Eureka Editor
The Archimedeans
Centre for Mathematical Sciences
Wilberforce Road
Cambridge CB3 0WA
United Kingdom

archim-eureka@srcf.net

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EUREKA

THE JOURNAL OF THE ARCHIMEDEANS

(The Cambridge University Mathematical Society: Junior Branch of the Mathematical Association.)

Editorial Committee: *J. T. Hansford, St. John's College; T. H. R. Skyrme, Trinity College; T. C. Spring-Rice, Girton College; M. Walker, Girton College.*

No. 7 MARCH, 1942

Editorial

We are unusually late in making an appearance this term, for which we hope to be forgiven. This is partly because it has been decided not to publish EUREKA in May this year, as we have found that although the theoretical date of publication is 1st May, it is very difficult under present conditions to collect material before the middle of the Easter term, and the heaviest work of editing then falls at a time when the committee wears, unaccountably, a slightly glazed expression, and seems unable to apply itself to the matter in hand with its customary force. We hope, however, that the average of two issues a year will be maintained in future, though the intervals may be somewhat irregular. No. 8 will be published during next Michaelmas term, all being well.

We realise that life is unsettled and time short, but, nevertheless, this is a magazine for Undergraduates, and we would very much like to have more active co-operation from you. Our pioneering predecessors hatched EUREKA in 1939 because they felt that students were "without a medium for stating their views, for discussing their present training and their future prospects, or for publishing their less orthodox or less mature researches." We do not feel that we have succeeded in remedying this defect, for although we are extremely grateful to the senior members of the University for their interest and support, it is noticeable that their contributions compose about 75 per cent. of the present issue, which is not as it should be. We know that that hypothetical being, the average undergraduate, has plenty to say, but we wish that he were bold enough to put his ideas on paper. In any case, we hope that he will excuse us for making this appeal yet once more.

Our address is—the Editor of EUREKA, c/o Mathematical Faculty Library, New Museums, Cambridge, from where, also, copies of this and other numbers may be obtained by post (price 9½d., post free).

Archimedeans' Activities

THE BRIDGE GROUP

THE Bridge Group has been holding fortnightly meetings on Friday evenings in Newnham College. Numbers have varied from six to twenty people in attendance. All types of bridge and all conventions are played, but usually the players sort themselves out, and the beginners soon learn. Everybody seems to enjoy themselves.

K. H. S.

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THE MUSIC GROUP

This year the Music Group has confined its attention entirely to gramophone recitals, but we are hoping, in the near future, to persuade the less shy members of the Group to give a performance of some chamber music.

Last term we began by holding our meetings fortnightly at Newnham, but towards the end of the term we moved to Trinity. This term we have endeavoured to hold weekly meetings, these taking place on Tuesday afternoons.

The attendance at our meetings has been fairly regular, and we are particularly pleased to welcome among our more enthusiastic members several freshmen.

V. W. D. H.

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THE CHESS GROUP

Hibernating.

■ ■ ■

An Editor's Apology

One quarter of the editor would like to join the ranks of Mathematicians who Apologise. In EUREKA 6, page 8, line 6 (Professor J. B. S. Haldane's article on the faking of genetical results), the word "lake" was misprinted "fake", a serious error for which we owe the most sincere apologies to Professor Haldane. The mistake was due to the same mathematician's lamentable ignorance of the language of biology, and not to any preferential direction of the subconscious mind.

Undergraduate Council Report

The main work of the Undergraduate Council in the Michaelmas term, 1941, has been social service, and the collection for medical aid to the Soviet Union. For the convenience of affiliated societies, the U.C. started to publish a weekly calendar of their functions.

The officers were the same as before, except that Mr. J. A. Newth was elected as a secretary, Mr. R. Hanes as treasurer, and Mr. R. N. Higinbotham as a member of the standing committee.

The Social Services Committee, which was started by the U.C., under the name of the Evacuee Care Committee, changed its name on 18th October to be more in keeping with the altered character of its functions. Later the committee felt that, in view of the increased scope of its activities, it would like to be independent of the U.C., though continuing to be associated with it. The Council decided that its status as a sub-committee of the U.C. was, in fact, anomalous, and the Social Services Committee was constituted an independent body, to maintain close relations with the U.C.

In November the War-Work Co-ordinating Committee was set up to obviate overlapping which was occurring in the war-work of various undergraduate bodies. On 18th November this committee was absorbed into the Social Services Committee, with the Rev. G. K. Tibbatts, of Magdalene, as chairman.

Blood transfusion, for which 500 names were obtained, volunteer porters for Addenbrooke's Hospital, and the manufacture of camouflage netting for the military authorities are some of the kinds of war-work that have been done. On the suggestion of I. S. E. Union, it was proposed to hold a war-work conference to discover how undergraduates could best help the war effort, but this had to be postponed.

A U.C. dance was held, and it is proposed to hold another in the Lent term. The collection for medical aid to the Soviet Union realised £208.

G. C. T.

■ ■ ■

NOTE. We are given to understand that there exists at the present time a body of erudite and justly eminent men, which meets weekly in Langham Place to answer questions put to it by the general populace, and that among these questions of late was one signed "Eureka, Cambridge." We should like to make it clear that to the best of our knowledge anything about the pseudonym which suggests ourselves was purely coincidental.

Finitism

By R. L. Goodstein

During the past 25 years the notion of *finitism* has gradually acquired a dominant position in mathematical criticism. Originally the term applied to a school of thought which developed alongside, but in opposition to, *formalism*, the view that mathematics is without content, a "mere" playing with patterns, yet it is within formalism itself that the idea of finitism has evolved, and in two of the outstanding works of present-day mathematics, Kurt Gödel's construction of non-demonstrable propositions and Gerhart Gentzen's proof that Classical Arithmetic is free from contradiction, the fundamental objective in each case is a clarification of the notion of *finitism* in a formal mathematical system.

What is finitism? Is it something entirely new in mathematics, the special creation of this century? Certainly, finitism is something new as far as the past few hundred years are concerned, but the present interest in finitism is in the nature of a renaissance, for finitism itself is as old as Critical Philosophy. As far as we can judge it to-day what Zeno wrote about was finitism (not the nature of physical space as was once thought), though on which side of the finitist fence Zeno took his stand is by no means certain. But at the peak of the mathematical development which took place between 1550 and 1850 a finitist *problem* was unthinkable; the attitude of mind which makes the problem can arise only when mathematics is deemed in some way to be completed and ready for a critical survey. That is why, in part, the professional mathematician regards finitism as something revolutionary and debasing; what is to him the current of life is to the finitist a rigid system, the *skeleton* of an organism of the past. For the practising mathematician there is only one mathematic, that which is *revealed* to him and comes to fruition within him, but the finitist sees, not something unique and inevitably true, a mathematical reality stored in the mind of a deity, but a subject for reformation.

To return to the first question, "What is finitism?" let us seek for a preliminary answer by considering to what in current mathematics the finitist takes exception. Suppose that $a_1, a_2, a_3, \ldots$ is a monotonic increasing sequence of integers bounded above by an integer A; current mathematics asserts that such a sequence has a greatest member, and this is held to be established in several ways. For instance, it might be argued that if there is no greatest member then there would be a term of the sequence which exceeded A in contradiction to the defining property of A, or, more elaborately, we might appeal to the theorem which asserts the convergence of a bounded monotonic sequence and deduce that from some a_n

onwards every a_p differs from a_n by less than unity and therefore, since the terms are integers, $a_p = a_n$ for all $p \geqslant n$, proving that a_n is the greatest term of the sequence (i.e. not less than any term of the sequence). What do we know about this greatest member? Is it the 1st, or 2nd, or 100th term of the sequence? Name any term of the sequence you please, can you say of this term that it is, or that it is not, the greatest term of the sequence? What do we know when we know "there *is* a value of n for which a_n is the greatest term of the sequence, but we don't know what this value is"? Why are we prepared to say that there *is* a number with a certain property even when it is clear that it is in the nature of the problem impossible to find this number? Existence, said Poincaré, is proved when non-existence is self-contradictory; if there is no greatest a_n then A is both greater than every a_n, and also less than some a_n, and therefore a greatest a_n exists. It seems, then, that we must choose either to admit that there *is* a greatest a_n (but that it is *undiscoverable*) or to admit that there is a number A both greater than some a_n and less than a_n. Fortunately, there is a way out of this dilemma. If we consider the integers a_1, a_2, a_3, and so on in turn, two situations may arise. We may reach an a_n which is equal to A − 1, and in this case we have found the greatest term, for every subsequent a_p must be less than A and not less than A − 1, showing that $a_p = A - 1$ and $a_n \geqslant a_m$ for any m. If, however, no matter how many terms of the sequence we construct we do not find a term as great as A − 1, *we cannot tell whether the sequence has a greatest term or not.* With this position the finitist is contented, that is to say he is prepared to admit that both of the propositions "the sequence has a greatest member," "the sequence has no greatest member" may be *unprovable.* Does this mean that there is some third possibility over and above the possibilities of having a greatest member and not having a greatest member? No! unless you call just this situation itself the third possibility, i.e. unless you mean by a third possibility just the negation "neither 'the sequence has a greatest member' nor 'the sequence has not a greatest member' is provable." But if we accept this position the consequences in conventional mathematics are almost overwhelming serious. For conventional mathematics accepts the method of proof known as *reductio ad absurdum,* which lays down the principle that *one* (and only one) of a proposition and its negation is provable, and therefore one of the propositions "the sequence has a greatest member," "the sequence has not a greatest member" must be provable, contrary to the position we accepted above. And if we challenge one of the fundamental methods of proof what confidence have we left in the other methods of proof!

Another difficulty is that although the method of *reductio ad absurdum* leaves us on the horns of a dilemma in the case we have just discussed there are cases when the use of the method seems beyond reproach. For instance, if we seek to prove that if $\{1 + (-1)^n\}/2$ is odd then n is even we might proceed thus: Suppose that n is odd then $(-1)^n = -1$ and so $\{1 + (-1)^n\}/2 = 0$, which contradicts the hypothesis that $\{1 + (-1)^n\}/2$ is odd, from which it follows that n is even, a conclusion to which no one takes exception. Why does *reductio ad absurdum* succeed in some cases but not in others; or to put the issue in a sharper form, if $p(n)$ is a mathematical proposition and $\bar{p}(n)$ its negation, why can we say, sometimes, that one of $p(n)$ and $\bar{p}(n)$ must be provable, whereas in other cases it may happen that neither $p(n)$ nor $\bar{p}(n)$ is provable? The finitist answer is this. When the proof or refutation of $p(n)$ for each value of n can be effected in a *finite number of steps* the principle "one of $p(n)$ and $\bar{p}(n)$ is provable" is valid, but not otherwise. For example, if "$p(n)$" is "n is an even number," then the principle is valid, because whatever value n may have we may determine in a finite number of steps whether n is even or not, whereas if "$p(n)$" is "a_n is the greatest member of a bounded monotonic sequence of integers (a_n)" the principle is not valid because we have no means of deciding in a finite number of steps whether any a_n is the greatest member or not.

A good illustration of the application of the test is afforded by the proposition "there is no greatest prime number." The conventional proof by *reductio ad absurdum* proceeds by showing that if N were the greatest prime number then the least factor (greater than unity) of $N! + 1$, being necessarily greater than N, is not prime, but the least factor (above unity) having no divisor must be prime, which is a contradiction. To justify the *reductio ad absurdum* we are required to show that "n is the greatest prime" is provable or refutable in a finite number of steps, for each value of n. Since the numbers greater than n form an endless sequence we cannot test the theorem by examining all these numbers; there is, however, a way in which we can test the result, and this is given to us, oddly enough, by the *reductio ad absurdum* proof itself, for in that proof we saw that whatever n may be the least factor (above unity) of $n! + 1$ is a prime greater than n. Yet the moment we justify the *reductio ad absurdum* in this way it becomes redundant for in saying that the least factor of $n! + 1$ is a prime greater than n we have a direct proof of the proposition that there is no greatest prime.

The weakness of the criterion "proof or refutation in a finite number of steps" is that the nature of a finite proof is by no means as obvious as it once seemed. When we say "a finite number of steps" must this number be specifiable in advance? Does not the very dilemma we sought to resolve make a re-appearance? Is a proof *finite* if we cannot say *how many* steps it contains? For instance, there are good grounds for believing that every decreasing sequence of transfinite ordinals (less than ω_1 say) is *finite*, but nothing is known regarding the number of terms such a sequence may have (except for ordinals less than $\omega^{\omega^{\omega}}$ for which definite limits can be set). The point of divergence of Gödel's proof of the impossibility of proving Arithmetic free from contradiction from Gentzen's proof of freedom from contradiction is just that Gentzen's conception of a finite proof includes this ordinal theorem whereas Gödel's does not.

My own view is that attempts to distinguish valid and invalid uses of logical principles like *reductio ad absurdum* are mistaken and that a preferable line to follow is the development of a mathematical system entirely free from the symbols and axioms of logic.

The point at issue between the finitist and his opponent is brought out very clearly in the Zeno paradoxes; in fact, one might say that Zeno's is one of the most successful statements of the case *against* finitism. Zeno, one might say, attempts to persuade you that it *is* possible to complete an endless process. For, Zeno observes, in passing from 0 to 1 you pass through $\frac{1}{2}$ and on the way from $\frac{1}{2}$ to 1, through $\frac{3}{4}$, and after $\frac{3}{4}$ you must pass through $\frac{7}{8}$ and so on, so that in passing from 0 to 1 you *complete* the *endless* process of passing through $\frac{1}{2}$, $\frac{3}{4}$, $\frac{7}{8}$, and so on. And if you can complete this endless process, the opponent of finitism can say, why can we not talk of completing the process of searching through all the integers (endless process) to find if, e.g., there is an even number which is not the sum of two primes? If it makes sense to talk of completing an endless process, why confine oneself only to finite ones, with such disastrous consequences to mathematics? Thus it becomes very important to the finitist to find a fallacy in Zeno's argument—and fallacy there is. An effective way of showing up the fallacy, which is subtle and not readily grasped, is to describe Zeno's process in terms of drawing dashes. In drawing a dash from 0 to 1, Zeno argues, you complete the endless process of drawing a dash from 0 to $\frac{1}{2}$, a dash from $\frac{1}{2}$ to $\frac{3}{4}$, a dash from $\frac{3}{4}$ to $\frac{7}{8}$, and so on. But do you? If you, in fact, drew a dash from 0 to $\frac{1}{2}$, and then one from $\frac{1}{2}$ to $\frac{3}{4}$, and so on, you would *never* complete the process of drawing a dash from 0 to 1, and, moreover, in drawing a dash from 0 to 1 you do *not* complete the series of operations of drawing a dash from 0 to $\frac{1}{2}$ and one from $\frac{1}{2}$ to $\frac{3}{4}$, and so on, for drawing a dash from 0 to 1 is *not* the same thing even as drawing a dash from 0 to $\frac{1}{2}$ and one from $\frac{1}{2}$ to 1; in each case you will have a dash from 0 to 1, but that does not make the operation of drawing a dash from 0 to 1 the *same thing* as drawing two dashes, one from 0 to $\frac{1}{2}$ and the other from $\frac{1}{2}$ to 1. You might say that in counting from one to a hundred by tens you pass through all the numbers from one to a hundred, but counting by tens is still not the same thing as counting all the numbers from one to a hundred. And because there are an unlimited number of fractions between one and a hundred it does not mean that you have completed the endless task of counting all these fractions when you count from one to a hundred (by ones).

The finitist maintains not that mathematics is limited, or that there is some greatest (finite) number, but that there is no actual infinite, only the infinity of endless succession, and that the concept of the infinite as such, the completion of an endless succession, is self-contradictory.

. . .

A Three-Dimensional Jig-Saw

By P. M. GRUNDY

A THREE-DIMENSIONAL puzzle, owing to the greater difficulty of working in three dimensions than in two, needs only a few pieces. This economy, demanded by the solver, is also wanted by the designer in order that a theoretical solution may be feasible. Before constructing such a puzzle it is, of course, essential to verify that the number of solutions is neither zero (which it could be, since the pieces must all be made separately) nor too large.

We take as the basis of our puzzle the simplest possible arrangement, the division of a cube of side $2l$ into 8 subcubes of side l, and investigate the incidences of the subcubes. Only the 12 interior faces and 6 interior edges (those which pass through the centre O of the main cube) need consideration. The incidences can best be represented on the plane diagram of an octahedron, the solid dual to a cube. For applications a precise rule for passing from the solid figure to the plane diagram is essential, so we agree to carry it out as follows. First, project everything from O to the surface of a sphere S with centre O. The 12 interior faces go into (great-) circular arcs on S, and since each subcube has 3 interior faces these arcs form a configuration of 8 spherical triangles, with 6 vertices projected from the 6 interior edges. Second, we project the spherical figure stereographically on a plane π from a point P of S not on any of the arcs. The result is the configuration of 8 triangles shown (with elaborations) in Fig. 2. Strictly speaking, their sides should be curved, but that is quite irrelevant. Note that ABC is one of them, and, because P is projected to infinity, we must agree to describe as the inside of ABC what would normally be called its outside. The 8 subcubes correspond to the 8 triangles, the 12 interior faces to the 12 sides, and the 6 interior edges to the 6 vertices in π.

To derive a puzzle from this scheme we modify the shape of the subcubes, without altering their arrangement, by sticking lumps on to some of their interior faces and hollowing out lumps from others. We shall only deal with the simple case when each "lump" is a right angled triangular prism whose hypotenuse is an interior face of the subcube concerned. The fact that the distinct pieces appear a little monotonous is actually an advantage; for an attempted solution can be begun in many ways, most of which are wrong.

Now concentrate attention on a single subcube Q and the corresponding triangle $\triangle$ in π. For each interior face f of Q we must indicate in the diagram whether a prism has been added to or subtracted from that face; and, if either, to which of the two interior edges of f the axis of the prism is parallel. This edge corresponds to a vertex of $\triangle$, so we get the required indication by drawing an arrow adjacent to that vertex, crossing the side of $\triangle$ corresponding to f. The arrow is drawn out from $\triangle$ ("positively w.r.t. $\triangle$") when the prism is added, in towards $\triangle$ ("negatively w.r.t. $\triangle$") when the prism is subtracted; while if no prism is added or subtracted the corresponding side of $\triangle$ is not marked. By attaching arrows to $\triangle$ in all possible ways, not more than one per side, we find that there are exactly 25 possible pieces. The arrows may be assigned arbitrarily, subject to the

Constructability Condition: Not more than two negative arrows may be attached to $\triangle$, and they must be adjacent to the same vertex.

The necessity (and sufficiency) of this condition is due to the fact that a given portion of a subcube can only be removed once. Hence two negative prisms must not overlap, which implies that their axes must be parallel.

When the puzzle is assembled we get arrows attached to each of the 8 triangles in π. The essential fact, which makes the diagram useful, is that the two arrows crossing any side, attached to the triangles having that side in common, coincide; or else neither triangle has an arrow crossing that side. Conversely, *if this holds, the pieces do fit together*. This fact immediately leads to the

Compatibility Conditions:

Total no. of +left arrows = total no. of −right arrows,

Total no. of −left arrows = total no. of +right arrows,

where an arrow attached to $\triangle$ is called left or right w.r.t. $\triangle$ according as it crosses a side of $\triangle$ at the left or right end, viewed from the inside of $\triangle$. These conditions are necessary, though not sufficient, for a given set of 8 pieces from the 25 to admit a solution; they thus help the designer to avoid choosing an insoluble set. The designer has also, of course, to avoid getting too many solutions.

While there are some vague principles which tend to keep the number low, this question can ultimately be settled only by actually finding the solutions.

The determination on paper of all solutions for a particular set of pieces is now a straightforward matter of trial, facilitated by the fact that partial information about the arrows attached to a given triangle can easily be recorded in the diagram. The knowledge that the pieces all satisfy the constructability condition is often useful, and various similar devices immediately suggest themselves once the pieces are given, the more so when some have been fitted into position in the diagram.

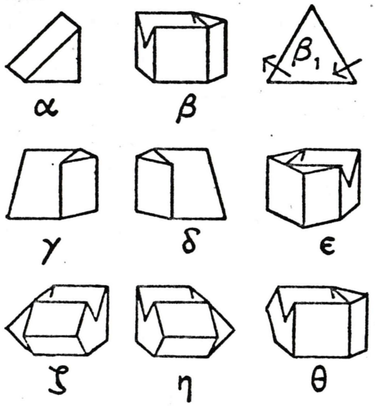

Fig. 1

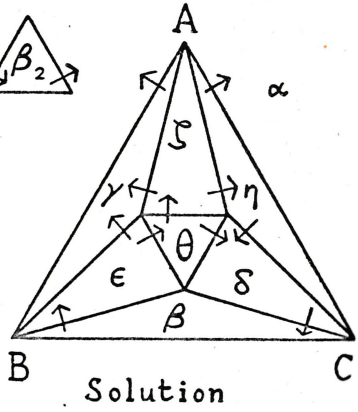

Fig. 2

As for the set $\alpha,\ldots,\theta$ of Fig. 1, it should be noticed that β has a pair of vertices either of which can be placed at O, and these two positions result in different arrow systems β_1 and β_2 representing the same piece β. This sort of ambiguity (which does not occur for $\alpha,\gamma,\ldots,\theta$) can frequently be cut out by the compatibility conditions; though not in the present case, because β_1 and β_2 are both self-cancelling. Of over a dozen likely-looking sets of pieces which I have tested by the above method, only the set in Fig. 1 has a unique solution.

Mathematics and the Naval Officer

By Instructor-Captain G. A. CLARKSON, R.N.

You would be surprised if you knew how much time and energy I have expended in explaining to mathematicians that their knowledge could *really* be put to some use! Most of them seemed to be under the impression that they had been studying art for art's sake. But do not blame the poor deluded creatures: it all started in their early youth when some misguided teacher showed them how to do "compound addition"—quite under the impression that this would lure them from the Wicked Paths of Practice into the Beautiful Realms of Fancy.

In a recent number of the *Mathematical Gazette* one of the said teachers suddenly came down to earth and wrote an article, entitled "Debunking Arithmetic", in which the futility of spending time on such sums as the addition of hours, minutes and seconds was, in the author's opinion, put into its proper perspective—("spending *time*" was quite unintentional, but, if I may say so, rather good!). But, as so often happens with generalisations, there was a flaw in the argument and the author played straight into the hands of those who go down to the sea in ships: "sums" of the compound addition variety involving time units form a very large part of the daily round of the navigating officer.

But I have been blown off my course: I was talking of mathematicians and their possible uses.

I, myself, have been accused of being a mathematician by some of my casual acquaintances: I did, as a matter of fact, manage to secure honours in Mathematics and Physics at a University (probably little known to most of my readers!) on the banks of the Isis. This was about 30 years ago—and of those 30 I have spent more than 25 as an Instructor Officer in His Majesty's Navy. This is just because I *was* (on paper) a mathematician-cum-physicist: I very soon found that the accent was on the "paper".

Few people are aware of the existence of the Instructor Branch of the Royal Navy: its counterpart in the Army is the Educational Corps. The branch exists primarily for the instruction of the Junior Officers of the Service in the gentle art of navigation; an art easily acquired by the *practical* mathematician—at the government's expense! The teaching of ratings is usually carried out by one of the Schoolmaster Branch, which contains officers who enter the Service with Warrant Rank and who act under the guidance of the Instructor Officers.

The Instructor Officer will find it to his advantage to have had some previous teaching experience and to have some knowledge of physics: he certainly does "join the navy and see the world" since all the larger ships and some of the smaller ones carry an "I.O." whose duties may involve, amongst other things, a knowledge of meteorology—also taught at government expense to the "elect". The average sailor (and, indeed, many of his brother officers), seems to expect the I.O. to be a walking compendium of knowledge: with a little ingenuity this impression is easily fostered! There is no doubt that the Naval Officer gets that variety which adds the spice to life since, as is seldom realised by "landlubbers," no Naval Officer is allowed to remain normally in any one job for more than a certain length of time—the average being about two to three years: shore jobs are interspersed with sea jobs and foreign stations with home stations.

Little did I think, when I took my degree in 1911, that I should ever find any use for "COT with an OT suggests OUTSIDE with an OT" (and a lot more), which was my introduction to the *Four Parts Formula* of spherical trigonometry by a learned Balliol professor. But, lo and behold, when a few years later (after my initiation period at a Prep School) the Great War projected me into the Instructor Branch of the Royal Navy, I hailed with joy and was able (thanks to excellent teaching) to quote correctly the "cot outside side sine inside side . . ." of my varsity days. By the way, it may have dawned on you that even spherical trigonometry has its uses: you certainly cannot navigate without it.

Does it surprise you to learn that Naval Gunnery Officers revel practically in d^2s/dt^2, though they may *call* it something else? Do you realise that one lives in a realm of relative velocity and that what a very eminent Cambridge mathematician has defined as "rotation of a moment of momentum or spin couple" is merely an everyday phenomenon on which the life of the sailor or airman may depend?

I can't help wondering how many of you recognise "precession" in the above disguise—but much more do I wonder how many of you have ever really consciously *seen* precession. Many of you will, no doubt, talk (I nearly said *torque*!) glibly about axes of rotation, couples, theta-dots or omegas or whatever notation happens to be *de rigueur*—but, I repeat, how many of you have actually *seen* precession? When, as an introduction to the theory of the Gyro-Compass (which, in case you do not realise it, indicates the direction of true north and is used in all large and many small warships), I demonstrate to a class of grown men the practical existence of precession, I see on the faces of most of my class that same rapture which dawns on the face of a small child who watches, for the first time, a conjuror produce a rabbit out of a hat: I really believe some of them think I have something up my sleeve! About sixty per cent. of my classes of budding I.O.'s have, at any rate, had the honesty to confess that precession had been to them merely a piece of book work—or words to that effect. Cambridge mathematicians should not be alarmed at this apparent heresy, for does not even the great Routh himself say that "whenever some useful instrument has been found, which did not require so lengthy a description as to unfit it for an illustration, it has been preferred as an example to a merely curious and artificial construction."

When an Instructor Officer is accepted for the Navy he starts life (after 3 or 4 months undergoing the necessary courses) as a "two-striper"—i.e. an Instructor Lieutenant—dropping the "Acting" rank given to him during his courses. For a year or two he remains "Temporary", which merely means that either party to the contract can, so to speak, give notice if not satisfied. Provided that he does not blot his copy book, the Instructor Lieutenant rises automatically through the rank of Instructor Lieutenant-Commander to the rank of Instructor Commander. He may retire in this rank (with a pension) or, if he is one of the lucky ones, be promoted (by selection) to the rank of Instructor Captain. The controller of the branch is the one and only Instructor Rear Admiral. At the risk of "teaching my grandmother" in these days when the Services are so much better known to the *Hoi Polloi* than in peace time, I should like to point out that a Naval Captain ranks much higher than an Army Captain—in fact the former is of equivalent rank to a Colonel and the latter to a Lieutenant R.N.

I have found it most illuminating to have taught navigation (mainly) to officers ranging from Cadets aged about 15 to fully fledged mathematical schoolmasters (entering the Navy as I.O.'s) some of whom were about twice that age. It gives me acute pleasure to see the latter making, sooner or later (usually sooner), *all* the mistakes made by the former! Only this very morning one of them got the wrong answer because he had made the equator join the poles—and he is by no means the first one who, in my experience, has done that! Silly, isn't it—*you* wouldn't do it, or so *you* think! All that is really necessary for practical navigation (as far as the paper work is concerned) is the ability to add and subtract correctly and use books of tables intelligently. At the end of a course in this subject I find that the mathematician and/or schoolmaster has at last become almost human: he has probably, by this time, been completely converted to Abraham Lincoln's theory that "the man who can't make a mistake can't make anything"!

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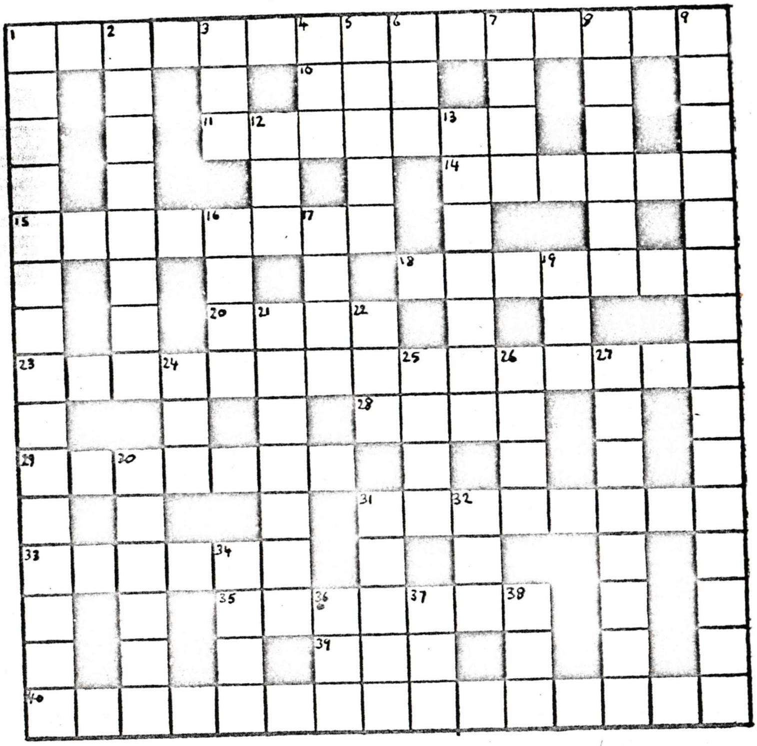
With motion irrotational in fluid incompressible,
A tiny little minnow swims along a line of flow,
And the greater its velocity—well, cutting out verbosity—
The greater its velocity the faster it will go.

"ARCUS."

A Mathematical Crossword

By The President



CLUES ACROSS

1. Cite moral Girton (anag) (15).
10. Cleric who is short for 2π (3).
11. This rule, introduced during the Great War, is not much fun (2, 5).
14. This of 5 is used to estimate relative values (6).
15. Taking this is often the last step in a proof (4, 4).
18. Brothers, in one direction (7).
20. This lady's first name makes her sound as if she were always complaining (4).
23. A curtailed remark appears in a legitimate risk, but the result is irrational (15).
28. This anagram of seat sounds as if it's a puzzler (4).
29. Seems always to be on the point of vanishing (7).
31. Discontinuous (8).
33. Never without cause (6).
35. Tending to percolate or intermix, when separated by porous septa (7).
39. Crooners idolise her (3).
40. 3,721 (5, 3, 7).

CLUES DOWN

1. Menace harsh tide (anag) (3, 12).
2. For example, $a_0a_1a_2 + a_0{}^2a_3$ (8).
3. Admit (3).
4. Trifle (3).
5. See 14 (5).
6. Maths. society wife (3).
7. Cyclic order (4).
8. Recent, but not American (6).

This is used in problems on restricted maxima and minima (9, 6).

[illegible] (3).

[illegible] and Cecil unanimous in this? ([illegible])

This may be pink, or a tree (4).

Evidently an elevated lady (4).

Associated with 1 down on occasion ([illegible])

Signifies (7).

Industrious, and sounds like a female relative in Lancashire (3).

[illegible], with a French accent (3).

This prefix reduces the 33 of the word it precedes (4).

26. Charge a 100 for this arm. See? (4).

27. This concurs quite often (8).

30. Indispensable in tensor calculus (6).

31. Sherlock Holmes is nowhere compared with this, when it comes to detecting (5).

32. French pronoun (3).

34. This is an everyday thing to a journalist, but to do it is a crime in an exam. (4).

36. Derivatives vanish here (3).

37. Heap in France (3).

38. The French believe, in the past (3).

(*The solution is on page* 25.)

A Mathematical Cipher

By Prof. HAROLD SIMPSON

The interesting note on ciphers in EUREKA of January, 1941, has suggested that some of your readers may like to try their skill in decoding the following sentence:—

AE*df*BC*m*ACED*hz*JN*a*M*e*A*f*J*b*AFG*n*QAL*g*AKF*n*J*a*IB*f*BF*fs*CA E*h*J*a*FG*dt*M*i*M*e*BH*f*AK*g*O*c*XJ*a*H*f*DCEA*h*FAK*ny*CI*ju*AD*h*IC*j*DE G*d*AGF*l*VAJ*g*J*a*PJ*k*ST.

If they have ever learnt typewriting, they may have met it before. The only merit claimed for the cipher here used (which must not be taken too seriously) is that, even if the key falls into the hands of the enemy, it will be worthless to him unless he is a mathematician!

Is it too much for you? Well then, here's the key:—

A $= x$, B $= x^2 - 1$, C $= y$, D $= y + 1$, E $= y - 1$, F $= x + y$,
G $= x - y$, H$=(2x-y-1)\ (2x+y+1)$, I$=(2x-y+1)\ (2x+y-1)$,
J $= x^2+y^2-1$, K$=x^2+y^2-y$, L$=(x^2+y^2+y)\ (x^2+y^2-y)$,
M $= 16x^2 + y^2 - 2y - 3$, N $= 2(x - y - \sqrt{2})^2 + 1000(x + y)^2 - 1$,
O $= 6yx^2 + y^2x + y^3 - x - y$, P $= 50(5y + 3)^2 - 5x + 2$.

$a = 1$, $b = 1 - 2x$, $c = 1 - 4x^2$, $d = 1 + \sigma - x^2$, $e = 1 - y$,
$f = 1 + \sigma - y^2$, $g=(\sigma+x)(1+\sigma-y^2)$, $h=(1-x)\ (\sigma+x)(1+\sigma-y^2)$,
$i = 2x - y$, $j=(1+y)\ (1+\sigma-2x-y)\ (1+\sigma+2x-y)$, $k=4-5x$,
$l = \sigma + y\,(10x^2 - y^2) - (2x^2 + y^2)^2$,
$m = \Pi\left\{(2 + \sigma)^p - \omega(x + y)^p - \Omega\,(x - y)^p\right\}$,

where $(2r - 1)p = 2r$, r being a fairly large positive integer and ω, Ω being all possible $(2r - 1)th$ roots of unity,

$$n = \Pi \{(2 + \sigma)^p - \omega (2x + y - 1)^p - \Omega (2x - y - 1)^p\}.$$

For the rest, trust your memory in case a copy of the key is stolen. The sentence is read as follows:—$A^2E^2 = \epsilon df$, $B^2C^2 = \epsilon df$, $A^2C^2E^2D^2 = \epsilon h$, (end of word), $J^2N^2 = \epsilon a$, $M^2 = \epsilon e$, etc.

Throughout ϵ and σ denote small positive constants; σ may be taken as $1/20$ and ϵ smaller still. Any symbol not in the key, such as Q or r, is a dummy marking the space between two words, while a pair of such dummies marks the end of a sentence. Now draw the curves with the above equations $A^2E^2 = \epsilon df$, etc., and the sentence is before you in block capitals! Perhaps it may be advisable to point out that in the key m is positive inside a curve very much like the square with sides $x = \pm 1$, $y = \pm 1$, but slightly enlarged and slightly convex, $m = 0$ being the rationalised form of $(2 + \sigma)^p = (x + y)^p + (x - y)^p$. Likewise n bears a similar relation to the rectangle with sides $x = 0$, $x = 1$, $y = \pm 1$.

▪ ▪ ▪

And thei that bee dulle witted, and yet be instructed and exercised in it (Arithmetike), though thei gette nothyng els, yet this shall thei all obtain, that thei shall bee moare sharpe witted than thei were before.

ROBERT RECORDE—*The Whetstone of Witte.*

▪ ▪ ▪

Little beast one doesn't smile on, almost smaller than ϵ,
(I am, of course, referring to that pest, the common flea)
It hops around quite happily in very large parabolae
And its maximum displacement equals v^2 over g.

"ARCUS."

▪ ▪ ▪

It has been observed that the rate of change of the birth rate in a certain Baltic country is very distinctly correlated with the immigration of storks into the country.

So there, you moderns!

A Shuffling Problem

By T. H. R. Skyrme

A way of shuffling cards well known, at least in theory, is that in which the cards are taken one by one from the top of the pack and placed alternately above and below those already removed. The main problem is to determine the least number of shuffles required to restore the original order. Its solution is well known, and we shall obtain it together with an analysis of the journeys of the various cards.

The pack shall contain N cards numbered consecutively from the top in the original order. There are two associated methods of shuffling differing in the relative positions of the first two cards. Thus for $N = 7$, after one shuffle the order of the cards is 7531246 in the first method, and 6421357 in the second. It is clear that the problem for $2K$ cards is the same as that for $2K-1$ cards in the first case, and the same for $2K+1$ as for $2K$ in the second case. By a "shuffle of N cards" we shall mean a shuffle of the first or second type according as N is odd or even.

The new order then becomes $N, N-2, \ldots, 1, \ldots N-3, N-1$. Thus the Nth card takes the first place, the $(N-1)$st the Nth place, etc. If we continue this series $1, N, N-1, N-3, \ldots$ where each card takes the place previously occupied by the preceding one we must eventually return to 1. If this does not exhaust all the numbers $1 \ldots N$ we begin a similar sequence with another one and continue till the order resulting from one shuffle is represented by a number of such sequences called the cycles of the permutation. It is then evident that any card occupies successively all the places in the cycle to which it belongs initially and no others. Thus for $N=7$, the cycles are (1764), (25), (3). We are interested in the number of these cycles and their order, i.e. the number of numbers each contains.

We suppose $2N+1=p_1^{a_1} \ldots p_s^{a_s}$ where the p_i are different odd primes, and let $2N+1=p_i(2Q_i+1)$ for any p_i. Also, for a given integer x, let $x^*=(1-x)^*$ be that number between 1 and $N+1$ which is congruent either to x or $(1-x)$, mod $(2N+1)$.

Then it is easily verified that one shuffle of N cards moves the card originally at x_0 to x_1^* where $x_1=2x_0-(N+1)$. Then a second shuffle moves it to x_2^* where $x_2=2x_1^*-(N+1)$. But if $z_2=2x_1-(N+1)$, then $z_2^*=x_2^*$ for

$$[2(1-x_1)-(N+1)] \equiv [1-(2x_1-\overline{N+1})], \text{ mod } (2N+1).$$

Hence by induction r shuffles moves it to the place x_r^*, where

$$x_r=2^r x_0-(2^r-1)(N+1) \equiv 2^r x_0-2^{r-1}-N, \text{ mod } (2N+1).$$

Thus the first card returns to the first place when $(2^{r-1}-N)^* = 1$ which implies $2^{r-1}-N \equiv 1$ or 0; $2^r \equiv 2N+2$ or $2N$; therefore $2^r \equiv \pm 1$, and clearly if this is satisfied $(2^{r-1}-N)^* = 1$. Let m be the least integer $r>0$ for which $2^r \equiv \pm 1$, mod $(2N+1)$. Then $2^m x_0 - 2^{m-1} - N = 2^{m-1} - N + 2^m (x_0 - 1) \equiv x_0$ or $1 - x_0$; or $(2^m x_0 - 2^{m-1} - N)^* = x_0$ for any x_0. Hence m is the required least number of shuffles needed to restore the original order.

Suppose the card at x_0 returns to its original position after $t<m$ shuffles, so that t is the order of the cycle containing x_0. Then $x_0 = (2^t x_0 - 2^{t-1} - N)^* = (2^{2t} x_0 - 2^{2t-1} - N)^*$ which implies that $(2^t \pm 1)(N+x) \equiv 0$, mod $(2N+1)$. By hypothesis $2^t \not\equiv \pm 1$, hence $N+x$ is a factor of $2N+1$, divisible by some prime p_i. Then $x \equiv N+1$ $= p_i Q_i + \frac{1}{2}(p_i+1) \equiv -\frac{1}{2}(p_i - 1)$, mod p_i. Let $y_{ri} = rp_i - \frac{1}{2}(p_i - 1)$, $r = 1 \ldots . Q_i$; then if $x_0 = y_{ri}$, $x_1 = 2y_{ri} - (N+1) = [2r - (Q_i+1)]p_i - \frac{1}{2}(p_i - 1)$. Thus the result of one shuffle on the Q_i cards y_r is to permute them among themselves in the same way as a shuffle of Q_i cards.

The total number of cards with positions y_{ri} is given by

$$\sum\left\{\left[\frac{N}{p_i}\right] - \left[\frac{N}{p_i p_j}\right] + \ldots .\right\},$$

$$= N - \tfrac{1}{2}\left\{2N + 1 - \sum\frac{2N+1}{p_i} + \ldots .\right\} = N - \tfrac{1}{2}\phi\,(2N+1),$$

where $\phi(t)$ is the number of numbers less than and prime to t. The remaining cards belong to cycles of order m, and m divides $\frac{1}{2}\phi\,(2N+1)$ as it must since $2^{\phi(2N+1)} \equiv 1$ by Fermat's theorem.

We can deduce some particular results. If $2N+1$ is prime, then $\frac{1}{2}\phi(2N+1) = N$, hence m/N; thus if N is also prime $m=N$. If $2N+1$ is composite then $m<N$. We can show very easily that if $N=2^q$ or 2^q-1, $m=q+1$, and it follows by the same argument that $m \geqslant \log_2(2N)$ for all N. This enables us to enumerate all N whose shuffles contain a cycle of k cards for any given K; for if $N>2^{K-1}$ than the cycle of k cards must be a cycle of cards y_{ri} and thus of a shuffle of Q_i cards where $2Q_i + 1/2N + 1$. Hence if N are the values of $N \leqslant 2^{K-1}$ which have a cycle of k cards, then any N of this type must have $2N+1$ a multiple of $2N_r+1$ for some r.

Thus for $k=1$, $2N+1$ must be a multiple of 3, so that one card occupies the same position after a shuffle if and only if $N \equiv 1$, mod 3. Similarly for $k=2$, $2N+1$ must be a multiple of 5; for $k=3$, of 7 or 9, and so on.

Finally, some numerical results easily deduced from the rules above. For $N=7$, $m=4$, and the cycles have orders $4, 2, 1$; $N=32$, $m=6$, and the orders are $6, 6, 6, 6, 6, 2$; $N=52$, $m=12$, and the orders are $12, 12, 12, 6, 4, 3, 2, 1$.

Notations for Rotations

By Bertha Jeffreys.

(*a*) *Dyadic.*

Let $\mathbf{r}\,(x_\alpha)$ be the position vector of a point P of a rigid body and when the body is rotated through an angle θ about a line through the origin with direction cosines l_α let P move to P′ with position vector $\mathbf{r}'(x_\alpha')$. Let **l**, **m**, **n**, be three mutually perpendicular unit vectors such that $\mathbf{l} \wedge \mathbf{r}$ lies along the positive direction of **n**.

Then

$$\mathbf{r}' = \mathbf{r}\,.\,\mathbf{ll} + \cos\theta\,\mathbf{r}\,.\,\mathbf{mm} + \sin\theta\,\mathbf{r}\,.\,\mathbf{mn}.$$

Now

$$\mathbf{r}\,.\,\mathbf{mm} = \mathbf{r} - \mathbf{r}\,.\,\mathbf{ll}$$

and

$$\mathbf{r}\,.\,\mathbf{mn} = \mathbf{l}\wedge\mathbf{r}.$$

Hence

$$\begin{aligned}\mathbf{r}' &= \mathbf{r}\,.\,\mathbf{ll} + \cos\theta\,(\mathbf{r} - \mathbf{r}\,.\,\mathbf{ll}) + \sin\theta\,\mathbf{l}\wedge\mathbf{r}\\ &= [\mathrm{U}\cos\theta + \mathbf{ll}\,(1 - \cos\theta) + \sin\theta\,\mathrm{U}\wedge\mathbf{l}]\,.\,\mathbf{r}\end{aligned}$$

where U is the unit dyadic. Denoting the factor in brackets by the rotation tensor R

$$\mathbf{r}' = \mathrm{R}\,.\,\mathbf{r}$$

and for successive rotations $\mathrm{R}_1, \ldots.\mathrm{R}_n$,

$$\mathbf{r}' = \mathrm{R}_1\,.\,\mathrm{R}_2\,.\,\mathrm{R}_3\ldots.\mathrm{R}_n\,.\,\mathbf{r}.$$

In suffix notation

$$\mathrm{R}_{\alpha\beta} = \delta_{\alpha\beta}\cos\theta + l_\alpha l_\beta\,(1 - \cos\theta) - \sin\theta\,\epsilon_{\alpha\beta\gamma}\,l_\gamma$$

(*b*) *Quaternion.*

The multiplication rules for 1, i, j, k are

$$1\,.\,i = i, \ldots.,\ i\,.\,i = -1, \ldots.,\ i.j = -j\,.\,i = k.$$

Then if x_α, x_α', l_α and θ have the same meaning as in (*a*)

$$x_1'i + x_2'j + x_3'k = \left[\cos\frac{\theta}{2} + \sin\frac{\theta}{2}\,(l_1 i + l_2 j + l_3 k)\right].$$

$$[x_1 i + x_2 j + x_3 k]\,.\left[\cos\frac{\theta}{2} - \sin\frac{\theta}{2}\,(l_1 i + l_2 j + l_3 k)\right].$$

Denoting the first factor by p, the last is p^{-1}. Hence for successive rotations denoted by $p_1, \ldots.p_n$,

$$x_1'i + x_2'j + x_3'k = p_1\ldots.p_n.(x_1 i + x_2 j + x_3 k).p_1^{-1}\ldots.p_n^{-1}$$

(*a*) and (*b*) are a footnote to a recent correspondence in the *Mathematical Gazette* (December, 1941).

(c) *Matrix.*

(i) Applying the unitary transformation

$$u_1' = u_1 a - u_2 \bar{b} \qquad a\bar{a} + b\bar{b} = 1$$
$$u_2' = u_1 b + u_2 \bar{a} \qquad (\bar{a} \text{ denotes conjugate complex})$$

to the quadratic form

$$c_{11}u_1^2 + 2c_{12}u_1u_2 + c_{22}u_2^2$$

we have that

$$c_{12}^2 - c_{11}c_{22} \text{ is invariant.}$$

Put

$$x_1 + ix_2 = c_{22}$$
$$x_1 - ix_2 = -c_{11}$$
$$x_3 = c_{12}.$$

Then $x_1^2 + x_2^2 + x_3^2$ is invariant and a rotation in 3-space is given by the unitary transformation in 2-space.

In particular, putting $a = \cos \theta/2$, $b = -\sin \theta/2$, we get

$$x_1' = x_1 \cos \theta + x_3 \sin \theta$$
$$x_2' = x_2$$
$$x_3' = -x_1 \sin \theta + x_3 \cos \theta$$

a rotation through θ about the x_2-axis, and putting $a = exp\,(-i\phi/2)$, $b = 0$,

$$x_1' = x_1 \cos \phi - x_2 \sin \phi$$
$$x_2' = x_1 \sin \phi + x_2 \cos \phi$$
$$x_3' = x_3$$

a rotation through ϕ about the x_3-axis. A general rotation can be built up from these.

(ii) Rotations in 4-space.

Applying the transformations

$$\begin{matrix} u_1' = u_1 a + u_2 c \\ u_2' = u_1 b + u_2 d \end{matrix}; \quad \begin{matrix} v_1' = v_1 \bar{a} + v_2 \bar{c} \\ v_2' = v_1 \bar{b} + v_2 \bar{d} \end{matrix}; \quad ad - bc = 1$$

to the bilinear form

$$c_{11}u_1v_1 + c_{12}u_1v_2 + c_{21}u_2v_1 + c_{22}u_2v_2,$$

then $c_{12}c_{21} - c_{11}c_{22}$ is invariant.

Putting

$$c_{21} = x_1 + ix_2 \qquad c_{11} = x_3 - ix_4$$
$$c_{12} = x_1 - ix_2 \qquad c_{22} = -x_3 - ix_4$$

then $x_1^2 + x_2^2 + x_3^2 + x_4$ is invariant.

In particular, with $d = 1/a$, $b = c = 0$, $x_4 = ict$, we obtain the Lorentz transformation corresponding to a velocity in the x_3 direction.

The Early Days of the Calculus

By S. Lilley

Every Archimedean knows of the almost simultaneous invention of the infinitesimal calculus by Newton and Leibniz and of the barren controversy which resulted. Less well known is the fact that this success was preceded by fifty years of work by others in which the concepts of the calculus were slowly evolved in an implicit form. The germ of the calculus is to be found in Kepler's method (1615) of evaluating the volume of a solid of revolution by dividing it into what he regarded as an infinite number of infinitesimally thick laminae. A similar idea was used by Cavalieri in his method of indivisibles (1635), in which he regarded a curve as composed of an infinite number of points, an area as composed of an infinite number of curves, and so on. Roserval (1602–75) improved Cavalieri's conception by regarding a curve as composed of elementary lines and so on; and Pascal (1623–62) worked on similar lines. Wallis, by 1655, had produced a method of finding areas under curves of the form $y = x^n$, by what was essentially a particular case of integration. In the fifteen years preceding 1673, Huyghens, working on the properties of the pendulum, used many particular cases of integration. Barrow, Newton's teacher and predecessor in the Lucasian Chair at Cambridge, showed how to find the slope of the tangent to a curve of given equation.

At the end of these many attempts Newton discovered his fluxions in 1665–6, but did not publish his work till 1704, while Leibniz's discovery, probably independent, came in 1675–6.

Such a sudden outburst of widespread research is sufficiently remarkable to call for explanation. To say that this was in general a period of great scientific activity leaves unanswered such questions as why it was such a period and why the activity took this form, rather than (say) research on the equally easy problem of discovering the conservation of energy (which did not interest scientists for nearly another 200 years).

In his essay on Newton, Hessen* suggests an extremely powerful method of attack on such questions. Space does not allow more than an inadequate summary of this essay, which is well worth the attention of every mathematician. He points out that the sixteenth and seventeenth centuries form a period in which the capitalism of the modern world was rising and throwing off the bonds of feudalism. The chief advances of rising capitalism were being made in navigation

* B. Hessen, "The Social and Economic Roots of Newton's Principia," *Science at the Cross Roads* (London, 1931), 151–203.

(the most important aspect of the new commerce), mining (the most basic of industries) and war (a colossal outburst of which accompanied the rivalries of the growing commercial powers); and he shows that a very great part of the scientific activity of the period was concerned with problems involved in these professions.

Navigation presented two outstanding problems: the determination of longitude and the forecasting of tides. The latter is clearly an astronomical problem, while the former reduces to the comparison of local times at different places. One method of comparing times is by observation of such astronomical phenomena as eclipses, occultations and the position of the moon among the fixed stars; and this requires the ability to forecast accurately the future positions of the moon and the planets. A large part of the astronomy of these centuries, the time of Copernicus, Brahe, Kepler, Newton and Flamsteed, is expressly devoted to the problems of longitude and the tides. And there seems to be little doubt that the general interest thus aroused in astronomy was largely responsible for the work of those astronomers (actually few in number) who did not explicitly acknowledge their interest in the problems of navigation. The other potential methed of finding longitude is by the use of the pendulum clock, and it is from researches concerned with clocks that a great part of the development of mechanics arose, from the work of such men as Galileo and his pupils Hooke and, above all, Huyghens.

The scientific problems arising from mining do not much concern the history of the calculus, but the growing importance of artillery in warfare gave rise to an intense study of the trajectory of a projectile, which was another important factor in the growth of dynamics. Galileo virtually considered only the pendulum, falling bodies and the parabolic trajectory, yet on this narrow basis he established the foundation of terrestrial mechanics. After his time the increasing power of cannon turned attention more and more to the trajectory under air resistance, investigated by Toricelli, Newton, Bernouilli, Euler and many others.

The chief object of Hessen's essay is to demonstrate the influence of these and other practical problems on Newton's *Principia*. It is not suggested that *Principia* is anything like a technical handbook to the problems of early capitalism (though Newton at times showed very practical interests), but rather that the widespread interest in astronomy as the sailor's science, in the pendulum clock, the flight of a cannon ball and so on was sufficient to turn the genius of Newton mainly towards those physical phenomena which were more or less directly related to these practical problems. The general theme of *Principia* is, of course, celestial dynamics, of tremendous importance in the forecasting necessary for the determination of longitude and of tides; the motion of bodies through a resisting medium, so important to ballistics, occupies a large part of the second book, which also deals with the motion of the pendulum—and so the catalogue continues. Whether or not Newton was conscious that his interests were being directed in the long run by certain pressing practical problems is not relevant; an unconscious influence is just as important as a conscious one.

Let us see then how these influences affected the development of the calculus. Kepler, who began our list of the predecessors of the fluxions, was primarily interested in astronomy. His computations required in many places the rectification of curves or evaluation of areas. His works contain many results that would now be expressed as trigonometrical integrals. True, he gets nearest to the calculus in a work (1615) which arises from the problem of finding the volumes of wine casks; but by this time much of his work on astronomy was completed and probably his experience in astronomical problems suggested the methods expressed later in more general form. Cavalieri was a pupil of Galileo, and had, no doubt, acquired some of his master's interest in astronomy and ballistics. Huyghens' approaches to the calculus all occur in his *Horologium Oscillatorium*, a work centered on the pendulum clock, avowedly for longitude determination, but extending far beyond practice into many problems of mechanics and mathematics in which Huyghens had become interested as a result of his work on the pendulum. He had discovered the isochronous property of the cycloid, and this involved its rectification, a process for which he used Cavalieri's method of indivisibles. Interest in the properties of the cycloid prompted him to consider involutes and evolutes generally and again much nascent calculus was needed. Lastly, his (the first) discussion of the compound pendulum, in which Σmr^2 and $I_z = I_x + I_y$ occur, again involved ideas akin to the calculus.

Wherever we find the cycloid in the seventeenth century, there we can suspect the influence of the clock and so of longitude. Thus it is significant that Roserval's approaches to the calculus are also chiefly concerned with the cycloid. Pascal, however, does not seem to have worked on problems closely connected with astronomy or ballistics, so that their influence, if any, on him must have been indirect.

Finally, when we come to the English names towards the end of the story, we have little need to seek direct interest in practical problems. We have only to read the first few volumes of the *Philosophical Transactions* to see how problems of navigation and ballistics continually interested the members of the Royal Society, and to realise that the ideas of those members who were not directly concerned in these things must have been influenced continually towards the many interesting problems involved indirectly in the mechanics of the heavens, the cannon ball and the clock. Also, two frequent visitors to Royal Society meetings were Huyghens, carrying on his ideas about the clock, and Leibniz, carrying away those problems which led to his independent discovery of the calculus.

It seems clear, then, that the majority of those connected with the rise of the calculus were interested more or less directly in astronomy, the pendulum clock or ballistics. These could be effectively mastered only by the calculus, and the piece-meal methods of the early workers were bound to find a synthesis within a very few years—as they did.

But in regard to the needs of navigation, the civilisation of Alexandria, 1800 years earlier, had been very similar—and had produced a similar outburst of astronomical research. Why, then, apart from Archimedes' method of exhaustions, had no effective approach to the calculus been made in that era? The answer is partly that the Alexandrians had to begin by developing trigonometry, on which the sixteenth and seventeenth centuries built, partly that the demands of navigation were much more acute when men sailed the Atlantic, than when the limits of navigation were Gibraltar and the Indus valley. But more explanation is required.

And I think the explanation lies in the development of ballistics and the clock. From Galileo on, workers in mechanics had the advantage of interest in the accessible phenomena of the projectile and the pendulum, as well as the inaccessible phenomena of the heavens; and there results an essentially different attitude. Alexandrian astronomy was concerned, as it were, with the ability to say, "This planet will be there at that time." Velocity and acceleration were not important concepts. This attitude is still found in Copernicus, who uses the word "motion" vaguely, but specifies position precisely as a function of time. But gradually mechanics, in its study of ballistics and pendulums, became more and more familiar with velocity and acceleration as precisely defined quantities. And the new concepts are soon applied to astronomy: Kepler's second law essentially reduces the variable velocity of a planet to the constant rate of change of an area. The concept of rate of change gradually grows in importance, till at the end of the story we find Newton saying in *Quadratura Curvarum* (1704), "Lines are described, not by the apposition of parts, but by the continued motion of points. . . . I sought a method of determining quantities from the velocities of the motion . . . with which they are generated; and . . . I fell by degrees upon the method of fluxions."

Thus there can be little doubt that in the fifty years of development which preceded the discoveries of Newton and Leibniz, the most important new factor was the growing familiarity with precise ideas on velocities and rates of change, which in turn arose from the widespread interest in the path of a cannon ball and the behaviour of the pendulum clock.

When we look at history in this light it does not seem strange that so much energy should suddenly have been diverted into channels leading to the calculus, nor that the eventual great discovery should have been made by two men almost simultaneously.

■ ■ ■

Solutions of May Problems

THE AREA OF GEODESIA.

The traveller entered and left the country by the same road and one might therefore, at first sight, expect him to have turned through an odd multiple of 180° while inside the frontiers. But from his readings of signposts he found that his movements amounted to a turn left through 179° 1′. He would deduce that the sum of the exterior angles of the polygon which his car had described was 0° 59′ less than it would have been on a flat earth. Assuming the earth is spherical, the "spherical excess" of the polygon was 0° 59′ or 0·0172 radians. Hence Geodesia subtends a solid angle 0·0172 at the centre of the earth and has an area of about 275,000 square miles.

J. G. O.

ACROSTIC.

1 2 3 2 1
6 5 5 3 7
93,000,000
6 2 5

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Solution to Crossword

Across.—1. Trigonometrical. 10. Rev. 11. No treat. 14. Factor. 15. Real part. 18. Western. 20. Lisa. 23. Incommensurable. 28. Tesa. 29. Epsilon. 31. Discrete. 33. Effect. 35. Osmotic. 39. Ida. 40. Sixty-one squared.

Down.—1. The Archimedeans. 2. Isobaric. 3. Own. 4. Ort. 5. Merit. 6. Eve. 7. Rota. 8. Centre. 9. Lagrange's Method. 12. Ova. 13. After us. 16. Palm. 17. Rose. 19. Tea. 21. Imports. 22. Ant. 24. Oui. 25. Semi. 26. R.A.M.C. 27. Bisector. 30. Suffix. 31. Diode. 32. Soi. 34. Copy. 36. Min. 37. Tas. 38. Cru.

Are you Hard and Sharp, or Soft and Vague?

By M. H. A. Newman

In an article on "Analysis" in the June, 1941, number of Eureka, Miss Cartwright had some rather hard things to say about abstract tendencies in analysis. The classical, or "Hardy-Littlewood" analysis, she says, is the "hard, sharp, narrow" kind, as opposed to the " 'soft, vague, broad' kind of some American and German mathematicians." This distinction was first made by Hardy himself, who said in his presidential address of 1929 to the London Mathematical Society*:—

> A thorough mastery of inequalities is to-day one of the first necessary qualifications for research in the theory of functions; at any rate, in function-theory of the "hard, sharp, narrow" kind as opposed to the "soft, large, vague" kind (I do not use any of these adjectives as words either of praise or blame), the function-theory of Bohr, Landau or Littlewood, as opposed to the function-theory of Birkhoff or Koebe.

It can hardly be disputed that the description "soft and vague" applied to a piece of mathematics, is about the worst that can be said of it, except that it is completely wrong. If the work of a candidate for a mathematical fellowship were so described, there would not be much doubt of the referee's opinion. Nor does Professor Hardy improve matters very much by his safeguarding parenthesis. If I say, in a testimonial for my cook, that she is dirty and uses foul language, and the potatoes are always hard, it does not substantially improve matters to add "but I do not use these words either as praise or blame."

The accusation of "vagueness" is a particularly hard one for analysts of the general abstract type to put up with. The Oxford Dictionary says for "vague": *indistinct, not clearly expressed or identified, of uncertain or ill-defined meaning or character.* The whole object of axiomatic theories is to give precise definitions of ideas that have hitherto been only vaguely formulated, and to unify in a single treatment arguments, from widely scattered branches of mathematics, which had previously merely been felt to have a family likeness. The very abstractness of these theories, and their independence of earlier work, excludes the possibility of any appeal to a vague intuition at any point of the argument. It is books on old-style analysis, such as the one mentioned by Miss Cartwright, that can afford to treat the theory of sets of points in rough and ready style, because the sets are situated in the familiar Euclidean

* *Journal Lond. Math. Soc.*, 4 (1929), p. 63.

plane; it can therefore be confidently assumed that all the statements made about them either have been proved by someone, or could be. When the sets are situated in general spaces even the simplest geometrical properties require careful demonstration from the axioms. This emphasis on precision and "cleanness" is not confined to the definitions and the details of the proofs. It is the aim of the axiomatic theories to isolate the central ideas of certain of the great theorems of mathematics in the purest possible form. Emmy Noether, perhaps the greatest of the axiomatic mathematicians after Hilbert, went so far as to consider it a blemish in a proof to proceed by cases ("case 1, $k \leqslant 0$, case 2, $k > 0$," etc.) unless the nature of the theorem clearly demanded it.

The "hardness" of the Hardy-Littlewood analysis refers to its extraordinary power of penetrating apparently solid rock in dealing with certain problems; the "softness" of the general methods accordingly refers to their failure to cope with these problems, because they cannot, so to speak, get close enough to them. The same situation arises in other subjects. In topology the oldest method of classifying surfaces and spaces is by means of their connectivities, and this concept has been repeatedly generalised until connectivities are now defined for spaces of the most general character. But if we are confronted with the apparently simple problem of distinguishing between differently knotted strings in ordinary 3-space, this concept is not powerful enough, all knotted loops having the same connectivity. The whole of the general theory glances off the problem, and we require the narrower but more penetrating homotopy invariants to make any impression.

But all that this example shows is that the general methods fail when applied to problems for which they are not suited. The purpose of abstract theories is to prove general existence theorems, and in this they are often successful when other methods fail. A notable example is the proof by Leray and Schauder* of existence theorems for partial differential equations of elliptic type, by means of a topological fixed point theorem for the space whose "points" are functions.

The very shortness and "readability" of many abstract proofs may disguise the difficulties that had to be overcome in discovering them. A new abstract concept is almost certain to present itself in the first place in an imperfect and hazy form, and before it can be used needs a great deal of laborious working over and trying out on examples, nothing of which appears in the final paper—indeed, in many cases the more work the less paper. But proofs of general existence theorems can be as tough, even in their final form, as any theorem in classical analysis. The proof that has recently been

* Leray and Schauder, "Topologie et equations fonctionelles," *Ann. de l'Ecole Normale*, 51 (1934), 45–78.

given by Pontrjagin of the fundamental theorem on continuous groups—that the *analytic* character of the functions defining the groups follows from the mere assumption that they are *continuous*—combines arguments from general analysis (integral equations over a continuous group), algebra (theory of group characters) and topology (theory of dimensions) into a proof which almost entirely avoids anything that could be called calculation, but yet has arguments as close and deep as anyone could wish for. The simplest cases of this theorem (e.g. for groups with two parameters) can be regarded as theorems of classical analysis, namely, that all continuous solutions of certain sets of functional equations are analytic; but classical methods are at least as unlikely to make any impression on it as the abstract methods are to provide a proof of Goldbach's theorem.

The point that has been put, perhaps at rather excessive length, in these paragraphs is merely that "classical" analysis is suited for some problems and "general" analysis for others. At the end of her article, Miss Cartwright says: "In this general and sometimes very abstract work the amount of calculation is comparatively small, and the methods are attractive; but it may be asked whether it actually does the job of solving definite problems as economically as the classical analysis." If by a "definite" problem is meant one that is too special for the general analyst to get to work on it, the answer is certainly "No"; but it is rather rough to call a steam hammer "soft" because it won't split atoms. After all, a cyclotron won't crack nuts.

. . .

Who said "I am pure, but not nearly as pure as Professor Hardy"?

. . .

D'you think the busy little bee
Finds making cells much fun,
And does it know that Pascal's rule
Applies to every one?

"ARCUS."

Instructor-Captain G. A. Clarkson, O.B.E., B.A., Royal Navy, our only outside contributor, spent the greater part of the last war in H.M.S. *Queen Elizabeth*, the Fleet Flagship. He was the Fleet Education Officer in both the Home and Mediterranean Fleets, and is now serving as Dean of the College and Professor of Navigation at the Royal Naval College, Greenwich.

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Book Review

Algebraic Solid Geometry. By S. L. GREEN, M.Sc. (London). Pp. 132. (Cambridge: at the University Press.) Price 6/-.

This book is based on courses of lectures given by the author to students of the Queen Mary College, London, reading for Pass and Special Honours Degrees in Arts, Science and Engineering. It aims at nothing more than forming a sound introduction to the subject, and fulfils this purpose admirably.

It assumes little more than a knowledge of elementary trigonometry and algebra (including determinants) and contains chapters on the plane and the Straight Line, the Sphere, the Central Quadrics, the Paraboloids and the Cone.

One criticism may perhaps be levelled at the book, and that is that in some cases the figures tend to be incomplete or inadequate. Solid figures are always potential sources of confusion, and the situation is not made easier if points are continually being mentioned in the text which are not marked on the accompanying diagram and whose position the reader has to imagine and memorise.

The value of the book is enhanced by the inclusion of a reasonable number of exercises at the end of each chapter, many questions being taken from University of London final examinations. The chapters themselves are interspersed with sets of examples, and the method of solving each one is briefly indicated below it.

Mathematical students will, of course, prefer a book which goes deeper into the subject, but for those whose courses require only a sound elementary knowledge of the subject this book is well suited. It would be particularly useful to those Part I Natural Science students whose curriculum includes that mathematical potpourri—"Mathematics as a half subject."

J. M. R.

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