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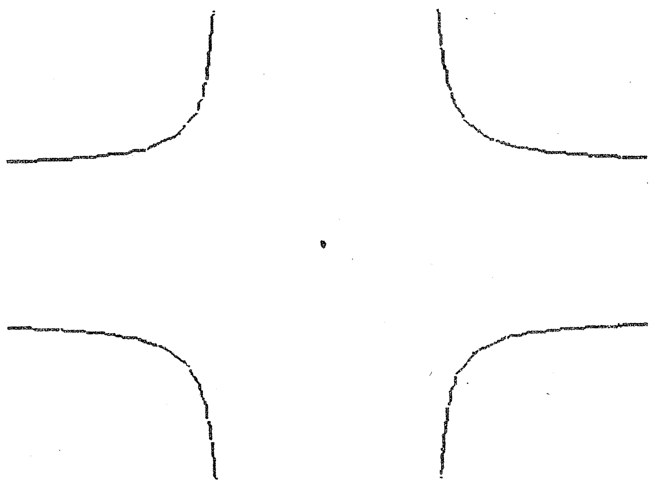

School of Mathematical Sciences – Monash University

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Function is a refereed mathematics journal produced by the School of Mathematical Sciences at Monash University. Founded in 1977 by Professor G B Preston, *Function* is addressed principally to students in the upper years of secondary schools, but also more generally to anyone who is interested in mathematics.

Function deals with mathematics in all its aspects: pure mathematics, statistics, mathematics in computing, applications of mathematics to the natural and social sciences, history of mathematics, mathematical games, careers in mathematics, and mathematics in society. The items that appear in each issue of *Function* may include articles on a broad range of mathematical topics, news items on recent mathematical advances, book reviews, problems, letters, anecdotes and cartoons.

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An Editorial and Two Corrections

Our principal article in this issue is something of a rarity. As the article itself comments, Mathematics is usually presented as a finished product: the elegant result of all the work that goes into its development. Omitted in most accounts are all the false leads and blind alleys that interrupted the solution of a problem or the elaboration of a theory.

However in his account of how he solved a (particularly difficult) Olympiad problem, Dr Kupka gives a "warts and all" description of the mental processes that eventually led him to the solution. We considered several drafts of this interesting paper, before finally settling on this one.

Part of the reason for the omission of all these interesting details from most published accounts is the simple factor of space and its conservation. This same consideration operated here. We have had to omit a lot of further detail, detail that is in fact highly relevant to a full discussion of this challenging problem. So, in a sense, this is an "open-ended" article, in that there is a lot for the reader to supply in addition to what is explicitly said.

In particular, it can become clear to a very attentive reader that Dr Kupka's proof actually does *more* than is asked. The required conclusion follows from slightly weaker assumptions than those supplied. Readers can verify this for themselves, and determine what those weaker assumptions are. A further investigation may then determine the weakest assumptions that can be imposed to ensure that the result still holds.

Sadly we have to own up to two errors in our two previous issues. Jim Cleary's article in the June issue contained a reference to the "display opposite". This should have read "display overleaf", as that is where the "Power Triangle" actually *was* displayed.

Rather more serious was the error in our August issue, in which the cover diagram was inadvertently printed upside down. In a sense, this should not matter; the shape of any geometric object is its shape is its shape is its shape. Nonetheless, its relation to the other diagrams and to the name (puptent) will be clearer if you hold the magazine upside down!

ooooooooooooooooooooooooooooo

The Front Cover

Our front cover for this issue is another curve from the list supplied by Cundy and Rollett in their book *Mathematical Models*. We showed one of them in our issue a year ago. That one was called "electrical machine", although a correspondent pointed out that it is also known as "the devil's curve". The one we use this time is called "policeman on point duty".

As with the previous case, it was produced using Maple, but the result needed some modification that was supplied by hand. The equation for the curve is

$$x^2 + y^2 = x^2 y^2$$

and it comprises five separate parts, one in each quadrant and a single point at the origin. This point is Cundy and Rollett's "policeman". If we write the equation in the form

$$y = \pm\frac{x}{\sqrt{x^2 - 1}}$$

we see at once that there are asymptotes at $x = \pm 1$, that (0, 0) is a point on the graph, but otherwise we cannot have points with x-values lying between –1 and +1. Writing the equation in the form

$$x = \pm\frac{y}{\sqrt{y^2 - 1}}$$

leads to similar conclusions in respect of the y-values.

The code that produces the picture goes:

```
>with(plots):with(plottools);
>implicitplot(x^2+y^2=(x^2)*(y^2),x=–3..3,y=3..3,
    colour=black, thickness=2,axes=none);
```

This code however omits to include the point at the origin, and so this was inserted by hand.

NOT BRILLIANT, JUST LUCKY: MY ADVENTURES WITH AN OLYMPIAD PROBLEM

Joseph Kupka, Monash University

Math Olympiad problems are hard! As an ordinary mathematical mortal, I stand in uncomprehending awe of whatever instinct, talent, or lightning-fast information processing capacity it must take for Olympiad contestants to solve such problems in the space of the one or two hours allowed in the timed examination. I feel well pleased with myself if I get one out after days of effort.

Problem 6 of the 2001 International Mathematical Olympiad has attracted much attention (see *Function*, February 2002). It reads as follows:

> Let a, b, c, d be integers with $a > b > c > d > 0$. Suppose that
>
> $$ac + bd = (b + d + a - c)(b + d - a + c). \qquad (*)$$
>
> Prove that $ab + cd$ is not prime.

This problem is "elementary" in the sense that it can be solved with very little (in fact no) specialized background knowledge. But it is nevertheless very difficult.

So difficult, in fact, that presenting a solution in the usual way can be intimidating, can unintentionally foster the I-can't-do-bloody-maths syndrome, can make the mathematician look like a mathemagician. Says he: "First you do *A*, then you do *B*, then you do *C*, ... and there's your answer!" Say I: "But why? Why did you decide to do *A* first, why did you introduce concept *X* in step *B*, why ... ?" Says he: "That's just the way it works out." Say I: "Oh, yeah!"

Oh yeah, indeed! So I am not going to present the solution in the usual way. Instead I am going to recount my exciting adventures in attempting, over some days, to find a solution. Readers may skim lightly over any mathematical details they find intimidating.

The actual process of finding the solution to a problem contrasts sharply with the cold hard logic of a finished presentation; it is more like a fishing expedition. You can romanticize it by calling it a voyage of discovery (... to seek out new theorems ... to boldly go where no mathematician ... but I guess you've heard that before). Start with the given information. Be proactive. Make deductions. Make algebraic transformations. Consider special cases. Books have been written about this. I may have read one once. Slowly put together a small encyclopedia of mathematical facts which are implied by the initial data. Some may be relevant to the stated goal, others may not be. Don't worry about this. Catch whatever fish you can. From time to time reflect on what you have caught to see if it may bring you closer to the goal. Continue to fish and reflect, continue until you give out or luck out.

My first stroke of luck, although I didn't realize it at the time, was a feeling of intimidation by Equation (*). The "obvious" thing to do is to expand the right-hand side. That would give a sum of 16 products of various pairs of *a*, *b*, *c*, *d*. Too complicated! I can't face this! I could cope better, I thought, if I paired off the terms to get an expression of the form $(A + B)(C + D) = AC + AD + BC + BD$. And there are many ways to do this. What would be good?

As a man of some experience, I am aware that $(x+y)(x-y)=x^2-y^2$. So I was able to see this form in (*) and get

$$\begin{aligned} ac+bd &= \{(b+d)+(a-c)\}\{(b+d)-(a-c)\} \\ &= (b+d)^2-(a-c)^2 \\ &= b^2+2bd+d^2-a^2+2ac-c^2. \end{aligned}$$

Now I can see an $ac + bd$ on both sides, so I subtract this and rearrange terms to get

$$ac+bd=a^2+c^2-b^2-d^2=(a+d)(a-d)+(b+c)(c-b). \qquad (1)$$

Seductive in its simplicity, this new expression looks like real progress. But after staring at it for much longer than a Math Olympiad candidate would, I finally concluded that it leads nowhere! A big zero! So put it on a shelf. It may or may not be relevant.

Now I'll explain why I am lucky. If I had multiplied the right-hand side of (*) out completely, a lot of terms would have cancelled, and I would have ended up with ... Equation (1)! That really would have been a dead end. Because I couldn't face doing this and obtained Equation (1) by pairing off terms, I am left with something to get on with: Try another pairing!

So almost at random, since I am still fishing here, I tried

$$\begin{aligned} ac+bd &= \{(a+d)+(b-c)\}\{(b+c)+(d-a)\} \\ &= (a+d)(b+c)+(a+d)(d-a)+(b+c)(b-c)+(b-c)(d-a). \end{aligned} \tag{2}$$

Wow! Is that ugly, or what? Well, maybe, but this is something I can work with. Look at the first term. We have

$$\begin{aligned} (a+d)(b+c) &= ab+ac+bd+cd \\ &= (ac+bd)+(ab+cd). \end{aligned} \tag{3}$$

If I substitute (3) into (2), I can cancel $ac + bd$ and rearrange terms to get an actual formula for $ab + cd$. That can't be a waste of time. The formula is

$$ab+cd = -(a+d)(d-a)-(b+c)(b-c)-(b-c)(d-a). \tag{4}$$

That felt good! And I can do it again.

$$\begin{aligned} (b-c)(d-a) &= bd-ab-cd+ac \\ &= (ac+bd)-(ab+cd). \end{aligned} \tag{5}$$

Substituting (5) into (2) likewise gives

$$ab+cd = (a+d)(b+c)+(a+d)(d-a)+(b+c)(b-c). \tag{6}$$

Fewer minus signs! I like this better than (4).

Now I had better remember what we are on about. We are trying to show that $ab + cd$ is not prime, which means it can be factored into the product of two smaller positive integers. Can I get this from either (4) or (6)? Scanning the right-hand side of (4) with my eagle eye, I (eventually) notice that both $d - a$ and $b - c$ appear twice. So I can factor them out to get, respectively,

$$ab+cd = -(d-a)(a+d+b-c)-(b+c)(b-c) \tag{7}$$

$$= -(b-c)(b+c+d-a)-(a+d)(d-a). \tag{8}$$

Doing the same thing to (6), in which $a + d$ and $b + c$ appear twice, gives

$$ab + cd = (a+d)(b+c+d-a) + (b+c)(b-c) \tag{9}$$
$$= (b+c)(a+d+b-c) + (a+d)(d-a). \tag{10}$$

Yow! Would you look at all those equations! Equations, equations, everywhere, but not (quite!) a factorization in sight. Which brings me to the case of Andrew Wiles, the famous Princeton mathematician who spectacularly proved Fermat's Last Theorem. He spent seven long years, largely working in secret, before he finally got it out. How did he manage to keep going for all that time without losing heart? The answer forms an important part of the creative process in mathematics.

During his 7-year "fishing expedition", Wiles produced important new mathematics which is interesting and valuable regardless of whether he had solved the problem or not. And this is the real value of Fermat's problem. It is not that the theorem itself is of such earth-shattering significance. It is, rather, that during the 350-year life of this problem, attempts at its solution produced more new and interesting mathematical results than perhaps any other single problem in history.

The situation I am facing here is a microscopic version of that faced by Wiles. As long as I can keep on making deductions, deriving new equations, whatever, then I can keep my spirits up and not get disheartened.

But I am a bit frustrated at the moment. This is an Olympiad problem, which means that it is very HARD, so it's a safe bet that there will be no explicit formula in terms of a, b, c, d which factorizes $ab + cd$. So, like it or not, I am going to have to play a little game of mathematical make-believe. If Equation (7), say, is not going to hand me a factorization on a platter, maybe I can get one by brute force.

So let us make-believe, or, as we usually prefer to say, let us *suppose* that there is a prime number p which divides each of the two big terms on the right-hand side of (7). (We say that *p divides* an integer n, and write $p|n$, if the ratio n/p is an integer and not just a fraction, e.g. $3|6$ because $6/3 = 2$, an integer.) Oh, no! Too complicated! I can't face this! So I shall suppose instead that $p|(d - a)$ and that $p|(b + c)$. So $d - a$ equals an integer times p, and so does $b + c$. But this means that I can factor p out of the entire right-hand side of (7), leaving a complicated expression which will be an integer, not a fraction. But the right-hand side of (7) is just $ab + cd$, so I can factor p out of this as well, which means that $ab + cd$ can be factored and so is not prime.

Have I solved the problem? Not quite! Remember that we are playing make-believe here. IF, by the merest chance, there happens to be a prime p which divides both $d - a$ and $b + c$, THEN $ab + cd$ is not prime. So I have solved a *special case* of the problem. Which means that in my further deliberations, I am allowed to assume that there is NO prime number p which divides both $d - a$ and $b + c$, or, as we say, that $d - a$ and $b + c$ are *relatively prime*. So I have caught another "fish" on my fishing expedition.

And I am especially pleased about this because exactly the same line of reasoning, applied to any of Equations (7), (8), (9), (10), will give me quite a large number of pairs of integers which I can assume are relatively prime. For example, (8) allows me to assume also that $a + d$ and $b - c$ are relatively prime. As usual, I have no idea as yet whether any of these facts will help me to a complete solution of the problem, but they do energize me to keep going.

What next? Well, just more of the same, more deductions, more equations. I have four different formulas for $ab + cd$. So I can certainly equate any two of these, and simplify, to see if something interesting pops out. Which pair to choose? I chose (7) and (10). Why? The reason is very unprofound. When I first thought to do this, I only had (7) and (10) down on paper. I had as yet to get (8) and (9). So I equated the right-hand sides of (7) and (10), I combined the terms with $d - a$ in them, and I combined the terms with $b + c$ in them to get

$$-(b+c)\big(2(b-c)+a+d\big)=(d-a)\big(2(a+d)+b-c\big). \qquad (11)$$

As I wrote this down, my mind was full of thoughts about one integer dividing another, so I immediately observed that $b + c$ divides the left-hand side of (11), hence it divides the right-hand side. But $b+c$ and $d-a$ are relatively prime. So $b + c$ divides $2(a + d) + b - c$. This is exciting! Why? Well, not because it solves the problem, but simply because it is a non-obvious deduction from the original assumptions. Mathematicians like to make non-obvious deductions. It makes them feel powerful.

Ah, but when I get excited I tend to make mistakes, so I had better check my deduction carefully. Firstly, the deduction would be nonsense if $d - a = 0$. But $d \neq a$ by assumption; in fact, $d - a$ is negative. So that's OK. Now every integer bigger than 1, such as $b + c$, may be factored into primes p. Take any such p. It divides the left-hand and hence also the right-hand side of (11). But we know already that p does *not* divide $d - a$. So it divides $2(a + d) + b - c$. This is a basic fact about primes. Now divide both sides of (11) by p, and then take another prime factor q of $b + c$, if there is such a prime. (We could have $q = p$, as, for example, if $b + c = 3^5$.) Again, q does not divide $d - a$, so it must divide what is left of $2(a + d) + b - c$ after division by p. If I keep going like this until I have used up all the prime factors of $b + c$, I see that I will get

$$2(b - c) + a + d = (d - a) \times (\text{an integer}).$$

But this *integer* is nothing other than $2(a + d) + b - c$ divided by $b + c$, which means that $b + c$ divides $2(a + d) + b - c$. OK. I guess that's all right. There is probably some big theorem somewhere which I could have used to get all this in one step.

I could equally well have deduced that $d - a$ divides $2(b - c) + a + d$. Why didn't I? Just didn't think of it. Instead, propelled as much by excitement as by logic, I chose to fiddle with (7) to get (8), to fiddle with (10) to get (9), and then to equate the right-hand sides of (8) and (9). Then, in an argument entirely similar to the above, assuming that $a + d$ and $b - c$ are relatively prime, and using the fact that $b \neq c$, I deduced that $a + d$ divides $2(b + c) + d - a$.

So many fish I am catching! My head is spinning. Confusion looms. I need to get my feet back on the ground. So I decide to be as explicit as possible about what I have deduced. I have deduced that there exist positive integers q_1 and q_2 such that

$$2(a + d) + b - c = q_1(b + c) \qquad (12)$$

$$2(b + c) + d - a = q_2(a + d). \qquad (13)$$

This is helpful. Because the minute I wrote down (12) and (13) explicitly, I was seized by a mad impulse. Why not multiply the right-hand sides of (12) and (13) together, and also the left-hand sides, to get one enormous equation? Why not? Because it would be crazy, that's why! What could one hope to get from the product of the left-hand sides apart from an unholy mess? I may as well put on my Crazy John costume and go jigging around trying to sell discount fridges.

If I were playing the role of mathemagician at this point, you would not be hearing any of this. Instead, armed with the wisdom of hindsight, I would loftily announce that if "we" (does it take two of us?) were to eliminate the 2 from the left-hand side of (12), we would get the first term in the factorization (*), and the second term in (*) comes likewise from (13). Therefore, "as the reader may easily check", the product of these two sides "clearly" reduces to just $3(a + d)(b + c)$.

But that just ain't the way it was! I was Crazy John, beset by confusion and uncertainty, I had a mad impulse, I acted on it, and the result took me completely by surprise. Here it is.

$$
\begin{aligned}
q_1q_2(a+d)(b+c) &= \{2(a+d)+b-c\}\{2(b+c)+d-a\} \\
&= 4(a+d)(b+c)+2(a+d)(d-a)+2(b+c)(b-c)+(b-c)(d-a) \\
&= 3(a+d)(b+c) \\
&\quad +\{(a+d)(b+c)+(a+d)(d-a)+(b+c)(b-c)+(b-c)(d-a)\} \\
&\quad -\{(a+d)(a-d)+(b+c)(c-b)\}.
\end{aligned}
$$

I was guided in these expansions by the very first "fish" I had caught, namely the "useless" Equation (1), and Equation (2). These equations tell me that the last two lines above just equal $[ac + bd] - [ac + bd] = 0$. I subtract $3(a + d)(b + c)$ from both sides to get

$$(q_1q_2 - 3)(a+d)(b+c) = 0.$$

Well, I'll be damned!

This was the moment of truth, the light at the end of the tunnel, and all that stuff. From now on I can behave the way a mathematician is supposed to behave: cold, logical, precise. So let's see: $a + d$ is positive, $b+c$ is positive, so q_1q_2-3 must be zero, or $q_1q_2=3$. Since these factors are positive *integers*, either $q_1=1$, $q_2=3$ or $q_1=3$, $q_2=1$. If $q_1=1$, then (12) gives

$$2(a + d) + b - c = b + c, \text{ or } a + d = c,$$

which contradicts the initial hypothesis $a > b > c > d > 0$. If $q_2=1$, then (13) gives

$$2(b + c) + d - a = a + d, \text{ or } b + c = a,$$

which does *not* contradict the initial hypothesis. But we also have $q_1 = 3$, so (12) gives

$$2(a+d)+b-c=3(b+c),\ \text{which implies that } a+d=b+2c.$$

Substituting $b + c$ for a in this last equality gives $d = c$, again contrary to hypothesis. Yow! That's it! Fist in the air! Done!

I guess I had better explain this little outburst. Earlier in my journey I assumed that both pairs $b + c$, $d - a$ and $a + d$, $b - c$ were relatively prime. From this assumption I (eventually) deduced that either $a+d=c$ or $d=c$. Both of these assertions are *false* (by assumption). Therefore the rules of logic dictate that my original assumption must be false. Therefore at least one of the two pairs above must have a common prime factor p. But I have already satisfied myself that this would imply that $ab + cd$ is not prime. Q.E.D.

Wow! Am I brilliant, or what? I don't think so. Equations (7)-(10) were a real goldmine, or minefield, depending on your point of view. So many possibilities for relatively prime pairs of integers! So many possible further equations! So many possibilities for one integer dividing another! The "fish" were almost leaping out of the water. I had to make choices, almost at random. I acted on a Crazy John impulse coming from I know not where. I could equally well have made other choices. What then? Who can say? Perhaps a different solution. Perhaps a fruitless wandering in the wilderness. This last has certainly happened to me! So: not brilliant, just lucky. And I had better not get too cocky either, because my luck could be just about to run out.

HISTORY OF MATHEMATICS

The Strange Case of Q A M M Yahya

Michael A B Deakin, Monash University

Perhaps I should preface this article with a disclaimer. It was already in an advanced state of preparation well before recent events at

Monash made it particularly topical. I briefly considered using some other material, but this would have made for difficult pressures of time, so I did not pursue this idea, but stuck to my original plan. It is purely coincidental that it results in this material being discussed at this particular time.

First, some background. Mathematical research is, for the most part, disseminated by means of publication in specialist journals. Before it sees print, however, it is assessed by an acknowledged expert, who recommends a decision to the editor. The assessor, or referee, usually concentrates on three questions in reaching a conclusion on whether to recommend publication or not. Of a submitted piece of work, the referee judges it on these three criteria: Is it true? Is it new? Is it interesting?

If all three questions are answered in the affirmative, then publication is recommended, otherwise not.

Of course, referees make mistakes from time to time. Sometimes work appears in print although it is incomplete or incorrect. (A "proof" of the 4-colour theorem was in print and accepted as correct for 35 years before it was found to be faulty.) Sometimes a researcher, unaware of an earlier result, inadvertently duplicates something already done. This is quite common because not all published work is widely available. I devoted my column for August 1997 to just this question. The third criterion, because it is subjective (and even subject to fashion), is often the most controversial of the three, and referees will differ on this, as will editors.

Published contributions to Mathematics are summarised in journals of review, of which the best known and most widely used is the US *Mathematical Reviews*. We can usually follow a mathematician's career by checking the listing in *Mathematical Reviews*. This may now be done online via

http://www.ams.org/authorlookup

(although the present form of this website is not as helpful as the one it replaced). Each review gives the bibliographic details of the relevant article. It is usual to include a brief description of its contents, and sometimes the reviewer also includes a few words of appraisal or comment.

Quite early in my own academic career, a colleague told me of the "exploits" of one Q A M M Yahya, whose papers seemed to replicate minor and earlier papers in somewhat obscure journals. A rumor mill had it that the seemingly improbable name was a pseudonym adopted by a consortium of disgruntled mathematicians whose aim was to discredit the refereeing process, and that it meant "Q A M M [*Quarterly of Applied Mathematics and Mechanics*]: Yah yah!".

My own later adventures with this author (detailed below) led me to doubt this interpretation, and very much later, in reading up the background for this article, I came to adopt the different view given here.

I began my search for the true story of this mathematician by looking up all his appearance in *Mathematical Reviews*. There are 13 of them. I will take them in order.

The first is a privately circulated pamphlet entitled *Complete Proof of Fermat's Last Theorem* and dated 1958. I have never seen this work but almost certainly the purported proof was fallacious. One presumes that Yahya himself sent the work to *Mathematical Reviews*; it is most unlikely that they would otherwise ever have seen it. No review was given, only the title. At this stage, Yahya's address was: Pakistan Air Force, Kohlat, West Pakistan. [West Pakistan was the name then given to what is now Pakistan; East Pakistan was what has since become the separate nation of BanglaDesh.]

Nothing more was heard of Yahya until 1963. In that year he published an article on "Leptonic decays in finite space". This appeared in the respected Italian journal *Nuovo Cimento*. This excursion into Theoretical Physics was a new development. There was also a new address: Department of Physics, Imperial College, London, but it was noted that the author was visiting from Dacca (modern Dhaka) in East Pakistan (under a travelling scholarship from the Colombo Plan for Commonwealth co-operation).

Perhaps it was the new address or else the change of subject matter or even both that gave rise to the rumor that there were several people using the name, but it seems simpler to accept the stated facts at their face value. It would not be unusual for a young researcher to follow a flawed if ambitious youthful effort with a more mature investigation in a quite different field.

This time *Mathematical Reviews* offered no independent account of the work, but merely reprinted a part of the author's own summary. (This is by no means unusual; I have resorted to this same practice in a couple of my own reviews for them.)

The fourth contribution was also a paper in Theoretical Physics, reviewed reasonably favourably and still from the London address. This too appeared in *Nuovo Cimento*.

Between them came a third paper published in a rather obscure Latin American journal, and returning to the area of Number Theory. The result was a (rather distant) relative of the famous Goldbach Conjecture (which states that all even numbers, apart from 2, may be written as the sum of two primes). Here the reviewer noted two related papers, and seems to indicate that Yahya referred to these. I cannot be sure on this point as the paper in question is not available to me, and so I am also uncertain as to why the reviewer raised the matter.

The fifth paper was still published from the Imperial College address, and this time it appeared in the *American Mathematical Monthly* as a short note. It concerned a standard inequality, which it offered in a somewhat strengthened form. One year later, the editor noted that the result, along with all the intermediate results used in its proof had already appeared in print, derived by a Chinese author writing in a Scandinavian journal. This time there was a distinct implication that all was not kosher.

It was Yahya's next effort that led to my "almost involvement" with this story. In 1968, I became the assistant editor of the *Journal of the Australian Mathematical Society*, then edited by Gordon Preston, who later founded *Function*. I was aghast to learn that, two years earlier, this journal had published a paper by Yahya, and passed this concern on to Gordon. However, it appeared that he had had misgivings about the paper himself, but that these had been allayed by the referee.

The referee pointed out that the result in question was known: in fact it was a special case of a more general theorem. However, the method of proof was in his opinion new. This was the basis of the recommendation to publish. It was in fact quite a nice little piece of work whose flavor I can give here.

If $f(x)$ is a function satisfying certain constraints, then we have a result known as Taylor's Theorem, which states that

$$f(x+a) = f(a) + x\frac{d}{da}f(a) + \frac{x^2}{2!}\left(\frac{d}{da}\right)^2 f(a) + \frac{x^3}{3!}\left(\frac{d}{da}\right)^3 f(a) + \dots .$$

It is a convention going back at least to Cauchy in the early 19th century, and possibly even earlier, to "factor out" an *f*(*a*) from the right-hand side and to write this expression as

$$\left(1 + xD + \frac{1}{2!}x^2D^2 + \frac{1}{3!}x^3D^3 + \dots\right)f(a),$$

where D is a shorthand for $\dfrac{d}{da}$, and then to go on to abbreviate the series still further by noting that the expression inside the parentheses is a formal expansion of the exponential, so that we end up expressing Taylor's Theorem as

$$f(x+a) = \exp(xD)f(a). \qquad (1)$$

Yahya took as his starting point the consideration that

$$\frac{d}{da}f(a) = \lim_{h\to 0}\frac{f(a+h)-f(a)}{h} = \lim_{h\to 0}\frac{\Delta_h f(a)}{h},$$

where the last expression defines the new term employed.

We thus have symbolically, $D = \lim_{h\to 0}\frac{\Delta_h}{h}$, and we may substitute this into Equation (1). This gives

$$f(x+a) = \exp\left(x\lim_{h\to 0}\frac{\Delta_h}{h}\right)f(a).$$

What Yahya sought to do was to take the limit at a different point in the calculation, and so to show that

$$f(x+a) = \lim_{h\to 0}\exp\left(x\frac{\Delta_h}{h}\right)f(a). \qquad (2)$$

My description so far has been intuitive rather than precise. Yahya showed that Equation (2) could be more carefully stated as a proposition about the convergence of series. He went on to prove this proposition and to note some consequences for the theory of the Poisson distribution in Statistics. Quite a nice paper, if not earth-shattering, and apparently original. Yahya's address was given at this time as "University of Dacca, East Pakistan".

This was Yahya's sixth paper. His seventh was a brief note discussing a property of some special functions known as Tchebychev polynomials. It was published in *Portugaliae Mathematicae* (as its name implies, a Portuguese journal). This journal then enjoyed quite a high reputation (indeed one of my own papers appeared there about this time). Again, it would seem that the result was original if only mildly interesting.

After this, however, things started to go badly wrong. Two related papers on Theoretical Physics appeared in a quite obscure journal, whose name would seem to be *Journal of Natural Science and Mathematics*. Both were reviewed by the same reviewer, who was extremely negative. His review of the first was long but hostile. A single quote will give the flavor: "In the fourth paragraph he mentions the known fact that .... He does not relate this fact to the rest of the paper."

And if that sounds bad, the review of the ninth paper was even more damning. I quote it almost in full, omitting only a few technical details. "The theorem and proof in this paper resemble Theorem 1.1 and the proof in an earlier paper by R. Prosser .... In fact, in the statement of the theorem the only difference is that the author uses S, T, A and D where Prosser uses A, B, U and $\mathcal{D}$. Taking into account the different notation used in the statement of the theorem, the author and Prosser use exactly the same equations in the proof with the author's equation (n) corresponding to Prosser's equation ($1.n$), $n = 3, 4, 5, ..., 9$. Prosser's paper is not listed in the author's bibliography."

In other words, Yahya had been caught out again in a blatant piece of academic fraud. He had copied Prosser's results, gone to some lengths to hide the fact and had passed off the work as his own. This is what is known in academic circles as plagiarism, and it is rightly condemned.

The final four papers all appeared in *Portugaliae Mathematicae*, whose standards would seem to have been slipping by this time. Paper number ten was a brief note on the roots of polynomial equations. The last three all contained purported proofs of Fermat's Last Theorem. All were shown in short order to be incorrect. These late papers came from Pakistani addresses, three from Dacca, one from Karachi. A further proof of Fermat's Last Theorem was promised in the final paper, but, as far as I know, it has never appeared.

The picture I form of Yahya is one of a moderately gifted worker, whose ambition outstripped his abilities, and who (for whatever reasons) resorted to unfair means in order to exaggerate his accomplishments.

Bibliographic Note

I earlier remarked that the present form of the website is less useful than a previous one. It is now more difficult to find the full list of Yahya's publications. Accordingly, I list the references in *Mathematical Reviews* for the benefit of readers who want to look further into this strange case. They are, in order: (1) 19,713c, (2) 27 #4576, (3) 29 #52, (4) 31 #4440, (5) 32 #4235, (6) 33 #7727, (7) 35 #3108, (8) 39 #1187, (9) 39 #1188, (10) 41 #3444, (11) 51 #10220, (12) 56 #252, (13) 80f:10018.

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COMPUTERS AND COMPUTING

Solving Non-Linear Equations: Part 2, Graphical Methods

J C Lattanzio, Monash University

Of the five methods I foreshadowed in my previous paper for the solution of a non-linear equation $f(x)=0$, the simplest (although the least accurate) is to sketch a graph of $f(x)$ over a range of values of x over which this function changes sign. It is usually helpful also to determine the turning points and points of inflection.

First consider a relatively simple example:

$$f(x)=x^3-3x^2-9x-6=0.$$

To find the turning points (maxima and minima), set the derivative of the left-hand side equal to zero. This gives

$$f'(x) = 3x^2 - 6x - 9 = 0,$$

which implies that $x = -1$ or 3.

These two values of x correspond to values of $f(x)$ of -1 and -33 respectively.

If we now proceed to the determination of the points of inflection, we set

$$f''(x) = 6x - 6 = 0,$$

so there is exactly one point of inflection at $x = 1$. We may note that $f(1) = -17$.

These calculations give us enough information to sketch the graph of $f(x)$, and this gives rise to Figure 1.

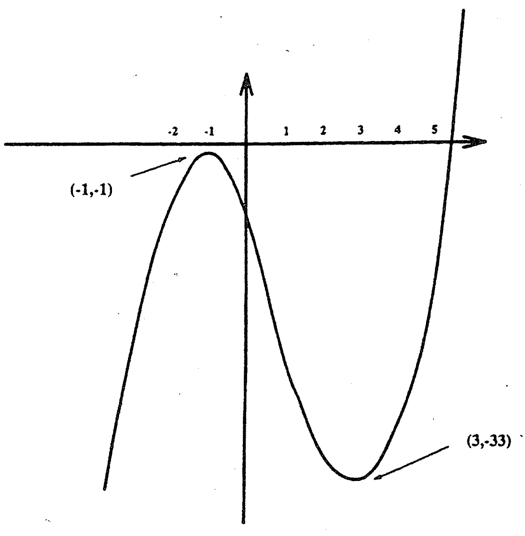

Figure 1: A sketch-graph of $y = f(x) = x^3 - 3x^2 - 9x - 6$.

From the sketch-graph, it is clear that the equation $f(x)=0$ has exactly one real root and that it occurs near $x = 5$.

Next consider a somewhat more challenging example:

$$f(x) = \tfrac{1}{2}x - \sin(x) = 0\,.$$

In this case, I leave the details to the reader, but the graphs of $\sin(x)$ and $\frac{1}{2}x$ have been superimposed in Figure 2.

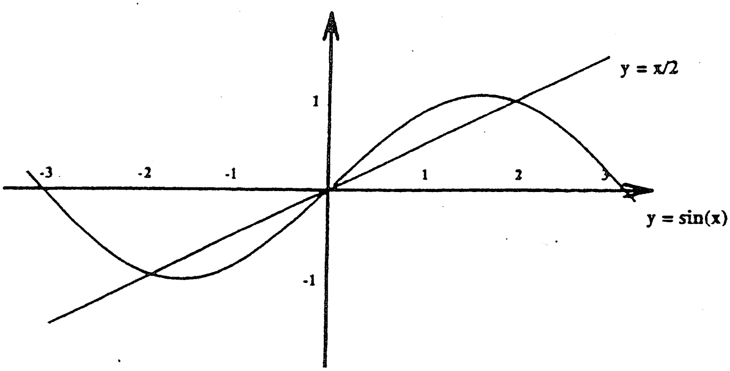

Figure 2: Sketch-graphs of $y = \frac{1}{2}x$ and $y = \sin(x)$.

From this graph, it is clear that this equation has three (real) roots, lying at $x = 0$ and $\pm X$, where $X \approx 2$.

The reader will note that the graphical approach tends to give information of a qualitative nature, rather than giving precise estimates of the roots. It is very useful for telling us how many roots there are, and even their (very) approximate locations. In order to obtain better estimates, we need to resort to one or another of the numerical methods I briefly described in my last article and which will form the subject-matter of those that follow.

OLYMPIAD NEWS

Hans Lausch, Monash University

Four Medals, including Gold, at 2002 International Mathematical Olympiad

Stewart Wilcox of North Sydney Boys High School has won a Gold Medal representing Australia at the 2002 International Mathematical Olympiad, held in Glasgow.

Australian students were successful in winning a total of 1 Gold Medal, 2 Silver Medals, 1 Bronze Medal and an Honorable Mention..

Stewart scored 31 points of a possible 42, achieving 4 complete solutions, and was placed equal 19th, among 479 of the world's elite students from 84 countries.

In the IMO Gold Medals are awarded to students placed in the top one-twelfth of students. In the history of our participation in these Olympiads, Stewart is only the ninth Australian to win a Gold Medal. He received it from Princess Anne, the Princess Royal, at the IMO closing ceremony.

The two Silver medallists were Nicholas Sheridan (23 points) of Scotch College, Melbourne, and David Chan (23 points) of Sydney Grammar School. Yiying (Sally) Zhao (15 points) of Penleigh and Essendon Grammar School, Melbourne, won a Bronze Medal, while Gareth White (13 points) of Hurlstone Agricultural School, Sydney, earned an Honorable Mention for a complete solution. He and Andrew Kwok (12 points) of University High School, Melbourne, both narrowly missed Bronze medals, being within 1 point and 2 points respectively.

Australia's total score was 117, placing it in 26th position of the 84 countries.

China was placed first, Russia second and the USA third. Three perfect scores were achieved, two from China, one from Russia. The United Kingdom, with 116 points, finished in 27th position.

The Mathematics and Informatics Olympiads are both administered in Australia by the Australian Mathematics Trust and receive support from the Federal Government through the Department of Education, Science and Training. Participation in the IMO is also supported by the Australian Association of Mathematics Teachers and the Australian Mathematical Society.

In the IMO, each country is represented by a team of no more than six competitors. They sit two separate papers, presented on two separate days, and each comprising 3 questions worth 7 points each. A time of $4\frac{1}{2}$ hours is allowed for each paper.

Here are the questions set.

1. Let n be a positive integer. Each point (x, y) in the plane, where x and y are non-negative integers with $x + y < n$, is coloured red or blue subject to the following condition: if a point (x, y) is red, then so are all points (x', y') with $x' \leq x$ and $y' \leq y$. Let A be the number of ways to choose n blue points with distinct x-coordinates, and let B be the number of ways to choose n blue points with distinct y-coordinates. Prove that $A = B$.

2. The circle S has centre O, and BC is a diameter of S. Let A be a point of S such that $\angle AOB < 120^\circ$. Let D be the midpoint of the arc AB which does not contain C. The line through O parallel to DA meets the line AC at I. The perpendicular bisector of OA meets S at E and at F. Prove that I is the incentre of the triangle CEF.

3. Find all pairs of positive integers $m, n \geq 3$ for which there exist infinitely many positive integers a such that

$$\frac{a^m + a - 1}{a^n + a^2 - 1}$$

is an integer.

4. Let n be an integer greater than 1. The positive divisors of n are $d_1, d_2, \ldots, d_k$ where $1 = d_1 < d_2 < \ldots < d_k = n$. Define

$$D = d_1 d_2 + d_2 d_3 + \ldots + d_{k-1} d_k .$$

(a) Prove that $D < n^2$.
(b) Determine all n for which D is a divisor of n^2.

5. Find all functions f from the set **R** of real numbers to itself such that

$$\big(f(x) + f(z)\big)\big(f(y) + f(t)\big) = f(xy - zt) + f(xt + yz)$$

for all x, y, z, t in **R**.

6. Let $\Gamma_1, \Gamma_2, \ldots, \Gamma_n$ be circles of radius 1 in the plane, where $n \geq 3$. Denote their centres by $O_1, O_2, \ldots, O_n$ respectively. Suppose that no line meets more than two of the circles. Prove that

$$\sum_{1 \leq i < j \leq n} \frac{1}{O_i O_j} \leq \frac{(n-1)\pi}{4} .$$

The 2002 Senior Contest of the Australian Mathematical Olympiad Committee (AMOC)

The AMOC Senior Contest is the first hurdle for mathematically talented Australian students who wish to qualify for membership of the team that represents Australia in the following year's International Mathematical Olympiad. This year about seventy students took part in that four-hour competition on 13 August.

Here are the questions (each worth seven points).

1. Find all solutions of the following system of equations:

$$\frac{x_1}{x_1+1}=\frac{x_2}{x_2+3}=\frac{x_3}{x_3+5}=\ldots=\frac{x_{1001}}{x_{1001}+2001}$$

$$x_1+x_2+\ldots+x_{1001}=2002.$$

2. Determine all positive integers x, y that satisfy

$$x!+24=y^2.$$

(If n is a positive integer, then $n!=n(n-1)(n-2)\ldots.1.$)

3. For each pair (k,l) of integers, determine all infinite sequences of integers $a_1, a_2, a_3, \ldots$ in which the sum of every 28 consecutive numbers equals k and the sum of every 15 consecutive numbers equals l.

4. Determine all functions f that have the properties:

(i) f is defined for all real numbers,

(ii) $|f(x)|\le 2002\le\left|\frac{xf(y)-yf(x)}{x-y}\right|$ for all x, y with $x\ne y$.

5. Let ABC be a triangle and D the midpoint of BC. Suppose $\angle BAD=\angle ACB$ and $\angle DAC=15°$. Determine $\angle ACB$.

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NEWS ITEMS

(1) Catalan's conjecture

Yet another long outstanding mathematical conjecture now seems to have been proved. Like the better-known Fermat's Last Theorem, it is a result in Number Theory, and it is easily stated (but, obviously, far from easy to prove). It states that 8 $(=2^3)$ and 9 $(=3^2)$ are the only consecutive prime powers. That is to say that the equation in positive integers

$$p^m - q^n = \pm 1$$

has no other solutions for m, n greater than 1. This conjecture was first stated by the Belgian mathematician Eugène Catalan in 1844. Even back then, some special cases were known. For example, Euler had already proved that the special case

$$2^m - 3^n = \pm 1$$

has no solutions other than $(m, n) = (3, 2)$.

Since Catalan's time, there have been numerous attacks on the problem, and considerable progress was made. It was even shown that only a finite number of possible solutions could exist, and bounds were placed on the maximum size of any such. These bounds remained too large for a computer search to be a practical proposition, however. In 1994, the number theorist Paul Ribenboim produced an entire book devoted to the conjecture, and this listed the progress made to that date.

Even the fact that there were only finitely many possible cases was not proved until the work of Robert Tijdeman in 1976. Now however Preda Mihailescu, a mathematician at the University of Paderborn in Germany, has recently announced a proof of this long-standing conjecture. He was able to extend and refine earlier partial results to the point where he was able to claim a proof of the conjecture.

Mihailescu sent his proof to several mathematicians on 18 April this year, and on 24 May he presented it publicly to a meeting of the Canadian Number Theory Association in Montreal. He is now preparing a paper for publication. Of course until the wider mathematical community has had the opportunity to check the proof, we should withhold final judgement, but many eminent number theorists have now examined the proof and express themselves satisfied.

For more detail, see the website

http://mathworld.wolfram.com/CatalansConjecture.html

which includes a useful reference list.

2002 Fields Medals

Every four years, an International Congress of Mathematicians is held and it is at that congress that the award of the Fields medals is announced. The Fields medal is regarded as Mathematics' equivalent of the Nobel Prize. (See *Function*, *Vol* **11**, *Part* 2.) The congress for the year 2002 was held last August in Beijing, and two medals were awarded. They went to Laurent Laforgue of the Institut des Hautes Études in France and to Vladimir Voevodsky, originally of Russia, but now working at the Princeton Institute for Advanced Study in the US.

Laforgue received his award for his contributions to the so-called Langlands Program. This is a set of interconnected conjectures whose solution would demonstrate deep connections between Algebra and Analysis. The Langlands Program has been described as the Grand Unified Theory of Mathematics, after the similarly ambitious attempts to unify Theoretical Physics. The conjectures fall into three subsets, and Laforgue proved all of those comprising one subset. As those of another subset were already proved, this now leaves only one set to go.

Voevodsky received his medal for his proof of the Milnor Conjecture. This had been outstanding for over 30 years and had proved to be very difficult. It lies within a branch of Mathematics known as algebraic K-theory, which in turn derives ultimately from Topology.

Because both this and the Langlands Program are highly technical, it is not feasible to give more detailed descriptions here. However readers wishing to know more on this matter could look up the websites

http://mathworld.wolfram.com/news/2002-08-21_fields/

and

http://www.ams.org/ams/fields2002-background.html

These also give details of further reading.

Primality is P

One of the major areas of current research is the study of computational complexity. As the numbers involved in a computation increase in size, how does this affect the amount of computation needed? For many important computations, the amount of time required increases exponentially as the numbers involved get bigger. The consequence is that as these numbers grow, the computation soon becomes impractically large.

For more on this matter, see *Function*, *Vol* **4**, *Part* **3**. If the size of the computation can be made to increase at a less formidable rate, the computation is said to be of P-class (where P stands for "polynomial", i.e. a function increasing less than exponentially). A problem is of P-class, if the running time is bounded by some power of the problem's size.

The standard method for testing whether a number n is prime or not is known as the Sieve of Eratosthenes. (For an account, see another early issue of *Function*: April 1982.) This actually does more than test whether n is prime or not, for it works by discovering a factor in the event that n is not prime. Its drawback is that the computational times increase exponentially with n.

It began to be suspected however that it might be possible to test whether n was prime or not without actually finding one of its factors and that such a process might be of P-class. This has recently been shown to be the case. Maninda Agrawal, of the Indian Institute of Technology, Kanpur, and two of his undergraduate students, Neeraj Kayal and Nitin Saxtena, have come up with a surprisingly simple algorithm that does just that. The running time is proportional to at most $(\ln n)^{12}$, and may be even lower.

One of the real surprises was the relative simplicity of the algorithm when it was found. It was a fresh approach and far from easy to discover. What it did was to ask a whole sequence of simpler questions about n. If all of a sequence of equalities hold, then n is prime;

otherwise it is not. The algorithm to test for primality is only 13 lines long. Here it is.

Input: integer $n > 1$

```
if (n is of the form a^b, b > 1) output COMPOSITE
r = 2
while(r < n)
    if (gcd(n,r) ≠ 1) output COMPOSITE
    if (r is prime)
        let q be the largest prime factor of r − 1;
        if (q ≥ 4√r log n) and (n^((r−1)/q) ≠ 1(mod r))
            break
    r ← r + 1
}
for a = 1 to 2√r log n
    if ((x − a)^n ≠ (x^n − a)(mod x^(r−1), n) output COMPOSITE
output PRIME
```

Although primality is involved with code-breaking the new algorithm has no direct or immediate implications in this area. However, because such a simple solution was found to so apparently difficult a problem, there is now the worry that some clever piece of "lateral thinking" may mean that codes thought to be secure might not be so safe.

Again a word of caution is in order. The new algorithm has not yet been formally published. It is however posted on the web at

www.cse.iitk.ac.in/primality.pdf

and has been widely and expertly scrutinised. It would seem to have passed muster. For more detail, see *New Scientist*, 17/8/2002, p 9 or the website

http://mathworld.wolfram.com/news/2002-08-07_primetest/

and the references given there.

HRT and Breast Cancer

Recently a lot of publicity was given to a large American study that showed increased risk of breast cancer arising from certain types of hormone replacement therapy (HRT) among post-menopausal women. In particular administration of both Estrogen and Progestin (E+P) seemed to increase to risk of invasive breast cancer (and also gave evidence of other risks and fewer of the hoped-for benefits).

The study was reported in *JAMA* [*Journal of the American Medical Association*] for August 17, 2002. It followed the progress of 8506 women receiving the E+P and 8102 "controls" receiving placebos. On average, each woman spent 5.2 years of the assigned treatment, before the study was stopped on ethical grounds. The E+P group developed 26% more cancers than did the controls.

The method of reporting these results drew a critical response from the *Age* columnist Ross Gittins (7/8/'02), who pointed out that the risk, even for those receiving the E+P remained low (under 2%). The study reported 38 cases of breast cancer per 10000 woman-years among the E+P group as against 30 among the controls.

Rather more serious is the besetting sin of much medical research of neglecting to publish the raw data. We are never told how many cases of breast cancer were actually found. From the figures given, we may estimate that there were 168 cases among the E+P group and 126.4 (!) among the controls. This last figure is impossible of course; probably there were 126 cases, and the reported results were rounded. All the same it would be nice to know.

Failure to report raw data means that the data-set is often unavailable for analysis directed to other purposes.

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PROBLEMS AND SOLUTIONS

Yet more on Problem 25.2.3

Problem 25.2.3 involved a geometric construction. It was discussed at length in our February and April issues for this year. We acknowledge receipt of a further analysis from our regular correspondent Keith Anker. It gives a new approach to this difficult problem, but it does not contradict the analysis we published last April. Moreover it is quite long. It has therefore been decided not to run it. Correspondence on this problem is now closed.

We now turn to the problems posed in that same issue.

Solution to Problem 26.2.1

This problem was the second of three "Professor Cherry problems" drawn from Todhunter's *Algebra*. It asked for a proof of the identity

$$\left\{(a-b)^2+(b-c)^2+(c-a)^2\right\}^2=2\left\{(a-b)^4+(b-c)^4+(c-a)^4\right\}.$$

Solutions were received from Keith Anker, Šefket Arslangić (Bosnia), J C Barton, J A Deakin, Julius Guest, Carlos Victor (Brazil) and Colin Wilson. Most of these began with a change of notation. Set $x=a-b,\ y=b-c,\ z=c-a.$ Then $x+y+z=0$, and we are to prove the identity

$$\left(x^2+y^2+z^2\right)^2=2\left(x^4+y^4+z^4\right)$$

under this condition. From here on, we follow Wilson's solution, although the others who took this route came up with very similar analyses.

Form the expression $2\left(x^4+y^4+z^4\right)-\left(x^2+y^2+z^2\right)^2$. Expand this and simplify it to find $x^4+y^4+z^4-2\left(x^2y^2+y^2z^2+z^2x^2\right)$ and now combine the terms differently to get

$$\begin{aligned}
x^4+y^4+z^4&-2\left(x^2y^2+y^2z^2+z^2x^2\right)\\
&=\left(x^4-2x^2y^2+y^4\right)-\left(2x^2+2y^2\right)z^2+z^4\\
&=\left(x^2-y^2\right)^2-\left[(x+y)^2+(x-y)^2\right]z^2+z^4\\
&=\left\{(x+y)^2-z^2\right\}\left\{(x-y)^2-z^2\right\}\\
&=(x+y+z)(x+y-z)(x-y+z)(x-y-z).
\end{aligned}$$

This last expression is now zero because of the condition on the variables *x*, *y*, *z*. The result is therefore proved.

A solution that proceeded along rather different lines was Barton's. This used a more sophisticated argument involving cyclic symmetry. It may be deduced from such considerations that if we expand over the original variables, then a lot of terms cancel. This allows a neat proof.

Solution to Problem 26.2.2

This problem asked for a proof that for real x, y, z, w

$$x^4 + y^4 + z^4 + w^4 \geq 4xyzw$$

and a list of all cases for which equality holds.

Again solutions were received from Keith Anker, Šefket Arslangić (Bosnia), J C Barton, J A Deakin, Julius Guest, Carlos Victor (Brazil) – 2 separate solutions – and Colin Wilson. Several of these solutions pointed out that the required result is a straightforward consequence of the inequality of the arithmetic and the geometric means. (See *Function, Volume* **8**, *Part* **1**, p 15 and elsewhere.) Others offered various proofs from first principles. Here is Deakin's.

Since $\left(x^2 - y^2\right)^2 = x^4 - 2x^2y^2 + y^4 \geq 0,\ x^4 + y^4 \geq 2x^2y^2$.

Similarly $z^4 + w^4 \geq 2z^2w^2$.

Hence $x^4 + y^4 + z^4 + w^4 \geq 2\left(x^2y^2 + z^2w^2\right)$

Again, because $(xy - zw)^2 = x^2y^2 - 2xyzw + z^2w^2 \geq 0$,

$$x^2y^2 + z^2w^2 \geq 2xyzw.$$

The combination of these results produces the required inequality. Close attention to detail shows that equality occurs if and only if both $x^2 = y^2 = z^2 = w^2$ and $xyzw$ is positive.

Solution to Problem 26.2.3

This problem, as printed, read:

Show that, if a and b are real numbers and n a natural number, then

$$\left(\frac{a+b}{2}\right)^n \le \frac{a^n + b^n}{2}$$

and hence solve the equation

$$\sqrt[n]{x-1} + \sqrt[n]{3-x} - \sqrt[n]{x-2} = 2\,,$$

where n is a positive integer.

It *should* however have read:

Show that, if a and b are *positive* real numbers ...

The mistake was pointed out by several correspondents. J C Barton offered the counterexample $a = -1$, $b = -2$, $n = 3$, and similar points were made by others. The error occurred when the chief editor trusted too blindly in his imagined ability to translate Romanian. However, most solvers corrected the error and proceeded to the solution.

We received solutions from Keith Anker, Šefket Arslangić (Bosnia), J C Barton, J A Deakin, Julius Guest, Carlos Victor (Brazil) and Colin Wilson.

Without loss of generality we may choose $b \le a$. Anker then put $a = c + d$ and $b = c - d$. Then the right-hand side of the stated inequality is $\frac{1}{2}\left\{(c+d)^n + (c-d)^n\right\}$. Then c and d are positive. Now consider the expansion of the above term by means of the binomial theorem. This will result in a sum of positive terms of which the first is c^n, as all the terms with minus signs cancel out. Thus the right-hand side exceeds c^n and this is what was to be proved.

The second part he solved by setting $a = \sqrt[n]{x-1}$ and $b = \sqrt[n]{3-x}$. In order for a and b both to be positive, we require $1 \le x \le 3$. Making this assumption, we find

$$\frac{a+b}{2} \le \left(\frac{a^n+b^n}{2}\right)^{1/n} = \left(\frac{x-1+3-x}{2}\right)^{1/n} = 1,$$

from which it follows that $a+b \le 2$. So

$$\begin{aligned} 2 &= \sqrt[n]{x-1} + \sqrt[n]{3-x} - \sqrt[n]{x-2} \\ &\le 2 - \sqrt[n]{x-2}, \text{ as shown above} \\ &\le 2, \text{ since we have shown that } x \ge 2. \end{aligned}$$

Thus the only possibility is $x = 2$.

Solution to Problem 26.2.4

This problem, wrongly numbered in our April issue, otherwise read:

> Let N be the product of four consecutive positive integers. Prove that N is divisible by 24 and is not a perfect square.

As in all the other cases, we received solutions from Keith Anker, Šefket Arslangić (Bosnia), J C Barton, J A Deakin, Julius Guest, Carlos Victor (Brazil) and Colin Wilson.

For the first part of the problem, Guest noted that the product

$$n(n+1)(n+2)(n+3)$$

necessarily contains multiples of 2, 3 and 4, from which the first result follows. Wilson argued differently noting that $n(n+1)(n+2)(n+3)/24$ is the number of ways in which 4 objects may be chosen from a group of n, where $n \ge 4$, and that this number is necessarily integral.

For the second part, Victor supposed that

$$N = n(n+1)(n+2)(n+3) = k^2.$$

This means that

$$\left(n^2+3n\right)\left(n^2+3n+2\right) = k^2.$$

Put $n^2 + 3n = m$ to find $m^2 + 2m - k^2 = 0$. This is a quadratic equation in m whose solution is $m = -1 + \sqrt{1+k^2}$. But m is to be integral so that $\sqrt{1+k^2}$ must also be integral. This is only possible if $k = 0$, and so $m = 0$ also. This in turn is only possible if $n = 0$, which is outlawed by the statement of the problem. Thus N cannot be a perfect square.

We close with a further collection of problems.

Problem 26.5.1 (submitted by Keith Anker)

Two contestants, A and B, play the following game. A's initial score is 1, B's is 0. A play consists of the toss of a coin. If it lands heads, 1 is added to A's score (and B's is left unchanged); if it lands tails, 1 is added to B's score (and A's is left unchanged). The game terminates if and when B's score equals A's score. What is the probability that the game will terminate, and what is the expected length of the game?

Problem 26.5.2 (submitted by Colin Wilson)

Show that for any triangle, the ratio of the square of its perimeter to the sum of the squares of its sides is greater than 2, but not greater than 3. For what type of triangle is this ratio equal to 3?

Problem 26.5.3 (submitted by Jim Cleary)

The cells of a 4×4 grid are to be filled with noughts and crosses. There are to be exactly 2 noughts and 2 crosses in each row and exactly 2 noughts and 2 crosses in each column. In how many different ways can this be done?

Problem 26.5.4 (based on a problem posed by Carl Fischer)

Q is a fixed point inside a rectangle. P is any other point inside the same rectangle. It is desired to minimise the mean distance $|PQ|$ over all P by appropriate choice of Q. Show that Q must be the centre of the rectangle.

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