

# CORRESPONDENCE

## TAC

SIR,—I read with very great interest your article on the TAC Report (*Nature*, **241**, 2; 1973).

Our company, which through the Greenwich local television channel can claim, we believe, some responsibility for broadening the minds of the legislators, was similarly disappointed with the TAC Report. However, our trade association—the Cable Television Association—has already reacted by stating that it will shortly be producing its own plan for the future of broadcasting in this country. I am convinced that this will promote lively discussion about the great potential and versatility which cable offers.

Yours faithfully,

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## Special Relativity Again

SIR,—Professor Ziman's admirable review<sup>1</sup> of Professor Dingle's book *Science at the Crossroads* covers most adequately "the question" raised by Dingle about special relativity, except for one point: Ziman invokes general relativity at a stage when it is not really needed.

In fact, while special relativity does not deal adequately with gravity, it does quite adequately cope with accelerated motion. In special relativity, just as in general relativity, the answer to Professor Dingle's "question" is: the fastest working clock between any two events is one that travels between them by free fall. Any other clock travelling between these events necessarily experiences inertial forces, which a physicist moving with the clock might interpret as being due to a (uniform) gravitational field; a physicist moving with the "fastest" clock would experience no such forces (he would be an "inertial observer").

This completely answers Professor Dingle's "question". It leaves unsettled the further question as to what it is that prescribes this particular structure for space-time. In special relativity, this structure is simply taken as given *a priori*; while this may not be thought to be a completely satisfactory answer (and general relativity gives a better

one), it is certainly at least a logically consistent answer.

Yours faithfully,

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<sup>1</sup> *Nature*, **241**, 143 (1973).

## Reprint Requests

SIR,—The interesting article by Briggs and Briggs<sup>1</sup> on reprint request patterns under the deliberately misleading title "Hormones and Blood Chemistry" has moved me to make several comments about the reprint courtesy.

In 1970, some similar experiments on information retrieval techniques were conducted in *Nature*<sup>2-4</sup>. At that time, I had a pleasant exchange of correspondence with one of the principals (V. R. Pickles of Cardiff) through which we found ourselves to be in general agreement regarding uses and abuses of the reprint privilege. However, several mistaken impressions still appear to be fairly generally persistent.

There can be no argument that there is abuse of the reprint privilege by people who could determine whether or not they really need a reprint before they order it, by dabblers, and by habitual collectors ("scientific pack rats"). However, there is another side to the requesting of reprints through information gained from sources such as *Current Contents*. Many scientists work in places which are relatively remote from adequate library sources and they must gamble a bit on judgments about *Current Contents* titles to stay in the literature in their fields. If I might use myself as an example, I freely admit to errors in reprint ordering in the past and I acknowledge that I have some reprints which I can't use, but in most of these cases I was misled by titles. It would be impossible for me to check the actual contents of some journals without travelling literally hundreds of miles. I, and others in similar positions, must beg the indulgence of colleagues and we ask not to be lumped with the careless and abusive users of the reprint privilege. Of course, it could be argued that almost everyone should have access to *Nature*, but even this might not be the case.

I also sense that some of the concerns about reprint ordering by American scientists may arise from some mistaken impressions which are held by some workers in other countries concerning general working conditions in science in the United States. While a few workers here may have such financial and/or technical resources available that they can depend exclusively upon computer-based information retrieval done by hired assistants, most of us carry on the day-by-day slog through the literature which is the common burden of scientists everywhere. Hopefully, workable modern information retrieval will continue to become more readily available to everyone everywhere in the next few years.

Finally, one might ask how often these "experiments" need to be conducted in *Nature*. Publication costs and space limitations certainly would enter into such a determination. If further research is needed, possibly the editors could design even better "experiments" if they would construct an occasional *Nature* entry which incorporated into a single title such terms as "cancer, heart disease, racial differences in IQ, energy crisis, ecocide in Vietnam, and biological basis for female superiority".

Yours faithfully,

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<sup>1</sup> Briggs, M. H., and Briggs, M., *Nature*, **240**, 490 (1972).

<sup>2</sup> Davies, D., McKenzie, D. P., Turner, J. S., and Pickles, V. R., *Nature*, **225**, 636 (1970).

<sup>3</sup> Pickles, V. R., Davies, D., McKenzie, D. P., and Turner, J. S., *Nature*, **226**, 881 (1970).

<sup>4</sup> Pickles, V. R., *Nature*, **226**, 1181 (1970).

## Nature's Parish

SIR,—At this juncture to ask the "academic community" to give Dr Kissinger "credit for his liberalizing influence in the past four years" is remarkable. As the use of violence during the Kissinger era has been liberal to the utmost extent, this statement (*Nature*, **241**, 1; 1973) ought, in fact, to be remembered and highly valued. The editor should be given credit for having given the ultimate expression to the complicity of the aca-

demic community in the crucifixion of Vietnam.

Yours faithfully,

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## Who was HeLa ?

SIR,—It is twenty-one years since George Gey established the famous HeLa cells in culture. It has been estimated that the weight of these cells in the world today exceeds that of the American negro from whose cervical tumour they originated. That lady has achieved true immortality, both in the test-tube and in the hearts and minds of scientists the world over, since the value of HeLa cells in research, diagnosis, etc., is inestimable. Yet we do not know her name! It has been widely stated that He and La are the first letters of her names but whereas one textbook says the names were Helen Lane another says Henrietta Lacks. My letters to the authors, inquiring the source of their information, like the letter to the hospital from which Gey's paper emanated, remain unanswered. Does anyone know for sure? Would it be contrary to medical ethics in the HeLa cell's coming-of-age year to authenticate the name and let He . . . La . . . enjoy the fame she so richly deserves?

Yours faithfully,

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## Entropy and Vitalism

SIR,—Without even having read my book<sup>1</sup>, Van Kley<sup>2</sup> refers to it as "a new form of vitalism" such that for evolution "different forms of the laws of thermodynamics apply". This is such a gross misinterpretation that I am compelled to object.

On page 22 I state: "I think our classical notions of entropy as they come to us from the presently established laws of physics and chemistry are totally inadequate in dealing with the living system. This does not mean that there is anything mysterious, supernatural, or vitalistic about the living system. It simply means that our classical notions are inadequate".

I should like to stress the word inadequate. For example, the laws of Newtonian mechanics are totally inadequate in explaining the shift in the perihelion of Mercury. Einstein's equations, which explained this quantitatively, are different in the sense that they are more general; Newton's equations are just a special case.

The concept of entropy in informa-

tion theory is far more general than in classical thermodynamics. Specifically, the entropy,  $H$ , as defined by Shannon<sup>3</sup>, is

$$H = -K \sum_i p_i \log p_i \quad (1)$$

where the  $p_i$  are probabilities of elementary events on a finite probability space and  $K$  is an arbitrary constant. If the  $p_i$  are all equal, then

$$H = -K \log p_i \quad (2)$$

or

$$H = k \log W \quad (3)$$

where  $W$  is the total number of elementary events on the space. But (3) is Boltzmann's definition of the thermodynamic entropy which appears as a special case under Shannon's more general definition.

Schrödinger<sup>4</sup> foresaw that we have given a positive name, entropy, to a negative concept—a measure of a kind of disorder. He proposed that we use the negative value of the entropy, the "negentropy", as a measure of the order or organization. I believe that Schrödinger was wrong. The true measure of the organization is the maximum value of the entropy,  $H^{Max}$ , minus the value we actually observe,  $H^{Obs}$ .  $H^{Obs}$  as a measure of the disorder has no structure, but  $H^{Max} - H^{Obs}$  as a measure of the organization is rich in mathematical structure which classical theory neglected but which my theory stresses. It is in this sense a re-definition and extension of the entropy concept.

Consequently, I believe my work reduces the aura of vitalism man has always associated with the living system.

Finally, Van Kley certainly cited the wrong reference for any anti-evolutionary statement. Chapter 9 of my book is initiated by the following quotation from "The Giants" by Kahlil Gibran.

"I am among those who believe in the Law of Evolution; I believe that ideal entities evolve, like brute beings, and that religions and governments are raised to higher planes.

"The Law of Evolution has a severe and oppressive countenance and those of limited or fearful mind dread it; but its principles are just, and those who study them become enlightened."

Yours faithfully,

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<sup>1</sup> Gatlin, L. L., *Information Theory and the Living System* (Columbia University Press, New York, 1972).

<sup>2</sup> Van Kley, H., *Nature*, **240**, 365 (1972).

<sup>3</sup> Shannon, C. E., *The Mathematical Theory of Communication* (University of Illinois Press, Urbana, 1949).

<sup>4</sup> Schrödinger, E., *What is Life?* (Cambridge University Press, London, 1944).

## Synthetic Food

SIR,—The present is an especially opportune time for the initiation of a massive, interdisciplinary programme of research and development on the total synthesis of food.

Political as well as scientific leaders are coming to realize that agriculture, in the race with population, can at best only maintain the present 2,000-calorie-a-day diet in the developing countries. The "Green Revolution" and other recent advances are serving to gain time, but in a few years the population will outstrip the food supply unless the growth of population is quickly checked—an unlikely possibility—or unless an independent source of food is developed—a possibility that can be realized.

Two circumstances favour the immediate initiation of a major programme for the total synthesis of food. First, there is the availability of many scientists, engineers, and other experts who are now unemployed and would respond with alacrity to a new and challenging opportunity. Second, industry is at a stage at which it could adapt the vast fund of scientific knowledge and engineering experience amassed in the manufacture of synthetic polymers to the production of food.

Why has not a start been made? The answer lies in the problem of securing support for a programme of sufficient magnitude and duration to assure success. Experience in the administration of research has shown that support for a major, imaginative new programme can be obtained only after those proposing the programme have already made a significant beginning on their own resources. Research laboratories today that are competent to undertake a programme on the total synthesis of food already have a full complement of productive projects. Thus a new programme could be undertaken only at the sacrifice of currently successful activities.

The situation is similar to that which led to the beginning of the plantation rubber industry in 1876. Henry Wickham, later Sir Henry, discovered the unusually quick germinating characteristics of the seed of the *Hevea brasiliensis*. He chartered a steamer to bring seedlings growing in baskets of earth from the Amazon to London. Sir William Hooker, Director of Kew Gardens, threw out a collection of rare orchids to make space for the tender, little known seedlings until they should be ready to send to Ceylon, and later to Malaya. Since that time the billions of rubber trees on plantations have all been descendants of these original specimens.

Are there Britons today who have the