = 4$ and $n = 8$ where the $z_j$ are bilinear forms with integral coefficients in the $x_j$ and $y_j$. These show that $G_4$ and $G_8$ are groups. The identities with $n = 4$ and 8 are associated with the multiplication of Hamilton's quaternions and Cayley's octonions respectively. When k is the real field it was shown by Hurwitz at the end of the last century that 1, 2, 4 and 8 are the only values of n for which there are identities (1) in which the $z_j$ are bilinear forms. Frank Adams showed indeed that when n is not 1, 2, 4 8 there are no identities (1) even if the $z_j$ are allowed to be any continuous functions of the $x_j$ and the $y_j$. It was thus totally unexpected when Pfister proved his

Theorem 1. (Pfister.) Let $n = 2^m$ be any power of 2. Then $G_n(k)$ is a group for all fields k.

Theorem 2. (Pfister.) Let n not be a power of 2. Then there is some field k (depending possibly on n) such that $G_n(k)$ is not a group.

Before we prove theorem 1 we must recall some first-year algebra and introduce some notation. We say that two non-singular quadratic forms f, g in n variables with coefficients in k are <u>equivalent</u> if there is a non-singular transformation

$$\underline{y} = T\underline{x},$$

say

$$y_i = \sum_{j=1}^{n} t_{ij}x_j \qquad (1 \leq i \leq n)$$

with

$$t_{ij} \in k \qquad \det(t_{ij}) = 0,$$

such that

$$g(x) = f(T\underline{x})$$

identically in the variables $x_1, \ldots, x_n$. Clearly equivalence is an equivalence relation in the technical sense. For any form f we denote by M(f) the set of non-zero elements c of k such that cf is equivalent to f.

<u>Lemma 1</u>. The elements of M(f) form a group under multiplication. It includes all the non-zero squares. If f is equivalent to g then M(f) = M(g).

<u>Proof</u>. If

$$f(T_j\underline{x}) = c_j f(\underline{x}) \qquad (j = 1, 2)$$

then

$$f(T_1T_2\underline{x}) = c_1 f(T_2\underline{x}) = c_1c_2 f(\underline{x})$$

and

$$f(T_1^{-1}\underline{x}) = c_1^{-1} f(\underline{x}).$$

If $d \in k$, $d \neq 0$ we have

$$f(d\underline{x}) = d^2 f(\underline{x}).$$

Finally if f is equivalent to g then cf is equivalent to cg for any $c \in k$. Hence f is equivalent to cf if and only if g is equivalent to cg. This is the last statement of the enunciation.

We shall denote by V(f) the set of <u>non-zero</u> values taken by f when the variables take values in k.

<u>Lemma 2</u>. Suppose that $b \in V(f)$ and $c \in M(f)$.

Then $bc \in V(f)$.

For if

$$b = f(\underline{d}) \qquad \underline{d} = (d_1, \ldots, d_n) \qquad d_j \in k$$

and

$$f(T\underline{x}) = cf(\underline{x}),$$

then

$$bc = f(T\underline{d}).$$

We now return to sums of squares, write

$$\phi_n = \phi_n(\underline{x}) = x_1^2 + \ldots + x_n^2,$$

and enunciate

Theorem 3. (Pfister). Suppose that n is a power of 2.

Then

$$M(\phi_n) = V(\phi_n).$$

If we can prove Theorem 3 then we shall have obtained Theorem 1 as well, since $G_n$ is just an alias for $V(\phi_n)$ and since $M(\phi_n)$ is a group by Lemma 1.

The key to Theorem 3 is

Lemma 3. Suppose that $d \in M(\phi_n)$ and that $1 + d = 0$.
Then $(1 + d) \in M(\phi_{2n})$.

For

$$\phi_{2n}(\underline{x}) = \phi_n(\underline{x}') + \phi_n(\underline{x}''), \qquad (2)$$

where

$$\underline{x} = (x_1, \ldots, x_{2n}), \underline{x}' = (x_1, \ldots, x_n), \underline{x}'' = (x_{n+1}, \ldots, x_{2n}).$$

Here $\phi_n(\underline{x}'')$ is equivalent to $d\phi_n(x'')$ and so $\phi_{2n}(\underline{x})$ is equivalent to

$$\psi(\underline{x}) = \phi_n(\underline{x}') + d\phi_n(\underline{x}'').$$

After the last sentence of Lemma 1 it will be enough to show that $(1 + d) \in M(\psi)$.

But now

$$(1 + d)\psi(\underline{x}) = (1 + d) \sum_{j=1}^{n} (x_j^2 + dx_{n+j}^2)$$

$$= \sum_{j=1}^{n} (y_j^2 + dy_{n+j}^2)$$

$$= \psi(\underline{y}),$$

where

$$y_j = x_j - dx_{n+j} \qquad y_{n+j} = x_j + x_{n+j} \qquad (1 \le j \le n).$$

This completes the proof of Lemma 3.

We also enunciate the trivial

Lemma 4. $M(\phi_n) \subset M(\phi_{2n})$.

This is an immediate consequence of (2) and the definitions.

We now revert to the proof of Theorem 3 in which $n = 2^m$ is a power of 2. We use induction on m and shall therefore assume that $V(\phi_n) = M(\phi_n)$ and deduce the corresponding equation for 2n. Trivially $1 \in V(\phi_{2n})$ and so in any case Lemma 2 gives

$$M(\phi_{2n}) \subset V(\phi_{2n}).$$

On the other hand it follows from (2) that any $c \in V(\phi_{2n})$ has one of the two forms

(i) $c = c_1 \in V(\phi_n)$

(ii) $c = c_1 + c_2 \qquad c_1, c_2 \in V(\phi_n)$.

In the first case we have

$$c_1 \in V(\phi_n) = M(\phi_n) \subset M(\phi_{2n})$$

by Lemma 4 and the induction hypothesis. In the second case we have $c_1, c_2 \in M(\phi_n)$ by induction, and so

$$c = c_1 + c_2 = c_1(1 + d)$$

where

$$d = c_1^{-1}c_2 \in M(\phi_n)$$

since $M(\phi_n)$ is a group (Lemma 1). But now $c_1 \in M(\phi_{2n})$ by the first case and $1 + d \in M(\phi_{2n})$ by Lemma 3. Hence $c = c_1(1 + d) \in M(\phi_{2n})$ since $M(\phi_{2n})$ is a group. This concludes the proof of Theorem 3.

Theorem 3 has an interesting consequence about the structure of a field k. Consider the integers n such that $-1 \in G_n$. (We recall that $G_n = V(\phi_n)$ is the set of non-zero elements of k which are the sums of n squares.) It is possible that $-1$ is not in $G_n$ for any n: in which case we say that k is formally real and reject it as uninteresting in the present context. Otherwise there is a least $s = s(k)$ such that $-1 \in G_s$. We call s the stufe of k. An alternative definition of s is that it is the smallest number such that there exist $b_1, \ldots, b_{s+1}$ in k, not all zero, such that $b_1^2 + \ldots + b_{s+1}^2 = 0$.

Lemma 5. Suppose that $G_n(k)$ is group for some given integer n. Then either $s \le n$ or $s \ge 2n$.

For suppose, if possible, that

$$n < s < 2n$$

and that

$$-1 = b_1^2 + \ldots + b_s^2.$$

Then

$$c + d + 0$$

where

$$c = b_1^2 + \dots + b_n^2 \in G_n$$

$$d = 1 + b_{n+1}^2 + \dots + b_s^2 \in G_n.$$

[How do we know that $c \neq 0, d \neq 0$?] But then

$$-1 = c^{-1}d \in G_n$$

by the group property. This contradicts the minimality of s.

<u>Theorem 4</u>. (Pfister.) If k is not totally real then s(k) is a power of 2.

This follows at once from Theorem 1 and Lemma 5.

<u>Theorem 5</u>. (Pfister.) Let S be a power of 2. Then there is a field $k = k_S$ such that <u>s(k) = S.</u>

Let n be any integer in the range

$$S < n < 2S.$$

Let $K = R(x_1, \dots, x_n)$ where R is the real field and $x_1, \dots, x_n$ are independent variables. Our required field is $k = K(y)$, where y is defined by

$$y^2 + x_1^2 + \dots + x_n^2 = 0. \tag{3}$$

By (3) we clearly have $s(k) \leq n$ and so

$$s(k) \leq S \tag{4}$$

by theorem 4 and since S is a power of 2. We have to show that there is equality in (4). If not, we should have a representation

$$-1 = \sum_1^{S-1} b_j^2 \qquad (b_j \in k).$$

On putting

$$b_j = c_j + yd_j \qquad c_j, d_j \in K$$

we deduce that

$$-1 = \sum c_j^2 + y^2 \sum d_j^2,$$

or say,

$$u + y^2 v = 0$$

where

$$u = 1 + \sum c_j^2 \in G_S(K)$$

$$v = \sum d_j^2 \in G_S(K).$$

But now

$$-y^2 = u/v \in G_S(K),$$

using again that S is a power of 2. Thus we have found a representation of

$$-y^2 = x_1^2 + \ldots + x_n^2$$

as the sum of fewer than n squares in $R(x_1, \ldots, x_n)$. And an old theorem of mine states precisely that this is impossible. (It would make this note too long to give a proof here. Although intuitively 'obvious' it is more difficult to prove than you might think.)

We are now in a position to prove Theorem 2, which the author has not lost sight of even though the reader has. It states that if n is not a power of 2, then there is some field k such that $G_n(k)$ is not a group. Since n is not a power of 2, there is a power of 2, say S, such that

$$n < S < 2n.$$

We take for k the field given by Theorem 5 for which $s(k) = S$. Then Lemma 5 shows that $G_n(k)$ is not a group.

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A. Pfister. Darstellung von −1 als Summe von Quadraten in einem Körper. J. London Math. Soc. 40 (1965), 159-165.

A. Pfister. Multiplikative quadratische Formen. Arch. Math. 16 (1965), 363-370.

A. Pfister. Quadratische Formen in beliebigen Körpern. Invent. Math. 1 (1966), 116-132.

There is a good account which makes use of subsequent simplifications in

W. Scharlau. Quadratic forms. Queen's papers in Pure and Applied Mathematics, No. 22. (Queen's University, Kingston, Ontario. 1969).

The paper of mine which I referred to is

J. W. S. Cassels. On the representation of rational functions as sums of squares. Acta Arith. 9 (1964), 79-82.


# Where are the Zeros of Zeta of S?

Professor Apostol has very kindly given us permission to reprint his song, which is to be sung to the (?) well-known tune of 'Sweet Betsy from Pike'.

> Where are the zeros of zeta of *s*?
> G. F. B. Riemann has made a good guess,
> They're all on the critical line, said he,
> And their density's one over $2\pi$ log *t*.

This statement of Riemann's has been like a trigger,
And many good men, with vim and with vigor,
Have attempted to find, with mathematical rigor,
What happens to zeta as mod $t$ gets bigger.

The names of Landau and Bohr and Cramér,
And Hardy and Littlewood and Titchmarsh are there,
In spite of their efforts and skill and finesse,
In locating the zeros no one's had success.

In 1914 G. H. Hardy did find,
An infinite number that lay on the line,
His theorem, however, won't rule out the case,
That there might be a zero at some other place.

Let $P$ be the function $\pi$ minus li,
The order of $P$ is not known for $x$ high,
If square root of $x$ times log $x$ we could show,
Then Riemann's conjecture would surely be so.

Related to this is another enigma,
Concerning the Lindelöf function $\mu(\sigma)$
Which measures the growth in the critical strip,
And on the number of zeros it gives us a grip.

But nobody knows how this function behaves,
Convexity tells us it can have no waves,
Lindelöf said that the shape of its graph,
Is constant when sigma is more than one-half.

Oh, where are the zeros of zeta of $s$?
We must know exactly, we cannot just guess,
In order to strengthen the prime-number theorem,
The path of integration must not get too near 'em.

These lines stimulated some unknown bard in DPMMS to post the following lines on the notice-board, entitled 'What Tom Apostol Didn't Know':

Now André has bettered old Riemann's fine guess
By using a fancier zeta of s.
He proves that the zeros are where they should be,
Provided the characteristic is p.

There's a moral to draw from this sad tale of woe
Which every young genius among you should know:
If you tackle a problem and seem to get stuck,
Just take it mod p and you'll have better luck.

Both Eureka and Prof. Apostol would be very glad to learn the identity of the author.

# Optimal Card-Shuffling

by **M. J. Mellish**

One shuffles a pack of cards by dividing it into two portions and merging them onto a flat surface by riffling with the fingertips. The problem that concerns us is: How many times do we have to do this before the pack is perfectly shuffled, and what exactly is the best method of shuffling? A pack of cards is said to be perfectly shuffled if either (i) All possible decks are equally likely (this is what we require for patience) or (ii) All possible deals of the deck into hands are equally likely (this is what we require for Bridge). The object of this paper is to find an explicit solution of the problem in case (i) and give some guidelines in case (ii) which may enable some interested reader to solve the problem in that case also. We will start by proving some subsidiary results after noting that any sequence of shuffles of a pack of N cards can be regarded as a random permutation of the integers from 1 to N.

Lemma 1. If $D(i), i = 1, K$ is the set of decks which can arise from shuffling a given pack once, and if there exist $x(i), i = 1, K$ such that $x(i) \geqslant 0$ and $\sum_{i=1}^{K} x_i = 1$, then there exists a shuffling strategy under which $\text{prob}(D(i)) = x_i$.

Proof. Define $P_{j,L}(\alpha) = \text{prob}(\alpha, L \mid \alpha)$, where $\alpha$ is a sequence of form like (L, R, R, L, R, L, L, . . . . .) telling us whether each of the first $(j - 1)$ cards fell from the right or the left. Then the shuffling strategy is defined by the $P_{j,L}(\alpha)$. Let $S(\alpha)$ be the subset of $D(i)$ which corresponds to shuffles beginning with $\alpha$ (the reader should satisfy himself that the obvious correspondence between shuffles and decks is $1 - 1$). Then we set

$$P_{j,L}(\alpha) = \sum_{(i:\ D(i) \in S(\alpha, L))} x_i \Bigg/ \sum_{(i:\ D(i) \in S(\alpha))} x_i$$

A simple inductive check shows that this is the required strategy.

Lemma 2. There is a one-one correspondence between decks obtained by shuffling a new pack of N cards m times or less and sequences $a_i, i = 1, N$ satisfying:

(i) $1 \leqslant a_i \leqslant N$

(ii) $i \neq j$ implies $a_i \neq a_j$

(iii) $a_i$ can be expressed as the union of p subsequences $b^k_{L_i}$, all of which satisfy $b^k_{L_i} - b^k_{L_{i=1}} = 1$, where p is not greater than $2^m$.

Proof. Label each card of the new deck with an integer from 1 to N, starting from the top and working down. Then any deck after m shuffles can be represented as a sequence satisfying (i) and (ii). Further, the $2^m$ subsets of the deck such that two elements of the same set were in the same portion of the pack after every cut (some of which sets may be empty) correspond to the subsequences $b^k_{L_i}$, for a shuffle cannot change the order of such a subset.

Conversely, suppose we are given the subsequences $b^k_{L_i}$ we may assume without loss of generality that each sequence $b^k_{L_i}$ is the maximal subsequence satisfying (iii) and containing $b^k_{L_i}$ (otherwise one would concatenate subsequences until this were true). Define a vector v(k), the number of whose components is the least integer greater than or equal to $\log_2 p$, such that $v_i(k)$ is the coefficient of $2^{i-1}$ in the binary expansion of k — 1. Assume also without loss of generality that $b^k_{L_i}$ is the least number not equal to one of $\bigcup_{j=1}^{j=k-1} b^j_{L_i}$ by ordering the subsequences in a canonical manner.

Then corresponding to this sequence $a_i$ we define a sequence of —int (—$\log_2 p$) shuffles and the sequences $b^k_{L_i}$ are all in the top half at the j'th cut if $v_{N\ 1-j}(k)$ 0 and are all in the bottom half otherwise. A simple inductive argument (left to the reader) shows that such a sequence of shuffles exist and is unique. Further, as p is not greater than $2^m$, — integer(—$\log_2 p$) is not greater than m.

We now proceed to define a sequence on optimal shuffling procedures $P_1, P_2 .. P_n$ such that, if $\{D_n(i), i = 1, kn\}$ is the set of all decks which can be obtained by shuffling a new deck n times or less, then all the $D_n(i)$ are equally probable after the successive application of $P_1 .... P_n$. We now define $P_n$ inductively; let $P_1 .... P_{n-1}$ be perfect shuffles and let $M_n(j)$ be the number of elements of $D_{n-1}(i), i = 1, k_{n-1}$ which can give rise to $D_n(j)$ after a single cut and shuffle. Let $\{D_n(k_{C,j}), j = 1, L_{n,C}\}$ be the set of all decks obtainable by a sequence of n shuffles in which C was the last cut. Then we define the probability that the cut in $P_n$ is C to be

$$\sum_{j=1, L_{n,C}} (M_n(j))^{-1}$$

and by applying Lemma 1 we can construct a shuffling strategy such that

$$\text{prob (cut} = C, \text{deck} = D_n(k_{C,j})) = \frac{1}{k_n \cdot M_n(k_{C,j})}$$

summing over all possible cuts we get prob (deck $= D_n(i)) = 1/k_n$. Thus we have defined a $P_n$. The reader might care, as an instructive exercise, to work out $P_1$, the optimal procedure for the first shuffle.

It is now easy to see that the smallest number of shuffles necessary to randomise a pack of N cards completely is —int(—$\log_2 N$). For by considering the deck in which the original order of the pack is reversed and applying Lemma 2 we see that $D_n(i)$, $i = 1, k_n$ is the set of all possible decks if n is not less than —int(—$\log_2 N$) and in this case $P_1 P_2 P_3 .... P_n$ will perfectly shuffle the pack. This is a nice result and is what one would expect from elementary information theory.

The corresponding nice result for case (ii) mentioned in the introductory paragraph would be that one required —int(—$\log_2 M$) shuffles, where M is the number of players. Unfortunately the niceness of the result is spoiled by the fact that it is false; the true value is —int(—$\log_2$(f(M, N))) where f(M, N) is the least f such that for any M hands of N/M cards $H^i_j$, $j = 1, N/M$, $i = 1, M$ there exists a sequence $a_i$ of N integers as defined in Lemma 2 with the number of subsequences $b^k$ less than or equal to f(M, N) and also

$$\bigcup_{j=1,N/M} H^i_j = \bigcup_{j=0}^{j=N-1} a_{(i+jM)}.$$

Unfortunately it is not clear how f(M, N) can be evaluated; all that is clear is that $M \leq f(M, N) \leq N$. Perhaps one of our readers would care to earn himself a place in the hearts of bridge-players everywhere by solving this problem?

# Autobiographical

by **W. L. Ferrar.**

I came up to Oxford in October 1912 as a mathematical scholar of Queen's. The city was vastly different from what it is now. Horse-drawn buses and trams provided the public transport. Trams ran along the Corn and stopped at Carfax, where the horses were unhooked from one end of the tram and led round (in the road) to the other end. The pedal bicycle was ubiquitous, though the occasional 'blood' sported a motorbicycle. Inside the colleges life was much the same as it is now; though there were many minor differences from today—for instance, we took all meals save dinner in our own rooms and our baths at Queen's were taken in hip-baths in a large communal bathroom which, at about 4 p.m., was a social centre of college life—the general atmosphere of a college, apart from the presence of young women wandering freely about the place, was much the same as the atmosphere today; the same groups, the same shouts, the same plethora of societies and clubs, the same mixture of idleness and industry.

Maths. Mods. was then, as now, the first mathematical hurdle to be jumped. Perhaps a glance at their subject-matter may be of interest. There was the old 'classical' algebra, with its theory of equations and congruences, some theory of numbers and determinants, but no matrices (a word unknown to the undergraduate of that day); Burnside and Panton and some dips into Salmon were accepted texts, while some of us struggled with parts of Chrystal's Algebra. A considerable skill in analytical conics was essential, together with some knowledge of the invariants of related conics. There was a fair range of pure geometry; it included geometrical conics and the geometry of triangles and circles on the pattern of 'a sequel to Euclid' (e.g. the nine point circle, inversion, reciprocation and coaxal circles). There was also projective geometry and, contrary to what later detractors have scathingly declared, one did not get far if one's knowledge was confined to a glib quoting of the circular points at infinity. Curvature, asymptotes and curve tracing (a demanding game of skill in the harder specimens) completed the picture. Trigonometry occupied much more territory than it does now, that is, if it survies at all; Hobson's trigonometry of triangles and quadrilaterals, infinite series and products and (unknown these forty years) spherical trigonometry. Our calculus was in a state of flux. Hardy's Pure Mathematics had appeared in 1908 and a translation of Goursat by Hedrick was also on the market. So our calculus at school had begun with 'little Edwards', progressed through masses of techniques and extensions in 'big Edwards' and was started down a fresh road with a study of Hardy or (for some) Hedrick-Goursat. The examination naturally reflected both the old techniques of Edwards' day and the new Hardy approach. The applied mathematics was the conventional statics, hydrostatics and dynamics done without vectors—these we had heard of, I think, but the wearisome fight about notation had not yet resolved itself and I knew of no one who used them in 1912.

Almost immediately after Mods. I was sent by my college to do a Long Vac. term at Cambridge. This was done so that I might improve my chances of 'getting the Junior' and, in the event, I scraped home by a narrow margin. The examination for the Junior Mathematical Scholarship and Exhibition was held just before Hilary term and any serious candidate needed to start work on it early in the Long Vac. The range of the examination was Mods. plus a goodly slice (undefined as far as I ever knew) of Finals work. The questions were meant to be difficult—and were. It was no uncommon thing for a minor scholar or an exhibitioner to score zero marks on a paper; it counted as no grave disgrace. I remember something of my own marks; the worst was 19 and the best 120. I was always, in examinations, liable to have two papers like that, one well above my normal form and one abyssmally and unbelievably bad. At Cambridge I studied under G. N. Watson, then a young don of Trinity. For the most part I read Bromwich and struggled with former Junior papers. Watson (a past master of his art) was more than once floored by the question I had asked him to do and was obliged to postpone its solution until the next tutorial.

In 1914 came the war. I joined the ranks of the Territorial Field Artillery (15 pounders from the Boer war) and went to France in the spring of 1915. I took two mathematical books with me and studied them from time to time—in all sorts of odd surroundings. The time came for the battery to move down towards the Somme. I was a signaller, loaded with telephone, signal-flags, a rifle and what have you; there was no room in my kit for books and, with a solemn hymn of hate to the Kaiser (a frequent accompaniment to any action forced on one by the exigencies of war) I threw the books over the hedge. I kept a set of old examination papers and at rare intervals refreshed my mathematical memory by working a question or two.

After the war I returned to Oxford. I arrived in April 1919 and was called upon to take my Finals in June 1920. After nearly five years of soldiering I had fifteen months in which to prepare to face my Finals examiners and when, in the early days of my demobilisation, I first tried to do some analysis I found that my memory had developed huge gaps; there was nothing for it but to revise from page one of an elementary book on the calculus. I learned in that fifteen months how to keep to the essentials, to leave the frills and to select what parts of the syllabus I had any hope of mastering. I did a first reading of the whole syllabus, but in my revision work before the examination jettisoned large parts of it. The policy served me well save in the advanced geometry paper. This covered solid geometry and higher plane curves. I banked on the solid and read very widely; the examiners set a couple of quite elementary solid questions and reserved all their teasers for higher plane curves; I scored $\beta$+++. Incidentally, the syllabus for the three advanced papers on the pure side was, as far as I ever knew it, (1) Algebra and Analysis, (2) Geometry, (3) Analysis. The present-day meticulous detail of notes on the syllabus was completely absent.

There were but few awards available for graduate study in those days and in my year there were none. By great good fortune I secured the post of assistant lecturer at Bangor, North Wales; initial salary £250 per annum. I lectured there for four years, during which time I published my first research paper, married and gained my Senior Mathematical Scholarship. I was then invited by Professor E. T. Whittaker to become his senior lecturer at Edinburgh and for one year I revelled in the lively research atmosphere of the Edinburgh Mathematical Institute. Whittaker himself was the main source of inspiration; there has, in my view, never been his equal in that regard. In 1925 I returned to Oxford as a Fellow of Hertford. I was torn between a desire to continue my Edinburgh life, with relatively light teaching duties and great insistence on study and research, and the kudos of an Oxford fellowship. I chose the latter and, in spite of a much increased teaching load, contrived to publish two or three research papers a year. For the first twelve years or so of my life at Oxford I was primarily a mathematician, tutoring, lecturing and researching, serving as a secretary of the London Mathematical Society and as an editor of the newly founded (1930) Quarterly Journal of Mathematics (Oxford series); and (I cannot resist the temptation to cry my own wares) I lectured on matrices before they were admitted to the Oxford syllabus.

I avoided internal college administration for as long as I could withstand the pressure on me to become involved. In 1934 came my turn as Senior Tutor, but the duties were light and interfered only a little with my other work. A year or two later I resolved to write my first book, Convergence. Half the incentive came from the fact that I had become tired of seeing my pupils fail so dismally at understanding what it was all about. I finished the book in November 1937. In the December of that year came a turning point in my career. The Bursar of Hertford, a young economist, had been asked to serve under the League of Nations at Geneva for a period of two years and three months. I agreed, somewhat unwillingly, to act as bursar during his absence and on the strict understanding that he would resume the office on his return. Within eighteen months the war came; the Bursar did not return to Hertford, but was called to a senior post in the Cabinet Secretariat; in short, I was firmly established as the bursar of a small college. The prime movers in the founding of the Invariant Society, Whitehead and others, were out of Oxford. I was middle-aged and was left in situ. I became President of the Society and most of the wartime meetings were held in Hertford. We did nothing spectacular, but we kept the society active throughout the war.

By accident rather than by design I became, in the early days of the war, secretary of Domestic Bursars, was pressed to stand for Hebdomadal Council and, almost inevitably once my feet were set on that road, became involved in university affairs; Council, Science Faculty, General Board, the Chest and several others, less central, activities. For two years I was vice-chairman of General Board and for another few years chairman of the Chest Finance Committee. My research languished under the load. On the other hand, I soon discovered two facts about myself; the first, having written one book my fingers itched to write another; the second, whereas the multitude of administrative duties impeded my thinking about research topics, the quiet concentration on writing a book about subjects that I had already mastered was a pleasant relief from the fuss and bother of dealing with administrative problems. In short, I became a writer of text-books rather than a researcher and I often feel that I have had the best of both worlds in this matter.

The writing of my Finite Matrices (1951) was a touch more demanding. I resigned from the Chest in order to cope with it and for a while I was less occupied with university affairs; I was still bursar of my college, of course, and continued to tutor, lecture and examine. In 1959 I became Principal of Hertford. The head of a college may be called upon to do much or little; I was called upon to do much. Among other things, most of the detailed handling of the negotiations about the Indian Institute and of the Hertford appeal for funds fell to my lot. I wrote but little until my last two years of office when, with some leisure accruing, the itch to write mathematics again beset me and I started work on my Mathematics for Science. In June 1964 came retirement; I was nearly 71 years of age. Since then I have published two books, but I have now signed off. The new topics of university study, the new angle of attack on the whole subject of mathematics and the new jargon about matters that I knew of old but described in quite different terms warn me that anything I might write would be out of date and a waste of effort.

I have had some forty to fifty years of close association with Oxford mathematics—it has a quality all its own—I have crossed swords with many people, have supported and encouraged many others and can look back on a full mathematical life that I have most thoroughly enjoyed. After seven years of retirement I still read a little mathematics, some old and some new, and, with the arrogance of an aged professional, curse the writers for their occasional lapses into obscurity. If you write a textbook, you must read your manuscript at least once with the sole object of trying to misunderstand it and even then clarity will elude you!

# Annual Report of the Archimedeans

by **K. A. Moore (President)**

The year 1972-3 was again a highly successful year for the Archimedeans, much of the credit being due to the amount of work put in by committee members. The year's talks were of high standard and very entertaining.

The Friday evening meetings tended to be in traditional style, and included Dr. I. Stewart (Warwick) on Catastrophe Theory, (which has interesting applications to the sociological 'boy meets girl' problem) and a more unusual evening when Dr. J. H. Mason (Open University) turned his audience into a problem solving group!

Following the success of the first ever lunch meeting in the previous year, these replaced tea meetings for this year. Speakers included Prof. Swinnerton-Dyer talking on Tripos, past and future, in which he managed to convince us that the present system is infinitely preferable to the older forms of examination.

In the Michaelmas term the traditional visit to Oxford, to play games with the Invariants, took place and was, as usual, a most enjoyable trip. To conclude the term, an 'Archimedes Bathday Party' was held. In the Lent term the Invariants paid us a return visit, for a problems drive, which conformed completely to tradition.

Going back in time to June 1972 the annual ramble (postponed from the end of April until after the exams, owing to apathy) was well supported, and two people actually reached the destination (Stretham). Following this precedent the 1973 ramble is also taking place in June, after exams. The annual punt party to Grantchester was the usual success, with one of the punts being sunk, and for the first time a croquet afternoon took place. This is to be repeated in 1973.

Next year the Friday evening speakers include Dr. D. J. A. Walsh (Merton) on Problems of Combinatorial Optimisation, and Prof. K. Harada (SRC Visiting Fellow, Cambridge), on Classification of Simple Groups of small 2-rank. As last year's lunch meetings were so popular, these are to be continued, and speakers will include Dr. J. H. Conway on The Least Uninteresting Number and Mr. R. D. Harding on Computers in University Mathematics Teaching.

Last, but not least, the society's triennial dinner will take place next November, and Lady Jeffreys and Prof. Swinnerton-Dyer are to be our guest speakers.

# Annual Report of the Snaedemihcra

**by Andrew White (Secretary)**

For our first meeting of the year we were extremely lucky to obtain Dr. J. B. Rhine, of Duke University, to speak on ESP—Fact or Fiction? Very regretably, Dr. Rhine was involved in a motor car accident while driving up from London, and was killed. Manfully, he spoke nevertheless, but although we are grateful to him for this I must say we found his arguments in favour of ESP unconvincing.

Our second meeting was a most interesting talk from Dr Variola on Topology, Klein bottles, and Möbius Strips.

The annual Debate featured Professor Lyttleton (against) and the vocal Professor Dingle (for) the motion that 'Special Relativity is Wrong'. Professor Dingle presented the arguments which he has presented in The Times and in Nature, tending to disprove Relativity. (It will be recalled that the Editor of Nature claims to have received a different exposure of Dingle's fallacy from almost every physics Ph.D. in the country, but has declined to print any of these, feeling perhaps that Dingle's error is too silly to deserve pointing out). Professor Dingle showed a curious inability to move from one frame of reference to another so as to appreciate other people's points of view. Professors Bohr and Bondi were present, but repeated their previous public statements that they disdained to point out Dingle's error and would not comment. Dissatisfied, we questioned the slightly aetherial presences of Professors Newton and Einstein. (Although everybody knows that Newton wrote more than any one normal person could, and although everybody knows how strikingly dissimilar the four well-known portraits of him are, we had not known before he appeared that he was vector valued, like the prophet Isaiah, having four separate bodies, one to each portrait). He said that if he had seen far, it was by standing on the shoulders of giants; it seemed to him likely that God in the beginning had created him in the likeness of Isaiah, and so he would not deign to comment. A disagreement then broke out, with three of the components claiming that they could not have been created by God in the beginning because they were clearly Trinity. An infinite sea appeared by the condensation point in the corner of the room, and the fourth Newton began to gurgle and cackle over the pretty pebbles and shells on the beach announcing after a while, that—perdition on the editor of Nature—he was proprietor of nature. He did not believe in the Trinity but would not comment. Professor Einstein said that if he had seen far it was by standing on the faces of Poincaré and others; he was a simple man, and could not understand Dingle. He would not comment. In the absence of the expert opinions we wanted, the meeting declined to vote. The proof of the correctness of relativity is left as an exercise for the reader.

Dr Rubeola gave an interesting talk the week afterwards on the Königsberg Bridge problem and rubbersheet geometry, though the material was slightly familiar to us.

The following proposed joint meeting on Cardinals was cancelled at the last moment because of a misunderstanding with the Jesuit debating society over the venue.

Another confusion, however, can now be cleared up; as a few suspected at the time, the speaker at the Lucasian lecture on hydrodynamics was Ronnie Barker.

Dr P'ong almost proved to nearly everyone's complete satisfaction the almost breathtaking theorem that it was almost certainly vaguely-true that in China, all integers are equal; since the integers are countable, this is certainly true presque partout. After this there was a joint discussion (pace the Mid-Anglia drugs squad) on the Hardy-Weinberg Law with the Genetical society; we waited and waited, and waited for something to happen between our Chi-Chi test and their shaggy stuttering F-t-test, but nothing did, so we debated Is sexual reproduction necessary to maintain within-species variety? After some experiment we voted that 'sex is not necessary, but it is certainly sufficient'.

The epoch arrived for our yearly time trip. As is well known, the intuitively-desired properties of 3-dimensional measure are such that if these are given to the measure of arbitrary sets, it is possible to dissect the sphere of unit volume into two genuine, identical spheres, each of unit volume. We heard of use being made of this by a self-taught prodigy at a very early date, so thirty of us set out to visit him. This person—regretably we did not catch his name—certainly had apprehended these properties of measure, and demonstrated them rather neatly and vividly, we thought, to his lecture audiences by taking objects from the audience—we provided the contents of 'our' lunch-packet—and multiplying these without limit. Neither our bread nor our fish was in any way tainted or de-natured by replication.

Unfortunately he proved, as so many great men of humble origin do, to have rather a sensitive temper, and taking exception to some quite harmless question about his parental background, became angry, and drove us by sheer strength of his anger from our host-mind into a herd of pigs. We were so put out that we clubbed together and put a pound each towards suborning an official to get him nailed in some way. Many of us have since regretted that we have cut short a promising career so hastily.

We arrived back in good time to hear the quite fascinating talk from Dr Varicella on Pulling socks inside-out and other facets of recreational topology, although one or two aspects were quite familiar. After that we went shoplifting, and achieved 12.5 cm, a new record. Dr Papule gave a popular talk on circular stochastic queues (in which customers enter queue A for partial service and to queue B; from there to C.... and eventually from Z to A), A-level mechanics, orbits, Sturm-Liouville theory, and Green's functions, under the general heading Futility, gloom, and despair; some topics of Sartre's existentialism. It was shortly after this that our then secretary, Kevin Hohenzollern-Sigmaringen-Linden-Boggs had his tragic accident. As you will remember, he tripped over a pole in King's Parade, and falling through the slit in the imaginary cut-plane, slithered round and down and down and round the Riemann surface of log(z), corkscrewing into the previously solid ground towards infinity and scrabbling desperately to retain some grip on the frictionless surface; the party of physicists who were eyewitnesses said that it was not a pretty way to go. However, we have some good news here. A working party of philosophers have examined the matter, and assure us that this event COULD NOT HAVE OCCURRED. We have posted a copy of their report to Kevin—down the cut-plane slit—and we are sure it will be a great solace to him.

The final meeting of the year was a talk by Dr Dengue on Doughnuts, teacups and Möbius bands. Although the material was fairly familiar to us, the talk was very well attended and warmly received, which made the announcement the next morning that Dr Dengue had been found brutally murdered all the more painful, so that we are more than happy to help in correcting a rumour which has circulated. As the circumstances of the murder—Dr Dengue was wrapped in a rubber sheet, with a doughnut-shaped rubber quoit constricting his neck—have suggested an obscure sexual fetishism of some sort, we are asked to point out that contrary to what the police stated, the book on genetics in his pocket was a treatise on statistical domination.

And now, Oxford. It still seems strange to us that our carefully planned experiments in spectral representation could have gone so wrong, but, alas, they did, and I regret to announce that for the third year running the Invariant Society has declined to allow us to visit them, for fear of a repetition of the last visit. They maintained this refusal despite the evidence your committee presented to them that over thirty thousand of the people who lost their souls were undergraduates, or working class, or otherwise of no account; their whole attitude is very irrational.

As a consolation, however, we have recently acquired the body of Herman Kahn, the 50 stone American think-tank expert, who, on eating an unusually heavy meal succumbed to the gravity of his work, and collapsed below his Schwarzschild radius. With the aid of a special amplifier his unanswerable cries of anguish can be clearly heard—it will of course take an infinite time for the pain of the collapse to fade—and so we felt it would cheer everyone up to display him. Which we are doing. (Arts School, Bene't Street, 2-4 pm, admission 5p).

Finally, our president, Dr Faust, is retiring, emigrating, and has announced his engagement, after the imminent death of his first wife, to Dr H. de Troy. Dr Faust wishes to deny utterly any connexion between his emigration and the theft of the number whose name is written 2 149 623 778 426. (This slightly counter-intuitive theft is now confirmed. We do not suppose that objects whose names we can copy, such as 'unicorn', are actually present in the world, and so it should be no surprise that, although we can copy it, actual count, now completed, shows the number itself to be missing). We wish Dr Faust all the best in his new wife in Canada.

# Instabilities due to Dissipation

by **E. J. Hinch**

Dissipation is the degeneration of a useful resource. The loss of kinematic energy by friction and the loss of potential energy by the diffusion of density differences are two common examples. Inevitably there is some dissipative process which extracts the excess energy of any oscillation about an equilibrium in a straightforward system.

Intuitively one could reasonably expect that adding some dissipation to an already stable system, i.e. a system in which disturbances always decay, would only enhance the system's stability. But is stability naively additive? No; the real world can be more cunning than one's immediate intuition, as the examples below will reveal. A stable equilibrium need not be quite so straight forward, but may represent a precarious balance in which the stabilizing influences exceed some constrained destabilizing forces. The addition of dissipation can possibly do more damage by upsetting the delicate part of the balance than by strengthening the stabilizing factors in other less important parts of the balance. The destabilizing forces can be released from their constraint without being overwhelmed by the additional dissipation.

To produce a counter-example to our misleading intuition we need only look to linear first order systems. With the state of such a system described by a vector x, the appropriate evolution equation is a linear relation between the rate of change of the state vector and the state vector itself.

$$\dot{x} = Ax$$

Most physical systems can be put into first order form with some ingenuity, in general with a nonlinear operator A. Linear systems are relevant as approximations for small disturbances about an equilibrium. The concrete examples will be finite-dimensional where x can be thought of as a column vector and A a square matrix.

A system will be called stable if the real parts of all the eigenvalues of the operator A are negative. Any disturbance will eventually decay in such a system. A system will be called dissipative if A is stable and is self-adjoint, in which case all the eigenvalues are real and negative. The extra condition of self-adjointness ensures the bilinear form xAx is negative definite, or in physical terms dissipation extracts energy.

We now disprove the proposition that given a stable S and a dissipative D then $A = s + \mu D$ is stable for all positive scalars $\mu$. The eigenvalues are certainly not additive in general. [In fact one should be prepared for the most unpleasant parametric dependence of eigenvalues.] One can establish that if $\mu$ is small then A is near S and stable, and if $\mu$ is large then A is near $\mu D$ and stable. This is, however, no guarantee that A is stable at intermediate $\mu$.

Consider the two dimensional counter example

$$S(w) = \begin{pmatrix} -4 & -w \\ w & 2 \end{pmatrix} \qquad D = \begin{pmatrix} -4 & 0 \\ 0 & -1 \end{pmatrix}$$

D is dissipative, and S is stable if $w^2 > 8$. If S is not too stable, $8 < w^2 < 9$, then A is unstable at the intermediate values of $\mu$, $(2\mu - 1)^2 < 9 - w^2$.

The trick in this example is the spin w of S—its antisymmetric part. An exercise left to the reader is to show that A would be stable if S were symmetric, i.e. dissipative. Without the spin, the chosen S would be unstable through one normal mode although it is more stable in the other mode. The addition of the spin constrains the instability, sufficient to suppress it if $w^2 > 8$. The essential effect of the spin is seen at slightly higher values, $w^2 > 9$. Beyond this critical value both modes rotate about the origin as they decay. As they rotate they average out the stabilizing and destabilizing forces, each mode therefore decaying with its half of the net stability $\frac{1}{2}(-4 + 2) = -1$. The additional dissipation was chosen to enhance the basic stabilization more than it detracted from the basic destabilization. If the spin is stabilizing but not too strong, $8 < w^2 < 9$, some strengths of the additional dissipation can arrest the spin and release the inherent instability from its spin constraint without being sufficiently strong to stabilize the system.

A physical example of dissipative destabilization is the phenomenon of salt fingers. Salt-fingers are an important mixing process of heat and salt in the oceans. An analogous mechanism mixes hydrogen and helium in certain parts of stars. The problem involves hot salty water overlying cold fresh water. In the initial system the stabilizing temperature effect on the density exceeds the destabilizing salinity, so that the heavier water is underneath. The addition of thermal dissipation, which tends to equalize temperature differences and acts much faster than the diffusion of salt differences, can release the constrained destabilizing influence of the salt and lead to unexpected vigorous mixing. A model of this process along the lines of the counter-example may be easily constructed with three state variables; the vertical velocity of a blob of water along with its temperature and salinity excesses above the surrounding water.

# Randomly Collecting Sets of Objects, or . . . it's another Spear-Thrower!

**by Colin Vout**

In the days when you used to find plastic Red Indians in cornflake packets, I had a very great difficulty in obtaining a full set. By the time the sixth different one had turned up we had about eight spear-throwers. It has long been my desire to wreak vengeance on these free gifts, and what better way than to write a mathematical article on them?

It is easy to calculate the expected number of packets before you get a full set. If there are m objects in the set then the expected waiting time for the first different Red Indian after having i of them is $m/(m-i)$; so the expected number of packets needed for a full set is $m \sum_{i=1}^{m} \frac{1}{i}$, which for large m is about $m(\log m + \gamma)$, where $\gamma$ is Euler's number. For six Indians it is 14.7.

The recurrence relation for the probability of having i Indians from a set of m after n packets is

$$P_{i,m,n} = \frac{m-i+1}{m} P_{i-1,m,n-1} + \frac{i}{m} P_{i,m,n-1}$$

to which the solution is

$$P_{i,m,n} = m!\ V_{i,n}/(m^n(m-i)!)$$

where $V_{i,n} = \frac{1}{i!} \sum_{r=1}^{i} \binom{i}{r} (-1)^{i-r} r^n$; this satisfies $V_{i,n} = V_{i-1,n-1} + iV_{i,n-1}$.

One can obtain a rather interesting identity by evaluating the expected waiting time until a full set is achieved, by both methods. Note that the probability of achieving a full set on exactly the nth packet is $\frac{m!}{m^n} V_{m-1,n-1}$. Then we derive

$$\sum_{s=1}^{m} \frac{1}{s} = \sum_{n=m}^{\infty} \sum_{r=1} \frac{n}{m^n} \binom{m-1}{r} (-1)^{m-1-r} r^{n-1}$$

The numbers $V_{in}$ themselves are interesting. When written out a triangle they appear to exhibit the same property as Pascal's triangle: that the numbers (except those at the ends) in the nth row are all divisible by n if and only if n is a prime. It is simple to show that in this triangle the 'if' holds. By Fermat's theorem, $r^{n-1} \equiv 1$ (modulo n); and since we can write

$$V_{in} = \frac{1}{(i-1)!} \sum_{k=0}^{i-1} \binom{i-1}{k} (-1)^{i+1+k} (k+1)^{n-1}$$

where $r = k + 1$, we have

$$V_{in} = \frac{1}{(i-1)!} \sum_{k=0}^{i-1} \binom{i-1}{k} (-1)^{i+1-k} = 0$$

The proof of the 'only if' is that if n divides all the numbers in the nth line then $2^{n-1}, 3^{n-1}, \ldots, (n-1)^{n-1}$ are all $\equiv 1 (\text{mod } n)$ which is impossible if n has any factors.

1

1 1

1 3 1

1 7 6 1

1 15 25 10 1

1 31 90 65 15 1

1 63 301 350 140 21 1

. . . . . . . . . . . . . . .

. . . . . . . . . . . . . . . . .

1 1023 28501 145750 246730 174487 63487 11880 1155 55 1

From this triangle we find that the median of the distribution of the probability for six different items in a set occurs between the twelve and thirteen packets, about two packets lower than the mean. After eighteen packets there is a four-fifths chance of having a full set.

All this working assumes a random distribution of Indians in cornflake packets. The psychological and game-theoretical complications of avoiding spear-throwers intent on capture would make a more detailed analysis almost impossible.

# Problems Drive

Here are some of the questions from the 1973 problems drive, the casus belli for the annual battle against the Invariant mathematicians of 'the other place'. The questions were set by D. J. Aldous and N. H. G. Mitchell. The answers are on page 49.

1. There were 8 entrants for the prison tetrathlon (which consisted of 4 events: pole vault, pistol shooting, wall climbing, mailsack race). In each event, the winner got 7 points, the second 6 points, etc. There were no ties. The rules for choosing the winner(s) were as follows:

For all pairs
(i) if A beats B in 3 or 4 events, B is eliminated.
(ii) Of those remaining, if A has a larger total of points than B, B is eliminated.
(iii) Of those remaining, if A's sum of squares of points is larger than B's, B is eliminated.

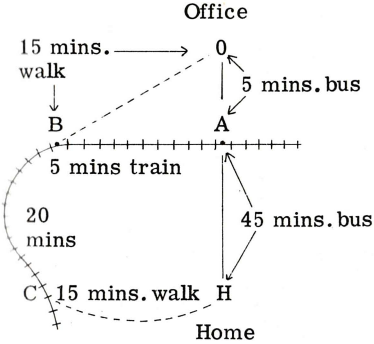
(a) How many prizes did the prison Governor have to buy, in order to be sure that there would be one for every winner?
(b) How many, at most, could he have been left with after the prizegiving?

2. A circle drawn on the Earth's surface is said to be imperial if the Sun never sets on it, i.e. at all times throughout the year, there is some point on the circle at which the Sun is above the horizon. The Earth is a large sphere of radius R. The angle between the equatorial plane and the plane of Earth's orbit is $\theta$.

(a) Find the length of the shortest imperial circle in terms of R, $\theta$.

(b) Specify an imperial circle which meets every other one in 2 points.

3. Mr H. Urry works irregular hours in the City, finishing some time (at random) between 4 and 7 p.m. He always tries to get home as quickly as possible. The available transport is as shown:




Trains leave A for B and C at 10 and 40 minutes past each hour.

Buses run from 0 to A and H, and Mr. Urry has found from experience that the waiting time for a bus is T minutes, where T is random in $0 \leqslant T \leqslant 15$, with uniform distribution.

How often does he catch a train?

4. In this alphametric, which is in the scale of 7, each letter stands for a different digit.

```
D A M T P +
D P M M S =
-----------
M A T H S
```

$(S \neq 3)$

Convert into decimal notation: (a) T M S

(b) A D A M S

5. Current first class postage rates are as shown:

Every week, a Scotsman sends a haggis by first class post. He minimizes the postage costs by subdividing the haggis into several packages if necessary, but naturally he never cuts the haggis into more pieces than necessary. A haggis may weigh any whole number of ounces up to 5 lbs.

| | | |
|---|---|---|
| Not over | 2 oz | 3p |
| | 4 oz | 4p |
| | 6 oz | 6p |
| | 8 oz | 8p |
| | 10 oz | 10p |
| | 12 oz | 13p |
| | 14 oz | 15p |
| | 1 lb | 17p |
| | 1 lb 8 oz | 24p |
| | 2 lb | 34p |
| Each additional | 1 lb | 17p |

Find: (a) The largest weight for which the haggis goes in 2 packages.

(b) The smallest weight for which the haggis goes in 3 packages.

(Ignore cost and weight of wrappings).

6. When the Professor woke up surrounded by 3 savages, he naturally wondered if he was in danger of being eaten. He quickly realised he was on the mystical island of Satisrevinu, where there are two indistinguishable tribes (who never intermarry), the Ivyleaves, who always tell the truth, and the Redbricks, who always lie. He elicited the following statements:

A: B is my brother.
C is not my brother.

B: I have no brothers.
There are no cannibals on this island.

C: A has no brothers.
B is the brother of no-one here at present.

Was he in danger of being eaten, and who was who's brother?

7. An Antigroup, A, is a subset of the strictly positive integers such that: If x, y are members of A, then x + y is not a member of A.

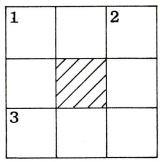
Find the greatest n for which the set $\{1, 2, 3, \ldots\ldots, n\}$ may be split up into three antigroups, and exhibit such a division.

8. Laura Biding is driving along a clear road at 30 m.p.h. (the legal limit) and approaching a green traffic light, which may start to change at any time. She knows that her reactions are good, that her car's maximum braking and acceleration are 5 and 3 m.p.h./second respectively, and that the amber light is illuminated for 3 seconds at a time. She always drives as fast as possible, subject to speed limits and never passing a traffic light when the red light is on.

(a) The light does in fact stay green. At what speed does she pass it?

(b) How many seconds would she have been delayed if the light had been red as she approached, not starting to change until she reached the traffic detector pad 33 feet before the light?

9. The solution to this cross-number puzzle (in base 10) contains every non-zero digit with one exception. What is the exception?




Clues: Across: 1. See 3 across
3. Eight times 1 across
Down: 1. Perfect square
2. Not divisible by three.

10. Following Britain's entry into the Common Market, chess, like everything else, has been decimalised, and is now played on a 10 by 10 board. Each player has two extra pawns, and two new pieces, known as Deans, which can move either like a Bishop or like a Knight.

(The pieces are shaped like sherry bottles, and start off between the Bishop and the Knight.)

(a) Find the maximum number of Deans which can be placed on the board without any Deans attacking any others.

(b) Find the minimum number of Deans necessary to occupy or attack every square on the board, and draw a diagram for this case.

# Some Critical Points

**by Manfred Gordon**

Variants of the statistical model here described have widespread applications in science and technology. I will touch on three applications because of their philosophical interest.

Consider first a problem posed by the geneticist Francis Galton about 100 years ago: the chances of survival of a family name. A simplified model makes the following assumptions:

(a) The medieval first bearer of a newly invented surname was allowed exactly f trials to produce a male offspring to carry on the family name, and

(b) each of his descendents (if any) in subsequent generations was, is, or will be allowed exactly f — 1 such trials before his death.

(c) Multiple male births are forbidden.

(d) A trial not resulting in a male birth is called fruitless (e.g. quintuplet girls).

(e) The chance $\alpha$ that a trial leads to a male birth is a constant, a measure of fertility of the members of a given family.

I am here concerned with statistics. The reader interested in the individual trials is referred to the relevant literature[1].

The founder of a family name forms the sole male member of generation zero of his family tree (fig 1). His male children (if any) form generation one, his male grandchildren (if any) generation two, etc. The expected (average) size $\langle T \rangle$ of a family tree is readily found for our model:

$$\langle T \rangle = \sum_{i=0}^{\omega} \langle n_i \rangle \tag{1}$$

where $\langle n_i \rangle$ is the average number of members of generation i in a family tree with a fixed f and $\alpha$. And the $n_i$ are readily evaluated: $n_0 = 1$ because exactly one man starts the family, and $n_2 = f\alpha$, because he performs f trials, each with chance $\alpha$ of bearing fruit. The expected number $n_2$ of the founder's grandsons is $f\alpha[(f-1)\alpha]$, because each of his sons is allowed $(f-1)$ trials with chance $\alpha$ of bearing fruit. And so on:

$$\langle T \rangle = 1 + f\alpha + f\alpha[(f-1)\alpha] + f\alpha[(f-1)\alpha]^2 + \ldots \tag{2}$$

or (summing the geometric series):

$$\langle T \rangle = \frac{1-\alpha}{1-(f-1)\alpha} \tag{3}$$

Let us consider f fixed and examine how the average size $\langle T \rangle$ of a family tree depends on the fertility parameter $\alpha$. The function $\langle T \rangle(\alpha)$ obviously has a singularity as $\alpha$ approaches from below the critical point:

$$\alpha_c = 1/(f-1) \tag{4}$$

This critical point is readily interpreted qualitatively. The mean size of family tree will be finite if a typical member of the tree (not the founder) produces on average $(f-1)\alpha < 1$ sons, i.e. if $\alpha/\alpha_c < 1$. In such a case a member of the tree fails even to reproduce himself adequately on average, and the name must die out. Even a fertility given by $\alpha = 0.99\alpha_c$ rarely produces a family which will last for 100 generations. On the other hand, if $\alpha = 1.01\alpha_c$, the average size of the family trees is infinite. This does not mean that any individual such family cannot die out. But each family then has a finite chance that its name will go on forever. This chance can be calculated to be, for example, 0.060 when $\alpha/\alpha_c = 1.01, f = 3$. Thus the critical fertility $\alpha_c$ is aptly named—it has a critical effect on possible family histories.

This singularity or divergence of $\langle T \rangle$ is typical of the class of cascade or branching processes which produce a kind of family tree by repeated trials in each generation, with some parameter corresponding to fertility. Such processes are of the Markov type. The existence of a singularity is by no means linked to the peculiar mating rule we have imposed. The reader is bound to have wondered why we propose to restrict each male to $f - 1$ matings, with an extra bonus for the founder of a name. (Indeed, even in a country with a free press, how is obedience to such a rule to be checked?).

The rule was chosen to produce family trees of a special simple structure. Fig. 1 brings out that our trees have points of only two possible valencies: each point of valency f, called a node, represents a male individual; each point of valency unity, called a terminal, represents one or more females, or just a happy memory. This simple tree structure becomes specially appropriate when we switch the discussion of our model from genetic to chemical applications.

The notion of valency of a point comes into its own in chemistry. Indeed, equation 3 was derived by P. J. Flory, the distinguished physical chemist, when he first identified the nature of the gel-point of a jelly-producing liquid about thirty years ago. Galation is a critical phenomenon which happens with great suddenness in time, or at a sharply defined temperature. Flory's explanation fits exactly the family-tree type of model described, and experiments amply confirm the model. At this point, I interrupt the argument to provide a

Crash Course in Chemistry for Mathematicians

An atom is a point. A bond is a line. After that, one gets by with ordinary graph theory.

**The Nature of Gelation**

In reality, chemistry is far simpler than the crash course has made it appear: large groups of atoms can be contracted to a single, artificial atom, by means of graph theory, because graph theory literally allows us to make rings around chemistry. As shown in figure 2, a typical chemical jelly-former or 'monomer' is reduced, in two stages—to the civilised form of a graph, on the right, which contains all information relevant for our purpose.

A large number of such monomer graphs represent the liquid in the beaker at the top of fig. 3. In presence of a little acid catalyst a process can be switched on, whereby the graphs begin to aggregate into the tree-like graphs shown at the bottom beaker of fig. 3. Here the formation of a chemical bond (actually formation of an ether group by elimination of water) is represented by the elimination of two terminals plus the insertion of a line between the two nodes concerned, to restore their valencies to $f = 3$. This process goes on many times, linking the graphs together quite at random; and the reverse process also starts up gradually, because bonds can split as well as form. Actually, the bottom of fig. 3 is supposed to represent the dynamic equilibrium which will exist when the bond formation and bond splitting rates have come into balance, and each kind of topological tree has reached a steady total number (concentration). The fraction of terminals which have been eliminated in passing from the starting state in the top beaker to the equilibrium state in the bottom beaker is denoted by $\alpha$. Now choose a node at random in the bottom beaker and plant it as the root of the family tree of which it forms part—e.g. the node marked x could become the root node of the tree in fig. 1. The reader will readily check

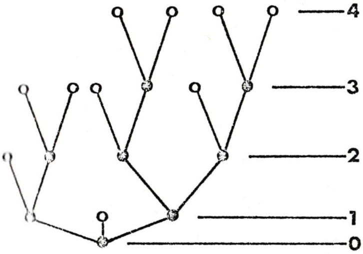



Fig. 1 Typical family tree for f = 3. Full circles: male individuals. Empty circles: fruitless trials.

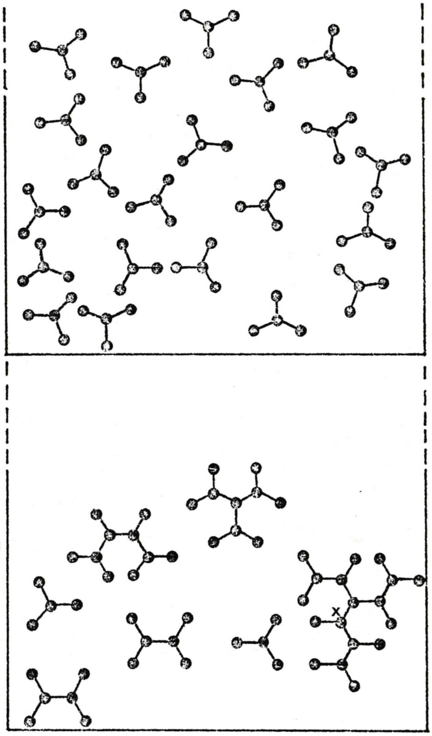
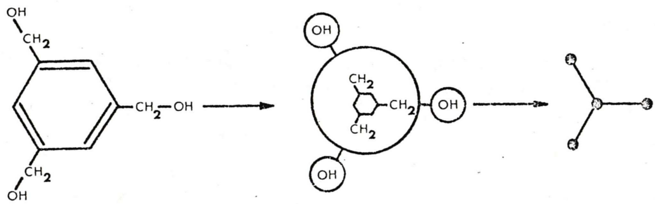



Fig. 2

Fig. 3




that the genetic model from which we derived eg. 3 shares with the chemical model of fig. 3 the same distribution of family trees, and therefore eg. 3 applies to both.

By adjusting temperature or pressure, the chemist can select the equilibrium state between bond formation and splitting, i.e. he can control $\alpha$. If $\alpha < \alpha_c$, he will obtain a liquid; if $\alpha > \alpha_c$, a jelly. At $\alpha = \alpha_c$ the system is just at the critical point, the gel point. It is easy to explain the physical phenomena qualitatively.

The resistance to flow of a liquid—its viscosity—is a measure of molecular friction. The friction that any one atom experiences is proportional to the size of tree of which it is a part: as the atom moves through the flowing liquid, it has to drag the rest of the molecule along. Thus the viscosity is proportional to the mean size $\langle T \rangle$, i.e. the mean number of nodes, of a tree to which a randomly atom (node) is attached. Thus the viscosity of a liquid is finite if $\alpha < \alpha_c$ (eg 3). It rises steeply as $\alpha$ approaches $\alpha_c$, and there it diverges: all flow stops at the gel point. As $\alpha$ increases further, beyond $\alpha_c$, the jelly becomes progressively tougher, its elastic (Young's) modulus rises rapidly. This modulus is calculated from graph theory also, but this is a little more advanced.

Life processes are generally observed to occur in the critically branched state of matter, i.e. close to, and on either side of, a gel point. If we look at the inside of a cell, e.g. a raw hen's egg, we find just that messy state, somewhere between a highly viscous liquid and a weak jelly.

If we study a chemical model system, such as our monomer in fig. 2, we find that at $\alpha = 0.99\alpha_c$, the liquid has a viscosity somewhere around 10 poise, about that of the cytoplasm of an amoeba. At $\alpha = 1.02\alpha_c$, the jelly has a modulus around $10^5$ dyn/cm$^2$, about that of a jelly-fish. It is easy to explain[2] why invariably Life is in such a mess, but this is not the place. Suffice it to say that the gel point is closely involved in the problem of the origin of life.

### A sobering thought

I turn to my third application, after genetics and chemistry. Let the root of the tree in fig. 1 represent a neutron, which starts a chain reaction producing a cascade of neutrons. Each of the neutrons in turn can produce a litter of further neutrons on the next generation, before its own energy is exhausted. Here the critical point is controlled, not by temperature or pressure, but by the critical mass of the neutron source. A fertility of $\alpha = 0.99\alpha_c$ represents an efficient nuclear power station. But $\alpha/\alpha_c = 1.01$ ends my story, not with a whimper, but with a bang.

To sum up all three applications: Critical aspects of the problem of survival of human family trees were statistically foreshadowed at the moment at which Life began on earth, and foreshadow how it may end.

The author most warmly thanks Professors D. G. Kendall and P. Whittle for hospitality, and Corpus Christi College for a Visiting Scholarship, in 1972.

### References

(1) But don't queue to see the film: it's a wash-out.

(2) M. Gordon, T. C. Ward, and R. S. Whitney. Polymer Networks, A. J. Chompff and S. Newman (Eds.), Plenum Press, New York and London 1970, 1.

# Notational Note

**by D. J. Miller**

Progress, in the matter of mathematical notation, is a slow business. New topics, obviously, develop a working notation at an early stage, but there is great conservatism among mathematicians with regard to the notation of traditional topics, despite the immense saving in time, effort, and obscurity that an innovation may give. Consider how slowly the notation 'iff' for 'if and only if' is advancing; only a small proportion of standard textbooks use it. This particular word should be a help to any subject in which rational arguments are written, whether scientific or literary, and cut out much otiose verbiage explaining that an implication can be reversed. Mathematicians have extended the principle to give 'onnce' (once and only once), 'inn', and others. Let us hasten the day when historians begin 'RRussia, inn the thirties...' and can miss a sentence about how conditions were different at all other times and in all other places, or 'Dr SSmith takes the view that....' and miss an unkind note that his view is entirely idiosyncratic. I hope you find this suggestion interesting, and not just innteresting.

The writing of $|x - y| < \epsilon$ as $x \underset{\epsilon}{=} y$ is a convention which could also do with rather wider use in mathematical textbooks, especially when more complicated expressions are near-equated. The Frege assertion sign '$\vdash$' can usefully be used for 'required to prove' in one's own working, and the corresponding denial sign '$\dashv$' should be used at the beginning of a reductio ad absurdum with the contradiction sign $\#$ at the end. A preliminary check of a convoluted proof, with contradictions within contradictions, would be to check that every $\dashv$ paired off with a $\#$.

Notation should, if possible, induce truth, in the sense that the ratio of dy to dx is dy/dx, which is 'obvious' only in, and because of, this notation. In vector spaces, therefore, if we use $\llcorner$ and $\angle$ for the much-written words 'independent' and 'dependent', then the use of of $\urcorner$ for 'spans' (as in $\{x_1, x_2, \ldots\ldots\} \urcorner V$) suggests that we could write $\{x_1, x_2, \ldots\ldots\} \sqsubset\!\sqsupset V$ for '$\{x_1, x_2, \ldots\ldots\}$ is a basis of V'.

Other much used words are eigenvalue, eigenvector, and eigenfunction, which could clearly be abbreviated $\not\lambda$, $\not\#$, and $\not f$, rather than 'e-value' &c. as they are at present. 'Open' and 'closed' in topology call for abbreviation not by their length, but by their frequency of use. A general principle has been suggested, that one should write an operator with two short vertical lines through it to indicate the adjective 'invariant under (operator)', but this seems too clumsy in this particular case, and makes 'closed' look rather like 'pi' if one is not too careful.

This note will have served its purpose if it encourages anyone to take an initiative rather than treat the present conventions as if they were handed down on tablets of stone. (Send your ideas to Eureka if you wish to make a bid to change the present conventions).

# Ode to the Negative Gaussian Curvature of Potato Crisps

**by Colin Vout**

Of all the unsolved problems that
  Confront us in this world,
The biggest mystery to me
  Is: why are crisps so curled?

Their curvature is negative;
  Whichever way they went
At first, you'll find to compensate
  They're oppositely bent.

Now, when it's plunged into the oil
  How does a crisp react?
Does it expand, and buckle up,
  Or does the thing contract?

Or does it seek to minimize
  Its surface area?
So, like a soap-film, there would be
  No max- or minima.

Considering an element:
  If forces balance out,
Then $\nabla^2$ crisp is zero and
  The same thing comes about.

But still, some crisps have, locally,
  A curving more than nought;
Though by and large its sign will be
  A minus, as it ought.

Imagine, now, the heated vat;
  The oil begins to bubble—
A sliced potato enters and
  Proceeds to bend up double.

Perhaps uneven heating makes
  One side shrink rather more;
Then if the crisp should overturn
  It curls up, as before.

And so the curve is negative;
  But now we wonder what
Would happen should it not reverse,
  For then the curve is not.

And anyway the temperature
  Is constant, I feel sure;
For otherwise some overcook
  While other crisps stay raw.

Or do the bubbles, rising up,
  Distort the crisp that way?
But crisps are sometimes bent in half;
  Explain that, if you may.

The facts we know about the world
  Should help us pass this hurdle;
Or is this an example of
  The theorem proved by Gödel?

O Archimedes, answer this,
  Our patron and our hero:
Why is the curvature of crisps
  So clearly less than zero?

(Footnotes: (i) In mathematick verse/When just for hell you're dabbler/The triangle inverse/Should be a $\nabla$ not $\nabla$.

(ii) A physicist has suggested a reason for the phenomenon in question is that the inner part of a potato contains more water than the outer; therefore it shrinks more on cooking; therefore there is in effect a circular frame holding the surface within it open; therefore the crisp is analogous to an open universe; and it is well known that a universe is open iff its Gaussian curvature is negative or zero. (How about a general cosmology of crisps?)—Ed.)

# Tomorrow is the day after Doomsday

**by J. H. Conway**

Lots of people have produced rules for working out the day of the week corresponding to any given date. One need merely add components for the century, year of century, month, and day of month, reduce modulo 7 and then start counting at the right place. But since the month components are essentially random numbers most people soon forget the rule. [In the version known as Zeller's congruence the month numbers are produced by a uniform formula, but this is just complicated enough to be easily forgotten.]

The Doomsday rule is one I worked out last year in an attempt to overcome these difficulties. In it one computes Doomsday for the given year and then computes dates in that year relative to Doomsday. The rule has the additional advantage that when one knows Doomsday for the year one has in effect the complete calendar for that year at one's fingertips—so all the man in the street need do is remember to update Doomsday at about the time he remembers to put the new year on his cheques.

## Doomsdays in a given year

Doomsday for a given year is defined to be the day of the week on which the last day of February falls. For 1973, Doomsday is Wednesday. The displayed table shows how to find a Doomsday in any given month. In February we see that a Doomsday is Feb 28 or 29 according as the year is ordinary or leap. In January, a Doomsday is Jan 31 or 32 in the same circumstances. (Of course Jan 32 = Feb 1, but we should think of it as Jan 32.)

**DOOMSDAYS**

Jan 31/32
Feb 28/29
Mar 3 + 4 = 7
Apr 4
May 5 + 4 = 9
June 6
July 7 + 4 = 11
Aug 8
Sep 9 − 4 = 5
Oct 10
Nov 11 − 4 = 7
Dec 12

Otherwise a Doomsday in the nth month is the nth day, if n is even, and the (n ± 4)th day, if n is odd. The sign is + for long odd months (31 days), and—for short ones (30 days), and it is fairly easy to remember than the only short odd months are September and November.

Summary: 'Last' in Jan and Feb, otherwise nth in even months, (n ± 4)th in odd ones.

By adding and subtracting 7s we can find other Doomsdays in these months, and then by the 'last-friday-was-the-twentyfirst-so-today-is-the-twentyfifth' technique we can locate any particular date.

## Examples.

August 19 1973? August is the 8th month, so that August 8th, and therefore August 22nd are Doomsdays (Wednesdays in 1973), so August 19th is a Sunday.

September 24th? September is the 9th month, and is short, so September 9 − 4 and September 26 are Doomsdays, so September 24th 1973 is a Monday.

If you do things exactly this way you will gradually remember more and more Doomsdays throughout the year. Don't say things like Sep 5 = Doomsday, 24 − 5 = 19 ≡ −2 (mod 7), so Sep 24 = Doomsday − 2. This kind of calculation is prone to sign errors and does not help you to accumulate more mental Doomsdays.

## Doomsdays for the century years.

One first needs to know Doomsdays for the century years. All the practical man need know is that Doomsday for 1900 was a Wednesday. (We say simply '1900 was a Wednesday'.) However, we assert that in the Julian system (which was used in England before September 1752) the years 0, 700, 1400, 2100, ... are Sundays (they were more Godly then!), and that each century after one of these retards Doomsday by 1 day. In the Gregorian system (after September 1752) 0, 400, 800, 1200, 1600, ... are Tuesdays, and each century after the most recent of these retards Doomsday by 2 days. In particular, 1900 = 3 centuries past 1600 = Tuesday − 6 = Wednesday, as we asserted.

## Doomsdays for years in a given century.

Each ordinary year has its Doomsday 1 day later than the previous year, and each leap year 2 days later. It follows that within any given century a dozen years advances Doomsday by 12 + 3 = 15 days ≡ 1 day. ('A dozen years is but a day.') So we add to the Doomsday for the century year the number of dozens of years thereafter, the remainder, and the number of fours in the remainder. It is easiest to say these numbers aloud, as in the examples. Remember, 1900 = Wednesday.
1946? We have 46 = 3 dozen and 10, so we say
'1946 = Wednesday, 3 dozen, 10, and 2 = Thursday.'

(Thursday being found by adding 3, 10, and 2 to Wednesday.)
1973? Wednesday, 6 dozen and 1 = Wednesday, as we know.
1990? Wednesday, 8 dozen, 5, and 1 = Thursday.

1752 September 2 (Julian)?

1400 = Sunday, so 1700 = Sunday — 3 = Thursday, so
1752 = Thursday, 4 dozen, 4, and 1 = Saturday, so
September 5 would be Saturday, and September 2 = Wednesday.

1752 September 14 (Gregorian)?

1600 = Tuesday, so 1700 = Tuesday — 2 = Sunday, so
1752 = Sunday, 4 dozen, 4, and 1 = Tuesday, so
September 5 and 12 would be Tuesdays, so September 14 = Thursday.

In fact in this country Wednesday September 2 and Thursday September 14 1752 were consecutive days, since this was the year we changed from Julian to Gregorian.

**Note on changes in the calendar.**

The Julian system in which every fourth year has an extra day was introduced by Julius Caesar on the advice of the astronomer Sosigenes. For some time the calendar had been at the mercy of Roman officials who more or less arranged things to suit themselves, and there had, for instance, even been one year with a month of 45 days. Sosigenes recommended a regular alternation of 30 and 31 day months which would have been adhered to had not both Julius and Augustus Caesar needed the months named after them to have 31 days, which was achieved by breaking the regular alternation and shortening February still more.

The Gregorian system, in which years divisible by 100 but not 400 are not leap years was introduced by Pope Gregory XIII. Roman catholic countries changed in 1582, but protestant countries resisted this piece of popery for several hundred years, and then changed at various times. Some eastern European countries changed only this century. Sweden managed the change most elegantly, by simply omitting all leap years between 1800 and 1840 inclusive. So in working out dates for the intervening period, one must be sure where one's problem originated.

The reason for the change was of course that the astronomical year has 365.2422 days rather than $365\frac{1}{4}$, and the inaccuracy had gradually accumulated until it was 10 or 11 days. There is still a residual inaccuracy which many people have remarked would be partly cured by making the years divisible by 4000 not leap years, but with any luck the whole ungainly system will be dead by then!)

Another annoyance is that the conventional starting date for the year has not always been January 1 (as we have supposed in the Doomsday rule). A number of different dates have been used at various times, even in this country. Jan 1 and Dec 25 (of what we should call the previous year) were both used at about the end of the first millennium, but March 25 then became more or less universal. So for instance March 24 1583 and March 25 1584 were consecutive days.

This convention for starting the year is the Old Style, the January 1 convention being the New Style. Unfortunately these terms are often used incorrectly to refer to the Julian and Gregorian systems, since in fact the Act of Parliament establishing the Gregorian system in England also finally decreed that the New Style was henceforth to be used for all legal purposes.

In fact the change from Old to New style dating had been accomplished long before. From about 1600 to 1700 opinion had gradually hardened in favour of the New Style. In the changeover period we usually find the double dating convention—thus February

14 $166^5/_6$ denotes the date which would be Feb 14 1665 (Old Style), and Feb 14 1666 (New Style). The situation is further complicated by the fact that dates in history books have often been transposed into New Style even when they refer to periods when Old Style was the only one in use. Also, dates in English history books for the deaths of French kings (say) might be in either the Julian or Gregorian system for the period between 1582 and 1752, according as the original source was English or French.

The fact that for various financial purposes the year starts on April 5 is an interesting consequence of the various changes. Originally it started in the first day (March 25) of the calendar year. When New Style was adopted, this remained the start of the financial year, although no longer the start of the calendar year. When we changed from Julian to Gregorian, this became April 5, since obviously no one was to pay a full year's interest on a year that was eleven days short!

So apply the method for historical dates with some caution. But the rule really comes into its own for dates within any given year. Throw your calendar away after a quick glance to find the Doomsday, for when you know Doomsday you will know it all! But when you impress your friends with the Doomsday rule, remember to give credit where credit is due! I do this now by noting that I found the Doomsday rule by simplifying (almost beyond recognition!) a rule given by Lewis Carrol in Nature, 1872.

# Triangles

**by Bernard Silverman & Paul Marx**

**Problem**

There are 100 points, no 3 collinear, on a piece of paper. We consider each combination of 3 of the points as the vertices of a triangle. Find an upper bound ($<1$) for the proportion of acute- (excluding right-) angled triangles in the set thus formed.

**Investigation**

Suppose $\lambda$ is such an upper bound for sets of m points. Let S be a set of n ($>$ m) points. We consider separately each of the ${}_nC_m$ subsets of S consisting of m points. Each gives ${}_mC_3$ triangles, of which we know at most $\lambda.{}_mC_3$ are acute. So altogether we have ${}_nC_m.{}_mC_3$ triangles, of which at most ${}_nC_m.\lambda.{}_mC_3$ are acute. But of course we have counted each triangle exactly ${}_{n-3}C_{m-3}$ times. So considering S as a whole, we have ${}_nC_m.{}_mC_3/{}_{n-3}C_{m-3} = {}_nC_3$ triangles of which at most ${}_nC_m.\lambda.{}_mC_3/{}_{n-3}C_{m-3} = \lambda.{}_nC_3$, i.e. a proportion $\lambda$, are acute. So $\lambda$ is also an upper bound for sets of more than m points, such as S.

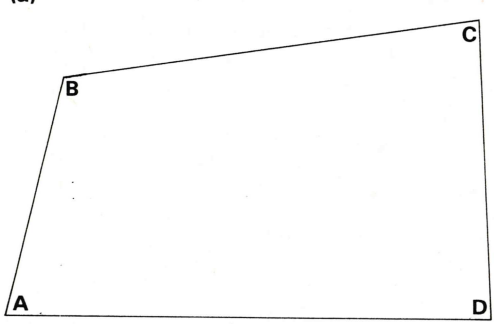
Now of course $\lambda.{}_nC_3$ need not be an integer, whereas the maximum number of acute triangles must be. So our upper bound for n points can be improved to $[\lambda.{}_nC_3]/{}_nC_3$ (where [ ] represents truncation).

We will get out best result by doing as many truncations as possible, which means considering one extra point at a time. Thus we obtain the recurrence relation:

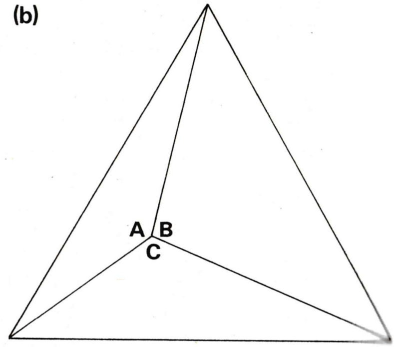
$$\lambda_n = [\lambda_{n-1}.{}_nC_3]/{}_nC_3 \qquad (*)$$

We now look for a suitable initial value for the recurrence relation. Let us consider

(a)




(b)




a set of 4 points, which will form either a convex quadrilateral (a) or else a triangle with 1 point inside it (b).

In (a) we have $A + B + C + D = 2\pi$; in (b) we have $A + B + C = 2\pi$. In either case there must be at least one non-acute angle, and so at least one of the four triangles must be non-acute. So we can take $\lambda_4 = \frac{3}{4}$.

Let us now attack the recurrence relation. Put off by the [ ] function we consider its general properties. Clearly we have a non-increasing sequence, and numerical evidence suggests very strongly that it converges to $\frac{2}{3}$. Encouraged by this we play with the first few numerical values and obtain: $\lambda_n = (2/3)(1 + (n - (n \text{ rem } 3))/n(n-1)(n-2))$ (where n rem 3 represents the remainder on division of n by 3).

By substituting this in (*) we verify that this is in fact its solution. So for the original problem we have: $\lambda_{100} = 3267/4900$.

Is this the best we can do? Can these upper bounds be attained? By drawing diagrams and doing a small computer search we obtained the following results:

| n | $\lambda_n \cdot {}_nC_3$ | largest number of acute triangles observed |
|---|---|---|
| 4 | 3 | 3 (see (a)) |
| 5 | 7 | 7 |
| 6 | 14 | 14 |
| 7 | 24 | 22 |
| 8 | 38 | 32 |

So this is still an open question. If one found that in fact one of the upper bounds could be reduced, all the succeeding upper bounds could also be reduced using (*).

Alternatively, we can try to construct a sequence of diagrams containing increasing numbers of points, for which we have a general expression for the proportion of acute triangles in terms of the number of points. This will give us, so to speak, lower bounds for the upper bounds.

As an example, consider the regular polygons. Here we have a proportion of acute triangles which is $\frac{1}{4} + O(1/n)$ as $n \to \infty$. This is not much use, but at least it disposes of the alarming possibility that the maximum proportion might $\to 0$ as $n \to \infty$.

# Well-Founded Games

by David Fremlin

1. Most mathematicians are familiar with the game of Nim, but perhaps I should begin by briefly describing it. Two players face each other over a (finite) number of (finite) piles of counters. Each in turn must remove counters from a pile; he must remove at least one counter and he may touch only one pile. The player who takes the last counter loses. (A version of this game was prominent in a fashionable avant-garde film of a few years back, L'Année Dernière à Marienbad.)

2. Nim is one of a large family of games which are determinate in the sense that, for any given starting position, it is possible to predict which player will win if both play correctly. In the case of Nim, this is a consequence of the fact that there are only finitely many games possible following a given starting position. For we can define 'P-positions' and 'N-positions' inductively, thus. The 'empty' position, with no counters in no piles, is an N-position. Suppose we have classified all positions with a total of n counters or fewer, where $n \geqslant 0$. Now say that a position with $n + 1$ counters.

(a) is an N-position if there is a move by which it can be transformed into a P-position;

(b) otherwise, is a P-position.

It is now easy to see that if you have an N-position, you can force a win, while if you have a P-position, you will lose against an efficient opponent. For in the latter case your move will necessarily result in an N-position, and now your opponent can turn it into another P-position.

3. The finiteness of the whole game of Nim is not essential here; what is important is the fact that any particular game is bound to finish. Let us define a well-founded game to be a quadruple $(G, R, T_N, T_P)$, where:

(i) G is a non-empty set.

(ii) R is a relation on G such that: if $A \subseteq G$ is non-empty, there is an $a \in A$ such that there is no $b \in A$ for which bRa. We interpret 'bRa' as 'there is a legal move transforming position a into position b', and say that b follows a. Thus the condition on R is that every non-empty subset A of G has a member which has no follower in A.

We see in particular (a) that there is no $a \in G$ for which aRa (for set $A = \{a\}$); (b) that there is no sequence $(a_n)$ in G for which $a_{n+1} R a_n$ for every n (for set $A = \{a_n : n \in \mathbb{N}\}$); (c) that there exist terminal positions in G, i.e. positions with no followers (for set $A = G$). Using the axiom of choice, it can easily be shown that property (b) is equivalent to the condition on R. Thus, subject to the axiom of choice, a game is well-founded iff any particular sequence of moves must terminate.

(iii) Now finally $(T_N, T_P)$ must be a partition of the set of terminal positions in G. We interpret $T_N$ as the set of terminal positions for which the last player loses; $T_P$ as the set of terminal positions for which the last player wins. Of course one of these may very well be empty. If $T_P = \emptyset$, as in ordinary Nim, we call the game negative; if $T_N = \emptyset$, the game is positive.

4. We can now prove the following.

**Theorem** Let $(G, R, T_N, T_P)$ be a well-founded game in the sense of §3 above. Then there is a unique partition $(G_N, G_P)$ of G such that:

$T_N \subseteq G_N$, $T_P \subseteq G_P$;

if $a \in G_P$, then there is no $b \in G_P$ such that bRa;

if $a \in G_N \setminus T_N$, then there is a $b \in G_P$ such that bRa.

**Sketch of proof** Say that a pair (U, V) of subsets of G is admissible if:

$T_N \subseteq U$, $T_P \subseteq V$;

$U \cap V = \emptyset$;

if $a \in V$, then every follower of a belongs to U;

if $a \in U \setminus T_N$, then there is a follower of a in V.

For instance, $(T_N, T_P)$ is admissible, since the conditions are vacuously satisfied. If $(U_1, V_1)$ and $(U_2, V_2)$ are admissible, apply the well-foundedness condition (ii) of §3 to

$$A = (U_1 \cap V_2) \cup (U_2 \cap V_1)$$

to show that $A = \emptyset$; it follows that $(U_1 \cup U_2, V_1 \cup V_2)$ is admissible. Now set

$$G_N = \{a: \text{there is an admissible } (U, V) \text{ with } a \in U\}$$
$$G_P = \{a: \text{there is an admissible } (U, V) \text{ with } a \in V\}.$$

Show that $(G_N, G_P)$ is admissible. Apply the well-foundedness condition again to

$$A = G \setminus (G_N \cup G_P)$$

to show that $A = \emptyset$ (for otherwise a bottom element of A could be added to one of $G_N$, $G_P$), and hence that $G_N$ and $G_P$ are the required sets. (This is a simple example of transfinite induction).

5. We can interpret the partition $(G_N, G_P)$ by saying that $G_N$ is the set of N-positions and $G_P$ is the set of P-positions, just as in §2 above; every follower of a P-position is an N-position, while every non-terminal N-position is followed by at least one P-position.

It is clear that knowing $G_N$ and $G_P$ gives you a strategy: if you are faced with an N-position, change it to a P-position; if you are faced with a P-position, knock the board over. Unfortunately the theorem does not give an effective method of determining which are the crucial P-positions. In the case of a finite game like Nim or chess, there can in theory be found by enumeration of cases. (Of course, chess is a three-valued game, since draws are possible. Exercise for readers: adapt the theorem above to this case, showing that G is partitioned into 3 sets). The case of chess, of course, is very theoretical indeed.

6. However, for Nim, an effective algorithm for deciding whether a position is in $G_N$ or $G_P$ is known; it was published by Bouton (1). It is based on a curious fact. Let $(N^*, R, T, \emptyset)$be the game of Nim in the terms of §3; $T_P = \emptyset$ because the only terminal position is an N-position. Now consider $(N^*, R, \emptyset, T)$, that is, the same game except that you win if you take the last counter. This is positive Nim, as opposed to the ordinary game, negative Nim. One would naturally suppose that they were quite different games. But in fact it is easy to see that positive and negative Nim have

nearly the same N- and P-positions. Now Bouton's analysis gives a very simple and elegant method of computing P-positions in positive Nim. (Hint: by direct calculation, as in §2, find all P-positions with $\leq 7$ counters in each of $\leq 3$ piles. Express your results in binary notation and look for a pattern. Answer on p. ii).

*7. The same arguments can be used for 'transfinite Nim'. In this case, the finite piles of counters are replaced by ordinals, possibly infinite; the number of piles, however, must remain finite. The trick is to work out what the binary expansion of an ordinal is. What you do is to identify the binary expansion of a finite integer k with a finite set $A \subseteq \underline{N}$; A is the set of places where 1's occur, i.e.

$$k = \sum_{n \in A} 2^n.$$

If $k = 0, A = \emptyset$. Now we can order the class of all finite sets of ordinals by writing

$$A < B \Leftrightarrow \exists\ \beta \in B\backslash A \text{ such that for } \alpha > \beta, \alpha \in A \Leftrightarrow \alpha \in B.$$

This is a well-ordering and consequently the class of finite sets of ordinals is canonically isomorphic to the class of all ordinals; thus each ordinal is naturally associated with a finite set of ordinals, and it is this finite set which behaves like a binary expansion. The Nim analysis is now easy to apply directly to these finite sets.

8. A whole class of games can now be tackled; this was done by Sprague (2). If G and H are positive finite games, their disjoint sum $G \oplus H$ is the game with position set $G \times H$ and follower relation given by

$$(a_1, a_2)R(b_1, b_2) \Leftrightarrow a_1 = a_2 \ \&\ b_1Rb_2 \textbf{ or } a_1Ra_2 \ \&\ b_1 = b_2.$$

The terminal positions of $G \oplus H$ are just the pairs (a, b) where a is terminal in G and b is terminal in H; they are all P-positions, as this is to be a positive game. It is easy to see that $G \oplus H$ is still finite. We observe that positive Nim is just a disjoint sum of copies of the trivial game $Nim_1$, positive-Nim-with-one-pile.

Sprague's work applies to any disjoint sum of positive finite games. He uses the notion of the rank of a position. If a is a position in a positive finite game, then r(a) is defined inductively by

r(a) = 0 if a is terminal;

r(a) = least non-negative integer not equal to r(b) for any follower b of a, if a is not terminal.

It is easy to see that r is well-defined; that a is a P-position iff r(a) = 0 (this is where we use the fact that the game is positive); and that r(a) is that unique integer such that the pair (a, r(a)) is a P-position in the disjoint sum $G \oplus Nim_1$.

The point is that if G and H are positive finite games, then (a, b) is a P-position in the disjoint sum $G \oplus H$ iff r(a) = r(b). From this we see that (a, b) is a P-position in $G \oplus H$ iff (a, r(b)) is a P-position in $G \oplus Nim_1$. Now a simple induction shows that if $G_1, \ldots, G_n$ are positive finite games, a position $(a_1, \ldots, a_n)$ in $G_1 \oplus \ldots \oplus G_n$ is a P-position iff $(r(a_1), \ldots, r(a_n))$ is a P-position in positive Nim; and we know all about P-positions in Nim.

From the arguments above it is clear that r(a, b) is a function of r(a) and r(b). I leave it to the reader to describe this function.

*9. This analysis works just as well for arbitrary positive well-founded games; but the rank function must now be allowed to take infinite ordinal values, and its definition requires a true transfinite induction. We then use the analysis of transfinite positive Nim (§7 above).

10. Grundy & Smith (3) made a determined assault on the problem of disjoint sums of negative games, like ordinary Nim. They shought a new kind of rank function which would be such that (i) $\theta(a, b)$ would depend only on $\theta(a)$ and $\theta(b)$ (ii) the value of $\theta(a)$ would determine whether a was an N- or a P-position. Their arguments are interesting and subtle but the most natural conclusion to draw from them is that the problem is very hard.

11. I will conclude this essay with a description of two particular finite games. The first is taken from (2). Its positions are the same as those of ordinary positive Nim, but there is a new kind of move; as an alternative to removing counters, you may break one of the piles into two, each part, of course, not empty. Calculate the Sprague rank of a single pile of n counters; the answer may surprise you.

12. The second game I shall call Nim-squared; I saw it in the Observer in 1961 or 1962. Imagine counters placed, not in piles, but in a rectangular array, as on the squares of a chessboard. For a move, you take counters away; as usual, you must remove at least one; and if you take more than one, they must either all belong to the same column or all belong to the same row. Thus

```
xxxx        xxxx        xxx         xxx
xxxx   →    x      →    x      →    x
xxxx        xxxx        xxxx        xxxx
xxxx        xxxx        xxx         xx
```

is a legitimate sequence. What are the N- and P-positions (a) for the positive game (b) for the negative game? I have no idea how to find them in general, but I cajoled the PDP-10 computer at the University of Essex into giving me a list of all P-positions that can be got into a 4 × 4 square. Observe that (i) permuting the columns (ii) permuting the rows (iii) reflecting about the diagonal, do not change the value of a position; so I give only one example of each type. The first list (Table 1) gives the

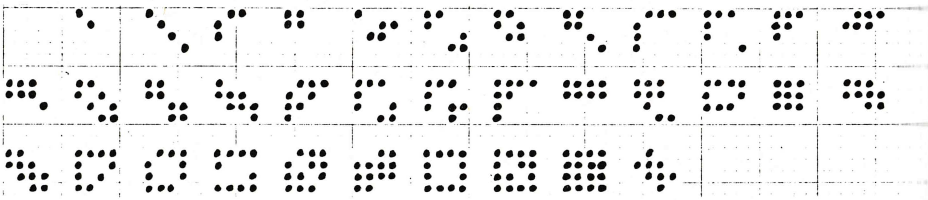


**Table 1**

P-positions for positive Nim-squared

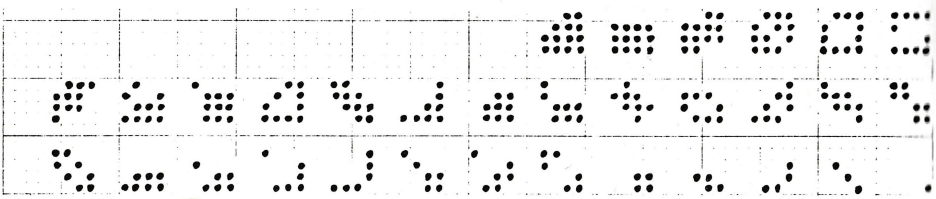


**Table 2**

P-positions for negative Nim-squared

P-positions for the positive game. (As is usual in games of this type, the N-positions greatly outnumber the P-positions). Unsurprisingly, many of these possess a rotational symmetry of some kind. The second list (Table 2) gives the P-positions for the negative game. Now I find it really surprising that the two lists overlap as much as they do. Has anyone any ideas?

**References**


(1) Bouton A. L., 'Nim, a game with a complete mathematical theory', Ann. Math. 3 (1902) 35-39.

(2) Sprague R. P., 'Über mathematische Kampfspiele', Tohoku Math. J. 41 (1935) 438-444.

(3) Grundy P. M. & Smith C. A. B., 'Disjunctive games with the last player losing', Proc. Cambridge Phil. Soc. 52 (1956) 527-533.


# $y^2 + D = x^5$

by Bernard M. E. Wren

The most thoroughly investigated single Diophantine equation must be $y^2 + D = x^3$; see, for example, chapter 26 of Mordell [1].

The equation $y^2 + D = x^5$ however, is much less represented in the literature, but does yield a small amount to elementary twiddling.

For example, squares modulo 11 are $\{0, 1, 3, 4, 5, 9\}$ and fifth powers modulo 11 are $\{0, 1, 10\}$ whence the equation is insoluble in integers if $D \equiv 4 \bmod 11$.

**Theorem**

Let D be a positive, squarefree integer, $D \neq 3$, with $D \not\equiv 7 \bmod 8$. Then if 5 does not divide the class-number of $\mathbb{Q}(\sqrt{-D})$, the equation $y^2 + D = x^5$ has no solution in integers except when $D = 1, 19, 341$ when the only solutions are

$$(x, \pm y) = (1, 0), (55, 22434), (377, 2759646).$$

**Proof:**

The case $D = 1$ goes back to 1850, see Mordell [1], p. 301, so henceforth we assume $D \neq 1$.

$x$ even implies $y^2 \equiv -D \bmod 8$ which is impossible under the given hypotheses. So $x$ is odd, $y^2 + D$ is odd, and thus $(y + \sqrt{-D}, 2) = 1$.

Then $(y + \sqrt{-D}, y - \sqrt{-D}) = (y + \sqrt{-D}, 2\sqrt{-D}) = (y + \sqrt{-D}, 2)$ since $(y, D) = 1$

$$= 1$$

Whence $(y + \sqrt{-D}) = \mathfrak{a}^5$ for some integral ideal $\mathfrak{a}$. Since 5 does not divide the class-number, $\mathfrak{a}$ is a principal ideal. Moreover, when $D \equiv 3 \bmod 4$, the multiplicative index of $\mathbb{Z}\left[\frac{1 + \sqrt{-D}}{2}\right]$ in $\mathbb{Z}[\sqrt{-D}]$ is 3 so that if

$$\mathfrak{a} = \left(\frac{a + b\sqrt{-D}}{2}\right) \text{ with a, b odd integers, then}$$

$$\alpha^5 = \left(\frac{A + B\sqrt{-D}}{2}\right) \text{ with A, B odd integers.}$$

Comparing coefficients of $\sqrt{-D}$ gives an impossible congruence modulo 2. Consequently $(y + \sqrt{-D}) = (u + v\sqrt{-D})^5$ for integers u, v.

Since $D \neq 1, 3$ the only roots of unity in $\mathbb{Q}(\sqrt{-D})$ are $\pm 1$ so that we deduce an equation

$$y + \sqrt{-D} = (u + v\sqrt{-D})^5$$

where the $\pm$ sign has been absorbed without loss of generality into the fifth power.

Equating coefficients of $\sqrt{-D}$ gives $1 = 5u^4v - 10u^2v^3D + V^5D^2$.

Accordingly, v divides 1 and $v = -1$ is impossible modulo 4. So $v = 1$ and $5u^4 - 10u^2D + (D^2 - 1) = 0$ giving

$$u^2 = D \pm \sqrt{\frac{4D^2 + 1}{5}}$$

So $4D^2 + 1 = 5t^2$, say, and $D \pm t = u^2$.

Substituting, $4(u^2 \mp t)^2 + 1 = 5t^2$

$$\text{i.e. } t^2 \pm 8u^2t - (4u^4 + 1) = 0$$

whence $(4u^2)^2 + (4u^4 + 1)$ is a perfect square, say $w^2$.

Then $w^2 - 1 = 20u^4$ and modulo 8, u is even.

Thus $\begin{cases} w \pm 1 = 2\alpha^4 \\ w \mp 1 = 8.20\beta^4 \end{cases}$ or $\begin{cases} w \pm 1 = 2.5\alpha^4 \\ w \mp 1 = 8.4\beta^4 \end{cases}$ for integers $\alpha, \beta$,

giving respectively $\alpha^4 - 80\beta^4 = 1$ and $5\alpha^4 - 16\beta^4 = 1$, the signs chosen modulo 4.

The latter equation is impossible modulo 8, and the former (N.B. Mordell [1] p. 275) has only the solutions $(\pm\alpha, \pm\beta) = (1, 0), (3, 1)$.

These values give $(u, w) = (0, 1), (6, 161)$ respectively; the former now gives $D = 1$ and the latter $D = 19$ or $341$ with corresponding solutions $(x = u^2 + Dv^2 = u^2 + D)$ $x = 55, 377$.

Ref:- [1] Mordell: Diophantine Equations.

# Another Proof of the Pythagorean Proposition

**by Arjun Tan**

It is refreshing to see that many recent text books on geometry are furnishing ancient proofs of the Pythagorean theorem which may date back to thousands of years before Pythagoras. An ancient Chinese proof, using the algebraic identity

$$(a + b)^2 - 4(\tfrac{1}{2}a.b) = a^2 + b^2 \quad (1)$$

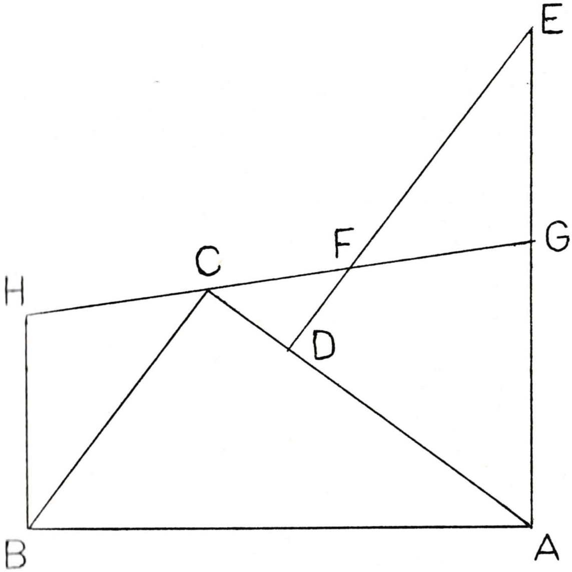
is given in Smith's History of Mathematics (also in Hogben's Mathematics for the Million, p. 63). This proof is believed to be known to the pre-Christian Egyptians also. Another elegant proof, known to the ancient Hindus and believed to be due to Bhaskara, is listed by Loomis (see also History of Hindu Mathematics by Dutta and Singh). This proof uses the identity

$$(b - a)^2 + 4(\tfrac{1}{2}ab) = a^2 + b^2 \qquad (2)$$

The celebrated proof by Leonardo da Vinci (1452-1519) is also listed by Loomis (The Pythagorean Proposition, p. 129).

Loomis has compiled over two hundred different and slightly different proofs of the Pythagorean proposition and classifies all proofs into four categories: algebraic, geometric, vectorial and dynamic. He also proposes that no trigonometric proof is possible.

A recent proof not listed by Loomis is given by Hyatt and Carico (Modern Plane Geometry for College Students, pp. 228-229). Another similar but different proof is given in Fig. 1.




Proposition. In triangle ABC, ACB is a right angle. To prove that

$$a^2 + b^2 = c^2 \qquad (3)$$

Construction. Triangle ADE is drawn equal in all respects to the triangle ABC. DF is drawn equal to CD = b — a. The rest of the construction is clear from the figure.

Proof. It is easily seen that triangles EFG and BCH are equal in all respects. Now

Area of trapezoid ABHG = $\Delta$ABC + $\Delta$CDF + $\Delta$BCH + area ADFG

or, $\tfrac{1}{2}c.(AG + BH) = \tfrac{1}{2}a.b + \tfrac{1}{2}(b - a)^2 + \Delta EFG +$ area ADFG or, $\frac{1}{2}c.(AG + GE) = \frac{1}{2}a.b + \frac{1}{2}(a - b)^2 + \Delta ADE$

or, $\frac{1}{2}c^2 = \frac{1}{2}a.b + \frac{1}{2}(b - a)^2 + \frac{1}{2}a.b$

or, $a^2 + b^2 = c^2$.

The above proof makes use of the algebraic formula for $(b - a)^2$. The corresponding proof with use of formula for $(a + b)^2$ is that of Hyatt and Carico. Both the proofs use the formula for the area of a trapezoid.

It is possible that this has been done before. The purpose of this article is not to claim originality but to publicise it.

# A New Geometry

**by D. M. Behrend**

This article is the outcome of an attempt made some years ago to construct an axiom system for plane geometry, using the area of a triangle as the basic concept. As one might expect, this turns out to be more suitable for affine than for Euclidean geometry.

A geometry here means a triple $(R, P, \alpha)$ where (i) R is a non-zero integral domain not of characteristic 2, (ii) P is a set of at least 3 points, (iii) $\alpha$ is a map $P^3 \to R$ satisfying the axioms below. We write ABC for $(A, B, C)\alpha$, which may be thought of as the signed area of the triangle ABC.

Axioms. Here A, B, C, X, Y are arbitrary points of P.

(1) $ABC = BCA = - ACB$ (whence $ABB = 0$).

(2) $ABC = XBC + AXC + ABX$

(3) If $A \neq B$ then $ABT \neq 0$ for some $T \in P$.

The next axiom comes in a weak and a strong form.

(4W) If $ABC = 0$ then $(ABX)(ACY) = (ABY)(ACX)$

(4S) $(AXY)(BCX) + (BXY)(CAX) + (CXY)(ABX) = 0$.

To see that (4S) $\Rightarrow$ (4W), first change the notation of (4S) by swapping A and X, then use (1). For the time being we shall assume only (4W).

A straight line is defined to be a subset of l of P such that

(i) l contains at least two points
(ii) If $X, Y, Z \in l$ then $XYZ = 0$
(iii) If $X, Y \in l$ and $X \neq Y$ and $XYZ = 0$ then $Z \in l$.

Prop. 1. If $A \neq B$ then the set $l = \{X \mid ABX = 0\}$ is the unique straight line containing both A and B.

Proof. If such a line exists it must be l. Conversely $A, B \in l$ and so (i) holds. For

(ii) suppose ABX = ABY = ABZ = 0; we require XYZ = 0. Take T such that ABT $\neq$ 0. By (4W)

$$(ABT)(AYZ) = (ABZ)(AYT) = 0.$$

Hence AYZ = 0, and similarly AZX = AXY = 0. If X = A we have finished. If X $\neq$ A take U such that XAU $\neq$ 0. By (4W)

$$(XYZ)(XAU) = (XYU)(XAZ) = 0.$$

Hence XYZ = 0. The proof of (iii) is similar.

If l, m are straight lines, l is parallel to m iff ABX = ABY for all A, B $\in$ l and all X, Y $\in$ m. Parallelism is clearly a reflexive relation, and is symmetric by Axiom (2). For transitivity see below (Prop. 8).

Prop. 2. If A $\neq$ B and X $\neq$ Y and ABX = ABY then AB$\|$XY. (Proof omitted).

Prop. 3. Distinct parallel lines have no point in common. (Proof omitted)

Prop. 4. Let l be a straight line, A $\in$ l, and B $\notin$ l. Let X, Y $\in$ l and suppose ABX = ABY. Then X = Y.

Proof. Suppose X $\neq$ Y. Then AB$\|$XY (Prop. 2). These lines meet at A and hence coincide (Prop. 3). Hence B $\in$ XY = l, a contradiction.

A closed geometry is one in which any two non-parallel lines have a common point. Not all geometries are closed. E.g. let R be the real line, P the Euclidean plane, and define $\alpha$ in the obvious way. This gives a closed geometry, but by deleting a suitable subset of P we can make a non-closed geometry.

An R-complete geometry is one in which the following holds: Given a straight line l with points A $\in$ l, B $\notin$ l, and given $\lambda \in$ R, there exists X $\in$ l such that ABX = $\lambda$. (This X is then unique by Prop. 4).

Prop. 5. If an R-complete geometry exists then R is a field.

Proof. Suppose (R, P, $\alpha$) is an R-complete geometry and let $\lambda, \mu \in$ R with $\mu \neq 0$. We construct geometrically $\xi \in$ R such that $\xi\mu = \lambda$.

Take a straight line l and points A $\in$ l, B $\notin$ l (clearly this can be done).

Let C, D $\in$ l be such that ABC = $\lambda$ and ABD = $\mu^2$.

Since $\mu \neq 0$, D $\notin$ AB; so there exists E $\in$ AB such that ADE = $\mu$. Let $\xi$ = ACE. Then by (4W) (ACE)(ADB) = (ACB)(ADE), that is, $-\xi\mu^2 = -\lambda\mu$. Thus $\xi\mu = \lambda$ as required.

Prop. 6. In an R-complete geometry, suppose A $\neq$ B and l is a straight line not parallel to AB. For each $\lambda \in$ R there is a unique X $\in$ l such that ABX = $\lambda$. (This follows from Prop. 5. Details omitted).

Prop. 7. An R-complete geometry is closed
(This follows from Prop. 6 at once).

Prop. 8. In a closed geometry:
(i) If A $\neq$ B and ABX = ABY = ABZ then XYZ = 0.
(ii) Parallelism is a transitive relation.

Proof. Suppose $A \neq B$ and $ABX = ABY = ABZ = \lambda$ (say). In proving that $XYZ = 0$ we may assume that X, Y, Z are distinct and that $\lambda \neq 0$.

If $AX \| YZ$ and $AY \| ZX$ and $AZ \| XY$ then

$$XYZ = AYZ + XAZ + XYA = 3(XYZ).$$

Since char (R) $\neq 2$ this gives $XYZ = 0$. If (say) AZ  XY let AZ and XY meet in Z′. Now $AB \| XY$ (Prop. 2) so $ABZ' = \lambda = ABZ$.

Hence $Z' = Z$ (Prop. 4, taking $l$ to be AZ). Hence $XYZ = XYZ' = 0$. This is (i), and (ii) follows easily.

If $G_i = (R, P_i, \alpha_i)(i = 1, 2)$ are two geometries over R, an embedding of $G_1$ into $G_2$ is defined to be a 1-1 map $\theta$: $P_1 \to P_2$ such that $X^\theta Y^\theta Z^\theta = XYZ$ for all $X, Y, Z \in P_1$.

Let R be a field and let $P_0 = R \times R$. For any $(x, y, z) \in P_0^3$ let $x = (x_1, x_2)$ etc. and define

$$(x, y, z)\alpha_0 = \begin{vmatrix} 1 & 1 & 1 \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}$$

It is straightforward to verify that $(R, P_0, \alpha_0)$ is an R-complete geometry. We shall denote this geometry by $G_0(R)$.

Prop. 9. Let $G = (R, P, \alpha)$ be a geometry over a field R. The following are equivalent:

(i) Axiom (4S) holds in G.
(ii) G can be embedded into a closed geometry
(iii) G can be embedded into an R-complete geometry
(iv) G can be embedded into $G_0(R)$.

Proof. It is immediate that (iv) $\Rightarrow$ (iii) $\Rightarrow$ (ii). Now assume (ii) and let $A, B, C, X, Y \in P$. Put

$$\Phi = (AXY)(BCX) + (BXY)(CAX) + (CXY)(ABX).$$

In proving that $\Phi = 0$ we may assume that G itself is closed and that the five points are distinct. If BC, CA, AB are all parallel to AX then all three lines coincide (Props. 8(ii) and 3). Hence

$$\Phi = (AXY)(BCX + CAX + ABX) = (AXY)(ABC) = 0.$$

If (say) $BC \nparallel XY$ let BC and XY meet in Z. Now

$$\Phi = (AXY)(ZCX + BZX + BCZ) + \ldots$$

but since $XYZ = 0$ we have by (4W) that $(AXY)(BZX) + (BXY)(ZAX) = 0$, and two similar equations. Hence

$$\Phi = (AXY)(BCZ) + (BXY)(CAZ) + (CXY)(ABZ)$$

$$= 0 + (ZXY + BZY + BXZ)(CAZ) + (ZXY + CZY + CXZ)(ABZ).$$

Since $BCZ = 0$ we have by (4W) that $(BZY)(CAZ) + (CZY)(ABZ) = 0$ and $(BXZ)(CAZ) + (CXZ)(ABZ) = 0$; also $ZXY = 0$; hence $\Phi = 0$.

Finally, we assume Axiom (4S) and prove (iv). Choose L, M, N $\in$ P such that $\kappa$ = LMN $\neq$ 0, and define a map P $\to$ R $\times$ R by $X^\theta$ = (XLM, $\kappa^{-1}$(XLN)). Suppose $X^\theta = Y^\theta$. Then XLM = YLM and XLN = YLN, whence also XMN = YMN by Axiom (2). For any T $\in$ P

$$
\begin{aligned}
(LMN)(XYT) &= (XMN + XNL + XLM)(XYT) \\
&= [(XTN)(XYM) - (XTM)(XYN)] + \dots \\
&= (XTL)(XYN - XYM) + \dots \\
&= (XTL)(MYN + XMN) + \dots \\
&= 0.
\end{aligned}
$$

By Axiom (3) this implies X = Y, so $\theta$ is 1-1. Let X, Y, Z $\in$ P.

By definition of $G_0(R)$

$$
\begin{aligned}
\kappa(X^\theta Y^\theta Z^\theta) &= [(YLM)(ZLN) - (ZLM)(YLN)] + \dots \\
&= (NLM)(ZLY) + \dots \\
&= (LMN)(ZLY + XLZ + YLX) \\
&= \kappa(XYZ).
\end{aligned}
$$

Hence $\theta$ embeds G into $G_0(R)$.

If we regard R $\times$ R as a vector space over R in the usual way, it is not hard to see that the map $\theta$ of Prop. 9 is uniquely determined up to an affine transformation of R $\times$ R of determinant 1. Further, if G is an R-complete geometry then $\theta$ is onto. Thus beginning with any geometry G = (R, P, $\alpha$) satisfying the axioms (1), (2), (3), (4S) we may first assume w.l.o.g. that R is a field, since any integral domain is contained in its field of fractions; and further we may assume that G is R-complete. Then all the theorems of plane affine geometry will hold in G.

It is quite amusing to try to deduce well-known theorems while working entirely in terms of the original axioms. For example, let A, B, C be points on a line l, not all coincident, and let X $\notin$ l. Axiom (4W) says that the ratio ABX : ACX is independent of X, and this ratio can be adopted as the definition of the ratio AB : AC. Now let ABC be a triangle and let L, M, N be points on BC, CA, AB respectively, none coinciding with a vertex. By Axioms (4W) and (4S)

$$
\begin{aligned}
&(BLA)(CML)(ANC) + (LCA)(MAL)(NBC) \\
&= (BCA)(CML)(ANL) + (LCN)(MAL)(ABC) \\
&= (ABC)[(LCM)(LAN) + (LCN)(LMA)] \\
&= (ABC)(LCA)(LMN).
\end{aligned}
$$

Hence Menelaus' Theorem: LMN = 0 iff (BL : LC)(CM : MA)(AN : NB) = − 1.

Finally here are two results (without proof) which may be regarded as theorems in affine geometry.

Prop. 10. Suppose A $\neq$ A′, B $\neq$ B′, C $\neq$ C′. Then the lines AA′, BB′, CC′ are either concurrent or parallel, iff

$$(BCC')(CAA')(ABB') + (CBB')(ACC')(BAA') = 0.$$

(Corollary: Ceva's Theorem).

Prop. 11. For any 6 points A, B, C, D, E, F define

$$\beta(A, B, C, D, E, F) = (ACE)(ADF)(BCF)(BDE) - (ACF)(ADE)(BCE)(BDF).$$

(i) $\beta$ is invariant under even permutations of A, ..., F and is multiplied by −1 under odd permutations.
(ii) The 6 points lie on a conic iff $\beta = 0$.

# On the Distribution of Decimal Digits in n!

by S. P. Castell

The generalised factorial operation, defined recursively by

$$\left.\begin{aligned} n! &= n(n-1)!, \\ 0! &= K, \\ n &= 1, 2, 3. \ldots . \end{aligned}\right\} \qquad (1)$$

is an operation on a set of non-negative integers which produces one string

$$S_n = a_R a_{R-1} a_{R-2} \ldots\ldots a_1 a_0 \overset{\text{df.}}{=} \sum_{i=0}^{P_n} c_{ni} r^i$$

of such integers from another

$$S_{n-1} = b_S b_{S-1} b_{S-2} \ldots\ldots b_1 b_0 \overset{\text{df.}}{=} \sum_{i=0}^{P_{n-1}} c_{n-1i} r^i,$$

where

$$\left.\begin{aligned} a_i &= c_{ni} \\ b_i &= c_{n-1i} \end{aligned}\right\}, \qquad \left.\begin{aligned} R &= P_n \\ S &= P_{n-1} \end{aligned}\right\}, \qquad K = \sum_{i=0}^{M} d_i r^i,$$

and, for all k,

$$a_k, b_k, d_k \in \{0, 1, \ldots\ldots, r-1\} \qquad (r > 1).$$

The term 'generalised factorial' is used to cover cases other than the usual K = 1, although the case K = 1 only is considered below. The number r is the radix of the particular number system in which $S_n$, $S_{n-1}$ are expressed. Thus, for r = 2 we have a binary representation, for r = 8 an octal and for r = 10, the situation discussed below, a decimal system.

Fixing attention, then, on K = 1, r = 10 we can readily compute the first few '1/10' factorials from (1):

$$0! = 1, 1! = 1, 2! = 2, 3! = 6, 4! = 24, 5! = 120, 6! = 720.$$

PERCENTAGE OCCURRENCE OF DIGITS IN FACTORIAL(N)

| N | DIGIT 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 |
|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 0 | 100 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 2 | 0 | 0 | 100 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 3 | 0 | 0 | 0 | 0 | 0 | 0 | 100 | 0 | 0 | 0 |
| 4 | 0 | 0 | 50 | 0 | 50 | 0 | 0 | 0 | 0 | 0 |
| 5 | 33 | 33 | 33 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| 6 | 33 | 0 | 33 | 0 | 0 | 0 | 0 | 33 | 0 | 0 |
| 7 | 50 | 0 | 0 | 0 | 25 | 25 | 0 | 0 | 0 | 0 |
| 8 | 40 | 0 | 20 | 20 | 20 | 0 | 0 | 0 | 0 | 0 |
| 9 | 16 | 0 | 16 | 16 | 0 | 0 | 16 | 0 | 33 | 0 |
| 10 | 28 | 0 | 14 | 14 | 0 | 0 | 14 | 0 | 28 | 0 |
| 11 | 25 | 12 | 0 | 12 | 0 | 0 | 12 | 0 | 12 | 25 |
| 12 | 44 | 11 | 0 | 0 | 11 | 0 | 11 | 11 | 0 | 11 |
| 13 | 40 | 0 | 30 | 0 | 0 | 0 | 10 | 10 | 10 | 0 |
| 14 | 18 | 18 | 18 | 0 | 0 | 0 | 0 | 18 | 18 | 9 |
| 15 | 30 | 7 | 0 | 15 | 7 | 0 | 15 | 15 | 7 | 0 |
| 16 | 28 | 0 | 21 | 0 | 0 | 0 | 0 | 7 | 28 | 14 |
| 17 | 26 | 0 | 6 | 6 | 6 | 13 | 13 | 6 | 13 | 6 |
| 18 | 31 | 0 | 12 | 12 | 6 | 6 | 6 | 18 | 6 | 0 |
| 19 | 33 | 16 | 11 | 5 | 11 | 5 | 5 | 0 | 11 | 0 |
| 20 | 36 | 5 | 15 | 5 | 10 | 0 | 10 | 5 | 5 | 5 |
| 50 | 29 | 10 | 4 | 9 | 10 | 3 | 13 | 7 | 7 | 3 |
| 100 | 18 | 9 | 12 | 6 | 6 | 8 | 12 | 4 | 8 | 12 |
| 200 | 20 | 6 | 14 | 10 | 9 | 6 | 7 | 9 | 6 | 8 |
| 300 | 19 | 6 | 10 | 8 | 8 | 9 | 10 | 8 | 8 | 7 |
| 400 | 20 | 8 | 9 | 9 | 8 | 9 | 8 | 7 | 8 | 8 |
| 500 | 19 | 7 | 9 | 9 | 9 | 9 | 8 | 9 | 9 | 7 |
| 600 | 19 | 8 | 9 | 7 | 9 | 8 | 10 | 8 | 9 | 8 |
| 700 | 19 | 9 | 9 | 8 | 8 | 8 | 8 | 9 | 8 | 9 |
| 800 | 20 | 8 | 9 | 8 | 9 | 8 | 8 | 10 | 9 | 7 |

Progressing further, at 20!, or soon after, we realise that it would be convenient to have a computer do this tedious work for us. The ICL 4120 computer has a 24-bit word and can thus accommodate a maximum signed integer per word of $2^{23} - 1$, that is, just over $8 \times 10^6$. Unfortunately, 20! is a decimal number 19 digits long (20! = 2, 432, 902, 008, 176, 640, 000); in fact, the ICL 4120 cannot accommodate factorials greater than 10! by the normal method of storing one whole number per word (10! = 3, 628, 800). Accordingly, a program was written, in ALGOL 60, to store one digit of (n − 1)! in each computer word, and to be able to multiply the number stored in this way by n, (n + 1), etc.. The largest factorial which can be accommodated now depends on the number of computer words available for storage of the $P_{n-1}$ digits in $S_{n-1}$, and not on the number of bits per word.

It then seemed natural to augment the program to count the number of times, $p_{nj}$, j = 0, 1, ...., 9, each member of the set {0, 1, ....., 9} appears in the strings $S_n$, n = 0, 1, ...., N, and to compute, moreover, the percentage occurrence of each such member, $100(p_{nj}/P_n)$. This idea was prompted by the established discussion concerning the distribution of digits in n decimal places of $\pi$ (see Neville [1] and Broadbent [2]). It seemed that the distribution of digits in the string $S_n$ formed by n! could be viewed similarly.

The largest value of N which it has been able to attempt with the computer configuration available (24K), and existing ALGOL program has been N = 800. 800! is a number of 1977 decimal digits (cf. Comrie [3]) which are distributed as shown.

It can be seen from these Figures that the tendency seems to be that all digits but zero are distributed equally, each occurring a little over 9% of the time, while zero occurs about twice as often as all others, a little under 20% of the time. Now it is obvious that, for any K, r, as $n \to \infty$, the string $S_n$ corresponding to n! terminates with a large block of zeros, since, when 1 is a round r, $r^2$, etc., one or more zeros are tacked onto the end of the string $S_1$ in addition to the replacing of digits and lengthening of $S_{l-1}$ produced by computing $l(l-1)!$ to give l!.

The following 'K/r-theorems' seem intuitively possible:

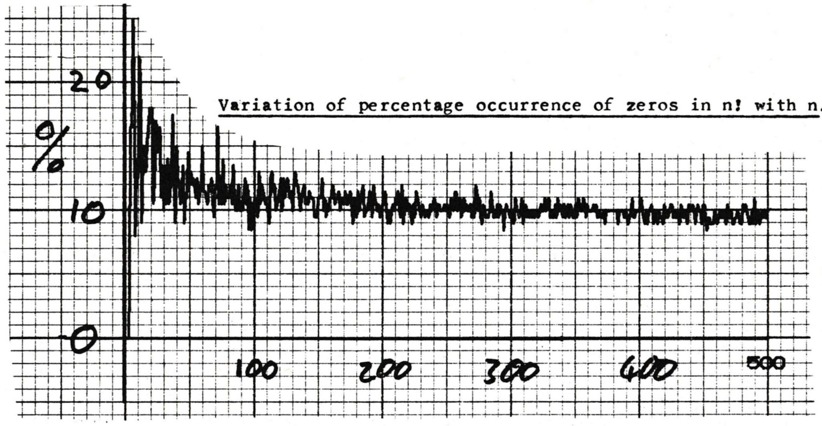
**Theorem 1**

For all K, r, as $n \to \infty$ (and, practically, for n larger than some $\overline{N}$), n! contains twice as many zeros as any other digit, the other digits appearing with equal frequency for $r \geq 3$.

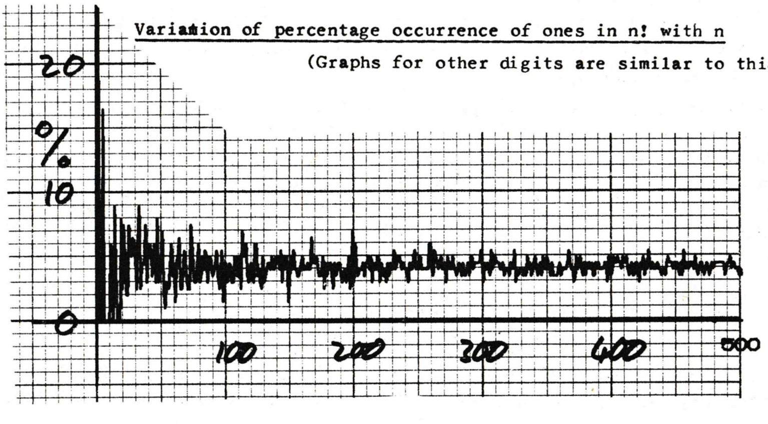
That is, if y is the percentage of zeros in $S_n$, x the percentage of any other digit, y and x satisfy

$$y = 2x, \quad (r-1)x + y = 100.$$

In the '1/10' case investigated above, r = 10 so that x = 9.0909..., y = 18.1818.....




Variation of percentage occurrence of zeros in n! with n.




Variation of percentage occurrence of ones in n! with n

(Graphs for other digits are similar to this)

**Theorem 2**

The number $\overline{N}$ for which Theorem 1 is practically valid varies directly with r.

To see why this is possible, just consider the number system with radix r = 800! (!). That is, perturbations due to the 'initial conditions' are extended more to the right as r increases. **Corollary** This effect also depends on K.

Can these 'theorems' be (dis)proved? More 'experimental' evidence can be obtained by carrying out the above computing for N > 800, for K = 2, 3, ..... and for r = 2, 3, ......

Returning to '1/10' factorials, it can be seen that digit 5's percentage occurrence shows a downward jump around 80! In fact, digit 5 occurs once only in 82!. In addition, Table 1 shows that digit 5 comes off the worst in the first 20 factorials. The full 'text' of 800! will be sent to anybody who cares to write.

**Acknowledgments**

I am grateful to the British Aluminium Company Ltd. for the use of time on their Research Division's ICL 4120 computer and digital graph-plotter.

**References**

[1] E. H. Neville: Mathematical Gazette, 35, 311, February 1951.
[2] T. A. Broadbent: Shanks, Ferguson and $\pi$. Mathematical Gazette, LV, 392, March 1971.
[3] L. J. Comrie: Chambers's shorter six-figure mathematical tables. W & R Chambers Ltd., London, 1970, page 337.

# Orthogonull Matrices

**by Tim Poston**

Leafing through a Scottish Maths Project Book brought the following horror to light: with no motivation, orthogonal matrices defined by $A^tA = I$, followed by one example of a particular matrix shown to be orthogonal, plus a couple of exercises doing the same. Then nothing. No attempt to explain (either geometrically or algebraically) what orthogonality meant, and no further development. Nothing.

Now any reader with a non-rotary mind (figure that one out) would naturally ask, what's so special about $A^tA = I$? What about $A^tA = 2I$? Or granted that 2I is less special than I, what about 0? Let's call a matrix such that $A^tA = 0$ an orthogonull matrix, and play with them a bit.

Sadly, $A^tA = 0$ implies $A = 0$ (how quickly can you prove that?), so it looks like the class of orthogonull matrices is rather small. But let's generalise, by way of what $A^t$ really means. If we have a linear map $\mathbf{A} : V \rightarrow W$ with a matrix A, its dual (between the spaces of linear functionals from V and W to the scalar field R) defined by $\mathbf{A}^*f(x) = f(\mathbf{A}x)$, 'goes the other way'; $\mathbf{A}^* : W^* \rightarrow V^*$. If $\mathbf{A}$ goes from V to itself, however, it is possible not to notice this. Now if V has an inner product (think of this as giving $x . y =$ (length of x)(length of y) $\cos \alpha$ so it defines lengths and angles) this gives an isomorphism $\theta : V \rightarrow V$ defined by $\theta x(y) = x . y$; a vector x goes to the linear functional 'taking inner product with x' on V. And if we are using nice coordinates (that is referred to a basis in which all the vectors are of length one, and any two at right angles) the linear map $\mathbf{A}^t = \theta^{-1}\mathbf{A}\theta$ is called the adjoint of **A** and has the nice easy matrix $A^t$; just the result of switching rows and columns in A. (Notation here varies, by the way.)

So we have

(i) $A^tA = I \Leftrightarrow \mathbf{A}^t\mathbf{A} = \mathbf{I}$ (change from matrices to maps),

$\Leftrightarrow \theta^{-1}\mathbf{A}^*\theta\mathbf{A} = \mathbf{I}$

$\Leftrightarrow \mathbf{A}^*\theta\mathbf{A}(x) = \theta x$ for all x in V

$\Leftrightarrow (\mathbf{A}^*\theta\mathbf{A}(x))y = \theta x(y)$ for all x, y in V

$\Leftrightarrow \theta\mathbf{A}(x)(\mathbf{A}y) = \theta x(y)$ for all x, y in V (definition of $A^*$)

$\Leftrightarrow \mathbf{A}x.\mathbf{A}y = x.y$

& (ii) $A^tA = I \Leftrightarrow \theta^{-1}\mathbf{A}^*\theta\mathbf{A} = 0$

$\Leftrightarrow \mathbf{A}^*\theta\mathbf{A} = 0$ ($\theta$ an isomorphism)

$\Leftrightarrow \theta\mathbf{A}(x)(\mathbf{A}y) = 0$, for all x, y

$\Leftrightarrow \mathbf{A}x.\mathbf{A}y = 0$, for all x, y

So the orthogonal maps are those that preserve the inner product, and the orthogonull maps those that kill it—which here only 0 does. But $\theta$ can be defined for <u>any</u> bilinear form w (bilinearity means exactly that $\theta$ and $\theta x$ must be linear), and writing w(x, y) = x@y, and if @ is non-degenerate (i.e. for any x in V, $x \neq 0$, there is a y such that x@y $\neq 0$) $\theta$ is injective and therefore an isomorphism, since $\dim(V^*) = \dim(V)$. So any nondegenerate bilinear form gives us a definition of 'adjoint', by $\mathbf{A}^t = \theta^{-1}\mathbf{A}^*\theta$, and a new 'transpose' $A^t$ for matrices if we choose bases and give a matrix for $\theta$; and (i) and (ii) go through.

Now, there are two important kinds of non-degenerate bilinear form besides inner products; indefinite metrics, where x@y = y@x but x@x may be zero for non-zero x (notably on $R^4$, $(x, y, z, t)@(x', y', z', t') = xx' + yy' + zz' - tt'$, the Lorentz metric of Relativity), and symplectic structures, where x@y = — y@x, which are central in classical mechanics. The corresponding orthogonal maps are the Lorentz (change of inertial frame) transformations and the symplectic maps, respectively. Both are much studied. The orthogonull maps are those **A**'s on whose ranges $\mathbf{AV} = \{\mathbf{A}x \mid x \in V\}$ the restriction of @ gives $\overline{x@y} = 0$; for instance, in $R^4$ with the Lorentz metric, they have to map the whole space to 0 or onto a light ray. In a symplectic space dim(**AV**) can be up to ½(dim(V)). An orthogonull map cannot be invertible, since V is not a possible range for it—we required @ non-degenerate on V to start with—and so do not form a group. They form a semi-group without identity, and there is not the slightest reason to suppose they lead anywhere. Still, it's a nice word and—as far as the victims of SMP get to know about it—neither do orthogonal matrices.

**EDITOR'S ACKNOWLEDGMENTS**

To the <u>Eureka</u> staff, Martin Mellish, Colin Vout, and James Macey; to Joe Conlon, the previous Editor, for his advice; to those who were willing to give hours of their time, without reward, to contribute; to P. de Fermat, for the bluff; to Peorth for the modification; and to K.

# Answers to the Problems Drive

1. (a) 8 (b) 8

Table of scores, 8 winners

| competitors | Event abcd |
|---|---|
| A | 7421 |
| B | 1247 |
| C | 4712 |
| D | 2174 |
| E | 6530 |
| F | 0356 |
| G | 5603 |
| H | 3065 |

Table of scores, no winners

| competitors | Event abcd |
|---|---|
| A | 0765 |
| B | 1076 |
| C | 2107 |
| D | 3210 |
| E | 4321 |
| F | 5432 |
| G | 6543 |
| H | 7654 |

2. (a) $2\pi R \sin\theta$. (Antarctic/Arctic Circle)

   (b) Any meridian, i.e. Great Circle through the poles (circle of longitude)

3. 5 days out of 18

4. (a) 215 (b) 7712 (0123456 = PDMATSH)

5. (a) 48 oz = 3 lbs (b) 21 oz = 1 lb 5 oz

6. No danger; B is A's brother (A is B's sister)

7. n = 13: $\{1, 4, 10, 13\}, \{2, 3, 11, 12\}, \{5, 6, 8, 9, \}$ 7 can go in any antigroup.

8. (a) 30 m.p.h. (b) 8 seconds

9. 5.

| | | |
|---|---|---|
| 1 | 2 | 3 |
| 6 | ////// | 7 |
| 9 | 8 | 4 |

10. (a) 18 (10 along bottom, 8 along top)

    (b) 8 is believed to be the minimum: we would be very interested to hear of any improvement on this figure.

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | |
| | | | | | | | | | |
| | | | X | | | | | | |
| | | | X | | | | | | |
| | | | X | | | | X | | |
| | | | X | | | | X | | |
| | | | X | | | | | | |
| | | | X | | | | | | |
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PRINTED BY Unwin Brothers Limited
THE GRESHAM PRESS OLD WOKING SURREY ENGLAND

*Produced by 'Uneoprint'*

A member of the Staples Printing Group