# LETTERS TO THE EDITORS

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## Genetical Effects of Radiation from Products of Nuclear Explosions

Sir John Cockcroft[1] has given a reassuring answer to the question of the genetic hazards of radiation from the products of nuclear and thermonuclear explosions disseminated to large distances. I think that he, or his biological advisers, have greatly over-estimated the simplicity of the problem. It can be attacked by several different methods. First we can fix a rough upper bound to the effect.

If an average human being has received 3 roentgens before becoming a parent, then a mean extra dose of $x$ roentgens spread over the world's population will increase the number of mutations arising in the next generation as the results of radiation by $\frac{1}{3}x$, provided $x$ is small.

In *Drosophila melanogaster* almost all mutations arise by a process with a temperature coefficient, and therefore presumably chemical. Isotopically labelled atoms are rarely incorporated into deoxyribonucleic acid except when it is replicated. This mutational process therefore probably occurs only or mainly during gene replication. Man has about twice as many nuclear replications per life-cycle as *Drosophila*, and a somewhat higher temperature. The chemical component in human mutation is therefore likely to be of the order of ten times that of *Drosophila* per generation. On the other hand, a human generation is about 1,000 times longer than a *Drosophila melanogaster* generation, so the component due to radiation may be expected to be about 1,000 times greater in man than in *Drosophila*. Man may have become adapted to longer generations by reducing mutation-rates; but he may also have a larger number of mutable genes. On the basis of such arguments, I suggested[2] that "it is quite possible that radiation may account for most human mutation". If so, the dose of radiation needed to double the human mutation-rates is a little more than 3 r. per generation, and not 50 r. as Cockcroft suggests. In fact, the effect of radiation is about ten times as serious as he believes.

It is also more serious for another reason. Cockcroft points out that since most British people spend most of their time indoors, the effective dose of radiation received by them is reduced to about a tenth of the outdoor dose. However, about half the human race is engaged in outdoor work, and most of the population in such countries as India live in very flimsy houses. Allowance for this fact raises the expected effect by a factor of two or three.

We do not know how much of human mortality is due to the action of natural selection in eliminating mutant genes. Haldane's[3] suggested figure of 10 per cent may be a good deal too high. But if this figure were accepted, an average dosage for the world of 0·01 r. (corresponding to Cockcroft's figure of 0·003 r. for Britain) could be responsible for about one three-thousandth of the $10^9$ or so deaths which occur in a generation, say, 300,000 deaths transferred from the ages later than the mean age of parenthood to the ages before it.

This figure is probably an upper bound. We can obtain an approximate lower bound as follows. Suppose men to have no more radiosensitive genes than *Drosophila*, as against the usual estimate of five to ten times as many, then we expect one lethal and two to four sublethal mutations per 10,000 r. The latter, which cause invalidism and death after some years of life, are the more serious. If they are all fully recessive, which again is optimistic, this implies about 2 deaths per 10,000 r., or about 2,000 deaths from the $10^7$ r. received by a generation of $10^9$ people receiving 0·01 r. on an average.

The true number is probably somewhere between 2,000 and 300,000. I doubt whether the experiments carried out on mice up to now allow of any great precision. Those whose results have so far been published were not so designed as to allow a calculation of the rate of production of lethal mutations; and had they been so designed, the argument from mice to men is of dubious validity. It may be possible to validate this argument by experiments on human and mouse tissue cultures.

The problem is not insoluble. The figures given above could, I think, be improved. I have, however, deliberately used estimates made before the question of the genetic effects of experimental explosions was raised. Estimates made later may have been influenced by conscious or unconscious political or ethical bias. Research on this problem requires extremely careful planning, including the realization that apparently hopeful projects may prove useless.

Until the results of such research have been published, many biologists will find it hard to share Sir John Cockcroft's optimism.

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[1] *Nature*, **175**, 878 (1955).
[2] *Proc. Roy. Soc.*, B, **135**, 147 (1948).
[3] *Amer. Nat.*, **71**, 337 (1937).

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In my lecture to the Parliamentary and Scientific Committee, I said that the Oak Ridge experiments suggested a doubling dose of about 50 r. for mice, but that the extrapolation to men was obviously an uncertain one. Dr. H. J. Müller has used a figure of 80 r. with similar qualifications.

I also emphasized the uncertainty of the biological data and the need for long-term genetic studies.

Since a Committee of the Medical Research Council has been appointed to study these problems, I will not pursue the matter further.

J. D. Cockcroft

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## Insulin Activity of Cord Plasma

It has been shown by histological methods[1] that the islets of Langerhans form a much larger proportion of the pancreatic tissue in fœtuses and newborn infants than they do in adults, and Fisher and Scott[2] extracted relatively more insulin from the pancreas of the fœtal and newborn calf than from the same organ in later life. Hartmann and Jaudon[3] found that the injection of insulin into newborn human infants led to a moderate fall of blood sugar, and Villee[4] has stated that the fœtal tissues of man respond *in vitro*, like those of adults, to the presence