FEEDBACK

(i.e. 10 ÷ 10 = 1) or I can give five people one apple each, twice (10 ÷ 5 = 2), else I can give four people two apples each with two left over or else give them 2½ apples each (10 ÷ 4 = 2½ or 2 remainder 2). I hope my rambling has demonstrated exactly what mathematical division is.

Interestingly, if reproducing cells are considered to divide (in the mathematical sense) then that would suggest that division of the integer one by two repeatedly (cells 'splitting' into two repeatedly) is equal to infinity, when in fact slicing an apple in half repeatedly will show that one divided by two repeatedly tends towards zero. Therefore 'division' of cells cannot be considered as being the same as mathematical division. Peno's postulate that 'one is a positive integer' has not been violated, but shown to be true.

Assuming that our culture started with one cell (it cannot start with less) as the cells reproduce we have more cells, so we have not violated the postulate that 'no positive integer has one as its successor! I conclude then that replication is no more than multiplication (and hence addition) and so multiplication, division, addition and subtraction can all be found in nature.

John R. Ridley
Masirah
Sultanate of Oman

Quality assurance

I find myself in broad agreement with Mr Whitehead (June Letters). I have recently helped to steer my company through a successful application for AQAP-1 and it is true that the focus of the system is on enhancing company quality performance, not on continuous knuckle-rapping. The only issue I would take with your correspondent is on Secton 4, where he implies that QA requirements can be relaxed when ordering from a supplier who can supply the sub-assemblies to BS 5750, components to BS9000, etc. This is simply not true: none of these releases provide any express or implied enhancement of quality. They are objective indicators that systems are in place to monitor and, if necessary, to correct problems if they should occur (or before they occur, if *successful* statistical methods are involved).

K. V. Castor-Perry
Beckenham
Kent

Toroidals and surface mount

I was interested to read the article in the March issue on the background to toroidal transformers. Alas (not surprisingly) it was a bit one-sided in the listing of pros and cons. The following comments based on my experience as a designer might perhaps help others.

Points in favour of toroids

- low radiated magnetic field
- low profile
- good rated-power to size ratio (but see below!)
- acoustically quiet

Against

- higher interwinding capacitance
- vulnerable to "shorted turn" failure if the mounting bolt is electrically connected both ends, even by accident!
- output power rating is also absolute maximum rating, i.e. a brick-wall current limit
- not continously rated at full power, so all right for class B audio, but for power supplies must be downrated to 60%
- typically twice the price per rated watt on small units
- Varistor strongly recommended to protect the primary against the "switch-off pulse" even on the smallest cores

I use toroids a lot, but for real power with "that little extra in hand just in case", a standard E/I core with its softer saturation characteristic is still by far the best.

On the subject of surface-mount techniques, the recent articles are highly relevant to the mass manufacturing side of the industry, but there is very little attention being paid to the maintenance of the product and the needs of the small quantity user.

My work includes repair and modification of modern small Japanese equipment full of s.m.ds. Only RS Components, so far, out of half a dozen industry suppliers I've checked, stock a range of R's and C's and getting hold of i.cs and other semiconductors is like drawing hens teeth from a storeman of the old school. S.m.t tools are also hard to find (I can recommend the DeSoutter pick-up pen) and very expensive for what is on offer.

If we are to be able to make use of the system then it should be more widely available and less like joining the Masons.

R. F. Stevens
Ickenham
Middlesex

Relativity

Dr Scott-Murray's June letter further confuses the issues raised by the Hafele-Keating experiments, in which two sets of atomic clocks were sent on a series of airline flights which took them round the world in opposite directions, and were compared before and after their journeys. In these experiments two distinct effects occurred, namely: –differential ageing, arising because the clocks carried round the world were exposed to accelerations which caused each set to traverse a closed path within the inertial frame in which, for the few days the experiment lasted, the centre of the earth was approximately at rest

– gravitational red shifts. As the altitude of each aircraft changed the corresponding change in the earth's gravitational potential affected the rates of the clocks it carried.

The results Dr Murray refers to as the '"acceleration potential" term', and as the 'Lorentz velocity term'" arise from alternative ways of calculating the *first* effect. The first way gives an approximate answer, since it is based on the 'Principle of Equivalence', which is itself approximate. The second way, which involves the implicit assumption that accelerations do not affect the rate of an ideal clock, gives the exact answer. The *second* effect can be calculated approximately from Special Relativity using the classical expression for gravitational potential, which is usually more than adequate.

To my knowledge no experiment involving the motion of finite objects, whether using rotors, or aircraft, or satellites, is claimed to verify terms in the Special Relativity formulae involving powers of v^2/c^2 greater than the first. However the experiments with his energy mesons which Dr Murray so carefully ignores verify the formulae with great precision.

As for his comments on the design of the Stanford electron linear accelerator, what Special Relativity actually says is that the measured velocity of an *isolated* electromagnetic pulse travelling in a vacuum is the same in all frames. The electromagnetic pulse travelling down a waveguide is a composite of many pulses absorbed by and re-radiated from the atoms in the wall of the guide, and so has a group velocity smaller than 'c'. Thus particles such as electrons may either fall behind the pulse travel in step with it, or run ahead of it.

In any case, as I pointed out in the March letters, to be accelerated the electrons must travel in step with the wavelets within the pulse, i.e. not at the group but at the phase velocity. The design problems arose from the requirement to excite the waveguide so as to keep the phase velocity smaller than the velocity of light, while avoiding the excitation of mixed modes.

C. F. Coleman
Grove
Oxfordshire

Re the misinterpretation of experiments. If we are given that unlike charges attract and that charges of one kind do not exist separately, it follows that two like charges will be attracted apart by their companion charges. There is no logical necessity for a repulsive force and no experiment can prove that such a force exists. The idea of a repulsive force has survived for 700 years but the record for the subversion of science by experiment probably belongs to the Sun God.

A similar situation exists with the electromagnetic experiments used to justify relativity. That theory treats the constancy of light velocity as a kinematic phenomenon. Classical physics treats it

FEEDBACK

electromagnetically by requiring that Maxwell's equations apply to all observers and then deriving the field relations. Both methods involve the Lorentz correspondence but, in the classical case, not as a kinematic relation between observers. The end result is the same for both methods so the experiments are no more a proof of relativity than of classical physics.

There appears to be a difficulty in distinguishing Newton's scheme for the description of phenomena from that phenomena. Thus we have such assertions as 'mass is constant in classical physics' followed by the derogation of classical physics. This is akin to saying the validity of a computer program depends on the data fed to it. In fact, Newton's requirement that space-time be neutral merely allows each branch of physics to have its own independent laws and imposes no restriction on any phenomena, so one would expect his scheme to be applicable whatever the experimental data may be. This argument needs to be faulted before the scheme is rejected and it is not clear how experiment can do it. Nor, of course, can relativity's soothsayer logic do it.

R. Berriman
Palmerston North
New Zealand

Tercentenary of Newton's Principia

The tercentenary of Newton's *Principia*, perhaps the greatest book ever written,is sadly being sullied by the limitations and alteratioins allegedly imposed on its contents by Einstein's relatively (Tom Ivall, Satellite Systems, *E&WW*, vol. 93, Feb. 1987 p.159. In fact all the republications of the *Principia* in the twentieth century are tarnished by this disgraceful smear within their own pages.

Naturally, there are inaccuracies and errors in the *Principia*, but Einstein did not correct them; nor did Einstein generalise Newton's laws. On the contrary, Einstein repudiated explicitly and unreservedly the most basic tenets of the *Principia*, though because of Orwellian doublethink the contemporary conventional physicists and historians seem incapable of recognising this little anomaly.

The most flagrant example is Einstein's rejection of Copernican heliocentricity (i.e. the physical superiority of the Sun's reference system over the frames of, say, Mercury or Earth, or Titan or Apollo 9, or the Orient Express) in favour of general relativistic polycentricity (i.e. the complete equivalence of all frames, both inertial and non-inertial). This point is significant because without heliocentrism there would have been no Kepler's laws, no Newton's laws, no rockets, and no artificial satellites.

Another elementary relativistic error is the extraordinary statement "force is a mathematical fiction, not a physical reality" which reduces Newton's second law (f=m a) to a mere definition of force. the absurdity of this queer relativistic precept becomes manifest when stated thus: "a mathematical fiction (force) produces a real effect (acceleration)". As for the third law, that is rendered pure fiction in its entirety. Of course a law of nature is a causal relationship between at least two (defined by other means) physical realities.

Evidently the only effective way to bring the relativists to their senses is to blast a punch in their faces, preferably by a champion boxer. (But perhaps the relativists can Lorentz-transform to a frame in which the force of interaction between fist and head is exactly zero?) Newton of course treated 'force' as the physical reality that it is.

A correct appraisal of the contents of the *Principia*, and of the achievement of Newton (Britain's, and possibly the world's, greatest ever scientist), will be possible only when the relativistic strictures are recognised as the blatant errors that they really are.

But these issues are not merely matters of academic concern. For the relativistic errors impede and prevent not only theoretical but also technological advances. We had the opportunity to point out in the *American Journal of Physics* (vol. 54, Nov. 1986. p.969) how the confusion stemming partly from the relativistic errors had hindered and delayed the invention of the laser and of the intensity interferometer; and in *Nature* (vol. 321, 1986, p.734) and *Electronics & Power* (vol. 32, 1986, p.789) how the confusion springing entirely from the same errors has so far prevented the development of the optical translation sensor.

T Theocharis
M Psimopoulos
Blockett Laboratory
Imperial College
London SW7

Planck's constant

In reply to Mr Akil's comments in April's Feedback and to Mr Brindleu's of May on my note about Planck's constant and the atomic fine structure constant, February 1987, I give the following answer. The relation $h=2\pi m_e c r_e (a-1)$ is got by the following premise:

Start with Bohr's basic atomic model of orbiting electrons around a proton nucleus. The forces acting in such a system are electrostatic forces and mass inertial forces. Assume the mass density is the same in both the proton and the electrons, and that the proton behaves like a current wire loop in a magnetic field, hence effected by a torsional force when turning out from a neutral position in the electrostatic field between the proton and electron. This oscillation creates small disturbances in the orbiting movement of the electron, giving rise to a resonance condition between this oscillation and the orbit time. That is the quantum condition. Then assume the frequency of radiating energy is in proportion to the medium value of the difference between two successive proton oscillating frequencies, then you have the solution in a nut-shell.

The fine structure constant approximately constitutes the quotient between the proton mass and the electron mass raised to the exponent of 2/3, given by the constant mass density of both proton and electron. The model generates Schrödinger's wave equations as well as other well-known relations from quantum mechanics. The model gives the proton an extension (radius) of about 35 fermi as well as new knowledge of the properties, structures and behaviour of elementary particles. Thus, I say that the relation above conceals deep secrets of the nature of matter.

Ove Tedenstig
Märsta
Sweden

Inductive coupling

Two minor points regarding Tom Ivall's article on my range-insensitive links (June issue): The first is that the approximate expression for the 'pulled' frequency is

$$\omega = \omega_0/\sqrt{1\pm k}$$

and not as stated. The second concerns Fig. 2. The 74HC04N is a hex inverter; one inverter is used for the first stage of the transmitter, and five stages in parallel for the second, output stage.

P.E.K. Donaldson
MRC Neurophysiological
Prosthesis Unit
London

Hugh Pocock

The obituary of Hugh S. Pocock in your May 1987 issue brings back to me many memories over a very long period, for I knew him well and can emphatically endorse your brief summary of his personal character and expecially its gentlemanly nature.

My first contribution to what was then *"The Wireless World & Radio Review"* was in the issue of 15th August 1923, and received the distinction of 'top billing' on the front cover. Even at that early date it was issue number 209, and cost 4d net (1⅔p), and was registered as a Weekly Newspaper. Unlike most of my subsequent contributions, this first one was entirely practical, being a detailed description of my invented machine made from second-hand parts to provide about 800 volts for my valve transmitter from the then-usual d.c. mains supply.

M. G. Scroggie (Cathode Ray)
Bexhill
Sussex

Feedback also appears on page 786.