The table shows that the A group and the S group agree fairly closely in their estimation of the twelve nouns, with the exception of "men", "English" and "physics". The S group awards "men" a less foolish, a more masculine and a hotter rating. The same group gives "physics" a less foolish, a very much more masculine and a less cold rating than "English" and identifies "physics" more closely with "men". The A group gives "English" a more wise and a less cold rating than "physics", but awards a feminine score to "English" and a masculine score to "physics". The implication seems to be that the A group prefers a somewhat feminine subject to study for A level, while the S group prefers a masculine one.

This feature can be highlighted by listing the difference between the mean factor scores given to "English" and "physics" by the two groups of pupils, subtracted in the order (chosen subject-rejected subject), on the three factor traits or dimensions. The results are shown in Table 2.

Table 2 Mean Factor Score Differences for (Chosen Subject-Rejected Subject)

	Wise- foolish dimension	Masculine- feminine dimension	Hot- cold dimension
A group	78†	-73†	92†
S group	25*	75†	09

Decimals and zeroes omitted.

* Significant at 5%, † significant at 1%.

The two main features of the table are very clear. The A group differentiates sharply between the chosen subject and the rejected one in each of the three dimensions, while the S group differentiates as sharply only along the masculine-feminine dimension. Further, the S group differentiation along this latter dimension is in exactly the opposite direction to that given by the A group.

Osgood and his co-authors have suggested that the distance between any two nouns in the three dimensional semantic space defined by the factor score axes is a measure of the difference of meaning between the two nouns¹⁰. The distances between "English" and "physics" and the other nouns included in the survey were evaluated by finding the root mean square differences between the three factor scores given to each noun, and the A pupils were found to place "English" close to "home" ($d=0.61$), "success" ($d=0.81$), "good schoolmaster" ($d=0.83$), and "father" ($d=0.96$), and relatively far away from "games" ($d=1.35$), "rules" ($d=1.38$) and "physics" ($d=1.41$). "Physics" was placed close to "rules" ($d=0.17$), "work" ($d=0.75$), and "boy" ($d=1.12$), and far from "father" ($d=1.84$), "home" ($d=2.00$) and "pleasure" ($d=$

2.34). The S pupils placed "English" close to "physics" ($d=0.79$), "work" ($d=0.82$), "rules" ($d=0.83$) and "home" ($d=0.93$), and moderately far from "games" ($d=1.46$), "pleasure" ($d=1.42$), "good schoolmaster" ($d=1.53$) and "father" ($d=1.53$). "Physics" was placed close to "work" ($d=0.24$), "boy" ($d=0.71$), and "success" ($d=0.74$), and far from "good schoolmaster" ($d=1.11$), "home" ($d=1.14$) and "pleasure" ($d=1.61$).

It is clear that the S group pupils do not differentiate nearly as sharply as their A group counterparts between school subjects (except on the masculine-feminine factor of Table 2). The elements of the "home", "good", "schoolmaster" and "father" are missing from their chosen subjects, and they even opt for a course which they rate at a great distance from "pleasure". Conversely, the A group pupils select a course which contains a strong flavour of the feminine "home" and "pleasure" concept found by other workers in this field^{13,14}. It could be pertinent to recall that, at the time these results were obtained, the adolescent trend towards unisex was strongly under way. The breaking down of the distinctions between the masculine and the feminine, and the desire to avoid being labelled as "arts" or "science" (more than 30 per cent of our population now study mixed arts and science courses to A level), seem in retrospect to have been concomitant phenomena.

Yours faithfully,

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UNISIST

SIR,—While the leading article "Slowly but Steadily with Information" (*Nature*,

234, 268; 1971) is a fair summation of and comment on UNISIST, I feel that it does less than justice to the problems of exchanging information and current awareness and of copyright.

One of the most prevalent fallacies is that the average worker in basic research (whether in university, government, or industry) can keep up with the state-of-the-art by the means mentioned in the article. A few research workers can, those in relatively restricted field, those who are leaders in their field, and possibly some in the academic sphere. However, most research workers cannot, particularly those who are engaged in multidisciplinary research (for example, ecology) and those in government and industry.

Therefore, for the majority of people engaged in scientific research the most important part of the information exchange process is to, somehow, sort out from the great mass of published and quasi-published literature that which is relevant to their needs. All this must be done with the minimum of effort on the user's part, so that he is able to put the maximum effort into "reading and puzzling out what the other fellow means", which has been rightly identified as another important part of the information exchange process. This is, of course, why mechanized systems for retrospective and current awareness searching of the world's literature are so important and command so much attention at meetings like UNISIST.

To downgrade the importance of these systems, particularly on a world-wide basis, by assuming that they are not of primary importance to the research worker, is a significant error of judgment.

What appears to have been missed about copyright is that the development of new technology (for example, photocopying) has made the various copyright laws un-enforceable in any practical sense. Therefore, however sympathetic one may be to the concept of copyright and the views of the leader writer, it is essential to realize that the laws of copyright must be re-cast in the light not only of the present but of the future. Any other view is a denial of common-sense.

Yours faithfully,

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Population Control

SIR,—In your leader "The Duke and ZPG" (*Nature*, 234, 499; 1971) you criticize the view that the British popula-

tion is growing too large, yet you make no attempt to examine the question "How large is too large?". It undoubtedly is true that it is much harder than it used to be for people to enjoy the beaches in summer; city streets are certainly more crowded; and the pressure of industry on the environment is obvious to all. Perhaps it is time the problem of the

optimal size of Britain's population was examined.

Although the birth rate has fallen since 1966, you admit that this is likely to be reversed in the next few years. The fact that a large part of our increased population is due to increased longevity (rather than a high birth rate) does not lessen the impact of the greater numbers. It is

irresponsible to preach complacency about population growth and to ridicule those who try to suggest remedies.

Yours faithfully,

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Obituary

Professor J. D. Bernal

Photo: Henry Grant AIIP

THE recent death of J. D. Bernal, after a long and tragic illness, has robbed British science of one of its most outstanding and controversial figures.

John Desmond Bernal was born seventy years ago in Nenagh, Ireland, the eldest of four children. His father was a Catholic gentleman farmer; his mother a university educated American expatriate and a Protestant. Bernal's background was further variegated by his attendance at English public schools. Partly and paradoxically as the result of this mixture of national and religious traditions, he became a passionate adherent of both Roman Catholicism and Irish nationalism. It was also during this period that Bernal found himself attracted to mathematics and the physical sciences.

His decision to specialize in X-ray crystallography was made during his undergraduate years at Cambridge, so he joined the group of young and brilliant crystallographers that Sir William Bragg had brought together at the Royal Institution. Here, under the general guidance of Bragg himself, and with such workers as Kathleen Lonsdale and W. T. Astbury, he learned the techniques of X-ray crystallography and in 1924 he achieved his first success,

using them to determine the structure of graphite. He was in some ways a poor experimentalist, being impatient in a field that required long periods of concentration and accurate measurement. The instrument which was then almost universally employed in British X-ray crystallography, the ionization spectrometer, was slow and tedious, although very accurate, and very early in his experimental career Bernal decided that this method was not for him. This led him to search for simpler, less painstaking methods, which resulted in 1926 in the publication of his first paper of major importance—on the technique of single crystal rotation photography. This paper, which included charts for indexing rotation photographs, made these methods widely available to an English speaking audience for the first time.

In 1927 he returned to Cambridge to a lectureship in crystallography, and it was here that he first began to attract his own scientific followers—Dorothy Hodgkin, I. Fankuchen, M. F. Perutz, and many others. Although he worked on a wide range of substances, in part through contact with the Biochemistry Department he early became interested in biological molecules. Thus, in the thirties, a period of intense political activity, he at the same time did much of his most important scientific work, starting with the sterols, turning to crystalline proteins, and going on to viruses.

In 1932, during a study of the sterols, he was able to demonstrate that the conventional chemical formula for one of these compounds was incorrect, by showing that the size of the molecule from its chemical formula could not possibly fit into the unit cell, the dimensions of which he determined by crystallographic means.

In 1934 he and Crowfoot published the results of work on the crystalline protein, pepsin. Up until this time X-ray work on the crystalline proteins had been held up by the fact that, despite the apparently suitable nature of the crystals, the diffraction effects were in fact highly diffuse, and gave little or no evidence of a highly organized or regular molecular architecture. A number of crystallographers

had worked on proteins, but with the exception of fibre workers such as Astbury, little advance had been made. Bernal hit upon the simple expedient of taking pictures of wet rather than dry crystals, and he found that the resultant X-ray photographs included detail down to the atomic level. It was immediately clear that all the necessary detail was present for a full structure determination, if only it could be interpreted.

This work on pepsin, then, laid the foundations for modern protein crystallography, and it posed the question—how can the detail revealed by the photographs be interpreted? The problem was one well known to all crystallographers—the phase problem—although made more difficult by the size of the protein molecules. Much of Bernal's later work involved attempts to overcome the phase problem, and although in the end he was ultimately unsuccessful, he offered his co-workers some valuable hints. Thus, in 1939, he suggested two possible approaches, one of which, the isomorphous replacement method, was that successfully employed by Perutz and Kendrew in their work on haemoglobin and myoglobin. Again, he was one of the few crystallographers who really appreciated at an early stage that advanced methods of computing would be needed to process the volume of data involved in protein structure determination, and after the war he organized a research project on automatic methods of calculation.

In 1937 he moved to become Professor of Physics at Birkbeck College, London, and while continuing his protein work he also collaborated with Fankuchen in pioneering work on virus structure and the properties of viruses in solution. His work in pure science was interrupted by the outbreak of war, although he still maintained a lively interest in all crystallographic and biological work.

In post-war years Bernal did much less experimental scientific work himself. This was partly as a result of his wartime administrative experience, and partly because his interests, always wide ranging, were leading him into more general and speculative areas. Thus, while he organized a group at Birkbeck