LETTERS

with these questions for some thousands of years. To make a useful contribution, *Wireless World* should perhaps give them rather more careful thought than is evident in the editorial.

There seems no case for saying that loyalty can exist only between people. Loyalty is given to ideas and principles, not to people as structures of flesh, bones and hair nor to organizations as management structures. Ideas and principles may be embodied by organizations as much as by people: surely an organization such as The Red Cross must be allowed as an example of this.

The argument that organizations are motivated only by self-interest can be applied equally to people. One can say that any person's good action is done ultimately for the resulting glow of self-satisfaction, but this is a sterile line which puts an end to any useful debate.

Wireless World seems very ready to judge for us whether our loyalties are worthy or not. Are you, or is anybody, really in a position to dismiss as unacceptable the possible loyalty of a wife to her mass-murderer husband? In any case you are really suggesting only the replacement of conventional allegiances by loyalty to the concept of 'Engineers Against Evil.' If some engineers decided to uphold their principles with action rather than just words, you might even find yourselves advocating loyalty to a terrorist group!

D. P. Leggatt
Farnham,
Surrey.

DEATH OF ELECTRIC CURRENT

I believe Ivor Catt bases his theory on Heaviside's "the current in the wire is set up by the energy transmitted through the medium around it."

Chapter ten of Hertz's book 'Electric Waves' is a reprint of his paper 'On the Propagation of Electric Waves by Means of Wires' first published in 1889, a year after the experiments which made him famous. The purpose behind the experiments described in this later paper was to test Heaviside's and Poynting's theory that, as Hertz wrote, "the electric force which determines the current is not propagated in the wire itself, but under all circumstances penetrates from without into the wire. . . ." Hertz went on to say "As a matter of fact the theory was found to be confirmed by the experiments which are now to be described; and it will be seen that these few experiments are amply sufficient to support the conception introduced by Messrs Heaviside and Poynting."

Hertz then described a set of experiments which used his invention of the coaxial cable and the balanced feeder or transmission line, and concluded his paper, "On studying the experiments above described, the mode in which we have interpreted them, and the explanations of the investigators referred to in the introduction, one difference will be found especially striking between the conception here advocated and the usually accepted view. (Weber's theory of electricity carried by charged particles acting instantaneously at a distance.) In the latter, conductors appear as the only bodies which take part in the propagation of electrical disturbances – non-conductors as bodies which oppose this propagation. According to our conception, on the other hand, all propagation of electrical disturbances takes place through non-conductors; and conductors oppose this propagation with a resistance which, in the case of rapid alternations, is insuperable. We might almost feel inclined to agree to the statement that conductors and non-conductors should, according to this conception, have their names interchanged . . . "

Hertz was even more specific in his Supplementary Note No. 24. "By the experiments in the following paper it is pretty plainly proved that in the case of rapid variations of current the changes penetrate from without into the wire. It is thereby made probable that in the case of a steady current as well, the disturbance in the wire itself is not, as has hitherto been assumed, the cause of the phenomena in its neighbourhood; but that, on the contrary, the disturbances in the neighbourhood of the wire are the cause of the phenomena inside it."

Catt's critics have a choice: either Hertz was a crank and a crackpot, or he was, as an experimenter and detective, in the same class as Faraday. If Hertz's diagnosis of his experiments with a transmission line is correct, the effect we call a current is caused by "the disturbances in the neighbourhood of the wire," what, in the neighbourhood of the wire, is being disturbed? Maxwell's ether?

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ELECTROMAGNETIC DOPPLER

If two objects have a relative velocity then the distance between them is changing (by definition). Anything that travels from one object to the other at regular intervals will travel different distances on successive trips. If the two objects are travelling away from each other then each trip will be longer (in distance) and will take longer (in time) unless the thing making the trip (wave crest, photon, bullet, or jogger for that matter) increases its speed to compensate for the increased distance. The fact that consecutive trips take longer (in time) means that consecutive arrivals are further apart (in time) than the corresponding departures. This is called the Doppler effect.

Special Relativity and Newtonian physics predict different values for the Doppler effect: they do not invoke different mechanisms. The mechanism is that unless you increase your speed you'll take longer to travel a greater distance.

You don't have to like Special Relativity but you must accept that it "explains" Doppler shift just as well as Newtonian physics does – indeed, the "explanation" is the same for both systems.

In your July issue, S. Kennaugh derives a formula for the magnitude of the Doppler effect. He describes his derivation as commonsense, elegant, but heretical. His derivation is Newtonian and relies on two assumptions (velocities can be added like pure numbers, and wavelength of a electromagnetic wave is constant to all observers) that are valid for Newtonian but not for special Relativistic physics. Neither of these assumptions is needed – I offer the following: Observer B is receiving radio signals transmitted by A at frequency f. A is moving at velocity v relative to B. "Wave crests" are transmitted at intervals of 1/f, during which time the distance from A to B increases by v/f. This extra distance is covered in time v/fc (c being the velocity of radio waves measured, like v, relative to B). Wave crests arrive at B at intervals of (1/f + v/fc) which corresponds to a frequency of fc/c+v – a Doppler frequency shift factor of c/c+v.

This derivation is correct for both Newtonian and Special Relativistic physics – provided that all values are observed by B. Note that (aside from the symbol 'c' for speed of wave propagation) there is nothing specific to electromagnetic Doppler in this derivation; it is equally correct for sound waves, or for any periodic transmission across an increasing distance.

In your July issue, J. Kennaugh raises another aspect of Special Relativity: suppose that the same radio wave is observed by two observers who are moving relative to each other. They each observe a different frequency for the wave but they observe the same propagation speed (c). Since $V = f\lambda$ holds good for all observers, these two observers obtain different wavelengths for the same wave. The two observers disagree about the length of the same physical "object". J. Kennaugh, it seems, does not like this prediction of Special Relativity. Although I can only agree with JK that Newtonian physics is simpler in many ways than Special Relativity, that doesn't make it correct.

In case your readers should think that I'm trying to avoid discussing the more disturbing predictions of Special Relativity, let me return briefly to the question of observed wavelengths. Special Relativity predicts (as pointed out by JK) that two observers of the same wave will see different wavelengths. Imagine now that one observer measures the time taken by the radio wave to travel between two objects at a distance of say in wavelengths apart. The other observer times the same radio wave travelling between the same two objects. Now Special Relativity predicts that the two observers measure a different distance (between the same two objects) traversed by the same wave at the same speed. They measure a different amount of time for the same physical event!

Predictions like these may be hard to understand and even harder to believe but it is important to realise that they are not contrary to logic. I'm sure that nothing I can say will shift JK (and many many others) from his view that Special Relativity contains some basic logical flaw, but if you are happy to continue publishing our letters then I am happy (for a while, at least) to continue defending Special Relativity.

Before I finish this letter I must challenge two things raised by James L. Smith also in your July issue. Firstly he suggests that the existence of a wave propagation medium (ether) changes the observed Doppler effect; as I've already explained, this is not true. Secondly he offers a choice between "Einsteinian" and "Non-Einsteinian" systems; this is a ludicrous oversimplification – Einstein proposed at least two systems (Special and General Relativity) and there are almost unlimited alternative (non-Einsteinian) systems – some predict an "ether", others do not.

S. J. Hobson
Hampton
Middlesex

HERETICS' GUIDE

It is reassuring to hear that the idea of the wave/particle duality or complementarity of light, "current half a century ago" (and chosen by Niels Bohr as his motto when knighted by the King of Denmark), is no longer a required belief in modern physics. Yet Mr Coleman (July 'Letters') continues to rely on the duality concept, even in this letter. In his fifth paragraph he refers to Doppler shift, a wave phenomenon (incidentally, is it really the ordinary conductivity electrons that radiate with

this Doppler shift, and how is it measured, please?); then he speaks of "a photon hitting the surface of a metal" and of "an area comparable with the square of its wavelength" in the same sentence. Duality may be dead but it seems reluctant to lie down!

I asked earlier why it was that only one of these (millions of) electrons is ejected by the impact of a single photon. Mr Coleman says the answer is simple, but he does not supply it. The work function (escape energy of a conductivity electron) from a metal surface is measurable, and no visible or ultra-violet photon carries more than a few times this escape energy. If that energy were distributed equally or even thermally among millions of electrons not one of them would have enough energy to escape – yet one of them does. Why? The quantum theory does not try to explain this but, characteristically, side-steps the real question by *assuming* the observed result.

The bunching of visual photons measured by Dontsov & Baz' (*WW* Letters, May, 1983) may perhaps assist in accounting for the interference phenomenon, but it is not required in order to explain the so-called "diffraction" of electrons or gamma-rays. (I have not felt entitled to ignore such facts – indeed, the early articles were concerned with little else!). The mechanism need not be wavelike. In a pin-ball game the ball bearings follow preferred tracks and finish up in preferred places that are predictable statistically but unpredictable individually. This is just what is observed when electrons and photons pass through crystals and are "diffracted". A pin-table doesn't look much like a diffraction grating to me.

W. A. Scott Murray
Kippford-by-Dalbeattie
Galloway

Dr Scott Murray on page 35 of the May 1983 issue of *Wireless World* uses the seepage of water through sand as an analogy for the tunnel effect. A more satisfactory analogy is that of frustrated total internal reflection of light waves.

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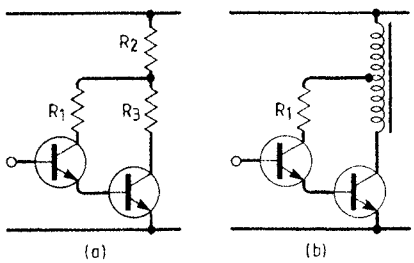
DARLINGTON DEVIATION

Mr Gray has pointed out (*WW*, March, 1983, p.62) the limitation of the Darlington in saturation and the advantage of the independent emitter-follower drive. When the main transistor is a power switch, and more especially when it is one of the devices which is operated at $I_b = I_c/5$, an intermediate configuration offers advantages.

The general form is shown in Fig. (a), and (b). In Fig. (a), R_2 and R_3 form the load to be heated. It is obvious that R_2 carries the total current in the two transistors. Provided that the first transistor is robust enough to accept a wide tolerance of current, the drop in R_3 need only be $(V_{CE\ Sat\ 1} + V_{BE2} - V_{CE\ Sat\ 2})$, about half a volt, and the load current is then, for the typical case, increased by some 10-15%. The introduction of R_1 gives better control at the price of lower efficiency.



When an output transformer is used, the circuit of Fig. (b) allows the drive transistor to be limited to anywhere between V_{CC}, for the emitter follower form, and $2V_{CC}$ for the Darlington.

T. Roddam
Arundel
W. Sussex

SATELLITE TV AERIAL ALIGNMENT

I must agree with all the difficulties that N. L. H. Cresdee describes in directing a satellite aerial dish. The problems are especially acute using the 12GHz band, with necessarily large dishes at present in use. In addition, if the receiving lens or reflector is slightly off-focus or boresight, the lobe pattern of the aerial is quite disturbed and you can easily end up optimising for a false or poor maximum.

However, there is one invaluable aid, both to directing and focusing that was not mentioned, and that is the sun. Far from the rarity it would appear to be in the UK, a visual sighting of the sun reflected on to your receiver lens will give you a positional fix close to that of the satellite orbit. The accuracy of this fix depends solely on your ability to know or calculate the local Solar Time.

J. Emmett
Supervisory Engineer
Thames Television

Sundials: Their Theory and Construction. A. E. Waugh, Dover.

RECHARGEABLE H.T. BATTERY

OVER recent months it has been my task to sort over the "debris" of 60 years of life of one family in the one house. One particular item that I have "found" is a Milnes rechargable h.t. battery (6×120 volt) that I know for fact has not been recharged or used in any way at all since 1956 – and almost certainly was last charged not later than 1952. Imagine my astonishment when a quick check with a multimeter revealed that several of the individual cells still – after 30 years or so – show a potential of at least one volt. I am writing to your journal because I know that your circulation penetrates those institutions where the "brains" are. Someone, somewhere – possibly engaged in storage battery research – could well be interested in this Milnes unit. I feel it would be criminal of me to cast it onto the rubbish heap without first making the effort to put it where it perhaps could be of use. I had over 35 years in radio communication and I only ever saw just two Milnes in my life – this one that my father bought sometime in the 1930's and one other where I first worked in 1943 at Somerton Radio Station: there cannot be very many in existence. I feel that a high percentage of those engaged in radio and electronics today would not even know what it was if one showed it them – hence my approach to you.

W. B. Pash
Somerton
Somerset

FORTH COMPUTER

In his article on a Forth Computer, Brian Woodroffe takes the dangerous step of comparing microprocessor c.p.us by preparing a number of examples of small isolated sections of code.

Whilst I do not wish to take a standpoint in favour of any particular device I would like to point out that this sort of comparision is, at best, worthless and can be misleading. To quote one counter example, the 8088 '+' operation could be carried out via the instructions:

```
POP  AX
MOV  BP  SP
ADD [BP]  AX
```

equal to the 6809 in terms of instructions or, if BP has a fixed relation to SP as is the case in most executing programs:

```
POP  AX
ADD [BP +α]  AX
```

where X is an assembly time constant. I hasten to point out that I am not trying to challenge his choice of processor but simply to point out that his reasoning is flawed (I have no doubt that any software engineer (sorry Mr Catt) familiar with the other c.p.u. mentioned could improve upon the quoted examples.

J. O'Connor
Crewe

BBC ENGINEERING

May I reply to R. G. Brown (June 'Letters') whose letter I certainly take as constructive criticism rather than abuse.

First of all, most non-news recording in the BBC today is effected on 1 in helical machines, the earlier 2 in quadruplex equipment being progressively phased out. The technical quality obtained with the 1 in helical system is unquestionably superior to that on quadruplex.

Secondly, a lower standard of recording quality is sensibly accepted for e.n.g. work, in the interest of portability. But e.n.g recording quality could not possibly be matched by a domestic video recorder.

So equipment and engineering standards can certainly not be blamed for Mr Brown's dissatisfaction.

On the other side of the coin, programme makers will always push their technical facilities to the limit. Any improvements in effective sensitivity (ie signal/noise ratio) or other technical parameters will tend to be exploited to give additional programme flexibility in the way of darker or higher-contrast scenes, even more multi-generation dubbing for editing purposes, and more complex special effects. This push for flexibility is very proper and it is the business of engineering to continue with improvements which offer it; but of course a balance must be struck between programme flexibility and technical quality, such that the major part of our audience is pleased with the former and content with the latter. I'm sorry if to Mr Brown's eye we have overstepped the line at times but the final judgements are subjective.

As engineers we remain continually vigilant to see that the flexibility/quality balance is about right. Comments such as those of Mr Brown are useful aids to this vigilance.

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