

correspondence

'Beast and Man'

SIR, — I have only just seen Professor Stuart Sutherland's very interesting review of my book *Beast and Man* (28 June page 83.) May I say something in reply to the difficulties he raises?

He says that the main aim of my book "seems to be to establish a secure basis for ethics" and that, like "most previous attempts to establish a secure basis for an ethical system", it fails. (The italics are mine). Two projects might be meant, of which the first is certainly not mine —

● A "secure basis for an ethical system" would be a justification of one particular set of moral views against others. This would itself be a piece of ethical reasoning.

● A "basis for ethics" by contrast seems to mean something much more general — a justification or explanation, from outside, of the whole range of thought which can be called ethical or moral. Confused people often run these two enterprises together, supposing that the second will do the work of the first. I think this is what Professor Sutherland objects to, and I share his objection. There can be no moral justification of morality as a whole any more than there can be a mathematical justification of mathematics as a whole; neither from outside nor from inside these areas of thought would such an enterprise make sense, nor is it needed. In the case of morality, the notion that it is needed, and is lacking, stems largely from Nietzsche's ambiguous talk of 'immoralism'. Nietzsche often spoke of this as a comprehensive attack on the very possibility of morality. But in fact his campaign was an intelligently limited one, directed against certain particular human weaknesses and particularly against contemporary bourgeois notions. It was conducted in entirely moral terms, powered by fierce and unmistakable moral indignation, and successful so far as his readers approved of it. It was a piece of ethics. The wider program of attacking or justifying morality as such remains obscure.

It was therefore no purpose of my book to provide defence against this hypothetical general attack. I am interested, as most serious moral philosophers have been, in explanation rather than in instant justification, in understanding rather than in proof. For ethics as for any other area of thought, we need to *understand* our basic concepts, to resolve conflicts among them, and to relate them systematically to those used in relevant neighbouring fields. That is the sense in which people speak of the 'foundations' of mathematics. Nobody supposes these to be a set of non-mathematical arguments, guaranteeing mathematical truths from outside. In the case of ethics, however, people do for some reason tend to expect this, and even when they give their attention to the concepts, have a remarkable tendency to insist that they shall be simple. Hume's question, "whether the foundation of morals lies in reason or in feeling?" has been only one of a whole series of grossly over-simple dilemmas, which it is the point of my book to defuse. I draw attention to the complexity of the relevant facts, to the unreality of various simple rulings, and particularly of those, fashionable in the first half of this century, which isolated moral thinking from the natural facts. During this epoch, a variety of reasons were given for tabooing many useful, indeed

essential, concepts such as 'natural' which refer to facts, but are necessary to explain our value judgments. I do not think I can now trespass on your space with a full answer to Professor Sutherland's worries about my treatment of these. But perhaps it will help if I emphasize that I do regard them as explanatory, and not as an underhand short cut to justifying local prejudice.

Yours faithfully,

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East Europe: valuable information

SIR, The need by science of 'extensive, systematic and friendly international ties' noted by your correspondent, Lee Lorch, (13 September, p 98) is one which I am confident is recognized by *Nature* and by the overwhelming majority of its readers. Having worked at various times in research laboratories in half a dozen countries, including the USSR, I have personally had the good fortune to benefit directly from existing ties and have, I hope, made a modest contribution to furthering them.

Lee Lorch accuses *Nature* of allowing itself to be used by propagandists "anxious to whip up an hysterical ill-will against East European science", an accusation so far-fetched that it would not merit refutation if it were not coupled with a scornful and vicious personal attack on your regular contributor, Vera Rich.

Her articles are a valuable source of information about science and technology in Comecon countries. Of course it is incomplete, of course it is biased; Ms Rich writes rather more often on environmental matters than on high energy physics, and as far as I know has never written about Polish research in general relativity. But omission and bias are not the same thing as prejudice, still less contempt and propaganda.

To be sure there have been occasions, sad to say not infrequent, when she has reported on the action of the authorities against individual scientists, their dismissal from employment, arrest, or worse. The cases which she has reported have been in the USSR and in East Europe — but that is the region of her specialist interest and knowledge and others in your journal have balanced the record of injustice with reports from elsewhere.

The achievements of Soviet science are beyond question; the distinction of her scientists and engineers and their contributions to the advancement of knowledge command the admiration and respect of the world community of science. The mutual benefits that flow from international cooperation, the free exchange of ideas and the unrestricted travel of scientists themselves are widely recognized. Your journal indeed has a responsibility, the greater because of its eminence, to report on developments in science in East Europe as elsewhere. But it has a responsibility too to report with the candour and good faith of a true friend of international cooperation the less favourable aspect of the policies of those countries insofar as they affect science and scientists. To pretend otherwise is no way to encourage the ties of friendship: it is to acquiesce in injustice.

Yours faithfully,

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'MHC' nomenclature

SIR, — Rolf Zinkernagel is too modest (14 June, page 618). The revolution which he and David Katz started has changed our thinking about the 'MHC' (major histocompatibility complex) to an extent which makes nonsense of the conventional nomenclature used in the otherwise excellent book which he reviews. His thesis is now generally accepted that MHC-coded molecules (other than complement components) evolved as signals for T cell subsets, which enable the immune system to make an appropriate choice between these subsets when combatting varied forms of infection. If one takes this point of view, present nomenclature is quite misleading. The very term, 'major histocompatibility complex' place the emphasis on transplantation, not infection. It perpetuates the historical accident which led to its discovery, but misleads as to its normal function.

Terms such as cytotoxicity-defined or lymphocyte-defined are open to the same criticism. The term 'I region' with the associated terms Ia-molecules, I-A, etc. implies falsely that H-2K and D molecules are not immunoregulatory. In fact, they are so, and in a way which appears exactly to parallel the immunoregulatory functions of Ia-molecules, except that they control cytotoxic T cells rather than helper T cells. Furthermore, gene substitutions at K and D regulate immune responsiveness of cytotoxic T cells just as substitutions in the I region regulate the responsiveness of helper cells. The whole complex, again with the exception of the complement components, seems to be equally immunoregulatory. Indeed, one really ludicrous usage is creeping in: to refer to the I-region genes as *the* IR genes. No doubt, many if not most gene substitutions perturb the immune system, just as most known substitutions perturb the nervous system: it's just that behavioural effects are more obvious.

We hesitate to suggest alterations, chiefly because it annoys others so much when one does so. T cell-activating complex? Cytotoxic cell activating molecules versus helper cell activating molecules? There are plenty of problems still: the aberrant presence of Ia-molecules in glandular epithelia and T cell products, the odd behaviour of I-J molecules, questions about restriction elements on B cells, and so on. What is certain is that so long as the present nomenclature is used we shall have to go on explaining to newcomers that the only way to understand the MHC — and hence what the majority of cellular immunologists these days are up to — is to forget about what the terms seem to mean.

Yours faithfully,

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Radio and the 'experts'

SIR, "A pity" — you say (23 August, page 619) — that more qualified people fail to participate in phone-in and open-line radio programmes.

The reason is simple: most of us are at work, so don't hear the programme anyway; those not at work presumably would not wish to advertise the fact!

Yours faithfully

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