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Editorial

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As one ages, time seems to contract. I'm not sure if this is due to daily living being filled with more responsibilities, or units of time becoming smaller fractions of our lives (probably some combination of the two.) This time contraction came to my attention recently, when I got ready to publish my edition of Noesis. Looking through past issues, I discovered that the last issue I received was November, 1989. I had been dimly aware that it had been a while since the last issue, but to discover that it was nine months was shocking. After the shock dissipated, I wondered why I hadn't received any of the nine issues. Thinking it very unlikely that nine successive issues went unpublished, I surmised that they had been mailed, but never received by me. I remembered that I had moved in December, 1988, leaving a forwarding address with the post office. Apparently, they stop forwarding mail after a year. I must have neglected to inform any of you of my change of address. I apologize for any inconvenience this may have brought to any of you.

I am still very interested in remaining a member of the society, and am taking my appointed turn as editor. There is some uncertainty as to the issue number. The last issue I received was November, 1989. Though unnumbered, that issue was the 43rd. Assuming that this journal has been continuous, this should be issue 53. I would greatly appreciate the editors of issues 45 through 52 sending me copies at the above address.

I came across some books about words that most of you should find interesting. The first is "There is no Zoo in Zoology" by Charles Elster. The title refers to the widespread mispronunciation of "zoology", which most people pronounce as if it had three consecutive o's. The book contains about 400 words commonly mispronounced, and I was quite surprised at the number of words that I've been butchering for many years. The second book is "The Logodaedelian's Dictionary" by George Saussy. This book contains many unusual words, such as gynotikolobomassophile.

I'm sure that most of you are familiar with the classic "Snow Plow" problem. (During a steady snowfall, a snowplow starts plowing at noon, plows 1 mile in the first hour, and 1/2 mile in the second hour. What time did it start snowing?) There is a similar problem, in which three plows start plowing the same road at noon, 1 PM, and 2 PM, from the same starting point during a steady snowfall. If the three plows later meet, what time did it start snowing? This problem is from the challenging collection "Ingenious", by L. A. Graham.

The remainder of this issue is devoted to several articles I have found interesting, and a probability problem that I spent some time with.

Faith in Fifth Force Fades

The case for the "fifth force" seems to be falling apart fast. Not only has a new experiment failed to find any evidence for it, but two earlier confirmations have now been withdrawn. "We're now saying that the evidence does not support the fifth force," declares Donald H. Eckhardt, who is a physicist at the Air Force Geophysics Laboratory in Bedford, Massachusetts, and a principal investigator on one of the experiments being retracted. "The case has not been established," agree the principal investigators on the other experiment, geophysicists Robert L. Parker and Mark A. Zumberge of the Scripps Institute of Oceanography in La Jolla.

The fifth force is supposed to be a new type of fundamental interaction beyond the four forces—strong, weak, electromagnetic, and gravitational—now known. Empirically, it is expected to show up as a tiny deviation from the inverse-square law of Newtonian gravity. If real, it would require major revisions in current theories.

The fifth force hit the headlines in January 1986, when Purdue University physicist Ephraim Fischbach and his colleagues found apparent anomalies in a 1922 measurement of the gravitational constant by the late Hungarian physicist Roland Eötvös. More direct evidence came from experiments such as Eckhardt's, which was conducted last year on a 600-meter television tower near Raleigh, North Carolina, and Parker's and Zumberge's, performed in 1987 in a 2-kilometer-deep borehole in Greenland. In each case, the scientists took gravity measurements at several different levels and found fifth-force type deviations from predicted Newtonian values.

Only one problem: the deviations disagreed in both magnitude and sign, raising suspicions about their significance. And, as Parker and Zumberge point out in the 2 November issue of *Nature*, those suspicions are well founded. The researchers show that the results of any such experiment are extremely sensitive to the corrections made for the gravitational effects of local geology. Indeed, they claim that equally plausible corrections can account for all the results *without* a fifth force.

Eckhardt, although skeptical of Parker and Zumberge's analysis, says he now concedes that his original conclusions are wrong for another reason. Subtracting out geological effects requires having ground-level gravity measurements for miles in every direction. But in eastern North Carolina, the survey teams tend to take their measurements by the roadsides instead of out in the swamps. "So you find that the gravity measurements are biased to high ground," he says.

And finally, there is a third nail in the coffin. In the 30 October issue of *Physical Review Letters*, James Thomas and his colleagues at the Lawrence Livermore Laboratory report on an experiment performed on a 465-meter tower at the Nevada Test Site, where the geological data are extremely complete. Their conclusion: no fifth force with an accuracy of better than one part in 10 million.

■ M. Mitchell Waldrop

Puzzling Out the Tectonic Plates

Something has to give in the Indian Ocean. Massive forces have touched off large earthquakes and crumpled the ocean crust in an area south of the Indian subcontinent. But, oddly, all this seismic activity is taking place in the middle of the Indo-Australia tectonic plate. In classic plate tectonics theory, earthquakes are concentrated around the edges of the jostling plates. So how do you get such large quakes and so much deformation where there's but a single plate? What may have to give is nothing less than one or another of the basic assumptions of plate tectonics.

One such assumption is that plates are always rigid and inflexible. Given all the tectonic activity in the Indian Ocean, many geophysicists had assumed that they had to make an exception to the rule of plate rigidity—the forces driving the Indo-Australia plate must be deforming it and triggering the seismic activity in mid-plate.

Now comes a group of geophysicists with an alternative explanation. The active area, they posit, may be a broad, diffuse boundary region between two plates moving independently after all. If these researchers are correct, they will have maintained the notion of rigid plates but overthrown the classical assumption that plate boundaries in the oceans are always narrow.

The group out of Northwestern University argues in a forthcoming issue of *Tectonics* that the Indo-Australia plate is not one plate but two—the India plate and the Australia plate—divided by a boundary 3300 kilometers long and a relatively enormous 800 to 1600 kilometers wide. That contrasts with the three conventional types of boundaries—the summits of mid-ocean ridges, deep-sea trenches, and transform faults—that are thousands of kilometers long but only some tens of kilometers wide.

This sprawling boundary would not only look peculiar but also behave peculiarly. Where the proposed Indian and Australian plates are thought to be ramming against each other along the eastern part of the boundary, neither plate is sinking beneath the other, as happens when the Pacific plate encounters Japan at the Japan trench. At the other end of the boundary, where the plates would be pulling away from each other, magma does not seem to be welling up to add to each growing plate, as happens at mid-ocean ridges. Instead, the putative boundary seems to be deforming along its entire length as the plates jostle each other.

It's no surprise, therefore, that many geophysicists remain unconvinced. To some, the proposed boundary is something of a semantic contrivance. "It's taking something the size of some smaller plates and calling it a boundary," notes Sean Solomon of the Massachusetts Institute of Technology. "Those of us who work at plate boundaries don't see much use in it." He and others would just as soon call it deformation in the middle of a nonrigid plate.

But those who try to understand how eight major and a half dozen minor plates drift around the globe see considerable merit in the idea. "The reason I like to think of it as a diffuse boundary," says Richard Gordon of Northwestern University, "is that it lets us quantify things and make predictions."

Indeed, Gordon has already tested one set of predictions derived from the diffuse boundary idea against observations and found that the concept stands up well. Building on earlier work by Douglas Wiens of Washington University in St. Louis, Gordon, along with Charles DeMets of the Jet Propulsion Laboratory in Pasadena and Donald Argus of Northwestern, gauged how the putative boundary between the India and Australia plates should behave given what can be determined about the motions of the surrounding plates.

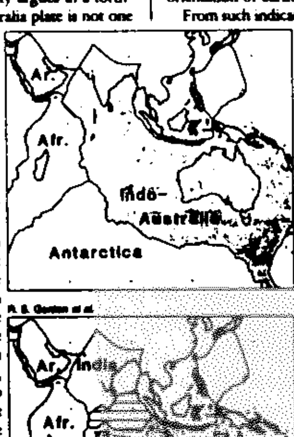
Gordon and his colleagues calculated the rate of plate motion during the past 3 million years from the record of Earth's flip-flopping magnetic field frozen into the crust as it continuously forms and spreads away from the mid-ocean ridges, much the way a tape recording is made. They gauged the direction the plates are moving from the orientation of sea-floor transform faults, which point in the direction of spreading, and from the orientation of earthquakes on the transform faults.

From such indicators, Gordon and his colleagues measured the motion of the presumed India plate relative to the adjacent Africa and Arabia plates and the motion of the presumed Australia plate relative to the Africa and Antarctica plates. From this, they calculated what the motion between the India and Australia plates should be. Toward the eastern end of the boundary, they predicted that the plates are converging at a slow but measurable 4 ± 3 millimeters per year. That fits the pattern of earthquakes, faults, and undulations in the sea floor in that area. Near the boundary's western end, the plates should be pulling away from each other at 6 ± 2 millimeters per year. That also fits the observed pattern of faulting there.

Although the success of these plate motion predictions would seem to justify the claim that the region is a new, diffuse boundary, that does not answer Solomon's fundamental complaint—"It doesn't tell us why" anything at all is happening.

Everyone agrees that the earthquakes and deformation along the equatorial Indian Ocean are ultimately related to the Himalayas 2500 kilometers to the north. Something about the strain of raising the world's highest mountain range by colliding the Indian plate with Asia has overwhelmed a portion of the plate. The final answer should lie in the detailed history of plate motions and plate behavior preserved in the sediments and crust of the Indian Ocean. Gordon leaves this spring to gather more of that history from this oceanographic hinterland.

■ RICHARD A. KERR



The plates before and after. *The conventional layout of the plates in the Indian Ocean (top) has a single, Indo-Australia plate (shaded). Studies of recent motions of all the plates in the area suggest the division of this plate into the India and Australia plates with a unique, broad boundary (hatched) between them.*

MEPHISTO X (WHITE)
vs.
DEEP THOUGHT (BLACK)
Queen's Gambit Accepted (D26/18 8 g4)

The first game which the "mature" DEEP THOUGHT has lost to another program. Appropriately it is the very positional, ever-dangerous MEPHISTO X program which achieves this feat against the tactically ferocious DEEP THOUGHT. The game follows a quiet course whereby Black never quite equalizes from the opening. White enjoys a strong, solid center which it never relinquishes. The reader may recall that MEPHISTO X almost beat DEEP THOUGHT at last year's 19th ACM NACCC in the same round with the same colors.

1 d4 d5 2 c4 d:c4 3 Nf3
MEPHISTO X follows the more solid, traditional lines of the Queen's Gambit Accepted which can also give White a nice edge.

3...Nf6 4 e3 Bg4 5 B:c4 e6 6 h3 Bh5 7 Nc3 Nbd7 8 g4 Bg6 9 Nh4 Be4?!
It is better to "lose" the bishop on g6 and recapture with the h-pawn with play on the half-open h-file.

10 N:e4 N:e4 11 Nf3 Nd6 12 Bb3 Qe7?
A very poor placement of the queen, interfering with the development of the king's bishop. Indicated is 12...c5 with counterplay. As the game continues, Black never gets to play the lever...c5.

13 Bd2 h5 14 Rg1 h:g4 15 h:g4 O-O-O 16 Ba5 b6?
Could Black really be a 2551 player? Why weaken the White squares around the king voluntarily? Indicated was 16...Nb6 or Kb8.

17 Bb4
The ensuing trade of this bishop seems unnecessary, but if 18 Ba3, DEEP THOUGHT may get some counterplay with ...e5. White should just try to play Rc1, Qe2, Bc6 and exploit the weakened white squares.

17...a5 18 B:d6 Q:d6 19 Qe2 Be7 20 O-O-O Rh3 21 Nd2 c6
This pawn remains weak throughout the game and MEPHISTO X quietly exploits this.

22 Rh1 Rdh8 23 R:h3 R:h3 24 Ne4 Qc7 25 Kb1 g5
Black could not play 25...c5 because of 26 d5.

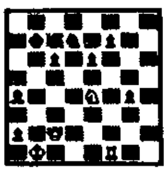


Figure 3 Position after White's 27 Ba4

26 Rc1 Kb7 27 Ba4
MEPHISTO X plays directly and simply. It is not the world's most tactical program, and over the years it has appeared reluctant to play line-opening pawn thrusts. Here 27 d5!? is one such try, but there could follow 27...e:d5 28 B:d5 Ne5 29 f4 g:f4 30 e:f4 Qd7! and perhaps MEPHISTO X's decision not to play 27 d5!? is correct.

27...Nb8 28 Nd2 Qd7 29 Bb3
Quite a sobering but disappointing move. It cannot be proven here, but intuitively it would seem that 29 Nc4!! must win. There are lines like 29...b5 30 Ne5 Qe8 (30...Qd5? 31 Bb3) 31 B:b5 c:b5 32 Qc7+ with a winning attack. Besides 30 Ne5 there is the threat of 30 Qb3 hitting b6 and a5. It is probable that DEEP THOUGHT saw these possibilities against itself.

CONT. ⇒ (7)

Back in my college days, I used to play poker fairly regularly. One of the players insisted that in 7-card stud, one had a better chance of getting a straight than one had for three of a kind. There are many books that give tables of the distribution of possible hands for 5-card stud, but I've never seen the distribution given for 7-cards. This problem was on my list of things to get to for many years, and I've finally been able to spend some time on it.

If you look at a seven card hand, the distribution of the card face-values is as follows:

4,3	C(13,1)C(4,4)C(12,1)C(4,3)	624
4,2,1	C(13,1)C(4,4)C(12,1)C(4,2)C(11,1)C(4,1)	41184
4,1,1,1	C(13,1)C(4,4)C(12,3)C(4,1)^3	183040
3,3,1	C(13,2)C(4,3)^2C(11,1)C(4,1)	54912
3,2,2	C(13,1)C(4,3)C(12,2)C(4,2)^2	123552
3,2,1,1	C(13,1)C(4,3)C(12,1)C(4,2)C(11,2)C(4,1)^2	3294720
* 3,1,1,1,1	C(13,1)C(4,3)C(12,4)C(4,1)^4	6589440
2,2,2,1	C(13,3)C(4,2)^3C(10,1)C(4,1)	2471040
* 2,2,1,1,1	C(13,2)C(4,2)^2C(11,3)C(4,1)^3	29652480
* 2,1,1,1,1,1	C(13,1)C(4,2)C(12,5)C(4,1)^5	63258624
* 1,1,1,1,1,1,1	C(13,7)C(4,1)^7	28114944

The total of these figures is 133784560, which = C(52,7)
This is all very straightforward, but the complexity enters when you consider that the entries marked with an asterisk also include straights, flushes, and straight flushes. I extracted these "pat hands" from the four entries under consideration using methods that left me less than fully satisfied. If any of you has an elegant way of doing this, I would relish hearing about it. For now, I will list the values I extracted.

	St. Flush	Flush	Straight
3,1,1,1,1	600	76620	50600
2,2,1,1,1	3600	459720	226800
2,1,1,1,1,1	20472	2079912	2530440
1,1,1,1,1,1,1	16912	1431392	3372180

Combining these two tables, the results are as follows:

St. Flush	41584
4 of a Kind	224848
Full House	3473184
Flush	4047644
Straight	6180020
3 of a Kind	6461620
2 Pairs	31433400
1 Pair	58627800
No Pair	23294460

Though it is a very close race, it is more likely to get 3 of a kind than a straight in 7-card stud. Interestingly, it is less likely to get no pair than it is to get 1 or 2 pairs. Of course, this may have been nothing more than an exercise in futility, for I haven't played poker in quite a few years, and I have no desire to begin again. This was just one of things that I wanted to know.

CONT. From (5)

29...Na6 30 Qe4 Nb4 31 a3 Nd5 32 Qg2 Rh8 33 Ne4 f6 34 Qg3 Rg8 35 Rh1 f5 36 g:f5 e:f5 37 Qh5
The game has taken another course since the previous note, but White still enjoys a comfortable edge via the solid central pawn chain and control of the h-file which Black once owned.

37...Rf8 38 Nd2 Bf6 39 Qh7 Rf7 40 Qh6 Qe6 41 Qg6 Rg7 42 Rh7 R:h7 43 Q:h7+ Be7 44 Kc1
Despite the foregoing exchange of rooks, White has made inroads into Black's position which is somewhat tangled.

44...Kc7 45 Nf3 Kd8 46 Ne5 g4 47 Qh8+ Kc7 48 Kd2 Kb7 49 N:c6!
Finally MEPHISTO X finds a tactical coup which converts its positional advantage into a material win (a pawn). If now 49...K:c6 50 Qa8+ Kd6 51 Qb8+ Kd7 (or Kc6) 52 Ba4+ and wins.

49...Q:c6 50 Qe5 Nc7 51 Q:e7 Qg2 52 Qh4 f4 53 e:f4 Qe4 54 Q:g4 Q:d4+
Black's checks quickly run out.

55 Kc1 Q:f2 56 Qf5 Qf3 57 Kc2 Kc6 58 Qe5 Nd5?
This loses quickly, but there was little Black could do to stop the f-pawn in any case.

59 Qe6+ Kc5 60 B:d5 Q:d5 61 Q:d5+ K:d5 62 Kd3 a4 63 Kc3 Kc5 64 f5 Black resigns.

Packing Your *n*-Dimensional Marbles

Some of the questions mathematicians ask sound silly: How many pennies can you lay on a tabletop? How many marbles will fit into a semi-trailer?

At other times the same basic question takes a somewhat more serious form: How many digital signals can occupy a noisy channel?

A recent discovery by Noam Elkies, a mathematician at Harvard University, has given researchers new insight into the mathematical theory that encompasses all three questions. Elkies's result is an unexpected application of a branch of number theory to a geometric problem known as sphere packing.

Sphere packing, in mathematical parlance, is a problem of cramming an *n*-dimensional space with *n*-dimensional "spheres," all of the same size, with the least amount of empty space in between. Coins are a good example of two-dimensional "spheres" (circles); marbles are examples in three-dimensional space.

In the mathematical theory that underlies telecommunication, a digitized signal is encoded as the coordinates of the center of a higher dimensional sphere packed in a higher dimensional space. After transmission down a noisy channel, the signal may no longer be exactly at the center, but as long as it's still within the sphere, the receiver can restore it to the center and read the signal exactly.

The easiest way to keep the signal clear would be to space the spheres far apart. But that's wasteful. The phone company can satisfy its customers and stockholders simultaneously by finding efficient ways to pack the signals. Similar problems crop up in other places: data compression, antenna design, and x-ray tomography. What everyone wants is the best way to pack spheres into the space their problem occupies.

Surprisingly—given its wide applications—the problem has so far only been solved in the simplest case: that of two dimensions, where the pennies fill 90.69% of the tabletop. In three dimensions the obvious candidate is the face-centered cubic packing of spheres beloved of crystallographers and fruit vendors.

Now, nobody is betting against the face-centered cubic packing as the best possible solution, but so far no one has been able to give a rigorous proof that there's nothing better. And in the really formidable dimensions—above a thousand, say—mathematicians are at a loss. "We don't know a better way of packing spheres than just picking them at random until there's no room left," Elkies says.

Although new sphere packings are discovered all the time, most of them have come from standard techniques in the subject. Elkies's approach, on the other hand, is brand new. It is based on the theory of elliptic curves, a branch of number theory that is concerned with finding solutions of certain polynomial equations. Though a newcomer to sphere packing, Elkies is an expert on elliptic curves; 2 years ago he applied the same theory to solve a 200-year-old problem related to Fermat's Last Theorem.

Elkies's result uses the theory of elliptic curves to construct a regular array of points, called a lattice, to serve as the centers for his spheres. These lattices have been long familiar to number theorists, but no one had looked at their sphere-packing properties before. Not every elliptic curve has the right kind of lattice, and part of the trick was finding the ones that do.

"Theoretically it's quite exciting," says Andrew Odlyzko, head of the Mathematics of Communication and Computer Systems Department at Bell Laboratories in Murray Hill, New Jersey. "It's a new way of approaching a famous unsolved problem."

Elkies' new packings may or may not be the best possible—his work doesn't address that issue—but they do give improvements on the best methods known so far in a number of dimensions, the largest being 1024. In several other cases his approach agrees with the previous best known packings—notably in dimension 24, which has a surprisingly efficient packing known as the Leech lattice. (Some new computer modems make use of the Leech lattice sphere packing.)

Elkies's discovery has no immediate practical application, says Odlyzko, "but that might change." For one thing, theoreticians are toying with the idea of upgrading modems to work in higher dimensional spaces. But whether it has immediate applications or not, Elkies' new discovery certainly gives mathematicians more space to play around in. ■ BARRY CIPRA

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