CORRESPONDENCE

Publication Speed

SIR,—Your recent editorial (*Nature*, **233**, 294; 1971) on multiple publication seems to me to put the responsibility of the journals for this practice at a lower value than the facts would merit. You merely suggest that the slowness of the whole process of publication has some effect on the willingness of scientists to "leak" to newspapers before publication. It is, however, quite clear that the frequent lack of efficiency in the processing of articles submitted to learned and technical journals could function as a strong incentive to submission of papers to several journals simultaneously. Old news is no news, in science as elsewhere, and scientists are concerned to have their material appear in print as quickly as possible. If journals appear to reject papers arbitrarily then there is, from the author's viewpoint, something to be gained from a course of action which avoids the possibility of being left six months or even a year behind the field with a perfectly good but homeless paper.

The experience of some members of this laboratory would certainly indicate the need for a certain amount of reform in the way journals are run. I myself am co-author of a paper submitted to a reputable journal in February 1970 which was at first rejected and then, having been refereed again, was accepted in a revised form. This took the remainder of the year although the actual rewriting took perhaps three weeks. This paper has still not appeared, after nearly two years. A second paper for the same journal has been refereed (this took seven months) and has just been rejected by an editorial fiat that the subject matter is not relevant to the journal's interest although the scientific merit is unquestioned. Why could this decision, if justified, not have been made by the editor when he received it? The nature of the subject matter was clear from the title and abstract. An article submitted for the features pages of the journal of an engineering institution was with the editor for three months, at the end of which time he had it set up in type, before he acknowledged receipt of the article. At this point he decided that it was more suitable for the proceedings and scientific papers section of the journal and submitted it to the papers committee of the institution. Another decision which would have been better made at the earliest opportunity.

If editors are willing to countenance in themselves such a lack of regard for the interests of contributors, then they must take the consequences, which may well include multiple publications. Authors should not be expected to sit helpless before this sort of treatment which, in many cases, decreases the value of their work. Editors should acknowledge receipt of papers, reply to authors' letters in less than the month which is common, should as a matter of course prompt referees into faster reading and should make their editorial as distinct from their scientific decisions at an early rather than a late date. They may then find themselves better served by contributors.

Yours faithfully,

I. D. GRIFFITHS

The Electricity Council Research Centre, Capenhurst, Chester

Forecasting

SIR,—In your editorial "How Much is Enough?" (*Nature*, **234**, 115; 1971) you discuss the difficulty of forecasting population growth, and you ask whether the forecaster—in this case the Government Actuary—should "settle for more modest but more realistic goals". An interesting question, but not confined to the prediction of population.

In any organization concerned with products—schools, hospitals, food supplies, aircraft, weapons—of long gestation, there is always a wish to know the future so that policies can be tailored accurately to meet world conditions as they will exist ten or more years hence. And so there is pressure to forecast not only populations but also social and political attitudes, economic conditions, technological developments and so on.

When these questions are posed, they are answered: "forecasting" has achieved a respectability undreamt of by the seers of the Middle Ages. The forecasts provided become, too often, the single set of assumptions on which planning is based. If the forecast should prove accurate, then the policy-makers are labelled "far sighted"; if not, they are, with hindsight, abused for having backed the wrong horse. But might it not be wiser to accept that forecasting is not an exact science. We have the evidence of grossly misleading population predictions; we know that political attitudes can change markedly within a few months; social attitudes appear essentially unpredictable in the longer term; technological forecasts merely reflect an obvious truth that the more money and effort is poured into any project the faster it will move. Against this sort of background, might it not be that the sensible approach is to seek to build a system reasonably matched to a wide range of possible futures rather than closely matched to a single predicted future?

Yours faithfully,

I. J. SHAW

"Woodilee", Mellersh Hill Road, Wonersh Park, Guildford, Surrey

Protecting Potatoes

SIR,—Under the headline "A Sticky End" (*Nature*, **233**, 93; 1971) your correspondent gave recognition to an interesting piece of work by Dr R. W. Gibson on the part played by glandular hairs in providing resistance to aphids in three species of potatoes (*Ann. Appl. Biol.*, **68**, 113; 1971). Gibson showed that glandular hairs on the potato species *Solanum berthaultii*, *S. tarijense*, and *S. polyadenium* discharged a gummy exudate which eventually immobilized the aphid and led to its death through starvation. As noted by Gibson this type of mechanism has potential value in the control of aphid population build-up. This effect may be further enhanced if combined with other types of resistance to aphids, such as the resistance to feeding demonstrated in several potato species, including *S. polyadenium*, by J. B. Adams at this research station. Control of aphid populations in such ways as these has the additional effect that it reduces the rate of spread of certain aphid-borne viruses.

The key to the utilization of these

traits lies in the practicality of incorporating them into the parental lines used in potato breeding programmes. Gibson recognized this, and noted that *S. tarijense* may be crossed with the cultivated potato. Your correspondent, however, gives the impression that the problems in utilizing these species are completely unknown. This is far from the case.

S. berthaultii and *S. tarijense* are diploid species and both can be crossed easily with the cultivated potato, the tetraploid *S. tuberosum*. The progeny from such crosses are sterile triploids which would still carry many undesirable traits from the wild species. However, both these species can also be crossed with haploids (dihaploids) of *S. tuberosum*, and with haploid derivatives, to give progeny which are diploid and, when the wild species is used as the seed parent, are also generally fertile. Undesirable traits may be eliminated by further crossing and selection. *S. polyadenium*, on the other hand, can at best only be crossed with *S. tuberosum* with very great difficulty.

It is clear that of the species studied by Gibson two, *S. berthaultii* and *S. tarijense*, can be used in a breeding programme fairly readily. Working at the diploid level, as I have suggested, has numerous advantages, and there are several methods of combining diploid parental lines and tetraploid cultivars at the final stage before variety selection.

Yours faithfully,

T. RICHARD TARN

Canada Department of Agriculture, Research Station, Fredericton, New Brunswick

Unambiguous Billion

SIR,—For a long time we have had to live with the annoying fact that Americans and Europeans cannot use the word billion without a risk of confusion. Perhaps it would be possible to persuade people on both sides of the Atlantic to add just two extra letters to indicate how big the billion is meant to be. To be specific, I suggest that 1 ambillion = 10^9 and 1 eubillion = 10^{12}. Thus ambillion becomes the name of the American billion—a cause of ambiguity.

Yours faithfully,

J. HILDEN

Department of Statistics, University Institute of Medical Genetics, Copenhagen

Retino-rectal Connexions

SIR,—Dr Robertson (*Nature*, **233**, 435; 1971) refers to an annoying visual condition which he calls tunnel vision. Surely he means hindsight?

Yours faithfully,

F. H. ALLEN
N. W. ISAACS

University Chemical Laboratory, Lensfield Road, Cambridge CB2 1EW

Obituary

Professor Arne Tiselius

PROFESSOR ARNE TISELIUS, who won the Nobel Prize for chemistry in 1948, died in Stockholm on October 29, aged sixty-nine.

Born in Stockholm in 1902, Tiselius moved early to Gothenburg and thence, after matriculation, to take up academic studies in Uppsala, the city which he was rarely to leave. Tiselius's interest in chemistry was first awakened at school, and his ability was noticed by The Svedberg, who quickly made him his valued and trusted research assistant. The master soon gave his protégé the freedom to plan and carry out his research. Tiselius chose to investigate electrophoresis and studied in particular the so-called boundary anomalies which occur in connexion with the electrophoretic migration of proteins. In his doctoral dissertation in 1930, Tiselius presented significant discoveries on the heterogeneity of serum proteins. After an important year of research at Princeton, he continued his development of moving boundary electrophoresis and designed the apparatus which made his name internationally known, the chief improvement being the elimination of convection. Tiselius showed by means of this apparatus that the electrophoresis of serum gave five distinctly migrating boundaries. These are albumin, and α, β and γ globulin as Tiselius designated them, and also a boundary dependent on the protein concentration. This work brought the world's leading immunochemists to Uppsala and the international contacts in many fields of physical chemistry and biochemistry were intensified. In 1938 Tiselius became the first to hold a newly created research chair in biochemistry at Uppsala.

Tiselius took an active part in the development of electrophoresis and especially preparative zone electrophoresis, but he extended his interest to chromatography which he realized was to provide biochemists with the necessary methods for further investigating the chemistry of proteins. He introduced a standard nomenclature for chromatography and contributed especially to the development of general principles for analytical and preparative chromatography.

For example, displacement chromatography and gradient chromatography originate from Tiselius's pioneer work. Gas chromatography can also be traced from the Uppsala school. These methods have extended far beyond the field of protein chemistry. Tiselius's last work as an active researcher was to investigate suitable conditions for the chromatography of proteins. His contribution was the introduction of calcium phosphate as an adsorbent, which method is still being used. When ion-exchangers and molecular-sieving gels were introduced he retired from experimental work but followed developments with never ending interest and gave generously of his knowledge to others.

After receiving the Nobel Prize in 1948 Arne Tiselius increasingly began to devote himself to research policy. He was President of the International Union of Chemistry IUPAC from 1951–1955.

Swedish science is greatly indebted to Arne Tiselius. He was, for example, one of the initiators for the founding of the Swedish Natural Science Research Council.

He became the presiding chairman of the Royal Academy of Science in 1956 and was very active in the Nobel Foundation, and held, among other posts, the chair between 1960–64. He took a keen interest in the Foundation's symposium committee and took the initiative for several of the international symposia. He was also a member of the Wallenberg Foundation.

Tiselius's contributions to the international coordination of research in chemistry have been of utmost importance and the appreciation resulted in eleven honorary degrees. He belonged to a great number of academies and learned bodies and was on the editorial committees of a number of scientific journals. His capacity for work was almost unbelievable and he was always in great demand.

Tiselius was deeply and personally involved in the problems arising from the effects of scientific research on society and man, and he expressed his anxieties for the negative effects in the Pugwash movement.

Arne Tiselius was not only a brilliant scientist, but also an unusual personality. He possessed a rare ability for listening to others' opinions, drawing conclusions, seeing the essentials, summarizing and then presenting the judgment and advice of a good friend and wise leader. He will be sorely missed.