NEWSLETTER

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NEWSLETTER

EDITOR

Donal Hurley

ASSOCIATE EDITOR

Patrick Fitzpatrick

The aim of the *Newsletter* is to inform Society members about the activities of the Society and also about items of general mathematical interest.

The *Newsletter* also seeks articles of mathematical interest written in an expository manner. All parts of mathematics are welcome, pure and applied, old and new.

Manuscripts should be typewritten and double-spaced on A4 paper. Authors should send two copies and keep one copy as protection against possible loss. Prepare illustrations carefully on separate sheets of paper in black ink, the original without lettering and a copy with lettering added.

Correspondence relating to the *Newsletter* should be sent to:

Irish Mathematical Society Newsletter
Department of Mathematics,
University College,
Cork.

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IRISH MATHEMATICAL SOCIETY

Minutes of the Ordinary Meeting held at 12.15 on 31 March, 1983 in DIAS.

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1. There were 15 members present. The President, A.G. O' Farrell, took the chair. The minutes of the previous ordinary meeting on 21st December, 1983, were read. A change in one item was agreed. Item 8 was amended to read "R. Bates mentioned that the Irish Mechanics Group might amalgamate with the IMS". The minutes were then signed.

2. The motion "that the Society abandon the idea of a prize for outstanding papers in the Proceedings of the Royal Irish Academy" was passed by ten votes to one with two abstentions.

3. The change in the constitution to allow two extra committee members was confirmed by seven votes to zero with seven abstentions.

4. The Secretary mentioned that the committee was considering entering into a reciprocity agreement with the Irish Mathematics Teachers Association, which would involve a reduction of approximately £1.50 in the subscriptions of reciprocal members. No objections were raised to this plan.

5. P. Boland mentioned that the committee was considering writing to the Department of Education protesting about recent changes in the regulations for maintenance allowances for post-graduate students. After some discussion, it was agreed that the President should draft such a letter mentioning only the anomaly that those with a B.A. degree in Mathematical Sciences from N.U.I. did not qualify for these allowances, while those with an almost identical B.Sc. degree did qualify. With the approval of

the committee, the letter could then be sent to the Minister for Education.

6. R. Enright drew attention to items in the Newsletter about prompt payment of subscriptions and about the 33% reduction on the Proceedings of the Royal Irish Academy available to members of the IMS. He also urged those present to encourage more institutions to join. He noted also the improved terms for institutional members agreed by the committee on 30th March.

7. With regard to the Irish Mechanics Group, it was announced that this organisation was to meet on June 2nd and 3rd in U.C.C.

R. Timoney (Secretary)

NEWS AND REPORTS

1983 Irish National Mathematics Contest

The fifth Irish National Mathematics Contest was held on March 1, 1983, and attracted 1797 entries from 116 schools, as against 1539 entries from 77 schools last year. The increase in interest in the competition is due in some part to the co-operation we received from the Department of Education which issued a circular about the contest to all schools under its aegis.

This year's winner is:

Patrick Joseph Gaffney,
Christian Brothers' College,
St. Patrick's Place,
Cork,

who scored 116. Patrick was placed second last year. In common with the top scorer from each of the foreign countries that use the American High School Mathematics Examination (AHSME) materials for their national contests, Patrick will receive an Honor Pin from the Mathematical Association of America Committee on High School Contests.

The highest team score (sum of the highest three scores by individual contestants) was returned by:

Coleraine Academical Institution,
Castlerock Road,
Coleraine,

which entered 35 students, of whom 17 scored 85 or more. Blair R. Cameron, Andrew R. Clark and Christopher A. Hunter/ Michael N. Bacon made up the winning team and between them achieved 311.

In the United States and Canada, the downward trend in participation which began last year continued. Only 407133 entries (versus 418009 in 1982) were received from 6190 schools (versus 6623 in 1982), a consequence possibly of the difficulty of the three previous examinations. However, the somewhat less difficult nature of this year's examination has been very well received and enrolment figures are expected to increase again in 1984, when the examination will be similar in difficulty to this year's.

The top scorer in the United States and Canada was:

James Yeh,
Mountain Brook High School,
Alabama.

James obtained full marks, i.e. 150, and becomes the 22nd student in the United States and Canada to do a perfect paper in the 34 year history of the competition.

With the addition of China, Mexico and Singapore, Ireland is now one of 15 foreign countries that participate in the AHSME. China's top scorer was:

Xiao-dong Che,
Tian She High School,
Shanghai.

Xiao-dong also returned a perfect paper.

The First Irish Invitational Mathematics Contest

All those who scored 85 or more in the Irish National Mathematics Contest were invited to participate in the Invitational Mathematics Contest (IIMC). This was held on Tuesday, March 22, 1983. It was a $2\frac{1}{2}$ hour, 15-question, short-answer examination, not multiple choice. The questions were prepared by the Mathematical Association of America Committee on High Schools Contests for the First Annual American Invitational Mathematics Examination.

Here are a few sample questions:

1. What is the product of the real roots of the equation
$$x^2 + 18x + 30 = 2\sqrt{(x^2 + 18x + 45)}\ ?$$

2. Let $a_n = 6^n + 8^n$. Determine the remainder on dividing a_{83} by 49.

3. What is the largest 2-digit prime factor of the integer
$$n = \binom{200}{100}\ ?$$

4. Find the minimum value of
$$f(x) = \frac{9x^2 \sin^2 x + 4}{x \sin x}$$
for $0 < x < \pi$.

5. For $\{1,2,3,\ldots,n\}$ and each of its nonempty subsets, a unique *alternating sum* is defined as follows: Arrange the numbers in the subset in decreasing order and then, beginning with the largest, alternatively add and subtract successive numbers (for example, the alternating sum for $\{1,2,4,6,9\}$ is $9-6+4-2+1 = 6$ and for $\{5\}$ it is simply 5). Find the sum of all such alternating sums for $n = 7$.

While 67 qualified for the Irish Invitational Mathematics Contest, only 53 were able to compete, for one reason or another. Results were returned on behalf of 47 contestants. The list was headed by

Kathleen McCormack,
C.B.S.,
Kilrush,
Co. Clare,

with a score of 8 out of a possible 15. This was a fine performance. Professor Mientka, Executive Director, AHSME, informed me that if Kathleen resided in the U.S.A., she would have been invited to their three-week International Mathematical Olympiad training session. This, I think, lends perspective to her achievement.

By comparison, only those who scored 95 or above on the AHSME were invited to take the American Invitational Mathematics Examination. A total of 1823 students qualified to compete in this examination, and 54 of these scored at least 10. These were invited to participate in the U.S. AMO which was held on May 3, 1983.

Here are two out of the five questions that were set for this:

1. Prove that the roots of

$$x^5 + ax^4 + bx^3 + cx^2 + dx + c = 0$$

cannot *all* be real if $2a^2 < 5b$.

2. Consider an open interval of length $1/n$ on the real number line where n is a positive integer. Prove that the number of irreducible fractions p/q, with $1 \leq q \leq n$, contained in the given interval is at most $\frac{1}{2}(n+1)$.

Finbarr Holland

Below are listed the first six students in the Individual Roll of Honour for the Irish National Mathematics Contest 1983.

Rank	Score	Student Name	School
1	116	Gaffney, Patrick Joseph	Christian Brothers' College, St. Patrick's Place, Cork.
2	111	Cameron, Blair R	Coleraine Academical Institution, Coleraine.
2	111	Murphy, John A	Oatlands College, Mount Merrion, Blackrock, Dublin.
3	107	O'Connell, John	C.B.S., The Green, Tralee, Co. Kerry.
4	105	Ferry, Denis	Pobalscoil Choich Cheannfhaola, Falcarragh, Co. Donegal.
5	104	Holahan, Shane A.	Belvedere College, Dublin,

The following schools attained the first three places in the School Roll of Honour

Rank	Score	Name of School	Team Members
1	311	Coleraine Academical Institution, Coleraine, Co. Londonderry.	Blair R. Cameron Andrew R. Clarke Christopher R. Hunter Michael N. Bacon
2	305	O'Connell School, Dublin 1.	Thomas J. Shortall Stephen. P. Dunne Michael. J. Murray
3	298	Pobalscoil Chioich Cheannfhaola, Falcarragh, Co. Donegal.	Denis J. Ferry Kevin. J. Gallagher Seamus A. McBride

LETTERS TO THE EDITOR

28th February, 1983

Dear Sir,

As a teacher of some experience I wish to comment on some of the points in the article on the Post Primary syllabus by Michael Brennan (December 1982).

No one could have any doubt about the fact that we have Euclidean Geometry as against that of Papy. Papy was a most important source for ideas, notation and definition in introducing Transformations. Transformations, so introduced, had some important effects.

The traditional approach had depended largely on congruence which used movement in and out of the plane ungoverned by axioms - hardly a satisfactory situation - but Axial Symmetry and its compositions, being isometries, provide for congruence of sets in general and triangles in particular. To my mind, congruence, thus readily deduced from material in the syllabus ought form an important part of present teaching and occupy a more explicit role in any future syllabus. The old approac also depended on a parallelism which led to the rather unsatis factory *reductio ad absurdum* tactic on proving the angle Theorem. In contrast, translation now deals directly and effectively with the Theorem.

Transformations have the further essential purpose of acting as a motivation and introduction to the idea of function which in turn fulfills the most important pedagogical purpose of unifying Algebra and Geometry. Mathematics is a unified activity.

Yours sincerely,
Hugh McTigue,
Meanscoil San Lughaidh, Coillte Mach, Co. Maigh Eo.

The following is Mr. Brennan's reply to the above:

Sir -

The most important effect of the Transformation approach to Geometry was to make Geometry more obscure.

Teachers *are* responsible to the Mathematical truth that they know, to see that there is proper government by proper axioms. But children do not need to know the details. Teachers are *not* responsible to children in the way that they are responsible to Higher Mathematics. Certainly they should tell the children the truth, but not the whole truth if the result will mostly be mystification, as at present.

As for the unifying effect of sets and transformations on school Mathematics, what about the disintegrating effect on children of getting E's and F's and NG's (and other grades too) during their years at school and doing Maths courses that are contrary to education? The vision of Unification comes only to a minority.

I do not think it matters what Geometry comes from the present shake-up provided that:

(i) it promotes intuition and logic in the child, and

(ii) the background is logical and elegant to the adult.

The present course fails on both counts.

Yours,
Michael Brennan,
Bower School,
Athlone.
16/6/83.

ROBERT CHARLES GEARY

MATHEMATICAL STATISTICIAN, 1896-1983

John. E. Spencer

Roy Geary, Ireland's greatest statistician, died in Dublin on 8 February 1983, after a long life devoted to mathematical statistics. He was born on 11 April, 1896 and was educated at University College, Dublin, 1913-18, obtaining a B.Sc. and an M.Sc. both with first class honours in 1916 and 1917, respectively. In 1918 he was awarded a Travelling Studentship in Mathematics and attended the Sorbonne in Paris, 1919-21. He was appointed to a lectureship in Mathematics at University College, Southampton in 1922 but returned to Dublin in 1923 as a statistician in the Statistics Branch of the Department of Industry and Commerce and remained there until 1949, apart from a brief period as Senior Research Fellow in the Department of Applied Economics in Cambridge, 1946-7. He was director of the Central Statistics Office in Dublin from 1949 to 1957, when he moved to New York for three years to head the National Accounts Branch of the United Nations Statistical Office. He returned to Dublin in 1960 to the Economic Research Institute (later known as the Economic and Social Research Institute) where he was to spend the rest of his life, as Director until 1966, as consultant thereafter, Among the many honours awarded him were three honorary doctorates from NUI, QUB and TCD and honorary fellowships of the Royal Statistical Society and American Statistical Association. He was President of the International Statistical Institute and a Council member of the International Association for Research in Income and

This appreciation relies heavily on other appreciations by the author published in *The Economic and Social Review*, April 1976 and April 1983, and a forthcoming obituary in *Econometrica*. The first two of these contain lists of Geary's works from which present citations can be identified and all three contain references to other work to aid in the assessment of Geary's achievements. A more rounded assessment of his personality and career can be gained by perusal of all the appreciations and obituaries.

Wealth. He was elected Fellow of the Econometric Society in 1951 and served as Council member from 1962-64.

Roy's first paper was published in 1925, a year within what Egon Pearson described as" a period of transition" and as "one of the two great formative periods in the history of mathematical statistics", 1915-1930. The discoveries of Karl Pearson, Edgeworth, Galton and Weldon and the early work of Gosset were to be expanded and unified by the work of Fisher who by 1925 had published the first edition of his enormously influential *Statistical Methods for Research Workers*. His work on likelihood, regression and correlation, his establishing of the distribution of Gosset's t-distribution and of the variance ratio had all just been published. The controversies with E.S. Pearson, Neyman and Wald on fiducial probability, interval estimation and hypothesis testing were still to come and the celebrated collaboration of Neyman and Pearson of 1926 to 1934 had not yet started. A new era in probability theory was also beginning. Liapunov had provided the first satisfactory proof of the central limit theorem under certain conditions in 1901 using the tool of characteristic functions but his work remained fairly unknown for some time and the fundamental refinements of Lindeberg in 1922, Bernstein in 1927 and Feller in 1935 were, in 1925, new or still to come. Indeed it was only by 1925 that Levy published what Cramer has described as "the first systematic treatise of random variables, their probability distributions and their characteristic functions" and the work of Khintchine and Kolmogorov was still in its infancy.

Accordingly Roy was beginning his work at a time which must have seemed full of excitement. Throughout the most productive part of his career, 1930-1956, he was greatly influenced by Fisher although he played no part in the controversies referred to above. Many years later (1976), he confessed in a letter to the author that many of his papers originated in the development of something in Fisher and that it was his great good fortune that his research lifetime coincided with so much of Fisher's. "Everything is in Fisher. One only had to dig it out a bit".

His first paper is richly promising as befits the early work of a researcher who is subsequently to gain a high international reputation. The paper shows fine technical ability, a natural flair for stochastic problems and a painstaking care for detail, qualities which were to manifest themselves throughout his later work. This early paper also provides an excellent example of the power of good theory when applied to real problems; here the problem concerned Irish agricultural statistics which had been thrown into some confusion during political troubles of the time. The theoretical result is as follows. Let $m+u_i$, $i=1...N$ be the values of N elements in year one and $m'+u_i'$, $i=1..N$ be the values in year two, where m and m' are the means in the two years. It is desired to measure ratio of true means m/m' by taking a random sample of n elements. The ratio as estimated by

$$\frac{\sum_1^n (m'+u_i')}{\sum_1^n (m+u_i)}$$

is shown to be approximately normally distributed for large n and N with a mean of m'/m. Geary also computes the variance and applies the result successfully to agricultural data.

This interest in finding the density of a ratio continued and five years later the classic (1930b) paper appeared. Let x_1 and x_2 be two jointly distributed normal variables with means μ_1 and μ_2 and variances σ_1^2 and σ_2^2 respectively, with correlation r. The difficult technical problem of finding the exact sampling distribution of $z = x_1/x_2$ was solved on the hypothesis that μ_2 was large relative to σ_2 so that the range of x_2 was effectively positive. On this assumption Geary proved that the ratio $(\mu_2 z-\mu_1)/\sqrt{(\sigma_2^2 z^2 - 2r\sigma_1\sigma_2 z + \sigma_1^2)}$ was distributed N(0,1). The result in its original form is still quoted today. An expression for the density of z where x_1 and x_2 are independent

but not necessarily normal variates was discovered in 1937 by Cramer for the case where x_2 is non-negative with finite mean. Writing $\Phi_1(t)$ and $\Phi_2(t)$ for the characteristic functions of x_1 and x_2, the density of z, f(z), if it exists was found to be

$$f(z) = \frac{1}{2\pi i}\int_{-\infty}^{\infty} \Phi_1(t)\Phi_2'(-tz)dt,$$

provided the integral converges. Geary (1944b) generalised this result to the case of non-independent x_1 and x_2 with joint characteristic function $\Phi(t_1,t_2)$ and the same condition on x_2. The generalised result is

$$f(z) = \frac{1}{2\pi i}\int_{\infty}^{\infty} \delta\Phi(t_1,t_2)/\delta t_2 dt_1$$

where the partial derivative is evaluated at $t_2=-t_1z$.

Between these two papers on ratio densities his 1933 paper appeared in which it was established, under normality, that

$$E(m_r/m_2^{\frac{1}{2}r})^p = Em_r^p/Em_2^{\frac{1}{2}rp}$$

where

$$m_r = \sum_1^n (x_i-\overline{x})^r/n.$$

The analysis, from a probabilistic point of view, of ratios constituted one of the three main inter-related themes with which Roy was concerned during his working life. The other two comprise the estimation of relationships between variables whose measured values are subject to error and normality tests and robustness studies of inference formally based on normality. This choice of problems illustrates his deep desire to direct his keen mathematical skill and imaginative flair towards problems of great practical importance and most of his contributions to these three fields are of lasting significance. A possible exception to this can be found in his work in the early 1940's on the estimation of relationships. This work, heavily based on the theory of cumulants, has theoretical importance but has not proved of lasting practical value as the estimators suggested tend to have high variance, in particular if the underlying variables have distributions which are nearly normal. His 1949a paper, however, remains fundamental and is one of his most cited contributions. It outlines an instrumental variable (IV) approach to estimation and allows the citing of Geary, with Reiersöl, as a pioneer of the IV method. The paper begins by considering the problem of estimating $\beta = -\beta_1/\beta_2$ in the model $\beta_1 X_1 + \beta_2 X_2 = 0$ where the X's are observed with error, the observables x_i being equal to $X_i + u_i$. Let z be the IV measured without error and suppose X_1, X_2, z have zero means, are joint normal and temporally uncorrelated. Geary writes the IV estimator of β as

$$b = (\Sigma x_2 z)/(\Sigma x_1 z) \quad (\equiv \text{num/den, say})$$

and shows, using Geary (1944b) that its density can be written

$$\phi(b)db = \frac{\{(n-2)/2\}!(1+y^2)^{-\frac{1}{2}n}}{\{(n-3)/2\}!\sqrt{\pi}} dy$$

where

$$y = \left\{ \frac{Ez^2(b^2Ex_1^2 - 2bEx_1x_2 + Ex_2^2)}{(bEx_1z - Ex_2z)^2} - 1 \right\}^{-\frac{1}{2}}$$

so that $y\sqrt{(n-1)}$ is distributed as t with n-1 degrees of freedom. By considering confidence intervals, it is shown that the precision of the method (asymptotically) is improved if z is chosen as highly correlated with X_1 and X_2 as possible and the idea of finding an optimal combination of instruments is treated.

The theory is then extended to the time series case where the true values of the observables are taken as non-stochastic with the errors normally distributed, independent of each other and of the other variables. The estimator b in this case is

the ratio of two independent normal variables so that Geary (1930b) applies by which $\{bE(\text{den})-E(\text{num})\}/\sqrt{\{b^2 \text{var}(\text{den})+\text{var}(\text{num})\}}$ is a N(0,1) variate. Of course, the expression involves unknown error variances while the previous expression only involves expectations of functions of observables which can easily be consistently estimated. Although the first model would seem to be inapplicable to the time series "sequences of n" case it is found that the operative first theory can be used with confidence in such a case for moderately sized samples with error variances not too large.

Roy's interest in testing for normality and appreciation of the need for robustness studies developed early. By 1930 large sample approximations to the densities of the statistics $\sqrt{b_1}$ and b_2, the sample analogues of the $\sqrt{\beta_1}$ and β_2, the classical measures of skewness and kurtosis, had been derived. By 1935, several of the lower order moments for normal $\sqrt{b_1}$ and b_2, useful in approximating their distributions, had been derived by various writers notably Craig, Wishart and Fisher. Roy was to contribute to several aspects of this broad range of topics. In *Biometrika* 1935 he criticised tests of kurtosis based on b_2 due to the slowness with which b_2 approached normality and suggested tests based on the ratio of the mean deviation to the standard deviation. In *Biometrika* 1936 he found moments of his ratio under normality and provided a table ofcritical values. Other papers dealing with small sample properties of $\sqrt{b_1}$ and b_2 culminated in his outstanding 1947 *Biometrika* paper (1947c) in which he derived approximations to the first four normal moments and asymptotic distribution of a generalization of b_2 and showed that while b_2 was asymptotically the efficient test for kurtosis in the class considered against a wide field of alternatives, there appeared to be little superiority over his own ratio for moderate sample sizes. Similar analysis was carried out on skewness measures. Recent reconsideration of his test, both theoretical and Monte Carlo, has been interestingly favourable. Geary's work on robustness is mostly contained in his 1936 JRSS paper and the

1947 paper just described. In the latter he considers the effect of nonnormality on Fisher's z ($=\frac{1}{2}\log F$) and one and two sample t-tests. Regarding the one sample t-test, for example, he approximated the first six moments in terms of the cumulants of the parent population and approximated the density of the non-normal t with a Charlier differential series with the normal t as generating function using his results for the first few cumulants of non-normal t, assuming a Pearson system population, and collecting terms according to their orders of magnitude. The resultant formula is used to approximate the true left hand tail probabilities. The main trouble is found to be asymmetry, with positive skewness leading to left tail rejection of the null hypothesis too often. This contrasts with the variance ratio test, where kurtosis is the key problem.

Further, as follows from general formulae in Geary (1947c), the approximation to order n^{-1} of var z ($z=\frac{1}{2}\log F$) is

$$\left(\frac{\beta_2-1}{4}\right)\left(\frac{1}{n_1}+\frac{1}{n_2}\right)$$

for two independent samples drawn from the same population, an expression which generalizes Fisher's normal approximation for $\beta_2=3$, and suggests that positive kurtosis would lead to too frequent rejection of variance equality on uncritical use of normality assumptions. Much of this is now standard knowledge.

In the course of these investigations he presented a method for calculating the exact small sample frequency of normal $\sqrt{b_1}$ to any required degree of accuracy (1947a) and calculated, with Worlledge, the seventh moment of normal b_2 (1947). The method of the former paper was an elaboration of that which he used in 1935b and 1936b in finding the distribution of his ratio and consisted of establishing a relation in integral form between the frequency ordinate for n (the sample size) with that for n-1, with a certain actual frequency being shown to be close to its Gram-Charlier representation.

While these three themes account for his most systematic work, other important and attractive results appear throughout his work. For example, he showed in the 1936 paper on robustness that independence of mean and variance imply normality, not just the well-known reverse. In a 1942 JRSS paper, he showed, under regularity conditions, that maximum likelihood estimators minimise the generalized variance for large samples. His 1954 paper in *The Incorporated Statistician* introduced a contiguity ratio of spatial autocorrelation, a generalization of the von Neumann ratio, together with sampling theory which has been used by geographers, sociologists and others and has been included in a book of readings in Statistical Geography. In a 1944 *Biometrika* paper he compared Pitman's 'closeness' with efficiency showing a certain equivalence result in the bivariate normal case and providing an early analysis of "the racing car problem", the estimation of b in the uniform [0,b] distribution. He was also a pioneer in national income accounting, a topic in which he consistently retained interest and influence.

In his later work through the 1960's and 1970's he concentrated more on economic and social issues showing great concern for problems of population movement, poverty, inequality and inflation. He was never an avid reader of other people's work (except Fisher) preferring to work things out for himself. He had a deep love, almost reverence for the arts and mathematics which he thought of as the sublime art. In no sense did he conceive of the application of mathematics as its justification. In fact, he was often sceptical and at times deeply antagonistic towards the use of mathematics in social problems, especially if little mathematical manipulation was involved, which he conceived to be the essence of mathematics. The essence of statistics, he thought, lay in the efficient manipulation of measures so as to derive inference from them. In partial response to a question from the author and earlier, apparently from Fisher as to the essential distinction between mathematics and statistics, he emphasised that mathematics had no place in statistics unless clearly relevant to a statistical problem. He frequently emphasised that a problem generally involved much more than its relevant statistics and resented any charge of materialism on statisticians as much as he resented the use of mathematics for its own sake in the ostensible address of a statistical problem.

He was undoubtedly himself a powerful and energetic mathematician and a magnificently creative statistician, with an unusual emphasis on applicability throughout his work at all times. He had an amazing all-round talent and was a man whose contributions to statistics will not be forgotten.

Department of Economics,
New University of Ulster.

PUTTING COORDINATES ON LATTICES

John Hannah

In this article I shall show how the problem of putting coordinates on certain types of lattices leads to the class of von Neumann regular rings, and discuss briefly the resulting connexion between ring and lattice properties. The article is based on a talk I gave at the Group Theory Conference in Galway on May 13th, 1983, and I would like to thank the organizers both for their invitation to speak and for tolerating the presence of a ring theorist.

Recall that a lattice is a partially ordered set in which any pair of elements a, b have a greatest lower bound $a \wedge b$ and a least upper bound $a \vee b$. We shall be considering complemented modular lattices in what follows. A lattice L is said to be ***complemented*** if it has a least element (denoted by 0) and a greatest element (denoted by 1) and if every element $a \in L$ has a complement $a' \in L$; that is, $a \wedge a' = 0$ and $a \vee a' = 1$. Such complements are not usually unique. We say that L is ***modular*** if whenever $a, b, c, \in L$ with $a \leq c$ then $(a \vee b) \wedge c = a \vee (b \wedge c)$.

<u>Example 1</u>: Let V be any vector space (possibly infinite dimensional) and let L be the set of all subspaces of V ordered by inclusion, so that $a \wedge b = a \cap b$ and $a \vee b = a + b$. Then L is a complemented modular lattice.

2. : Von Neumann, studying rings of operators on Hilbert spaces, came across rings whose sets of projections (that is, self-adjoint idempotent operators p, so that $p = p^* = p^2$) formed lattices if $p \leq q$ was taken to mean that $p = qp$ (so that q is a left and right identity for p). Although there was no simple algebraic formula for $p \wedge q$ and $p \vee q$ in this case, von Neumann was able to show that this lattice was complemented and modular.

Consider for a moment the special case of Example 1 where $V = \mathbb{R}^3$ and L is the lattice of subspaces of V. We can view L as the real projective plane $\mathbf{P}$ with the one-dimensional subspaces being the points of $\mathbf{P}$ and the two-dimensional subspaces the lines of $\mathbf{P}$. In this picture of L the operation $\wedge$ is still intersection but $a \vee b$ corresponds now to the line joining the points a and b. The original space $\mathbb{R}^3$ is now the usual homogeneous co-ordinates for $\mathbf{P}$ and von Neumann wondered if such a "co-ordinatization" was available for his lattice of projections too. It would of course be desirable for such co-ordinates to be related to the original ring structure of his examples, and to see what we should expect we shall look more closely at Example 1.

Notice that the lattice of subspaces of $\mathbb{R}^3$ is "the same" as the lattice of right ideals of the ring $M_3(\mathbb{R})$ of 3x3 matrices with real entries. Indeed a subspace U of $\mathbb{R}^3$ corresponds to the right ideal $\hat{U}$ of $M_3(\mathbb{R})$ consisting of all matrices whose columns belong to U. Thus, for example, the subspace U consisting of all vectors of the form

$$\begin{pmatrix} x \\ 0 \\ 0 \end{pmatrix} \quad \text{is sent to the set } \hat{U} \text{ of all matrices of the form} \quad \begin{pmatrix} x & y & z \\ 0 & 0 & 0 \\ 0 & 0 & 0 \end{pmatrix}.$$

(We could equally use left ideals of $M\ (\mathbb{R})$: since left multiplication in $M_3(\mathbb{R})$ corresponds to row operations we could make the rows of the matrices come from U in that case.)

To see what sort of right ideals should correspond to the lattice elements in general we need to consider the infinite dimensional case of Example 1. In this case we can represent the elements of V as infinite columns almost all of whose entries are zero. For our ring this time we take the ring of all linear transformations on V, represented as infinite square matrices (having dim V rows and columns) each column of which has only finitely many nonzero entries. This representation allows us to use the same lattice isomorphism as above: a subspace U is sent to the set $\hat{U}$ of all matrices whose columns come from U. For example:

$$U = \begin{bmatrix} a \\ a \\ 0 \\ 0 \\ 0 \\ \cdot \\ \cdot \\ \cdot \end{bmatrix} \longmapsto \hat{U} = \begin{bmatrix} a & b & c & \cdots \\ a & b & c & \cdots \\ 0 & 0 & 0 & \cdots \\ 0 & 0 & 0 & \cdots \\ 0 & 0 & 0 & \cdots \\ \cdot & \cdot & \cdot & \\ \cdot & \cdot & \cdot & \\ \cdot & \cdot & \cdot & \end{bmatrix}$$

(Notice that using left ideals here would lead to a different ring since these infinite matrices lack the left-right symmetry which the transpose operation imposes on $M_3(\mathbb{R})$. The choice between left and right here is made when we decide whether to write V as row or column vectors.)

The set $\hat{U}$ is again a right ideal but not all right ideals arise this way (as they do in the case of $M_3(\mathbb{R})$). In fact $\hat{U}$ is a principal right ideal. To find a generator for $\hat{U}$ simply choose a generating set for the subspace U, making sure that the set contains dim V elements, and use the matrix whose columns make up the generating set. The fact that right multiplication corresponds to column operations allows us to generate all of $\hat{U}$ from this one matrix. This example leads us to the definition we have been seeking:

<u>Definition</u>: *Putting co-ordinates on a lattice* L means finding a ring R such that L is lattice-isomorphic to the set $\mathcal{L}(R)$ of principal right ideals of R.

We cannot expect any old ring to be suitable for this purpose: in most rings $\mathcal{L}(R)$ is not even a lattice, let alone complemented and modular. Von Neumann showed that the key property here is being complemented. Indeed let aR be any princpal right ideal of R. If aR has a complement in $\mathcal{L}(R)$ then there is some $b \in R$ such that $aR \wedge bR = 0$ and $aR \vee bR = R$.

Regardless of what $\wedge$ and $\vee$ mean in $\mathcal{L}(R)$ (all we have at this stage is the partial order of inclusion) this gives $aR \cap bR = 0$ and $aR + bR = R$. Thus we can write

$$ax + by = 1.$$

Hence

$$axa + bya = a$$

so that

$$bya = a - axa \in bR \cap aR = 0$$

and so

$$a = axa.$$

Thus, in a co-ordinatizing ring for a complemented modular lattice, for any element a there is some x such that $a = axa$. Rings with this property are said to be (*von Neuman*[n]) *regular*, and von Neumann showed that if R is regular then $\mathcal{L}(R)$ is a complemented modular lattice with $A \wedge B = A \cap B$ and $A \vee B = A + B$.

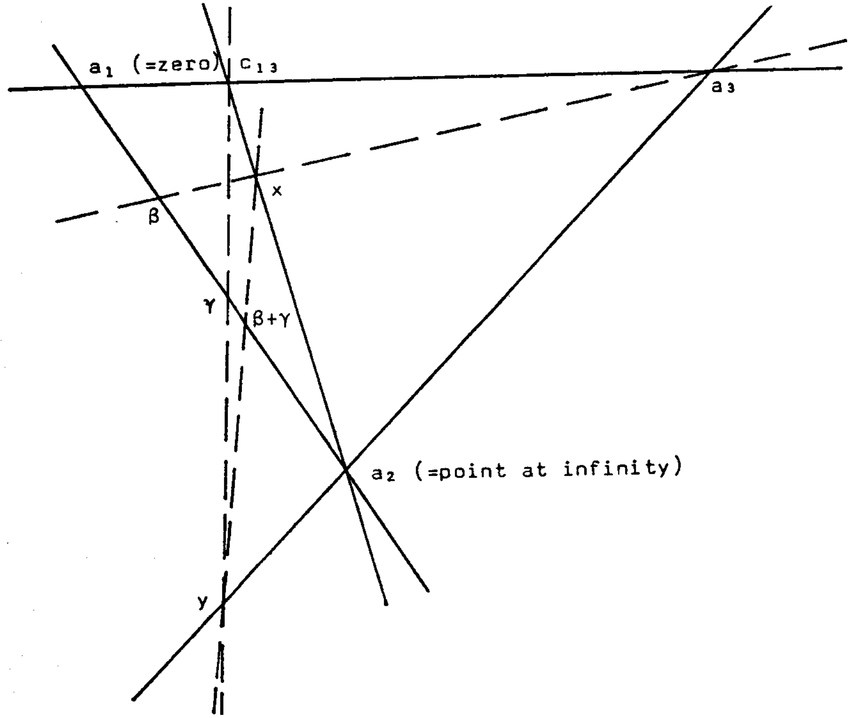
Example 3: If V is any vector space then the ring of all linear transformations of V is a regular ring.

4. It is not hard, using Maschke's Theorem and Example 3, to see that if F is any field and G is any locally finite group having no elements of order equal to the characteristics of F then the group algebra $F[G]$ is a regular ring. Auslander and others have shown that these are the only regula[r] group algebras.

5. The ring of integers Z is not regular.

The original problem, finding what we now know should be a regular ring which co-ordinatizes a given complemented modular lattice, has been solved only in a few special cases. Von Neumann produced a solution which looked after his lattic[e] of projections by imitating the usual construction of co-ordin[in]ates in a projective plane. For this method the lattice mus[t] have a "homogeneous n-frame" with $n \geq 4$. Such a frame consists of n independent lattice elements $a_1, a_2, \ldots a_n$ (so tha[t] $a_i \wedge (a_1 \vee a_2 \vee \ldots a_{i-1} \vee a_{i+1} \ldots \vee a_n) = 0$ for each i) with the properties that $a_1 \vee a_2 \vee \ldots \vee a_n = 1$ and each distinct pa[ir]

a_i, a_j are *perspective* (that is, have a common complement c_{ij} so that $a_i \vee c_{ij} = a_j \vee c_{ij} = a_i \vee a_j$ and $a_i \wedge c_{ij} = a_j \wedge c_{ij} = 0$). This frame plays a similar role to the frame in $\mathbb{R}^3$ consisting of the x, y and z axes. Indeed if we look at the lattice L of all subspaces of $\mathbb{R}^3$ we can get a frame by letting a_1, a_2, a_3 be the x, y and z axes (respectively); a common complement c_{13} of a_1 and a_3 would be, for example, the line $x = z$, $y = 0$. Using the projective plane picture of L we can retrieve the usual addition of points on the line $a_1 \vee a_2$ by using just the lattice operations shown in the diagram: the solid lines represent the frame, the broken lines give the construction for the sum of any two "finite" points β and γ on the line $a_1 \vee a_2$.



1. Join β to a_3 to meet the line joining c_{13} and a_2 at x.
2. Join γ to c_{13} to meet the line joining a_2 and a_3 at y.
3. The line joining x and y meets $a_1 \vee a_2$ at $\beta + \gamma$.

Adding points on $a_1 \vee a_2$

A similar diagram can be used to define the product $\beta\gamma$. In terms of the original lattice and its frame we are making a ring from the set of complements of a_2 in $a_1 \vee a_2$: two such complements β and γ are added and multiplied according to the rules

$$\beta+\gamma = ([(\beta \vee a_3) \wedge (c_{13} \vee a_2)] \vee [(\gamma \vee c_{13}) \wedge (a_2 \vee a_3)]) \wedge (a_1 \vee a_2)$$

$$\beta\gamma = [\beta \vee c_{23}) \wedge (a_1 \vee a_3)] \vee [(\gamma \vee c_{13}) \wedge (a_2 \vee a_3)]) \wedge (a_1 \vee a_2).$$

Von Neumann used these same operations in his more general setting, the extra dimension ($n \geq 4$) being needed to verify that these operations had the desired properties. It can then be shown that the original lattice is the same as the lattice $\mathcal{L}(R)$ where R is the ring of mxn matrices over the ring just constructed. Furthermore this ring is unique up to isomorphism (only a 3-frame, as illustrated in the diagram, is needed for this assertion; the uniqueness breaks down in lower dimensions since, for example, the lattice {0,1} may be co-ordinatized by any division ring). In von Neumann's original setting - where the lattice was the set of projections of a ring of operators - it turns out that there is often an embedding of the original ring in the co-ordinatizing ring, and so the aim of getting a ring compatible with the original structure is achieved in these cases.

In the general setting it is now natural to seek lattice characterizations of the various classes of regular rings and we conclude by considering an example of such a characterization. In a recent paper Munn introduced the class of *bisimple* rings: if the ring R has an identity element, we say it is bisimple if for any pair of nonzero elements a, b there is a third element c such that aR = cR and Rc = Rb. These are just the rings whose multiplicative semigroups are bisimple. Any division ring is bisimple but all other bisimple rings are quite large; for example if S is the ring of all linear transformations on a countable-dimensional vector space, and if I is the ideal of finite-rank transformations then the factor ring

S/I is bisimple. Munn showed that all bisimple rings are simple (in the above notation we have RaR = RcR = RbR) and regular, and so we can ask which complemented modular lattices correspond to these rings.

A hint is given by another of Munn's results: any pair of nonzero principal right ideals of a bisimple ring R must be isomorphic as R-modules (replacing a by the c given by the definition we may assume the right ideals are aR, bR where Ra = Rb; then a = xb and b = ya and left multiplication by y gives an isomorphism from aR to bR). Hence in the lattice $\mathcal{L}(R)$ any two nontrivial intervals $[0,A]$ and $[0,B]$ are isomorphic as lattices. Unfortunately such lattice isomorphisms throw away too much of the ring structure for this property to characterize bisimple rings. However a stronger isomorphism is provided by the notion of perspectivity that we have already met: if lattice elements A, B are perspective they have a common complement C, say, so that

$$A \vee C = B \vee C = A \vee B$$

with

$$A \wedge C = B \wedge C = 0.$$

Hence

$$A = \frac{A \vee B}{C} = B$$

where the isomorphisms will be module isomorphisms if we are working inside (R). Perspectivity by itself is too strong for our purposes (since if $A < B$ we want $A \cong B$ but clearly cannot have A and B perspective) but a simple splitting trick allows us to get round this problem:

<u>Result</u>: Let R be a regular ring with identity and let $\mathcal{L}(R)$ be its lattice of principal right ideals. Then R is bisimple if and only if $\mathcal{L}(R)$ satisfies

(*) ... for any nonzero a, b $\in \mathcal{L}(R)$ there are splittings

$$a = a_1 \vee a_2 \quad \text{where} \quad a_1 \wedge a_2 = 0$$

and

$$b = b_1 \vee b_2 \quad \text{where} \quad b_1 \wedge b_2 = 0$$

such that a_1, b_1 and a_2, b_2 are perspective pairs of elements of $\mathcal{L}(R)$.

The key idea of the proof here is that if A, B are principal right ideals of a bisimple ring R such that $A \cap B = 0$ then A and B are perspective (as before we may assume $A = aR$ and $B = bR$ where $Ra = Rb$; then $c = (a+b)R$ is a common complement of A and B).

A stronger result is also true: any complemented modular lattice satisfying the condition (*) is easily seen to possess a homogeneous 4-frame (or else be the lattice $\{0,1\}$) and so, by von Neumann's result, can be co-ordinatized by a (necessarily bisimple) regular ring.

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TEN COUNTEREXAMPLES IN GROUP THEORY

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Introduction

Many major theorems in the theory of finite groups have been proved by the minimum counterexample technique, which works as follows. We assume that the theorem is false and let G be a counterexample of smallest possible order. The assumption that G exists is then used to force a contradiction and the theorem in question is thereby established. In practice, the contradiction frequently arises from the existence of a counterexample of order less than that of the presumed minimum counterexample (m.c.e.). This technique of course is merely a disguised form of induction or the method of infinite descent used in number theory.

However, even when a conjecture about finite groups turns out to be false, it is often of interest to discover an m.c.e., or "least criminal" as it is often called. Note that an m.c.e. need not be unique. Searching for an m.c.e. is a very good method of becoming familiar with the groups of small order and perhaps the size of an m.c.e. is an indication of how plausible the conjecture was in the first place!

In this article we discuss ten "not implausible" conjectures about finite groups and produce an m.c.e. in each case. We outline the arguments used in establishing that a given group is an m.c.e.

The material in this paper is based on the author's M.A. thesis "Minimum Counterexamples in Group Theory", University College, Cork, 1982, prepared under the supervision of Dr. D. MacHale. I wish to thank Dr. MacHale for suggesting this problem and the Mathematics Department of U.C.C. for their co-operation and facilities.

Preliminaries

In what follows G and H will always denote finite groups and p and q will denote prime numbers. We make use of the following facts from elementary group theory.

1. If $H \lhd G$ and $|H| = 2$ then $H \leq Z(G)$.
2. (a) If $|G| = pq$ and $q \not\equiv 1 \pmod p$, then G has a normal Sylow p-subgroup.
   (b) if $|G| = p^2q$ then G has a normal Sylow subgroup, ([1], page 97)
3. For any finite group G, Inn $G \simeq G/Z(G)$.
4. If $N \trianglelefteq G$, then G/N is abelian iff $N \geq G'$. ([1], page 59)
5. If $H, K \leq G$ so that Hchar $K \trianglelefteq G$, then $H \trianglelefteq G$. ([2], page 73)
6. Let $H \leq G$, $K \trianglelefteq G$, and $\Phi(G)$ denote the Frattini subgroup of G, then (a) $K \leq \Phi(G)$ iff there does not exist $H < G$ so that $HK = G$.
   (b) $K \leq \Phi(H) \Longrightarrow K \leq \Phi(G)$. ([1], page 269)
7. If $H < G$ then (a) G/H_G can be embedded in $S_{|G:H|}$
   (b) $N_G(H)/C_G(H)$ can be embedded in Aut(H). ([1], page 74, 84)
8. If G is a transitive permutation group on a set Ω, then the stabilizers $G_\alpha (\alpha \in \Omega)$ are all conjugate to one another and if $|\Omega| = n$, then $|G:G_\alpha| = n$. ([4], page 15)
9. There exist exactly two non-abelian simple groups of order less than 360 namely A_5 of order 60 and PSL(2:7) of order 168.
10. If G is a non abelian group then G is insoluble iff some subgroup H of G (possibly H = G) contains a non abelian simple factor group (possibly the trivial factor group H/1).
11. The following is a list of all the groups of order less than 16.

    C_n, $1 \leq n \leq 15$; $C_p \times C_p$, p = 2 or 3; D_n, $3 \leq n \leq 7$; $C_2 \times C_4$; $C_2 \times C_2 \times C_2$; Q; $C_2 \times C_6$; A_4; Q_6.

Conjectures

Conjecture 1: $G' = G \Longrightarrow Z(G) = 1$.

The motivation for this conjecture is of course the fact that if G is abelian then G' is trivial and also G' is in some way a measure of the commutativity of G. Now we know that if G is soluble then G' is properly contained in G and so a counter-example can only be found within the class of insoluble groups. $G \simeq A_5$ satisfies the conjecture. Hence all groups of order less than 120 are ruled out as counter-examples (9, 10). We show $G \simeq SL(2:5)$ of order 120 is an m.c.e. The map det : $GL(2:5) \to \{1,2,3,4\}$ is an onto homomorphism whose kernel is SL(2:5). Hence $|G| = 480/4 = 120$. $G/Z(G)$ is simple non abelian ([1], page 66) $\Rightarrow$ Z(G) is maximal normal in G (Isomorphism Theorem) $\Rightarrow$ Z(G) is the only proper normal subgroup in G. [For suppose there exists $H \neq Z(G)$ such that $H \lhd G$. Then $HZ(G) \trianglelefteq G$ and HZ(G) contains Z(G) properly. Hence $HZ(G) = G$. Now

$$Z(G) = \left\{\begin{pmatrix}1 & 0\\0 & 1\end{pmatrix}, \begin{pmatrix}4 & 0\\0 & 4\end{pmatrix}\right\} < \left\{\begin{pmatrix}1 & 0\\0 & 1\end{pmatrix}, \begin{pmatrix}2 & 0\\0 & 3\end{pmatrix}, \begin{pmatrix}4 & 0\\0 & 4\end{pmatrix}, \begin{pmatrix}3 & 0\\0 & 2\end{pmatrix}\right\} = K,$$

and $K \simeq C_4$. Hence by (6) $Z(G) \subseteq \Phi(G)$ and H = G]. G non abelian $\Rightarrow G' \neq 1$. $G/Z(G)$ non abelian $\Rightarrow G' \nsubseteq Z(G)$ (4). Since $G' \trianglelefteq G$ we must have $G' = G$.

Conjecture 2: Conformal groups are isomorphic, i.e. if G and H have exactly the same number of elements of each order then $G \simeq H$.

This conjecture is true for abelian groups but unfortunately non abelian groups do not fit this pattern. In fact within the groups of order 16 we can find 3 non isomorphic groups all of which are conformal. We also see that an abelian and a non abelian group may be conformal. For a counter-example it is clear that we need two non isomorphic, non cyclic groups of the same order and this condition rules out all the groups of order less than 16 except the groups of order 8 and 12, (11), and these groups are eliminated by counting elements of each order. $G \simeq C_4 \times C_4$ and $H \simeq Q \times C_2$ supply us with an m.c.e. as both groups each contain three elements of order 2 and twelve elements of order 4. G is abelian. H is non abelian. The group $K = \langle x, y : x^4 = 1 = y^4, xy = yx^{-1} \rangle$ is also conformal with G and H. We also note that this m.c.e. is not unique as among the groups of order 16 there exist two other groups which are conformal, namely:

$$\langle a,b,c : a^2 = 1 = b^2 = c^2, \quad abc = bca = cab \rangle$$

and

$$\langle a,b,c : a^4 = 1 = b^2 = c^2, \ ab = ba, \ ac = ca\,b, \ bc=cb \rangle$$

<u>Conjecture 3</u>: Given a nilpotent group G, there exists a finite group H such that $G \simeq \Phi(H)$.

For any group G, $\Phi(G)$ is nilpotent so this conjecture poses the interesting question "is every nilpotent group the Frattini subgroup of some group?" Since $\Phi(C_{p^2}) \simeq C_p$, $\Phi(C_8) \simeq C_4$, $\Phi(C_4 \times C_4) \simeq V_4$, $\Phi(C_9 \times C_4) \simeq C_6$ and S_3 is not nilpotent all groups of order less than 8 are ruled out as counter-examples (11). $G \simeq Q$ is an m.c.e. For suppose there exists H such that $\Phi(H) \simeq Q$ and let $C = C_H(\Phi(H))$. Then $C \trianglelefteq H$. Now M/C is max. in H/C $\Longrightarrow$ M max. in H(Isomorphism Theorem) $\Longrightarrow$ $M \supseteq \Phi(H)$. Hence $C\Phi(H)/C \subseteq \Phi(H/C)$ ......(*). Also $C\Phi(H)/C \simeq \Phi(H)/(C \cap \Phi(H))$ (Isomorphism Theorem). Now $Z(\Phi(H))$ char $\Phi(H) \trianglelefteq H$ ($Z(G)$ and $\Phi(G)$ are char in G) $\Longrightarrow$ $Z(\Phi(H)) \trianglelefteq H$ (5). $|Z(\Phi(H)| = 2$ ($\Phi(H) \simeq Q$). So $Z(\Phi(H) \leq Z(H)$ (1) and clearly $C \cap \Phi(H) = Z(\Phi(H))$ Hence $C\Phi(H)/C \simeq V_4$. Hence $\Phi(H/C)$ contains a subgroup isomorphic to V_4 (*). On the other hand $H = N_H(\Phi(H))$ and $C = C_H(\Phi(H))$ so H/C can be embedded in $Aut(Q) \simeq S_4$ (7). But all subgroups of S_4 have Frattini subgroup of order 1 or 2 which gives a contradiction.

<u>Conjecture 4</u>: If G is not simple then G has a normal Sylow subgroup.

A p-group cannot furnish us with a counter-example and groups of order pq and p^2q are ruled out by (2). Hence all groups of order less than 24 are ruled out as counter-examples. We show S_4 of order 24 is an m.c.e. S_4 prime $\simeq A$ of order 12. Hence no Sylow 2-subgroup is normal (4). A_4 and hence S_4 contains eight elements of order 3 $\Longrightarrow$ a Sylow 3-subgroup is not unique and so not normal.

<u>Conjecture 5</u>: If G is not simple then G has a non-trivial endomorphism.

The motivation for this conjecture comes from the fact that if G is not simple, G certainly has a non-trivial homomorphic image as there is a 1:1 correspondence between the normal subgroups of G and the homomorphic images of G. Now we know that if G is soluble then G contains a normal subgroup of prime index, H say, and by Cauchy's Theorem, G/H can be embedded in G. Hence G has a non-trivial endomorphism with kernel H. So this rules out as counter-examples all groups of order less than 120 (9, 10). We show that $G \simeq SL(2{:}5)$ of order 120 is an m.c.e. By conjecture 1, Z(G) is the unique normal proper subgroup of G and $G/Z(G) \simeq A_5$. Hence the only possibility for a non-trivial endomorphism α is $\ker \alpha = Z(G)$. But G contains no subgroup, H, isomorphic to A_5, otherwise $H < G$, contradicting the uniqueness of Z(G). Hence $SL(2{:}5)$ has no non-trivial endomorphism.

<u>Conjecture 6</u>: If an automorphism α of G sends every conjugacy class of G onto itself then α must be inner.

Let A denote the set of all automorphisms of G which send each conjugacy of G onto itself. Then $A < Aut(G)$ by checking closure. Clearly $Inn(G) \leq A$. Hence in eliminating groups as counter-examples we merely show $|A| < 2|Inn(G)| = 2|G/Z(G)|$, (3). All groups of order less than 32 can be eliminated as counter-examples. Much of the detail is just routine so we merely outline the arguments one may use. We note here that if $x \in G$, then $|G:G'| \leq |C_G(x)| \geq 2|Z(G)|$ and $K(x)$, the conjugacy class of x, has $|G:C_G(x)|$ elements. If G is a non-abelian 2-generator group of order less than 32 then x and y can always be chosen such that $G = \langle x,y \rangle$ and $|K(x)||K(y)| < 2|G/Z(G)|$ and so $|A| < 2|G/(Z(G)|$. If G is a 3-generator group of order less than 32, then $z,x,y \in G$ can nearly always be chosen such that $G = \langle z,x,y \rangle$ where $z \in Z(G)$ and $|K(x)|\,|K(y)| < 2|G/Z(G)|$. There is only one exception, namely if G is a non abelian 3-generator group of order 18 with trivial centre. Clearly in this case we have $G = \langle a,b,x \rangle$ where $|a| = |b| = 3$ and $|x| = 2$. $\langle a,x \rangle$ is a proper subgroup of G and clearly $\langle a,x \rangle \simeq S_3$. Hence $a = g^{-1}a^2 g$ for some $g \in \langle a,x \rangle$, We conclude that if $k_1, k_2 \in \langle a,b \rangle$ and $k_1 = g^{-1}k_2 g$ $(g \in G)$, then $k_1 k_2 = 1$ .....(*) (Otherwise G is a 2-generator group). Now $G = \langle a,b,x \rangle$ where $|K(a)| = |K(b)| = 2$ and $|K(x)| = 9$. So $|A| \leq 36$. But α defined by $a \longmapsto a^2$, $b \longmapsto b$, $x \longmapsto x$ is one of the 36 possible maps in A and $\alpha(ab) = a^2 b$ but $ab\,a^2 b = b^2 = 1$ contradicting (*). Hence $\alpha \notin A$ and $|A| < 36 = 2|G/Z(G)|$.

In analysing the groups of order 24, it may be useful to divide them into the following four categories: (1) both Sylow subgroups normal; (2) only the Sylow 3-subgroup normal; (3) only the Sylow 2-subgroup normal, and (4) no Sylow subgroup is normal. In (1) $G \simeq D_4 \times C_3$ or $G \simeq Q \times C_3$ and G is a 2-generator group. In (2) $G \simeq H_\phi \times K$, where $|H| = 8$ and $|K| = 3$. G may be a 3-generator group but one generating element can always be chosen from the centre. In (3) $G \simeq H_\phi \times K$, where $|H| = 3$ and $|K| = 8$ and G is a 2-generator group. In (4) $G \simeq S$ and $\mathrm{Aut}(S_4) = \mathrm{Inn}(S_4)$. An m.c.e. of order 32 can be found in [4], page 24.

<u>Conjecture 7</u>: If every maximal subgroup of G has prime power index, then G is soluble.

The converse to this conjecture is true and in fact the conjecture itself is "very nearly true" as $PSL(2:7)$ seems to be the only non abelian simple group with the property that

every maximal subgroup has prime power index. Now G insoluble of order less than $168 \Rightarrow G \simeq A_5$ or $|G| = 120$ (9, 10). Let P be a Sylow 5-subgroup of A_5. Then $N_{A_5}(P)$ is a maximal subgroup of A_5 of index 6. Hence by (9), (10) and the Isomorphism Theorem all insoluble groups of order 120 also have maximal subgroups of composite index. The subgroup of A_7 generated by $\{(1234567), (26), (34)\}$ is simple of order 168 (See [4], page 18). Let P be a Sylow 7-subgroup. $|G:N_G(P)| = 8$ is the number of Sylow 7-subgroups (Sylow Theorem). Hence the normalizers of the eight Sylow 7-subgroups form a single conjugacy class of 8 maximal subgroups each having index 8 in G. Also G is a transitive permutation group of degree 7, so $\{G_i : 1 \leq i \leq 7\}$ (where G_i is the stabilizer of i in G) is a conjugacy class of 7 subgroups in G each having index 7 in G (8). Now every proper subgroup of G of composite index is contained in one of the above maximal subgroups of prime power index. But G is insoluble as it is simple.

<u>Conjecture 8</u>: Every finite group G has a maximal subgroup of prime index.

Here we use the minimum counter-example technique. Let G be an m.c.e and N a maximal normal subgroup of G. Suppose $|N| > 1$. Then by hypothesis G/N contains a maximal subgroup of prime index, H/N say, $\Longrightarrow$ H has prime index in G. (Isomorphism Theorem). Hence $|N| = 1$ and G is simple. The two simple groups of order less than 360 (9) are eliminated by (8) and conjecture 7. $G \simeq A_6$ of order 360 is an m.c.e. for if there exists $H < G$, such that $|G:H|$ is prime then by (7) and the simplicity of G, $G \leq S_5$ which is impossible.

<u>Conjecture 9</u>: Let $K \trianglelefteq G$. Then if both K and G/K are supersoluble then G is supersoluble.

This conjecture is true for solubility. Groups of order pq are ruled out by (2) and finite p-groups are nilpotent and hence supersoluble. This eliminates all groups of order less than 12. In A_4 of order 12, $1 < V_4 < A_4$ is the only possible normal chain and V_4 is not cyclic. But V_4 and A_4/V_4 are supersoluble.

Conjecture 10: If H is a subnormal subgroup ef G, then the sequence from H of subgroups of G formed by taking successive normalizers in G reaches G.

If H is subnormal in G, then by definition there exists a normal chain from H to G. However the conjecture is not true. One of the characteristics of nilpotent groups is that every proper subgroup is a proper subgroup of its normalizer and so all finite p-groups are eliminated as counter-examples. In groups of order pq or p^2q a non-trivial subnormal subgroup is either normal or its normalizer is normal. This rules out all groups of order less than 24. $G \simeq S_4$ of order 24 is an m.c.e. since in S_4.

$$\{1\} \lhd \{(1), (12)(34)\} \lhd \{(1), (12)(34), (13)(24), (14)(23)\}$$
$$\lhd A_4 \lhd S_4$$

is a composition series. So $H = \{(1), (12)(34)\}$ is subnormal in G but $N_G(H)$ is a Sylow 2-subgroup of G, has index 3 in G and is not normal in G. Hence $N_G(H)$ is self normalizing.

We conclude with three "partially solved" problems and the author invites comments or solutions.

(1) The kernel, K, of a Frobenius group G is abelian.

If G is an m.c.e. then $120 \leq |G| \leq 256$.

(2) G non-abelian $\Rightarrow$ Aut(G) non-abelian.

If G is an m.c.e. then G is a p-group and $|G| \geq 32$.

(3) Every group G has a p-complement where $p \mid |G|$.

If G is an m.c.e., $120 \leq |G| \leq 360$.

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SUBDIVISION OF SIMPLEXES - IS BISECTION BEST?

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Problem 1

A closed and bounded interval in $\mathbb{R}$ is to be subdivided into 2 intervals by insertion of a single point. These 2 intervals in turn are to be subdivided into 4 intervals by insertion of a point in each. Continue this process. Let d_n be the length of the longest interval at the n th stage. How should the insertion points be chosen so as to minimize d

This is not very difficult! Obviously points should be inserted at the midpoints of intervals, i.e. the optimal poli is to bisect intervals at each stage.

Why is Problem 1 of interest? (No doubt for many reade this is a much harder question than Problem 1 itself!). Wel if we wish to solve $f(x) = 0$, $f:[a,b] \to \mathbb{R}$ with $f(a)f(b) < 0$, and we want after some fixed number of function evaluations t find an interval of minimum length that is guaranteed to contain a root of f, then Problem 1 shows that the classical bisection method is best. (For any other algorithm there is some function f for which the interval found is longer). In this article we shall examine the relevance of bisection to an n-dimensional generalization of Problem 1 which we'll call Problem n. This problem has as yet no complete solution. I arises in the comparison of methods used to solve $f(x)=(0,0..$ for $f : \mathbb{R}^n \to \mathbb{R}^n$, but we shall discuss it purely from a geometric viewpoint.

Generalizing Problem 1 to $\mathbb{R}^n$

We first replace closed and bounded intervals by n-simplexes (triangles when n=2, tetrahedra when n=3). The reason that we generalize intervals to triangles and not rectangles is that if $g : \mathbb{R}^2 \to \mathbb{R}$, then on each triangle in $\mathbb{R}^2$ there is a unique affine function which interpolates g at the vertices

of that triangle; this is very useful when approximating a root of g, and rectangles do not have the same property. Similarly in higher dimensions.

Definitions: For $n \geq 1$, an n-simplex $S^n = (a_0a_1...a_n)$ is the closed convex hull of n+1 points $a_0, a_1, ..., a_n$ in $\mathbb{R}^q$, $q \geq n$, such that the vectors $a_1-a_0, a_2-a_0, ..., a_n-a_0$ are linearly independent. The points $a_0, a_1, ..., a_n$ are called the vertices of S^n. Any m-simplex $(1 \leq m \leq n)$ formed by taking the closed convex hull of any m+1 vertices of S^n is called a face of S^n. The one-dimensional faces (a_ia_j), $0 \leq i < j \leq n$, are called the edges of S^n. The diameter of S^n, $d(S^n)$, is the length of the longest edge of S^n in the Euclidean norm.

We subdivide any n-simplex $S^n = (a_0a_1...a_n)$ as follows. Choose a point $y \in S^n$. Form all n-simplexes $(a_0a_1... a_{i-1}y\, a_{i+1} ... a_n)$. Note that if m is the minimum dimension of a face of S^n containing y, then the subdivision yields m+1 n-simplexes.

roblem n

iven an n-simplex S^n subdivide it as just described. This is e first stage. Similarly subdivide the resulting n-simplexes inserting a point in each. This is the second stage. ntinue thus. Let A_k denote the set of all n-simplexes T^n tained at the k th stage. Define

$$d_k = \max_{T^n \in A_k} d(T^n)$$

find an algorithm for inserting points which will yield

$$d_{kn} \leq Cr^k \text{ for } k = 1,2,3...$$

ere $r > 0$ (independent of S^n) is as small as possible and C pends on S^n only. (We consider d_{kn} rather than d_k as expience shows it's a more natural measure).

Example: For the n=1 case with $S^1 = [a,b]$ the bisection method yields an equality:

$$d_k = (b-a)(1/2)^k, \; k = 1,2,3...$$

Only partial results have been obtained for Problem n. The principal reference is [7]. There it is shown (essentially) that for any n and any algorithm one must have $r \geq \frac{1}{4}$, but it is also conjectured that in fact one must have $r \geq \frac{1}{2}$. An algorithm for which $r = \frac{1}{2}$ is exhibited in [7]; it is based on Whitney's simplical subdivision [8, pp 358-360]. This algorithm may be fairly described as a generalization of the one-dimensional bisection method. Nevertheless a different generalization has become established as the "n-dimensional bisection method" [1,2,3,4,5,6]. We shall concentrate on this latter algorithm as it is simple to describe, it is clearly a generalization of the one-dimensional method, and yet it has not been satisfactorily analysed up to now.

The n-Dimensional Bisection Method

For $n > 1$, given an n-simplex T^n choose any edge $(a_i a_j)$ of T^n whose length is $d(T^n)$. Let b be the midpoint of this edge. Bisect T^n into $(a_0..a_{i-1}\ b\ a_{i+1} .. a_n)$ and $(a_0 .. a_{j-1}\ b\ a_{j+1} .. a_n)$. That is, b is the point inserted into T^n to subdivide it.

In attacking Problem n this method is intuitively attractive. To decrease the diameter of an n-simplex one must divide edges, and the bisection method bisects the longest edge. It's intuitively reasonable that the method will yield n-simplexes of diameters shrinking to zero, and this fact was implicitly assumed in [4]; however a proof did not appear until later [1].

To demonstrate the elementary nature of the arguments which can be used in relation to Problem n, we shall give a new proof of (a slightly stronger result than) the main theorem of [1].

Lemma. Given a triangle (2-simplex) of diameter d, the length of the median obtained by joining the midpoint of the longest edge to the opposite vertex is at most $\sqrt{3}\, d/2$.

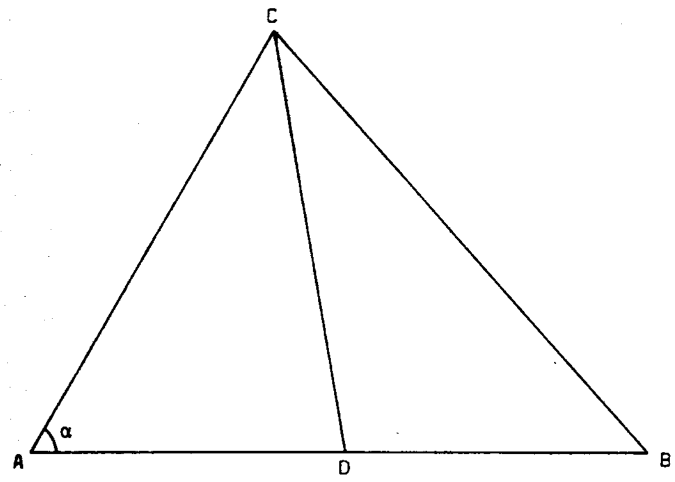

FIGURE 1

Proof. See Figure 1. There $AB \geq AC$, $AB \geq BC$, D is the midpoint of AB. Now

$$BC^2 = AC^2 + AB^2 - 2AC.AB\cos\alpha$$

so $AB \geq BC$ implies $\cos\alpha \geq AC/(2AB)$

Hence
$$\begin{aligned} CD^2 &= AC^2 + AD^2 - 2AC.AD\cos\alpha \\ &\leq AC^2 + AB^2/4 - AC.AB.AC/(2AB) \\ &\leq 3AB^2/4 \text{ as required.} \end{aligned}$$

Now any new edge $(a_k b)$ say formed by the bisection method on an n-simplex T^n lies in the triangle $(a_i a_j a_k)$ just as CD is in ABC above. From the Lemma it follows that the length of the new edge is at most $\sqrt{3}\,d(T^n)/2$.

Theorem. Let S^n be an n-simplex having exactly m+1 vertices as endpoints of edges of length greater than $\sqrt{3}d(S^n)/2$. Then after m iterations of the bisection method the diameter of any resulting n-simplex is at most $\sqrt{3}d(S^n)/2$.

Proof. Bisected edges have length at most $d(S^n)/2$. New edges have length at most $\sqrt{3}d(S^n)/2$ by the Lemma. So we need only show that after m iterations any edge of S^n whose length exceeds $\sqrt{3}d(S^n)/2$ has been bisected.

Let $S^n=(a_0a_1..a_m.a_n)$ where without loss of generality we assume that among all the a_k only $a_0, a_1,...,a_m$ are endpoints of edges whose lengths exceed $\sqrt{3}d(S^n)/2$. At the first bisection S^n becomes

$$S_1^n = (a_0..a_i..b..a_m..a_n)$$

and

$$S_2^n = (a_0..b..a_j..a_m..a_n)$$

where b is the midpoint of (a_ia_j) and $0 \leq i < j \leq m$. Consider S_1^n. By the first paragraph of the proof only the vertices $a_0,...,a_{j-1}, a_{j+1},..,a_m$ can be endpoints of edges exceeding $\sqrt{3}d(S^n)/2$ in length. That is, S_1^n has at most m vertices with this property. Similarly for S_2^n.

At the next iteration we will obtain 4 n-simplexes each having at most m-1 vertices which are endpoints of edges exceeding $\sqrt{3}d(S^n)/2$ in length. Repeating this argument n times in all proves the theorem.

Corollary. (Notation as in Problem n). For the n-dimensional bisection method we have

$$d_{kn} \leq C(\sqrt{3}/2)^k, \quad k = 1,2,3...$$

Proof. An n-simplex has n+1 vertices so in the Theorem we have m=n at most. Thus the Theorem implies that

$$d_n \leq d(S^n)\sqrt{3}/2.$$

Applying the Theorem again to each of the 2^n n-simplexes present after n iterations gives

$$d_{2n} \leq (d(S^n)\sqrt{3}/2).(\sqrt{3}/2) = d(S^n)\ (\sqrt{3}/2)^2.$$

Repeating this argument yields the Corollary.

In the notation of Problem n we have shown $r \leq \sqrt{3}/2$ for the bisection method. However for n=1 we clearly have $r = \frac{1}{2}$, and in fact for n > 1 all the computational evidence is that $r = \frac{1}{2}$ also. For n=2 it has been proven that $r = \frac{1}{2}$ [6] but the proof relies on a case by case analysis which is unlikely to extend to n > 2. Proving that $r = \frac{1}{2}$ for the bisection method when n > 2 is an open problem which I feel should not be too difficult - if one can find the right approach!

We close by pointing out that in fact $r \geq \frac{1}{2}$ for the bisection method.

Proposition. In the notation of Problem n we have $r \geq \frac{1}{2}$ for the bisection method.

Proof. For every n-simplex T^n let $V(T^n)$ denote the n-dimensional volume of T^n. Note that

$$V(T^n) < d^n(T^n) \qquad .... (*)$$

When an n-simplex is bisected it's not difficult to show that its n-dimensional volume is halved. Thus after kn iterations (k = 1,2,3,...) of the bisection method applied to S^n (say) the volume of any n-simplex T_k^n obtained is $V(S^n)/2^{kn}$.

Suppose now that the bisection method yields $r < \frac{1}{2}$ in Problem n. Then choosing a sequence of n-simplexes T_k^n, k = 1,2,3,... (notation as above) we have

$$\frac{V(T_k^n)}{d^n(T_k^n)} \geq \frac{V(S^n)}{C(2r)^{kn}} \to \infty \text{ as } k \to \infty.$$

This contradicts (*). Hence $r < \frac{1}{2}$ is impossible.

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A BIOGRAPHICAL GLIMPSE OF WILLIAM SEALY GOSSET

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William Sealy Gosset, alias 'Student' was an immensely talented scientist of diverse interests, but who will be primarily remembered for his contributions to the development of modern statistics. Born in Canterbury in 1876, he was educated at Winchester and New College, Oxford, where he studied chemistry and mathematics.

At the turn of the 19th century, Arthur Guinness, Son & Co. became interested in hiring scientists to analyse data concerned with various aspects of its brewing process. Gosset was to be one of the first of these scientists, and so it was that in 1899 he moved to Dublin to take up a job as a 'brewer' at St, James' Gate. In 1935 he left Dublin to become 'head brewer' in London but died soon thereafter at the young age of 61 in 1937.

After initially finding his feet at the brewery in Dublin, Gosset wrote a report for Guinness in 1904 on "The Application of the Law of Error to work of the Brewery". The report emphasised the importance of probability theory in setting an exact value on the results of brewery experiments, many of which were probable but not certain. Most of the report was the classic theory of errors (Airy and Merriman) being applied to brewery analysis, but it also showed signs of a curious mind at work exploring new statistical horizons. The report concluded that a mathematician should be consulted about special problems with small samples in the brewery. This led to Gosset's first meeting with Karl Pearson in 1905.

Karl Pearson (1857-1936) headed at University College London an industrious biometric laboratory which was very much concerned with large sample statistical analysis. Pearson had developed an extensive family of distribution curves, written an important paper introducing the χ^2 goodness of fit criterion, and initiated the journal *Biometrika*. Gosset was introduced by Pearson to correlation coefficients and large sample theory. It was not long before correlation coefficients of various types were being used extensively in new work at the brewery. Gosset soon realised however that modifications of Pearson's large sample theory were needed for the special small sample problems that were encountered in the brewery.

In 1906, Gosset received a year's leave from Guinness for specialised study. Most of the year he spent in close contact with Pearson's biometric laboratory in London, where he worked on small sample problems. It was during this time that he laid the basis for his most famous work "The Probable Error of a Mean" which was published in *Biometrika* in 1908.

Student published twenty-two papers, the first of which was entitled "On the Error of Counting with a Haemacytometer" (*Biometrika*, 1907). In this first paper, Student illustrated the practical use of the Poisson distribution in counting the number of yeast cells on a square of a haemacytometer. Up until just before World War II, Guinness would not allow its employees to publish under their own names, and hence it was that Gosset chose to write under the pseudonym of 'Student'.

Most statistical analysis at this time dealt with large sample theory. When investigating the mean of, say, a normal population, it was standard procedure to (1) calculate the sample mean $\overline{x}$, (2) calculate the sample standard deviation = $(\Sigma(x_i-\overline{x})^2/n)^{\frac{1}{2}}$, and (3) use the 'normal' probability tables to make statements about the mean. This was of course reasonable for large n, but what about when dealing with small sample sizes? It was thus that Student's classic paper "The Probable Error of a Mean" read:

> *The usual method of determining the probability that the mean of the population lies within a given distance of the mean of the sample is to assume a normal distribution about the mean of the sample with a standard deviation equal to $s/\sqrt{n}$, where s is the standard deviation of the sample and to use the tables of the probability integral ...*
>
> *There are experiments, however, which cannot be easily repeated very often; in such cases it is sometimes necessary to judge of the certainty of the results from a very small sample, which itself affords the only indication of the variability. Some chemical, many biological, and most agricultural and large scale experiments belong to this class, which has hitherto been almost outside the range of statistical enquiry.*
>
> *Again, although it is well known that the method of using the normal curve is only trustworthy when the sample is "large", no one has yet told us very clearly where the limit between large and small samples is to be drawn.*
>
> *The aim of the present paper is to determine the point at which we may use the tables of the probability integral in judging of the significance of the mean of a series of experiments, and to furnish alternative tables for use when the number of experiments is too few.*

"The Probable Error of a Mean" was one of the first papers
ch a clear distinction was made between population par-
s and sample estimates of them (for example (u,σ^2) *vs*
). Assuming a random sample $(x_1,\ldots,x_n)$ from a normal
tion with mean u and variance σ^2, Gosset went on to cal-
the first 4 moments of s $= \sum_1^n (x_i-\overline{x})^2/n$, and then
ed that the distribution of s^2 is given by a Pearson
II curve. (Although the conclusion was correct, the
nt was not complete. To show that a statistic has a
ic distribution it is not in general sufficient to show
ts first 4 moments coincide with those of the distribut-
question.) In modern language, Gosset inferred that
$)^2/\sigma^2$ is a χ^2 random variable with n-1 degrees of freedom.
esult had actually been discovered by Helmert in 1876,
s 'unknown' to English biometers in 1908. Next Gosset
that $\overline{x}$ and s^2 were uncorrelated, and inferred (again
tly but without sufficient reason) their independence.
n derived the distribution of $z = (\overline{x}-u)/s$ to be of the
(z) = $C(1+z^2)^{-n/2}$. This was of course the main result

of the paper. In about 1922, the use of z was replaced by R.A. Fisher and Gosset with $t = \sqrt{n-1}\,z$, and hence is now known as Student's t. (See "On the transition of Student's z to Student's t" by C. Eisenhart).

Gosset checked the adequacy of his theoretical distributions for s^2 and z (equivalently t) on the basis of a data set of height and middle finger length of 3000 criminals. This was one of the first examples of a "simulated" experiment to be used in statistical research. Tables for his z statistic were also given, and Gosset concluded his paper with 4 practical illustrations of his method (e.g. the 't-test').

Due to the general lack of interest in 'small sample' statistics at the time, Gosset's method was not extensively used outside of the brewery for many years. It was mainly due to the promotional efforts of R.A. Fisher (1890-1962) that it eventually did become widely used. R.A. Fisher was a student at Cambridge in 1912 when he first wrote to Gosset with a rigorous proof of the frequency distribution of the z statistic. In a subsequent letter, Fisher showed that one should be dividing by n-1 instead of n in the formula for the sample standard deviation. Not fully understanding Fisher's mathematics, he wrote to Pearson:

12th September 1912 — *Woodlands, Monkstown, Co. Dublin.*

Dear Pearson,

I am enclosing a letter which gives a proof of my formulae for the frequency distribution of $z(=x/s)$*, where* x *is the distance of the mean of* n *observations from the general mean and* s *is the S.D. of the* n *observations. Would you mind looking at it for me; I don't feel at home in more than three dimensions even if I could understand it otherwise.*

The question arose because this man's tutor is a Caius man whom I have met when I visit my agricultural friends at Cambridge and as he is an astronomer he has applied what you may call Airy to their statistics and I have fallen upon him for being out of date. Well, this chap Fisher produced a paper giving 'A new criterion

of probability' or something of the sort. A neat but as far as I could understand it, quite impractical and unserviceable way of looking at things. (I understood it when I read it but it's gone out of my head and as you shall hear, I have lost it.) By means of this he thought he proved that the proper formula for the S.D. is

$$\frac{\Sigma(x-m)^2}{n} \quad \textit{vice} \quad \frac{\Sigma(x-m)^2}{n-1}$$

This, Stratton, the tutor, made him send me and with some exertion I mastered it, spotted the fallacy (as I believe) and wrote him a letter showing, I hope, an intelligent interest in the matter and incidentally making a blunder. To this he replied with two foolscap pages covered with mathematics of the deepest dye in which he proved, by using n *dimensions that the formula was, after all*

$$\frac{\Sigma(x-m)^2}{n-1}$$

and of course exposed my mistake. I couldn't understand his stuff and wrote and said I was going to study it when I had time. I actually took it up to the Lakes with me - and lost it!

Now he sends this to me. It seemed to me that if it's all right perhaps you might like to put the proof in a note. It's so nice and mathematical that it might appeal to some people. In any case I should be glad of your opinion of it.

(The rest of the letter is concerned with tuberculosis death rates, a matter which W.S.G. was already in correspondence about with K.P.)

Yours very sincerely,
W.S. Gosset

An indication of the importance Karl Pearson put on Gosset's small sample theory at this time can be seen from his reply to this letter on 17 September 1912. Commenting on the subject of the standard deviation, Pearson remarked that it made little difference whether the sum of squares was divided by n or n-1,

because only naughty brewers take n *so small that the difference is not of the order of the probable error!*

R.A. Fisher, on the other hand, felt that Gosset never fully realized the importance of his discovery. In a tribute to

Student in the *Annals of Eugenics* in 1939, Fisher wrote

> *How did it come about that a man of Student's interests and training should have made an advance of fundamental mathematical importance, the possibility of which had been overlooked by the very brilliant mathematicians who have studied the Theory of Errors?...*
>
> *One immense advantage which Student possessed was his concern with, and responsibility for, the practical interpretation of experimental data. If more mathematicians shared this advantage there can be no doubt that mathematical research would be more fruitfully directed than it often is.*

Gosset was also keenly interested in the problem of determining the distribution of the sample correlation coefficient r of two normal random variables (which were say uncorrelated). This was the subject of another 1908 paper in *Biometrika* entitled "The Probable Error of the Correlation Coefficient". Using the same criminal data as in "The Probable Error of a Mean" paper, he simulated an experiment whereby he observed 750 sample values of r (sample size 4) from a bivariate normal population with zero correlation. A graphical representation of the results suggested to him that for sample size 4, the distribution of r was rectangular (i.e. uniform on $[-1,1]$). With his knowledge of Pearson's family of distribution curves, he felt a (Pearson) type II curve was the only suitable one for the distribution of r based on samples of size n. He wrote

> *... working from* $y = y_0(1-x^2)^0$ *for samples of size 4, I guessed the formula*
>
> $$y = y_0(1-x^2)^{(n-4)/2}.$$

He then showed by simulation that the formula worked well for samples of size 8. He made some comments about the distribution of r when sampling from a population with non-zero correlation, but this more general problem was out of his grasp. Gosset concludes the paper in writing

> *It has been shown that when there is no correlation between two normally distributed random variables,*

> $y = y_0(1-x^2)^{(n-4)/2}$ *gives fairly closely the distribution of r found from samples of size n..*
>
> *Next the general problem has been stated and three distributions of r have been given which show the sort of variation which must occur. I hope they may serve as illustrations for the successful solver of the problem.*

(The successful solver was, of course, R.A. Fisher, who published the results (which in particular justified Gosset's "guesswork") in *Biometrika* in 1915.)

Guinness, as a large consumer of barley, was very interested in agricultural experimentation. Gosset eventually became very involved in the planning and interpretation of such experiments, many of them carried out under the supervision of the Irish Department of Agriculture. His knowledge and advice on such matters was held in high regard and he corresponded extensively with many other experimentalists. It can be said that Gosset did a considerable amount of pioneering work in the areas of Analysis of Variance and Experimental Design, and he had a lot to do with Fisher getting so interested in these areas. He did, however, come into open controversy with R.A. Fisher and his Rothamstead school on the subject of balance (as opposed to randomness) in experimental design. In a letter to Fisher on 18 April, 1928, he wrote:

> *The fact is that there are two principles involved in the Latin square of which I attach the greater importance to the balancing of the error and you to the randomisation. It is my opinion that in the great majority of cases the randomisation is supplied to any properly balanced experiment by the soil itself though of course where the ground has been used for experimenting before or for any other reason has met with a "straight edge" lack of uniformity in recent years it is better to supply it artificially.*

Gosset was not completely happy with Fisher's randomised blocks because he felt that often a greater accuracy could be obtained with a balanced arrangement within the blocks. He was furthermore unwilling to use a plot arrangement determined by the toss of a coin, if the arrangement so obtained was biased in relation to already known fertility knowledge of the field. In his final paper "Comparison between Balanced and Random Arrangements of Field Plots" published posthumously in *Biometrika* in 1938, he wrote:

> *It is, of course, perfectly true that in the long run, taking all possible arrangements, exactly as many misleading conclusions will be drawn as are allowed for in the tables, and anyone prepared to spend a blameless life in repeating an experiment would doubtless confirm this; nevertheless, it would be pedantic to continue with an arrangement of plots known beforehand to be likely to lead to a misleading conclusion.*

Although this was a subject on which they never agreed, their friendship and mutual respect for one another did not suffer. In his tribute to Student, Fisher wrote

> *Certainly though he practised it, he did not consistently appreciate the necessity of randomization, or the theoretical impossibility of obtaining systematic designs of which both the real and the estimated error shall be less than those given by the same plots when randomized; this special failure was perhaps only a sign of his loyalty to colleagues whose work was in this respect open to criticism.*

The theory of evolution is another area of interest that intrigued Gosset. Darwin's Theory of Natural Selection suffered a setback at the turn of the century with the discovery of Mendel's work. Many geneticists came to believe that hereditary traits are conditioned by a 'limited number of genes' and that selection involving a limited number of genes cannot lead to important evolutionary effects. They felt that evolutionary progress and development must occur as the result of new mutations.

Gosset (and Fisher) however did not believe in this 'mutation theory'. About 1932 he became interested in some experimental work of H.L. Winter involving oil in maize. He felt that here there was evidence of a large number of genes at work, and that by (natural) selection, important evolutionary changes could take place. Using Winter's work, Gosset

wrote two papers in support of the Theory of Natural Selection ("Evolution by Selection - The Implications of Winter's Selection Experiment", *Eugenics Review*, 1933, and "A Calculation of the Minimum Number of Genes in Winter's Selection Experiment", *Annals of Eugenics*, 1934). He sought the support of Fisher (and his mathematical ability) in this cause, and persuaded him to write up a note in this direction for *Nature*. The following letter, written to Fisher in early 1933 on this matter, is a good illustration of the lively sense of humour that Gosset possessed.

Holly House,
Blackrock,
Co. Dublin.
16.1.33

Dear Fisher,

When I persuaded you to write up the mathematics of myriad gene selection in Nature I was so pleased with the idea of having got it done properly that I overlooked the fact that I have put you in the position of appearing to "butt in".

That being so I am taking the liberty of suggesting an opening sentence, not of course that I would wish you to use exactly my words, but something of the sort should give you a locus standi.

"In the January number of the Eugenics Review, Student has drawn attention to an experiment in selection carried out by Winter and has shown that the remarkable result of that experiment is consistent with the theory that the genes which affect the percentage of oil in maize are to be numbered by hundreds; further that given such large numbers of genes continued selective breeding will necessarily result in the production of individuals and ultimately of sub-races completely outside the original range of variation.

The argument has, of course, a mathematical basis and Student has invited me to examine it in a more general way than he has considered appropriate to an article in the Eugenics Review".

Of course if you want to be truthful you can substitute "can himself" (which has the merit of brevity) for "considered appropriate ... Review" but people who don't *know Student might consider it rude, which would be a pity.*

And here I think I hear you murmur "Damn the man, why doesn't he refrain from teaching his granny. He's as fussy about his little bit of stuff as a hen with one chick".

To which I reply, "I am, curse you, for the very good reason that I'll never have the chance to incubate an egg which interests me so much".

Cluck. Cluck. Cluck. Cluck.

Fact is that until just recently I was so much taken up with the first part of the thing, "myriad genes", that I overlooked the fact that the second is really an essential cog in the mechanism of Darwinian selection. For at least twenty-five years I've been reading that the continued accumulation of infinitesimal variations can do nothing and all the time I've felt in my bones that Darwin was right.

Cluck. Cluck. Cluck. Cluck.

And now I have been vouchsafed a vision,.... and am filled with insufferable conceit ... for the nonce I too am among the prophets, a mere Obadiah, but still among the prophets. And if anyone were to offer to make me a Doctor of Divinity on the strength of it, I'd accept with conscious pride and flaunt a scarlet gown through the scandalized streets of Oxford.

Bear with me, Fisher, laugh with me tonight; tomorrow ... when I'm sane again ... when I know that my little bit was discovered in 1896 and put into better words than mine often since then when I have been shown that my essential cog will hardly ever fit into the machine and when it does is a clog ... then I'll laugh with you - at myself.

Cluck. Cluck.

Yrs. v. sincerely,

W.S. Gosset

Gosset led a very full life and pursued a variety of hobbies. He was a keen gardener with a particular interest in fruit. In the late 1920's, he developed some logan-raspberry hybrids, two of which went by the names of "jamberry" and "Paddyberry". He made two barley crosses (known as Student I

and Student II) in his own garden and accelerated their development by having one generation grown in New Zealand. Gosset was also a good carpenter and built several boats. One of these, built with a rudder at each end, was for the particular benefit of fly fishermen. The design of this boat was described in 'Field' in March 1936. Gosset also took an avid interest in fishing, hunting and golfing. However, as Fisher writes in his tribute to Student in 1939,

> *His life was one full of fruitful scientific ideas and his versatility extended beyond his interests in research. In spite of his many activities it is the student of Student's test of significance who has won, and deserved to win, a unique place in the history of scientific method.*

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COMPUTER EDUCATION IN IRISH SECOND LEVEL SCHOOLS

Michael D. Moynihan

In this article I will attempt to outline the progress of Computer Education in Irish Second Level Schools.

In 1971, the Department of Education initiated a course for teachers with a Mathematics or Science background who were interested in Computer Education, but had no previous knowledge of computers. Professor Bajpai of Loughborough College of Technology in England was invited to run this week-long course at University College Galway. At its conclusion, participants were able to write FORTRAN programs and process them on an IBM 1800 Computer.

From those whose active interest was maintained over the succeeding year, and from others who had been attracted in the meantime, sufficient support was gained to consider the formation of an embryo computer society and a repeat of the course. The inaugural meeting of the Computer Education Society of Ireland (CESI) was held in January, 1973, and, encouraged by the Department of Education, the society proceeded to devise a policy on Computer Studies in schools.

In the search for suitable material, the members were attracted to the materials published by ICL-CES in Britain. Two members undertook an ICL training course and on returning they ran a week-long course for teachers from twelve schools in preparation for the introduction of a pilot scheme early in 1974. The Department of Education sanctioned this pilot scheme and provided the necessary textbooks for the participating teachers. The scheme was greatly facilitated by the free processing time provided by local industry - cards were punched at an ICL installation and were processed on the ICL computer at the Sugar Company in Dublin.

The scheme, being the first of its kind, generated a great deal of interest among students, teachers and parents and we had hopes that the course content might be re-evaluated and that another scheme, with even more schools taking part, would emerge the following year. This was not to be. 1975 was a bad year for courses and the Department of Education was unable to finance a teacher-training course. CESI was at the cross-roads. We could either disband or go it alone. It was felt that we had gone too far to give up. So we charged a nominal fee and ran a beginner's course at Coláiste Choilm, Swords, Co. Dublin, where the first computer in an Irish School (a DEC PDP8) had just been installed. This proved to be a very successful course and since then CESI has assumed an important role in the training of teachers of Computing.

In 1973, paralleling CESI activities, Trinity College, Dublin, began a postgraduate diploma course for teachers of all disciplines, to equip them to run Computer Science courses if and when official departmental approval for such courses was forthcoming. This was a one-year course, directed by Fr. Cyril Byrne. Twenty-seven teachers took part. The course was (and still is) a great boost to computer education in that it gives teachers some qualifications to teach the subject; last year even with the limit raised from 30 to 50, the course was booked out an an early date. In 1979, some teachers who had done the diploma course went on to take a Master's Degree in Computer Practice.

Ireland has a long tradition of voluntary effort in education and to introduce Computer Studies at all called for a supreme effort on the part of teachers. Usually an interested teacher was forced either to teach the subject outside normal school hours (where it had to compete with traditional school activities) or to teach it under the guise of another subject (since Computer Studies is not recognised for the payment of incremental salary!). Neither case was satisfactory and, indeed, most of the work was done as an extra-curricular activity at some inconvenience to teacher and pupil alike.

As years went by, however, more and more schools were offering Computer Studies. There was a tremendous interest among students, although it was unfortunate that the teacher did not have the time or resources to teach the subject adequately. Schools had great difficulty in acquiring computer facilities; industry, while interested in our work, was unable, for the most part to process programs for schools. However, CESI built up valuable contacts with people in the computer industry and third-level colleges. The National Institute for Higher Education (N.I.H.E.), Limerick, and Thomond College have always been more than willing to accomodate teachers in the Limerick area. Trinity College, Dublin; University College, Galway and Maynooth College have also helped. With the help of the Computer Bureau at University College, Cork, we have set up a schools Computing Centre at Coláiste an Spioraid Naoimh, Cork. We see the centre as a positive response to cater for schools, who, in the absence of government grants, are unable to obtain processing facilities.

The advent of the microprocessor about 1979, has brought hardware down to a price that is within reach. It is because of the microprocessor that many schools are able to do Computer Studies today and it was estimated that there were 600-1000 micros in our schools before November 1981. The majority were APPLE II, COMMODORE PET, TRS-80 or ACORN ATOMS, nearly all bought out of school funds. When one considers how tight a school budget is, it was a remarkable achievement to have so many micros in our schools.

Over the years CESI has grown in numbers, confidence and expertise. Together with our friends in industry, we kept constant pressure on the Department of Education to introduce Computer Studies into our schools, and this pressure has produced agreement from the Department on two important issues. First, as of September 1980, Computer Studies appears on the curriculum as an optional extra on the Mathematics syllabus at senior level. The course is non-examinable, but is monitored by Department Inspectors, and candidates who reach a certain proficiency are awarded a certificate to this effect. Now, at least, some of the time spent on the subject can be recognised for salary purposes. Secondly, the Department has set up an Advisory Committee to examine the implications of introducing Computer Studies into schools.

It is hoped that when the Advisory Committee delivers its findings that Computer Studies will appear on the curriculum as a subject in its own right and that modules on computing will be attached to many existing subjects. We insist that it be divorced from Mathematics at the earliest opportunity. We also feel that the subject should be introduced into schools at first year level as we have found that the younger a child is allowed to start Computing, the better his/her understanding becomes.

In the 1981/1982 school year, the Department of Education tendered for the introduction of microcomputers to Irish schools. APPLE Computer Co., who have a manufacturing plant in Cork, won the contract. In all, 310 systems were given out to schools not having a computer, where there was a teacher competent to use the system.

Each school got the following:

48K APPLE II and 2 Disk Drives
16K Language Card
Centronics Printer
Z-80 Card
2 COMAL 80 Diskettes
80/80 Assembler
APPLE Pilot, APPLEwriter
APPLE POST, DOS Tool Kit
Manuals for APPLE, DOS 3.3, Pilot, APPLEwriter.

Schools that already had an APPLE system got enhancements to bring their system up to 64K with printer and/or DOS 3.3. Schools with multiple APPLE systems only received enhancements for one system.

COMAL-80 is a structured BASIC developed in Denmark in the mid-seventies. It operates under the CP/M operating system and needs a minimum of 64K RAM.

Many Irish schools are presently experimenting with COMAL and plans are afoot to upgrade systems other than the APPLE to enable it to take COMAL. There is a separate version of COMAL available to run on the Commodore Pet.

However, there is as yet no Syllabus Committee on Computing and the Department of Education has no official policy on the matter of teacher-training in Computing. (In fact, all Computer Courses scheduled for the summer of 1983, bar one, were cancelled.) Because of this, Computer Education at Second-level schools has been set back indefinitely.

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{***Editorial Note:*** Members of the I.M.S. interested in the CESI and/or the Journal ***Ríomhiris na Scol*** published by the CESI, should contact the author of the above article.}

"SHUFFLE THE CARDS, PLEASE"

Jim Leahy

Since playing cards was invented, the ranks of card magicians and gamblers have produced quite a deal of mathematics. What follows has been culled from the writings of such people and writers as Martin Gardner, Philosophy graduate, amateur magician and writer of the "Mathematical Games" column in "Scientific American" from 1957 to recent times, and Chicago card magician, Edward Marlo. All this places what follows in the field of Recreational Mathematics so shuffle the cards, please and we'll begin.

Of all card shuffles, that known variously as the "faro", the "weave" or "riffle shuffle" is the most interesting. To execute a faro shuffle, divide the deck in half exactly if the deck contains an even number of cards and as nearly in half as possible with an odd number deck. Now with half a deck in each hand let cards drop alternately from the thumbs thus weaving the two halves together. With odd decks the first card to fall must come from the "half" with the extra card. With even decks if the first card to fall is from the half that was formerly the bottom of the deck the original bottom and top cards retain their respective positions. Magicians call this an out-shuffle because the top and bottom cards remain on the outside. If the first card to fall is from the original top half we get what magicians call an in-shuffle. For odd decks a faro is an out-shuffle if prior to shuffling the deck is cut below the centre card and an in-shuffle if it is cut above the centre card.

Since any shuffle is merely a permutation from the symmetric group S_n, if the deck contains n cards, then the same shuffle repeated a number of times equal to the order of the permutation will restore the deck to its initial order. If n is odd a deck of n cards given x repeated faro shuffles of the same type will return to its original order if $2^x \equiv 1 \pmod{n}$.

For example if we use a full deck with a joker making 53 cards then x = 52 that is 52 in-shuffles (or 52 out-shuffles) are required to restore a 53-card deck.

If the deck is even the number x of out-shuffles is given by

$$2^x \equiv 1 \pmod{(n+1)}.$$

For a normal pack of 52 cards, 52 in-shuffles or 8 out-shuffles restore the original order while 5 out-shuffles and 10 in-shuffles will restore a piquet pack (32 cards).

The reader might like to test the formulae using packets of cards. A perfect faro is difficult to do but this can be circumvented by doing "Reverse faros", i.e. undoing a faro shuffle by stripping out every second card. Obviously if x faro shuffles restore original order then x reverse faros will do the same thing. The easiest way to do the reverse faro is to deal the cards singly into two piles turning the cards face-up as you deal. In putting the piles together if the original top card remains on top it is called an out-sort, if not an in-sort. Do it with ordered packets of cards so that you can see what is happening as you do it.

Alex Elmsley, a British computer programmer and a skilled card magician, discovered a remarkable formula connected with the faro shuffle. Elmsley wished to find the most efficient combination of in-shuffles and out-shuffles - terms incidentally coined by Elmsley - to cause the top card of a deck to go to any desired position from the top. If you want the card for example to go to the 20th position subtract one to give 19 and write this in the binary form 10011. Now let I and O stand for in-shuffle and out-shuffle respectively. The required answer is then the following sequence of faros IOOII. This works regardless of the size of the deck and is done in the least possible time.

We may also use this formula doing reverse faros to bring a card at any chosen position to the top by following the sequence of binary digits backwards. For example to bring the 20th card to the top execute the following sequence of in-sorts and out-sorts IIOOI. A brief analysis shows that in effect what you are doing here is that during each shuffle you are placing the pile containing the card in question on top. Now there is a very old card trick called "Gergonnes Pile Problem" (after Joseph Diez Gergonne, the French Mathematician who in 1813 was first to analyse it experimentally) in which 27 cards are dealt into three piles, face up, and a spectator thinks of any card indicating which pile it is in. The cards are picked up and dealt out again in three piles and the spectator again indicates the pile containing his card. After the procedure is repeated a third time the spectator finds his card at a position in the deck previously specified by him. The trick depends on the order of picking up the piles. Suppose you wish to bring the chosen card to position 22. Find the ternary equivalent of 21, i.e. 210, and reverse the digits to give 012. Now on the first deal place the pile containing the chosen card on top indicated by 0, on the second deal in the center indicated by 1 and on the last deal on the bottom indicated by 2. The card will now be found at position 22. If you want to bring the chosen card to the top then the ternary equivalent of 1-1 = 0 is 000 which tells you to place the pile containing the chosen card on top each time.

Now we find that, if we use two piles instead of three and binary in place of ternary that the above process works for any number of cards although we may need to deal more than twice if the number of cards is greater than 4 depending on the initial position of the chosen card. This all suggests a method for generalising the faro shuffle or rather the inverse faro. (One would need three hands to weave three packets of cards together!) If we deal three piles and pick up in one of the 6! ways, do analogous formulae exist, for example, for finding the number of times such a shuffle must be executed to restore the deck to its original order? I know of no literature on this problem. However, I have found that if we

pick up the three piles so that the top card remains on top and the centre pile is replaced in the centre then the number x of such shuffles is given by

$$3^x \equiv 1 \pmod{(n-1)}, \text{ if } n \text{ is of the form } 3k, \text{ and}$$

$$3^x \equiv 1 \pmod{n}, \text{ if } n \text{ is of the form } 3k-1.$$

I could make no conjecture regarding n of the form $3k+1$. The following table gives the number of such shuffles required to restore the original order for decks of 3 through 21 cards.

NO. OF CARDS	3	4	5	6	7	8	9	10	11	12
NO. OF SHUFFLES	1	3	4	4	6	2	2	6	5	5
NO. OF CARDS	13	14	15	16	17	18	19	20	21	
NO. OF SHUFFLES	11	6	6	15	16	16	52	4	4	

At the opposite end of the shuffling spectrum some surprising results occur in the realm of probability. Despite random shuffling the probability will be high that certain properties of a deck are preserved, a fact often made use of by confidence tricksters offering what appear to be good odds to the uninitiated. For example, what are the odds that as two standard decks are dealt simultaneously that at least once the same card will be dealt at the same time. This does not sound like a good bet but in fact is a winner two out of three times. This is of course the classic problem of matching letters with addressed envelopes. The probability, q_{52} say, of failing to get two cards to match is given by

$$q_{52} = 1 - \frac{1}{1!} + \frac{1}{2!} - \frac{1}{3!} + \ldots + \frac{1}{52!} = \frac{1}{3} \text{ approx.}$$

Note that $q_n \to e^{-1}$ as $n \to \infty$.

Harry Blackstone, well-known American stage magician and son of the "Great Blackstone" who rivalled Houdini in Vaudeville in the 1920's, in his book [1] gives the approximate correct answer to this problem but gives an erroneous and simplistic method of getting it.

In the same book he gives the following "betcha": have someone call out any three card values like Ace, Queen and nine. Bet him you can find two of these values together somewhere in the deck. Sounds like a long shot but is far from it. Blackstone himself failed to work out the odds and then as he says "took the simple problem to a probabilities professor at a West Coast University. After two weeks he was still struggling with the problem". Well, I struggled with the problem and eventually, by actually counting the possible combinations found the probability of success to be almost 0.9. The method I used is too long to reproduce here but I would be interested in hearing from anyone who can provide a "nice" solution.

Another good bet is to have someone cut a deck into three piles and turn the top cards. You bet that two will be the same suit. Can you figure the odds? There are many more and some may be found in the references below.

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BOOK REVIEWS

"A DICTIONARY OF STATISTICAL TERMS"

M.G. Kendall and W.R. Buckland.

Published by *Longman, 1982, 213 pp.,* £12.95 (stg.)

The last 60 years have seen an enormous growth in statistical literature. This dictionary of 3,000 terms offers an invaluable service by standardizing terms and notation that have arisen in this period, up to 1978. This fourth edition is a revised and extended version of the first edition of 1957. Moreover, it is the only manual of its kind.

The major areas of statistics are covered. Terms from probability theory, decision theory, multivariate analysis, stochastic processes, nonparametrics etc. are included. In addition, terms associated with specific applications in the disciplines of economics, psychology etc. are given; thus the entries range from 'least favourable distribution' to 'Laspeyres' Index'. The authors have interpreted terms in the broad sense; for instance theorems such as Cochran's theorem, technical terms such as 'variance' and particular concepts such as 'estimation' are included, as well as the usual 'tests of hypotheses' and various statistical distributions.

The manner in which the entries are explained reveals a British rather than an American approach to statistics in that the explanations are mostly given in a non-mathematical context. For instance, percentiles are described as 'The set of partition values which divide the total frequency into one hundred equal parts', rather than in relation to distribution functions. This non-mathematical bias means that terms such as 'probability measure', a Von Mises functional' and 'contiguity' are omitted. Emphasis is on placing the terms in the context from which they arose. As mentioned on the sleeve of the book, 'the authors have tried to attribute terms to those who originally introduced them into the literature'. Thus

they appeal to a wide audience as the reader is given access to the concepts involved.

A distinctive feature is that well known terms are treated at great length. There is, for example, a fascinating description of 'degrees of freedom', and of the concept of 'probability' which give a flavour of the historical controversies surrounding these terms.

Some terms largely extinct in the literature are given extensive treatment, for example 'clisy'. On the other hand, the concept of 'permutation distributions' and 'permutation tests' is confined to three lines. Extensive treatment is given to distribution theory and parametric inference while terms from nonparametric statistics such as 'Walsh averages' and "McNemar's test' are not mentioned. So the treatment is rather uneven, perhaps reflecting the authors' personal biases. Another criticism is that the last ten years have seen a rapid growth in the field of robust statistics. Necessarily, the dictionary cannot be expected to contain all the new terms which have arisen from this field, but those it does contain from the growth in robust statistics are unsatisfactory, in the light of recent developments. An example of this is that in the definition of robustness, the terms qualitative and quantitative robustness are not used.

In conclusion it is clear that this is a very welcome addition to the literature. It should be of great value to graduate students, consultants in statistics and researchers in statistics from other disciplines. It is a useful reference book for statisticians generally, and it is hoped that with future revisions, it will eventually become the guide for all statisticians.

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"GEOMETRICAL INVESTIGATIONS"

John Pottage.

Published by *Addison-Wesley Publishing Co., Reading, Mass., U.S.A.* *(xxi + 480 pp.)* £26.95 (stg.)

Here they are again - Galileo's famous trio - Salviati, Sagredo and Simplicio, reconstructed with great literary skill by the Professor of the History and Philosophy of Science in the University of Melbourne. In fourteen dialogues they discuss various geometrical problems about ratios of perimeters, areas and volumes, leading up to the evaluation of the perimeter of an ellipse and other matters. The dialogues are followed by interesting historical notes and geometrical problems for the reader to solve for himself, with some hints to help him. The book is beautifully reproduced, with many diagrams.

The keyword of the book is heuristic, a word of which I had forgotten the meaning, possibly for Freudian reasons. It means, of course, the art of discovery, but also includes a method of teaching in which the student finds out things for himself. Throughout my life I have hovered between mathematics and physics, and if I now regard myself as a mathematician rather than as a physicist, it is because when I was a schoolboy about 1912, physics was taught heuristically and mathematics was not. The heuristic method, it seemed to me, was essentially dishonest, involving a pretence that you did not forsee the result of the experiment before you did it. And all this dithering took up so much time that you really got nowhere.

In these dialogues I was at first fascinated by the discussion in which the particular was gradually generalised, but it seemed to me that Salviati was holding back. The others seemed to have learned little during their absence of three centuries and so they were feeling their way. I was shocked

when Salviati obtained the perimeter of an ellipse by integration; had I been writing the dialogue I would have made Sagredo and Simplicio shout with one voice: "Why the hell have you been wasting our time?"

It reminds me of Churchill saying "Give us the tools and we will finish the job!" In mathematics and physics we use tools, and it is a delicate question in education how much attention to devote to their manufacture and how much to their use. No teacher in his right mind would ask a student to accept, *ex cathedra* , that the derivative of x^n is nx^{n-1}, but neither would the student want to go through the proof every time he needed to differentiate x^n. The tool has been made: now it is up to him to use it. It is fascinating to think of Euler using imaginary numbers as a tool without knowing what he was talking about.

We should be grateful to Professor Pottage for reviving the dialogue as a scientific literary form. It has great possibilities. Why not switch from geometry to physics? It was Galileo's physics, not his geometry, that got him into trouble. Perhaps we might meet his great trio again, representing, let us say, Einstein, Bohr and Schrödinger, but which of them should take the dominating role of Salviati I do not know.

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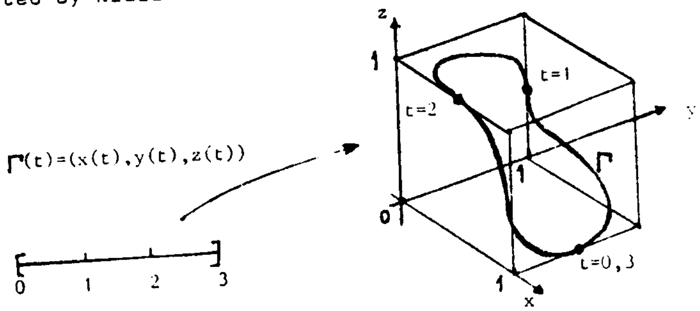
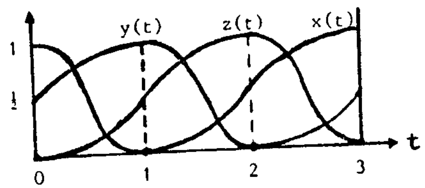
PROBLEMS

First, the solutions to the March problems.

1. Does there exist a smooth closed curve Γ in $\mathbb{R}^3$ such that no pair of tangent vectors to Γ are parallel but of opposite sense?

The answer is 'yes' as shown by the following curve, which was suggested by Niall O'Murchu:





If $t_1, t_2 \in [0,3]$ then at least one of the co-ordinate functions (say x) has the property that $x'(t_1)$, $x'(t_2)$ do not both vanish or have opposite signs. The tangent vectors at $\Gamma(t_1)$, $\Gamma(t_2)$ cannot then be 'parallel but of opposite sense'.

2. With the aid of a battery and a bell, how should an electrician identify n (>2) indistinguishable wires which run from the bottom to the top of a building? Only one visit to the top is allowed.

Here is one solution - there are others, based on the use of triangular numbers.

If $n = 2k + 1$, $k > 0$, connect the bottom ends in k pairs and label the singleton a. At the top the wires can then be labelled

$$\{a\}, \ \{b_1,c_1\}, \ldots\ldots, \{b_k,c_k\},$$

and reconnected in the form

$$\{a,b_1\}, \ \{c_1,b_2\}, \ldots., \ \{c_{k-1},b_k\}, \ \{c_k\}.$$

At the bottom b_1 can now be labelled (it is connected to a at the top) and also, therefore, c_1. Knowing c_1 leads to b_2 etc.

If $n = 2k + 2$, $k > 0$, connect the bottom ends in k pairs leaving two unlabelled singletons. At the top the wires can be labelled

$$\{a_1\}, \ \{a_2\}, \ \{b_1.c_1\}, \ \ldots \ \{b_k,c_k\}.$$

and reconnected in the form

$$\{a_1\}, \ \{a_2,b_1\}, \ \{c_1,b_2\}, \ldots, \ \{c_{k-1},b_k\}, \ \{c_k\}.$$

At the bottom a_2 can now be labelled (it is connected to b_1 at the top) and also, therefore, a_1, b_1 and c_1. Knowing c_1 leads to b_2 etc.

It is intriguing that the problem can be solved in all cases except n=2, though even this can be done if the electrician has another piece of wire. If he also has a supply of resistances and an ammeter then he can solve the problem without coming back down again!

Now, some more problems.

1. The following problem was suggested by Pat Fitzpatrick who heard it from Peter Cameron. It appeared a few years ago on a problem page of another journal, but is so pretty that it's well worth repeating.

A car park has spaces numbered 1,2,...,n. One morning n drivers arrive in turn, each wishing to park in a certain space (which they each choose at random). A driver who wishes to park in space k does so unless it is occupied, in which case he chooses the first vacant space from k+1, k+2,...,n. If these are all occupied he leaves in disgust.

The probability that all the drivers can park is $(n+1)^{n-1}/n^n$. Prove this.

2. Ship A is moving due east at constant speed and, at a certain moment, ship B is moving due north at the same speed towards A. If B maintains this speed but continuously alters course towards A how closely can B approach A ?

3. Des McHale asks the following:

Say that a group G is n-abelian, $n \in \mathbb{Z}$, is

$$(xy)^n = x^n y^n, \quad x,y \in G).$$

For which n is it true that:

G n-abelian implies G(n+1)-abelian ?

Also, what happens if n+1 is replaced by n-1 ?

As usual I'd welcome any historical references to the problems discussed here.

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CONFERENCE REPORTS

GROUPS IN GALWAY CONFERENCE (13-14 May, 1983)

The main speakers at the Conference were Richard Watson (Maynooth), John Hannah (U.C.G.), Peter Cameron (Oxford), Martin Liebeck (Cambridge) and Colin Walter (U.C.D.). Gerard Enright (M.I.C., Limerick) was unable to give his talk due to illness and Tom Laffey (U.C.D.) and Sean Tobin (U.C.G.) very kindly stepped in at short notice to fill the gap. Other short communications were given by Roderick Gow (U.C.D.), James Ward (U.C.G.), Brian Abrahamson (Flinders, Adelaide), Michael Barry (Carysfort) and Martin Newell (U.C.G.).

The Conference certainly brought home the range and variety of Group Theory and its applications, a fact not fully appreciated nor understood by those not in the area. In the last year alone, three advanced (i.e. postgraduate at least) textbooks have come on the market, namely Curtis and Reiner "Methods of Representation Theory" Vol. I containing over 819 pages (with volume II to follow), W. Feit "The Representation Theory of Finite Groups" (502+ pages) and Huppert and Blackburn "Finite Groups", Vols. II and III containing over 1000 pages. These books are destined to become standard references in their own branches of Group Theory. In the 70's and early 80's also, numerous advanced textbooks on permutation groups, groups of graphs, cohomology of groups, Galois groups, varieties and classes of groups, topological groups, Lie groups, combinatorial group theory, crystallographic groups, group rings and matrix groups have appeared and the subject is expanding into most unlikely areas such as applications in Computer Science and Geology. (For all this and as a contribution to Mathematical Education note that it is, I believe, agreed among Group Theorists that Group Theory is certainly not a topic for Secondary Schools' curricula.)

The first main speaker at the conference was Richard Watson who spoke on "Some aspects of profinite groups". A profinite group is a topological group which may be considered as the inverse limit of finite groups and he gave an interesting survey of these groups and the problems associated with them. These groups arise naturally as subgroups of the Galois group of a Galois extension and this seems to have given an impetus to research in the area. Other problems arise from trying to discover which theorems from finite group theory will extend to profinite groups and whether other concepts such as free or relatively free group can apply to the class of profinite groups.

John Hannah spoke on "Putting coordinates on lattices". John in his usual excellent expository style explained how von Neumann wanted to "put coordinates on" his lattice in a way compatible with ring operations - von Neumann was considering rings of operators on Hilbert spaces in which the set of projections formed a complemented modular lattice. This led to von Neumann regular rings and the rest of the talk explained the examples and problems associated with these *(See this issue, pages 21-28)*.

Peter Cameron spoke on "Infinite permutation groups". In particular, he described some of the properties of Rado's graph, which is the unique countable ultrahomogeneous graph, and showed how it gives rise to a number of interesting permutation groups. He also gave an account of some work on the levels of homogeneity and transivity of permutation groups.

Martin Liebeck's talk entitled "Some applications of the classification of finite simple groups to permutation group theory" explained how the recent classification - together with a result of O'Nan-Scott can be utilised to prove theorems on permutation group theory. As a sample, he proved four theorems (and one "almost-theorem") and also interestingly pointed out how a classification of the maximal subgroups of the finite simple groups woul lead to other theorems and reported on work being done in this area.

Colin Walter spoke on "Automorphism groups of graphs" and having explained that he was an Algebraic Number Theorist only recently becoming involved with graphs, then proceeded to show his versatility with an excellent survey of the area. He proved a number of theorems on connections between transivity, girth, valancy of a graph and its automorphism group of which I state the one I found most interesting:- Theorem (Tutte) "There is no 3-valent t-transitive graph with $t > 5$".

The shorter talks also contributed much to the enjoyment of the programme. They consisted mainly of announcements and explanations of recently acquired results of the speakers. Roderick Gow spoke on "Permutation representations of some classical groups on the cosets of certain classical groups"; James Ward on "On subrings which permute with their conjugates"; Brian Abrahamson on "Quaternion monomials"; Michael Barry on "Computing dimensions of irreducible modules"; Sean Tobin on "Groups with $[n \to t]$"; Tom Laffey on "Maximal commutative subalgebras of algebras" and Martin Newell on "Metabelian groups of exponent 8, II".

We have asked the main speakers to write up their talks in survey form for the Newsletter and hopefully some of these will appear in future issues.

The Group Theory Conference in Galway now appears to be an annual event. Its appearance coincides with the formation of the I.M.S. and has continued annually since then. We are grateful to the I.M.S., R.I.A., U.C.G. and some of the participants for financial support. We also thank the Mathematics Department of U.C.C. for arranging for Martin Liebeck to be present in Ireland for the Conference and for most of his financial support. (It should be noted that the total budget for this Conference is less than the (single) fee for another Conference held in Galway around this time.) Most of all, we would like to thank the participants for their continued support by their presence and participation. This year, we had 22 participants, a group small enough for mathematical and social interaction and large enough for viability. Their presence and interest encourage us and we will continue .....

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THIRD CONFERENCE ON APPLIED STATISTICS IN IRELAND

The Third Conference on Applied Statistics in Ireland was held in the Slieve Donard Hotel, Newcastle, Co. Down, on March 28/29, 1983. This followed the pattern set by the two previous conferences by inviting contributors to present a 25 minute paper on an applied statistics topic of their choice. However as a new innovation this year a special session on Statistical Computing was advertised in advance and several statisticians involved in this area were asked to provide appropriate papers. Micro-computers were provided for running statistical programs and a display of modern micro-computer equipment was arranged.

The Conference program was divided into five sessions. In the first session Professor D. Conniffe (ESRI) discussed unrecognised similarities between statistical theory in different fields leading to duplication of methodologies with particular reference to Biometrics and Econometrics. Dr. F. Murtagh (UCD) considered the theory of correspondence analysis and gave three examples of its use. Professor R.E. Blackith (TCD) described the interpretation of some politically sensitive epidemiological surveys resulting in bizarre statistical treatment.

In the session devoted to statistical computing Dr. M. Stuart (TCD) drew attention to recent advances in information technology and indicated some ways in which electronic devices

may be used by statisticians to enhance more traditional data collection procedures. Mr. C.E. Rogers (NUU) described GENSTAT, a general purpose package intended mainly for use by professional statisticians. Dr. J. Bradley (ESRI) outlined the features of the TROLL computer system devised for econometric modelling applications. Mr. L. O'Reilly (Central Bank of Ireland) described some packages and programs available for analysing time series. Miss A. Timberlake (Timberlake Clarke Ltd.) presented an overview of the specific needs for data management by statisticians and of the software availability in this area with particular reference to SIR, RAPPORT, SAS, P-STAT and SPSS. In the final paper of this session Dr. D.M. G. McSherry and Mr. D. Duffy (QUB) described and demonstrated a micro-computer package, INTERSTAT, for carrying out a range of parametric and non-parametric statistical tests interactively.

The third session was devoted to Biometrics. Dr. M. Mulvihill (TCD) presented a paper describing a problem in the selection of AI sires in Ireland involving the importation of sires from Britain and concluded that genetic environment interactions occur such that reranking of bulls occurs between Ireland and Britain. Mr. E.A. Goodall and Dr. D. Sprevak (QUB) defined a stochastic model to describe the milk yield of a dairy cow. Dr. J. Connolly (AFT) presented a paper dealing with some aspects of the design and interpretation of plant competition experiments and emphasised the role of plant density as a critical variable in these investigations. Professor S.M. Lavelle, F.H. Given and U.S. Khoo (UCG) presented a paper on computer-aided diagnosis in improving the accuracy of clinical diagnosis.

In a session on Technometrics Dr. S. Ferguson and Dr. D. Sprevak (QUB) described the influence of random fluctuations in the geometric dimensions of semiconductor devices on their performance. Mr. E.G. McEntee (Ulster Polytechnic) gave a case study involving the determination of the optimum sampling frequency in monitoring the quality of output of a continuous process. Mr. R. Beattie (Gallagher Ltd.) described the application of cumulative sum techniques for laboratory quality control, balancing the need to monitor both long- and short-term movements. Dr. S.I. McClean and J.O. Gribbin (NUU) described an estimation procedure for failure time data which is both right censored and left truncated with particular reference to manpower planning.

In the final session Dr. P. Boland (UCD) provided a biographical sketch of W.S. Gosset in which he quoted from Gosset's personal correspondence with Fisher and other eminent statisticians of his time. *(See pages 45-56 in this issue)* Mr. E.S. Gillespie (Ulster Polytechnic) provided the final paper of the conference on the predicting and fitting ability of alternative models for flood estimation in which he tested the models by means of a data splitting technique.

The Conference was organised by Professor D. Conniffe, Professor A.A. Greenfield, Dr. A. Unwin and Dr. S.T.C. Weatherup. It is planned to hold another conference on applied statistics again next year. Details will be circulated later.

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