

# CORRESPONDENCE

## Club of Rome

SIR,—The editorial in *Nature* of March 16 (242, 147; 1973) gives a brief review of the voluminous report of the Science Policy Research Unit of the University of Sussex on the *Limits of Growth* (*Futures*, March 6), and calls it, hopefully, "Almost the Last Word on the Club of Rome". This is liable to give a false impression of the Club of Rome, of the *Limits* and to some extent even of the Sussex Report.

In my opinion the Sussex Report is not so much an axe as, rather, so far the best constructive critique of the *Limits*. It is fair, courteous and thorough. Marie Jahoda in her "Postscript on Social Change" writes, "We know from our experience in studying Forrester's and Meadows's work that it took a group of highly trained experts in a variety of fields many months of hard effort to grasp fully the technique, the assumptions and the shortcomings of world dynamics". Almost every page of the Report bears this out. It is a very welcome change after the wholesale rejection of *World Dynamics* and the *Limits* by many economists, whom one must strongly suspect of not having read the books, but just leafed through them with disgust. The Sussex Group have read them, digested them and much of what they write, though not all of it, can be considered as a fair commentary, which will give the reader a deeper understanding of these works, while showing up some of their weaknesses.

The least satisfactory are the computer runs of the Sussex Group. They cast doubt on the reliability of the world models by running them backward, with the result that they "retrodict" a catastrophe in the past. I need not deal with this, as it has been answered by Meadows in the same number of *Futures*. An error of 1/1,000 in some parameters is liable to make the model run off its track in the past, while an error of the same order would be hardly noticeable in a forecast. The Forrester-Meadows models share this property with many other complicated physical systems. Nor need we be impressed with the difference which they find in forecasts by substituting expanding in place of shrinking resources. This is not a "small change" in the assumptions.

On the other hand, in view of what we have experienced from economists, it is an agreeable surprise to find that chapter 6, *The Capital and Industrial Output System* by Christopher Freeman with the economists Julien and Cooper,

is particularly constructive. They make it clear that it is hopeless to treat an aggregated system, in which Africans and Americans are lumped together, dynamically with a single set of operators. This is macroeconomics taken too far. One cannot expect from such a forcibly homogenized model anything but coarse approximations, and nothing more has ever been claimed by its authors.

This type of critique, of which there are many more examples in the long report, is welcome to the Club of Rome as a dialectical aid towards the next step. It does not find us at all unprepared. We have never considered the world models as oracles of unescapable fate. Perhaps the words "Predicament of Mankind" were misleading. A predicament is not a trap from which one can never escape; it was meant to be a warning and a challenge. A warning against "ostrichism" and a challenge to take up the fight with the dangers which have been revealed. As far as I can see, the Sussex Report nowhere states that the dangers are imaginary, though it often suggests that they might be exaggerated.

Are they exaggerated? I think that if the Club of Rome has anything with which to reproach itself, it is rather that it has not warned in time that while the global danger may be a hundred years ahead, its first serious wave is right upon us. We are far from living in a unified world in which the resources are equitably or at least reasonably shared. While the Club of Rome warned of an overshoot in the consumption of non-renewable resources some time in the next century, the highly industrialized nations and in particular the United States have long overshoot the limit at which they could live safely within their own means. A grave fuel and energy crisis is now striking at the industrialized countries, long before the resources of the world are exhausted—because we have not foreseen it in time.

The fuel and energy crisis which is starting now is likely to last for two decades, and it is scant satisfaction for us of the Club of Rome that from now on we are likely to meet much less complacency, much less of the "I am all right, Jack!" attitude. When the Sussex Group states that it considers the political difficulties as more urgent and important than the physical limits, it is in perfect agreement with the Club of Rome. From the start its founders have considered international cooperation as their most important target, and they

were successful in establishing a worldwide network of intellectuals, not entirely without political influence.

There are many minor deficiencies in the Forrester-Meadows models which could be amended, but it is clear that no aggregated world model can be satisfactory. The Club of Rome has long understood this, and now, among other projects, it is sponsoring a *regionalized* computer model of the world. This ambitious project, headed by Professors Mesarovic and Pestel, divides the world into 10 regions, taking account of their self-interest and also of their political philosophy. If successful, this project may give answers to many questions which the *Limits* have left open. Only one thing is certain; the dangers, the instabilities, will be found very much nearer to us than the global physical limits.

As regards the ultimate, physical limits, we believe that these can be pushed further away, but we are not satisfied with pious hopes. The Club of Rome is going ahead with plans for mobilizing creative technology in all countries. We are not "doomwatchers" but doomfighters.

Yours faithfully,

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## Princeton in Trouble

SIR,—Many of us at the Institute for Advanced Study were sad to read your leader about Princeton (*Nature*, 242, 217; 1973). Not only is your statement of the facts extremely misleading, but also your underlying philosophy is difficult to understand. Your writer seems to believe that an academic institution, by its very nature, should be a dictatorship, and that in case of a conflict between faculty and director the faculty must go.

The background of the present dispute can be described briefly as follows. It had been unanimously agreed that the faculty as a whole should supervise the formation of the fledgling "Program in Social Sciences" by examining the credentials of candidates for the first three professorships. Precisely because these first appointments would determine the future direction of the program, it had been agreed that such faculty supervision was essential; and in fact the program got off to an excellent start with the unanimous faculty approval of Clifford Geertz as the first professor.

Going further back, the following promise was made in 1947 by Frank Aydelotte, then director of the Institute: "I have given an undertaking to the faculty that I would never recommend to the trustees an appointment of which the faculty did not approve". This principle has been a firm tradition ever since, until the present altercation. It is our hope that negotiations now in progress between faculty and trustees will lead to a reaffirmation of Aydelotte's undertaking.

For readers who may have been misled by your headline, let me emphasize that the Institute has no connexion with Princeton University, which has a long record of excellent administration.

Your faithfully,

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## Careers

SIR,—The worsening market situation for scientists and technologists described in your supplement, "Careers for Scientists", and the threat of further reductions in scientific establishments, especially in the largest companies, leads to one irresistible conclusion: the need for scientists and technologists to join this union set up by the Council of Science and Technology Institutes just over a year ago. It is one reason why membership is already 4,000 and climbing steadily.

Of course, as several of your contributors pointed out, the picture is not all black. More and more science graduates are entering managerial employment, which we believe to be essential if the science-based industries are to develop their full potential, or scientific work at a supporting level. Viewed, therefore, as a whole, we would agree with Mr Davies that we are not producing too many highly educated people and can do with more, provided that planning is sensible and that people are told what to expect. This, too, emphasizes how vital it is that scientists and technologists moving into unfamiliar fields make sure that they can avail themselves of the assistance which APST can provide.

Yours faithfully,

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## Citation and Distinction

SIR,—Garfield<sup>1</sup> continues to make superficially plausible claims for the Science Citation Index, which his company publishes. Obviously Nobel Prize awards and citation frequency are partly

caused by the same factors. It may be coincidental that Nobel Prizes are not given in the evolutionary half of biology (with adjacent fields such as stratigraphic geology), where the literature is extraordinarily diverse and is poorly represented in the Index.

In Garfield's lists<sup>2</sup> of most-cited and highest-impact journals, eighty-one and seventy-one respectively of the first 100 are devoted exclusively to chemistry, physics, biochemistry, physiology, and medicine. This partly reflects the same bias in the source, but also the fact that there are, for example, more organic chemists than evolutionary biologists.

In a forthcoming work where I propose a new scientific law with considerable applicability, I make few references. But the result depends directly on, and would have been impossible without, a large proportion of all the work in evolutionary biology that has been done in the past. A simple-minded cost-effectiveness approach like Garfield's would have prevented this work from being done.

Any action taken on the basis of such biased results as Garfield's clearly discriminates against the areas under-represented. These areas are as a whole probably more dependent on extensive availability of literature than is any other area of science.

Yours faithfully,

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<sup>1</sup> Garfield, E., *Nature*, **242**, 485 (1973).

<sup>2</sup> Garfield, E., *Science*, **178**, 471 (1972).

## Human Papova Viruses

SIR,—We would like to correct some of the facts in your News and Views article "Human Papova Viruses" (*Nature*, **241**, 308; 1973).

A human papova virus of the polyoma virus subgroup "first came to light" when Zu Rhein and Chou observed by electron microscopy virus particles in brain cells of patients with progressive multifocal leucoencephalopathy (PML) (*Science*, **148**, 1477; 1965). Many attempts have been made to grow this virus *in vitro* without success.

Simultaneously with our report of the isolation of BK polyoma virus from the urine of a renal transplant patient (Gardner *et al.*, *Lancet*, **i**, 1253; 1971), Padgett and her colleagues described the successful culture of JC polyoma virus directly from the brain of a patient with PML (Padgett *et al.*, *Lancet*, **i**, 1257; 1971). It was unfortunate that no reference to this important work was made in your article. Later, Weiner and his co-workers reported the isolation of two further strains of polyoma virus from the brains of two patients with PML (*New*

*Engl. J. Med.*, **286**, 385; 1972). Both of Weiner's isolates appear to be antigenically identical and biologically similar to the simian polyoma virus (SV40) whereas JC, the other virus isolated from PML, appears antigenically unrelated to SV40 by immunofluorescence.

Our BK polyoma virus has only a very minor antigenic cross-reaction with SV40 and shows many differences from this virus biologically, including the property to agglutinate human O and guinea-pig erythrocytes to high titres. It seems therefore that we are indeed dealing with a group of different human polyoma viruses. In the discussion of human papova viruses it must not be forgotten that human common wart virus is also a member of the papova virus group although it belongs to the papilloma virus subgroup.

Yours faithfully,

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# Announcements

## Miscellaneous

Elections to the US National Academy of Sciences:

Edward Hamblin Ahrens, jun., **Rockefeller University**; Robert Wayne Allard, **University of California, Davis**; Andrew Alm Benson, **Scripps Institution of Oceanography**; Howard Alan Bern, **University of California, Berkeley**; James Daniel Bjorken, **Stanford University**; Harold Charles Bold, **University of Texas**; John Tyler Bonner, **Princeton University**; Frederick Herbert Bormam, **Yale University**; Gordon Howard Bower, **Stanford University**; Felix Earl Browder, **University of Chicago**; Donald David Brown, **Carnegie Institution of Washington**; Arthur Earl Bryson, **Stanford University**; Bernard Budiansky, **Harvard University**; John Werner Cahn, **Massachusetts Institute of Technology**; Robert Merritt Chanock, **George Washington University**; Albert McCavour Clogston, **Sandia Corporation**; Ansley Johnson Coale, **Office of Population Research, Princeton, New Jersey**; George C. Cotzias, **Brookhaven National Laboratory**; Philip Ernest Converse, **University of Michigan**; Ellis Brevier Cowling, **North Carolina State University**; James Edwin Darnell, jun., **Columbia University**; Albert Dorfman, **University of Chicago, School of Medicine**; Otis Dudley Duncan, **University of Michigan**; Isidore Samuel Edleman, **University of California Medical Center, San Francisco**; Walles Thomas Edmondson, **University of Washington**; Edmond