FEEDBACK

FFT

David Gibson (Letters Sept.86) is being unfair to the Fourier transform! The Fourier transform, put simply, is a mathematical tool which can transform or 'map' from one domain to its inverse. Frequency and time are inverse domains, and so the Fourier transform can be used to map from one to the other. There are limitations to what the transform can handle, (many of which are overcome by using the Laplace transform – but that's another story), but low-pass filters whether real or 'perfect' are not among them.

A non-repeitive frequency domain function, such as low pass filter (l.p.f.) whose frequency response never quite reaches zero at any frequency, has a finite-length time-domain transform. This is the realm of real filters. As a filter approaches perfection, and has more and more ultimate attanuation, the time-domain response last longer and longer (the filter 'rings') and in the limit, a perfect filter with infinite attenuation in the stop-band would have an infinitely long time-response (it would 'ring' for ever). The Fourier transform of such a frequency response is sint/t where t is the reciprocal of the filter bandwidth.

A feature of all time-domain transforms is that they are symmetrical about t=0, and so there is apparently as much response before t=0 as afterwards.

This leads to the realisation that the 'well known phenomenon' mentioned by Mr Gibson that the Fourier transform predicts that a perfect l.p.f. has a time response which begins before the impulse at t=0 is going to be true also for real l.p.s. So what has gone wrong?

The answer is that this is a misapplication to the transform. The process of predicting a filter's time-domain response resulting from an excitatory impulse involves convolution. It would need a complete article to satisfactorily explain what convolution is; however the salient features of convolution are as follows:

a) it runs for the time interval required for the resulting impulse response to die away to zero. Let us call this time interval t.
b) as the convolution proceeds, the excitatory impulse is scanned from 0 to t in the time domain, while the filter's time-domain response* is scanned backwards from t to 0

Fourier transforming the convolution gives us

$$F[f(t)*h(t)]=Ff(\tau).Fh(t-\tau)$$
$$=F(\omega).H(-\omega')$$

where τ runs from 0 to t (1) during the convolution

F represents the Fourier transform, and * represents convolution.

If f(t) is the vanishingly short unit impulse, and h(t) is the wanted response to this impulse, then the L.H.S. of equation (1) is simply equal to h(t). On the R.H.S. of the equations, Ff(t) is unity, and so we can rewrite equation (1):

$$h(t)=H(-\omega') \qquad (2)$$

What equation (2) says is that the impulse response is given by the inverse Fourier transform of not $H(\omega)$ (the frequency response) but $H(-\omega')$, *the frequency response run backwards, with an offset of t.* Now t is simply the time required for the impulse response to delay to zero, so we take the inevitably symmetrical $H(\omega)$, reverse it, and move it so that t=0 correspondents with the point where the response reaches zero.

As the l.p.f. response becomes steeper, t becomes longer, and tends towards an infinitely large value for a perfectly steep filter. This accords with experience, which is that the sharper the wanted filter cutoff, the more elements the filter requires, and the larger the group delay becomes. The mystical perfect filter would need infinitely many elements, t would be inifinite, and so the filter would never given any output (thereby never having a chance to 'ring' indefinitely as suggested earlier in the analysis)

Brian J Pollard
Watford

*i.e. the time response to a vanishingly short unit impulse – the response we are interested in deriving.

Relativity

Professor Butterfield has presented a welcome and delightfully simple approach to explaining the seeming oddities of relativity. I am, I confess, still not fully convinced of the "correctness" of the whole concept (Butterfields' article aside).

The invited assumption that "energy has inertial mass" is not at all straightforward (like the reasons to assume c is constant). I would suggest the only reason it seems to be more acceptable is that from experience we know it is worse to be hit by a fast moving object than by a slower moving object. To hedge a 'little' extra mass on top of this is, I suppose, more easily grasped. However, to explain it (this small extra mass) one needs the results based on this assumption! (equation 5).

The evidence in favour of relativity is overwhelming, considering observed effects in nuclear physics (e.g. particle decay times) and astronomy (gravity lens), yet I feel sure most people would agree that although relativity gives a possible (and quite usable) explanation for observed events, it does not give a reason and is far from complete.

Indeed, if one were to assume our hypothetical observers were blind, and must use sound to transmit and perform the necessary logical steps, we see that these poor observers believe their object has disappeared upon reaching (or passing) the sound barrier: all talk of aether and such aside, no convincing could persuade our blind friends that the object was still there.

Despite my allegiance to relativity, I'm still anxious to know whether anyone has tried, for example, weighing a spinning centrifuge rotor or a very large charged capacitor to measure any weight differences between the high and low energy states. Other relativistic ideas which bother me are, I'm sure, often suggested brain teasers, such as "What happens to a top spinning at a high angular velocity?" or what happens to the magnetic field of a superconducting ring as it is lowered into a black hole?"

It seems gravity is inextricably involved, and will remain a dark horse until a grand unified theory is put forward. I fear, that by then, the mathematics will be quite incomprehensible to this humble reader. Meanwhile, time has once again won by eluding true definition, and quietly slipping by.

Michael J Snoswell
Adelaide, 5067
South Australia

Professor Butterfield is being fishily eel-like with his shoals of evidence, and apparently cannot digest the worm which he nibbles at. In all my letters over the past couple of years or so I have never doubted Einstein, and my attack has been an effort firstly to enlighten those who do not understand the matter, and secondly to enable the specialists of the world to understand it better than they do, and that without inaccurate presumption.

Before wielding my two-edged fishing rod with its horse's head handle, let me congratulate the Prof. upon coming up with creation and catastrophe as a vital part of the picture (poor Albert) even if he only did it subliminally with a Freudian slipping clutch like the monetarist snake!

The Prof. states: "Since a packet of energy should not be instantaneously movable from A to B as we see it, then it must have inertial mass!"

NOT SO, Prof.: if the energy is carried by a wriggling device which has inertial mass, it will be similarly delayed. Can't you see that? Such a device is the LSM, the basic postulate which Einstein seems to me to have had at the back of his mind when he stated that he could not conceive an empty space with nothing in it, a postulate which he found difficult to communicate to the specialists of the world because he had not completed his concept. I say again, back to the drawing board, Prof.!

The concept at which I believe Einstein would have arrived, had he had enough time, is in the hands of Mr Editor who might publish it when and if he sees fit, or may not because it is better for improvement of mind to come from within by personal solving of the puzzle, rather than from without: logic which is a part of personal experience is more easily believed than that which is provided by others.

FEEDBACK

I recommend that Prof. Butterfield differentiates again in order to clarify his mind by separating energy from mass, so making it an individual entity: he might then learn something to his advantage (which simplifies his envisaged mind-boggling rules) and discover how Einstein evolved out of Newton without denying him. A second further differentiation will bring him to time alone, and there is nothing simpler than that.

Finally, I congratulate Prof. Butterfield in recognising (if again subliminally) that I am a Leo, but crouching in his den and lying in wait!

James A. MacHarg
Wooler
Northumberland

In his June article 'Relativity Simplified' Professor Butterfield attempts to derive Special Relativity by considering accelerated motion rather than uniform motion (i.e. motion in which no internal stresses are generated). By so doing he completely obscures Einstein's major achievement. That achievement was to realise (with some assistance from Minkowski) that the contemporary view that in a vacuum electromagnetic effects propagate as waves in an ether could be replaced by the view that in a vacuum the propagation of electromagnetic and gravitational effects is determined by the geometrical structure of the space-time continuum. This view takes some swallowing, but I do not believe that any consistent picture of Special Relativity can be developed without it. Generalisations which attribute increasingly elaborate structures to the space-time continuum underlie not only Einstein's very successful theory of gravitation, but also the much more recent developments in the theory of the early history of the universe.

In all these theories the idea of simultaneity (i.e. of a universal time) has to be abandoned. However Special Relativity retains reasonably close links with classical physics because in its simultaneity remains a valid concept in any particular inertial frame. The weakness of most presentations of the theory is that no attempt is made to find an explicit expression for the effect of simultaneity breakdown, and Professor Butterfield's is no exception. I have given the required expression elsewhere[1]. His statement, at the beginning of the section 'Simultaneity and measuring rods', that 'the length of an object *must* be defined in terms of simultaneous measurements at the two ends', is therefore incorrect. Instead this is merely a convention, and a bad one at that, since it leads to all the familar difficulties with the assertion that the length of a rod changes as a result of its motion, even though it is free from internal stresses.

In the section "Light and the 'Impact Theory'" the moving clock can be regarded as passing two distinct clocks at rest in the L_1 frame at the positions x_1 and x_2. These encounters remain distinguishable events in the frame L_2 of the moving clock however fast it moves, even though in that frame the measured time interval between them may be very small. Thus all his conclusions about 'impact theory' are simply special pleading. It is true that the 'interval' in space-time between any two events on the world line of a photon is zero, but in this complex four-dimensional space the statement turns out to embody Einstein's hypothesis about the invariance of the velocity of light.

I have little doubt that the key to understanding Special Relativity is to concentrate on unaccelerated motion and on events. Accounts in terms of such concepts as time diltation (time intervals), and especially of the so-called Lorentz-Fitgerald contraction (space intervals) tend to run into difficulties, which can usually be resolved as soon as one takes the trouble to specify the events involved.

C.F. Coleman
Grove
Oxfordshire

Reference

C.F. Coleman, *Eur Jour. Phys.* 4(1983)240-247

In view of the interest in relativity shown by many *Wireless World* contributions, it is opportune to draw attention to the experimental discovery reported by E.W. Silvertooth in the 14th August issue of *Nature*. His findings are important because they clearly invalidate Einstein's Principle of Relativity. I have had the opportunity of discussing this experiment at some length with Mr Silvertooth and offer the following comments on its significance.

Essentially, it has been found that the spacing between nodes in a standing wave set up by two oppositely-directed light rays from the same laser source varies with orientation of the apparatus. The effects observed, which were monitored over several months, indicate a detection of the Earth's motion relative to the preferred frame in the direction of constellation Leo, at a speed indicated by a typical measurement to be 378 km/s. This should be impossible according to Einstein's theory.

The feasibility of the experiment, which was sponsored by the US Air Force, depended upon the prior development and fabrication of a special type of standing wave sensor, described in *Applied Optics* in 1983[2]. This could scan through the laser beam and detect the position of the standing wave nodes with high precision.

Crucial to the justification for the experiment was the realization that the null finding of the 19th century Michelson-Morley experiment is not a conclusive test of light speed anisotropy. It merely tells us that the round-trip transit of a light signal over a given distance is invariant with motion or change of orientation of the apparatus. Though consistent with isotropy, this can equally be consistent with anisotropy, meaning that the light speed in opposite directions along any given orientation of the light path and referenced in the preferred (ether) frame can be the same, but that it might vary with the orientation. There could be physical interactions between the two waves that set up the standing wave system, modifying the common light speed in the preferred frame.

An example of such a condition is provided by a resonant spherical cavity of fixed radius R moving through the preferred optical frame. It can sustain radial oscillations of fixed round-trip duration 2R/c in a direction of a motion-component velocity v given by:

$$R/(c'-v)+R/(c'+t)=2R/c$$

provided the light speed increases from c to c' equal to $c(1+v^2/c^2)$. The conventional interpretation of the Michelson-Morley experiment is that c' must equal c, but this need not be the case if we accept light speed anisotropy.

In reality one can suppose that the properties of the ether are regulated by resonance effects which account for the null of the Michelson-Morley experiment. However, the properties just described must then show up in an experiment which measures the spacing between the standing wave nodes and this is exactly what the Silvertooth experiment accomplishes. In effect, over the test length L, he measures the difference:

$$L/(c'-v)-L/(c'+v) = 2(L/c')(v/c)$$

and so determines the v with first-order precision.

The fact that the test gives a positive result and not a null confirms the existence of an ether and disproves Einstein's theory. It sustains an argument by Ives[3] that the Sagnac effect, by which rotation is sensed optically as if there were an ether, need not depend upon a rotation of the apparatus to justify its operating principle. Indeed, Silvertooth claims that his experiment is a linear adaption of the principles of the Sagnac experiment.

References

1. E.W. Silvertooth, *Nature, 322,* 590 (1986).
2. E.W. Silvertooth and S.F. Jacobs, *Applied Optics, 22,* 1274 (1983).
3. H.E. Ives, *Jour. Opt. Soc. Am., 28,* 296 (1938).

H. Aspden,
Department of Electrical Engineering,
University of Southampton

The vector equation below gives the magnetic field $\bar{H}$ associated with an electric field $\bar{E}$ moving with velocity $\bar{V}$ in free space of permittivity e:

$$\bar{H}=e\bar{V}\times\bar{V}$$

If $\bar{E}$ is due to an electric charge of mass m, its magnetic energy in free space of volume τ and permeability u, can be equated to the kinetic energy to obtain

$$\tfrac{1}{2}u\int H^2 d\tau=\tfrac{1}{2}ueV^2\int E^2 d\tau=\tfrac{1}{2}mV^2$$

which gives $E_n=\tfrac{1}{2}mc^2$

where $E_n=\tfrac{1}{2}e\int E^2 d\tau$ is the elec-

trostatic energy of the charge and $c=(ue)^{-\frac{1}{2}}$ is the speed of light in free space.

I would be grateful to have readers' comments on the discrepancy, by a factor of ½, between the derivation made here and Einstein's law $E_n=mc^2$

M.D. Abdullahi,
NCATC,
Zaria,
Nigeria.

Mathematical Rake's Progress

In the September letters, D. Gibson states that, if Fourier transforms are used to calculate the response of an ideal low-pass filter to a pulse input, the calculated output begins before the input pulse itself. Since he fails to mention the phase response of the filter, it seems that he has arbitrarily assumed that the filter has no affect on the phases of the components of the spectrum of the input signal.

However, in the frequency domain one cannot make an arbitrary choice of the amplitude response as a function of frequency, and then go on to do the same thing for the phase response, without running the risk of describing a physically unrealisable waveform filter, and that is what he has succeeded in doing. An amplifier with a pass band stretching from d.c. to a sharp high-frequency cut-off can be regarded as a kind of low-pass filter. It is notorious that the sharper the cut-off is made the more rapid is the phase variation near cut-off, and the more ringing one sees on the leading and trailing edges of square-wave pulses passed through it. If Mr Gibson carries out a calculation using realistic values of the phase shifts produced by his low-pass filter he will find that the response to any feature of the input waveform comes after that feature, as it should.

The conditions under which a filter transfer function may represent a physically realisable filter have been expressed in terms of Hilbert transforms (see Bracegirdle's book on Fourier transforms). It is worth remarking that Fourier transforms can be applied to functions of spatial coordinates as well as to functions of time, in which case the preferred direction given by 'time's arrow' does not exist, and the entities analogous to filter transfer functions are subject to no such limitations. Entities of this kind are widely used in image processing.

C.F. Coleman
Grove
Oxfordshire

Reference
R. Bracegirdle, The Fourier Transform and its Applications, McGraw-Hill Electrical and Electronic Engineering Series, McGraw-Hill, New York, 1965.

S5/8

In reply to Mr Hayward's letter in the September issue, I agree that international standardization is desirable and has always been a goal I have sought to achieve, and will continue to seek after S5/8 becomes a British Standard.

Criticizing the quality of DIN connectors is as pointless as praising the quality of D-type connectors. Any connector for which there is a volume demand will attract the attention of the 'cheap and nasty' manufacturers and I have seen atrocious examples of both D-type and "Centronics" type multiway connectors as well as very poor quality BNC and UHF coaxial types. Judging DIN connectors by the worst available is a gross injustice. There are high quality metal-shell versions available, such as those maufactured by Futters (as sold by RS and Verospeed), that are quite satisfactory for both home and office use. Beyond that, I am confident that Belling-Lee would be pleased to inform you of the industrial and military success of the Bleecon DIN range. The already popular DIN connector is all set to become even more widely used for data interconnections, with the inevitable trend toward serial transfer.

Beyond quality of construction, how much performance is one to expect from a connector fitted to an item of IT equipment with a three or five year life? Just how many insertions and withdrawals is it likely to experience?

The question of bipolar signalling is a non-question. It is nothing more than a historical hangover from polarized teleprinter magnets and has no place in modern, low-power, single supply equipment. The choice of Epson portable equipment, as an example, for the use of RS-232 in a battery-operated environment is interesting, as it proves my argument. Yes, Epson have put an RS-232 interface into all three of their portable computers. However, in the PX-8, the RS-232 interface adds over 55 components and such a significant battery drain that a software-controlled switch circuit is used to disable it when not in use, to save power. Epson first used this approach in their HX-20, where turning on the RS-232 interface roughly doubles the battery consumption. The lower the power consumption of the processor, memory and display, the worse the problem becomes.

The use of RS-232 voltage levels by Epson to communicate with their PF-10 battery disc drive is particularly pointless. It is a non-standard interface, communicating in a non-standard protocol at a speed outside the RS-232 specification, and solely dedicated to Epson equipment, yet they felt it necessary to go to considerable expense to use bipolar signalling in units that will never be called upon to interface with RS-232 equipment and where there is no technical justification whatever for using d.c.-to-d.c. converters to generate bipolar voltages to send data down one metre of cable.

There is no question that differential, balanced signalling, such as RS-422, or the newer RS-485 party-line system, offers better data transmission, but at a financial and power cost which has rendered it less popular. RS-449 was not exactly a runaway success and, more recently, the SCSI standards offers the choice between single-ended (open-collector) or differential interfaces – guess which most manufacturers are implementing!

The MC3486 receiver and MC3487 transmitter are out of the question for battery-operated equipment, together having a worst case power consumption of 190 milliamps, a value embarrassing even in mains equipment, with devices such as the highly capable 64180 containing processor, memory manager, dual uarts, bit-rate generators, dual timers, interrupt controller, dual d.m.a. controller and clock generator, all consuming only 20 mA maximum at 4MHz. The days of "200mA doesn't matter" thinking in equipment design are over. Also, I do not consider the MC3486/7 pairing as low-cost. It costs at least three times as much as 1488/9 combination and over five times as much as an HC14 gate, not taking into account the cost of providing that 200mA and ventilating the heat dissipation thereof!

Finally, to achieve a "plug-and-run" interface (the goal of S5/8 and, I suspect, the goal of us all), a standardized data structure is essential. Eight-bit working is now the norm and there is no point in accommodating shorter word sizes by changing the framing. Lateral parity is also questionable. It is seldom possible to organize retransmission on a per-character basis and block error detection is much more efficiently performed using CRC, or similar, techniques and does not impose a 10% burden on the character frame.

On the question of the data signalling rate, the increasing use of microprocessors in devices removes the need to match the data interchange speed to the physical restrictions of the attached device. 9600 bauds is already a de-facto standard with IBM, DEC and Intel and is being incorporated into new standards including the ISO Standard 'smart cards'.

A.S. Hardie
Technical Director
Oval Automation Ltd
Littlehampton
Sussex.

Having reviewed the early proposals for S5/8, and contributed, along with many others (all unheralded), to the development of a useful idea into a proposed standard, I read Mr Hardie's articles with interest and waited for the criticism. So far, you have published only one letter (from L. Hayward, September, p.13) and, as expected, his criticisms miss the point in all except one area: the lack of international agreement.

Mr Hayward objects to using DIN circular (audio) connectors on the grounds that they are not