Correspondence

Reprint Communication

SIR, One of the important communication chains in science is the distribution of reprints by the author of a published journal article. Authors differ widely in their habits of reprint distribution: some send out great quantities wholesale, while others modestly send out just a few or even none.

We write to plead that authors at least send reprints to living authors of works in the bibliography of the reprinted article. Both of us have had the surprising experience of discovering much later that one of our publications had been cited in an article that we would have wished to study earlier.

The reasons for sending reprints to cited authors are, as we see them, first courtesy, and second (more important) using a bibliographical connexion to help move information along the network of scientific connexions. Distribution of reprints to previous authors increases the opportunities for serious criticism and communication of current developments. The earlier author will have often become an information centre for his specialized subject.

Yours faithfully,

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Scientific Responsibility

SIR,—Regarding Dr Siekevitz's article (*Nature*, **227**, 1301; 1970), I should like to point out that it is no use discussing scientific responsibility without first making clear what value judgments are accepted when defining responsibility. Value judgments are related to what one considers the ultimate end of existence and the following analysis might prove helpful.

In the classical view of the world, which was accepted also by science up to, say, the middle of this century, human existence was considered as a given and static fact. In those conditions, metaphysical analysis leads to the conclusion that the aim of being is being, as Schopenhauer so brilliantly demonstrated. In other words, no other aim can be found for an individual (or collective) existence than to go on to exist. It is easy to show that all desires, aims and pleasures can in fact be reduced to this one aim, behind which no other hidden finality can be discovered. But because being in this world cannot realize its final goal, all existence having an end, this view is self-destructive, as are our ethics and the society which are derived from it. The only escape is to deny the reality of existence and to transpose it in another non-physical world. However, as science increased the scope of physical explanations and consequently appeared to increase the reality of physical existence, this route of escape became less and less plausible.

Because science also increased the power of human beings, it enabled them to pursue their aim with greater and greater destructiveness, without coming any nearer to it. This, in short, is the reason for the present unrest.

Science itself offers a ray of hope. It stems from the discoveries of Darwin, the implications of which are only now, one hundred years later, being fully grasped. The view of the world as a world of evolution enables us to conclude that the aim of being is becoming. This is, of course, what Julian Huxley, Bertrand Russell and Teilhard de Chardin have said before, and contrary to the former view that the aim of being is being, it is not self-destructive. What is perhaps not so easily appreciated are the very important practical consequences of such a philosophy for everyday life. For it follows immediately that every individual is responsible not only for his existence, but for all future existence, which is much more important in quantity and quality. This means a shift of emphasis from "rights" to "duties", from exploitation to conservation, from (material) living standard to ecological reality, from consumption to recycling, from economics to biology. It means more and not less science, but it means the end of politics, business, economics, and trade unions as we know them.

By the way, it means the end of war as understood by Dr Siekevitz, but his obsession with this problem looks trivial when one considers the much more formidable problems which must be solved before.

Finally, it enables us to distinguish without difficulty between sincere and insincere contestation; the former defining new duties, the latter seeking new rights.

Yours faithfully,

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Accelerating Somatic Cell Genetics

SIR,—Under the above title you write (*Nature*, **228**, 318; 1970): "The standard technique for detecting linkage between human genes and assigning them to particular chromosomes involves correlating the chromosomal make-up with the biochemical properties of interspecific hybrid cells".

I think it is a little premature to refer so soon to this technique as "standard", and to do so—however unintentionally—is to slight the work of those who have given us most of what we know about the linkage map of man. I refer to the standard method of observing segregations in families, and inferring from them linkage groups and map distances. This has been at its most successful in the assignment of loci to the X chromosome, but several autosomal linkage groups have been established, and in some cases assignments to particular autosomes are well supported, the segregation data then being supplemented by cytological information.

The hybridization technique is a most important new development, but its very novelty precludes the use of the word "standard" to describe it.

Yours faithfully,

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Definition of Molecular Weight

SIR,—Many biologists are now using the dalton as a unit of mass, but according to information from Dr W. E. Cohn, no Commission of any of the International Scientific Unions, or indeed any other body, has officially recognized this unit. Its usage brings again into discussion the question of the definition of molecular weight and the most appropriate units to express it.

What is the dalton? Its meaning is clear from usage and from current definitions of standards of atomic weight[1].

It is a unit of mass, which can be defined by the statement that one atom of the carbon isotope ^{12}C has a mass of 12 daltons. Thus one dalton equals N^{-1} g $= 1 \cdot 663 \times 10^{-24}$ g, where N is Avogadro's number. This is obviously identical with the officially defined "unified atomic mass unit" or a.m.u.[2]. It is also identical with the "avogram", a name proposed for this mass unit in 1949, approved in 1950 by the American Chemical Society on the recommendation of its Committee on the Nomenclature of Physical Chemistry (chairman, T. F. Young)[3].

What is meant by molecular weight, and in what units is it to be expressed? The Commission on Physico-Chemical Symbols, Terminology and Units (I.1) of the IUPAC Physical Chemistry Division considers "molecular weight" to be the relative molecular mass of a substance, that is, the ratio of the mass of one molecule of the substance to one-twelfth the mass of an atom of ^{12}C (personal communication from Dr Martin A. Paul, of the Division of Chemistry and Chemical Technology, National Research Council, Washington, DC; secretary of Commission I.1 of the Physical Chemistry Division of IUPAC). It is therefore a pure number and is dimensionless. The Commission also defines the "molar mass", with symbol M, as the amount of a substance containing as many elementary units (properly specified by some formula such as $C_{11}H_{12}N_2O_2$) as there are carbon atoms in exactly 0·012 kg of carbon-12. Thus molar mass is commonly expressed in g $mole^{-1}$. Many references to "molecular weight" obviously mean a quantity which is identical with molar mass as already defined. For example, in the well-known Svedberg equation for the quantity M from sedimentation coefficient (s), diffusion coefficient (D) and partial specific volume ($\bar{v}$) (ref. 4):

$$M = RTs/D\ (1 - \bar{v}\rho)$$

The same is, of course, true for M as determined by measurements of osmotic pressure or other colligative properties. The dimensions of M are g $mole^{-1}$. Svedberg and Pedersen[4] and many other authors including myself[5] have referred to M as "molecular weight", and this usage, to my knowledge, has almost never been challenged. Clearly a mass in daltons is numerically identical with molar mass in g $mole^{-1}$, but it is clearly incompatible with the IUPAC definition of molecular weight.

One convenience of the dalton is that biochemists and cell biologists can describe structures for which the term "molecular weight" is inappropriate: ribosomes, mitochondria, other cellular organelles, bacteriophages, and so on. In 1959, for example, Tissières *et al.*[6] reported the "molecular weight" of 70S ribosomes from *Escherichia coli* as approximately $2 \cdot 6 \times 10^6$. But ribosomes are complex organized structures, containing many different kinds of protein molecules and several kinds of RNA molecules, and it would be more logical to say, "the mass of the *E. coli* ribosome is $2 \cdot 6 \times 10^6$ daltons", so avoiding the implication that the ribosome is a molecule.

The unit is also useful to specify the mass of the unit cell of such crystals as those of proteins, which contain water and salts or other substances as well as the molecule of primary interest. Low[7] used the avogram for this purpose in reporting her studies on serum albumin crystals. If V is the volume of the unit cell in cm^3, and ρ is its density, then the mass of the unit cell in daltons is $NV\rho$. Thus the mass of the unit cell of the "wet" human mercaptalbumin mercury dimer was 590,000 daltons, and analysis showed that 44·6 per cent of this was protein. Another use is in reporting the content of various substances in a cell. Thus a human lung cell has been reported to contain 6 pg of DNA[8], which corresponds to $3 \cdot 6 \times 10^{12}$ daltons. The content of DNA in daltons, divided by the mean residue weight of the nucleotides in the DNA, also expressed in daltons, gives the number of nucleotides in the DNA of the cell.

I believe it would avoid confusion to maintain the IUPAC definition of "molecular weight" as a ratio. The term "relative molecular mass", as Dr Paul has pointed out, is a clearer expression of the nature of this quantity than is "molecular weight". Thus it would be correct to write: "the molar mass of protein X is 25,000 g"; or "the molecular mass of protein X is 250,000 daltons"; or "the relative molecular mass (that is, molecular weight) of protein X is 25,000". All these statements are different ways of expressing the same fundamental facts. It would, however, be incorrect to say: "the molecular weight of protein X is 25,000 daltons", for the dalton is a unit of mass, and molecular weight is dimensionless. I hope the nomenclature committees of IUPAC and of the International Union of Biochemistry will be stimulated to clarify these matters and to make investigators aware of the somewhat subtle distinctions involved.

Yours faithfully,

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[1] *Handbook for Authors*, 95 (American Chemical Society, Washington, 1967).
[2] Bateman, G. M., *et al.*, *J. Chem. Educ.*, **26**, 113 (1949).
[3] Crane, E. J., *Chem. Eng. News*, **28**, 1841 (1950).
[4] Svedberg, T., and Pedersen, K. O., *The Ultracentrifuge*, 5 (Oxford, 1940).
[5] Cohn, E. J., and Edsall, J. T., *Proteins, Amino Acids and Peptides*, chap. 16, 17, 19 (Reinhold, New York, 1943; Hafner, New York, 1965).
[6] Tissières, A., Watson, J. D., Schlessinger, D., and Hollingworth, B. R., *J. Mol. Biol.*, **1**, 221 (1959).
[7] Low, B. W., *J. Amer. Chem. Soc.*, **74**, 4830 (1952).
[8] *Handbook of Biochemistry* (edit. by Sober, H. A.), first ed., H-59 (Chemical Rubber, Cleveland, 1968).

Another Review Journal

SIR,—I have two points about R. J. Blin-Stoyle's review under the title "Another Review Journal" (*Nature*, **228**, 390; 1970).

The "proliferation of journals" in physics is not adequate ground for not publishing more. This new periodical may be more useful than some already in existence.

Neither is the "sufficiency of natural homes" for each of the articles. The editors would presumably not have commissioned these articles if this periodical hadn't been published.

I suggest that many who deprecate new journals do so because they are unable or unwilling to organize themselves effectively to monitor and select published information. Summary and titles journals help these people. If such secondary journals are not effective enough, criticism should be directed at them, not at the source publications.

Yours faithfully,

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Hunting Down Pornography

SIR,—It has apparently (*Nature*, **228**, 203; 1970) been proved to the scientific mind's satisfaction that the hunting down of pornography by proper authorities is no more useful than was the annual hunting of eagles by members of some American Indian tribe who, by the way, were careful not to destroy the species.

But the missionary zeal of your Washington correspondent in committing assorted politicians to the secular hell of perversion is somewhat disturbing, especially when one remembers what happened to tribes who lost their taboos as a result of well meaning mission work.

Yours faithfully,

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