graph in Fig. 2 is only slightly affected by omission of this point.

We agree, from a theoretical standpoint, with Finkel when she states that it might be better to use the tumor rather than the tumor-bearing animal as the statistical unit. However, we must also agree with Wollman [*J. Natl. Cancer Inst.* **16,** 198 (1955)] that this is a very difficult unit to use for the following reasons: (i) new tumors may appear over such a wide range of time intervals after treatment that the experimental animals may die from the first tumor before all potential tumors are detected; (ii) if the tumor metastasizes, it may be difficult to distinguish between a metastasis and a tumor of independent origin; and (iii) the early tumors may coalesce, resulting in an underestimate when scoring late. The first of these two objections would be especially applicable to the sarcomas, and the second would be especially applicable to the reticular tissue tumors.

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Reprint Exchange Center

All of us who consider the scientific reprint as a valuable research and teaching tool, and as a necessary part of our personal reference libraries, have experienced the frustration which accompanies the reply to a specific request indicating that the author's supply of reprints has been exhausted. At the same time, we are all largely guilty of harboring reprints which are no longer pertinent or useful. In addition, we frequently shelve, indefinitely, reprints which were never of any real interest to us and which came into our possession by means unknown or forgotten. In short, many thousands of reprints are taken out of circulation permanently every year, their purpose for existence defeated, when they should be available to those who could make serious and profitable use of them.

It would seem that a free reprint exchange center could be created with the cooperation of interested scientists. Such a center would accept the voluntary contributions of reprints from individual and institutional files, catalog them, and, in turn, make them available to other scientists in response to general or specific requests without charge.

Although I am certain that there are agencies, institutions, and individuals better equipped by virtue of experience, finances, and facilities to undertake such a program. I would, nevertheless, be happy to attempt to establish such a cen-

ter at Duquesne University. In that regard, I welcome the comments, suggestions, and cooperation of scientists everywhere who share these views.

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Village Solidarity

Peter Suzuki [*Science* **132**, 891 (1960)] makes a most important point and one which I had almost despaired of seeing in a "neutral" journal—that of "village solidarity." While he may overstate the case by saying that what "is generally taking place in many of the underdeveloped countries is a ruralization of the cities. . ." (since the majority of these cities were hardly urban within the meaning this term has assumed in recent years), yet it must be stated repeatedly that, with few exceptions, aid administrators continue to err in failing to recognize that this group attitude is a prerequisite if their rural programs are to meet with success. The opposite view, held in the past and still the principal method of assistance—that this sense of common identity and common purpose must be altered or eradicated before change can take place—has resulted in the almost unqualified failure of rural development programs carried on by the International Cooperation Administration and its predecessors throughout the so-called "underdeveloped nations." I am therefore quite happy that Suzuki has supplied us with an additional case study to substantiate my criticism of the direction these programs have taken. ["*Social and political aspects of Philippine economic development*," Philippine Council, Institute of Pacific Relations (Kyoto Conference, Pacific Council, Institute of Pacific Relations, 1954) (mimeographed)].

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Developmental Selection of Mutations

We should like to comment on the interesting and provocative report by L. L. Whyte entitled "Developmental selection of mutations" [*Science* **132**, 954 (7 Oct. 1960)]. Whyte is well known as a perspicacious and imaginative thinker and, as always, his writings are worthy of consideration. If we understand his report correctly, however, the problem to which he alludes does not really exist.

Whyte's main point seems to be that there is a class of mutations whose role in evolution has not been appreciated by students of the evolutionary process. These are the mutations which prevent "internal organizational efficiency permitting continued growth." In other words, he is referring to lethal and detrimental mutations whose effect is manifest during morphogenesis, as opposed to those genes whose morphogenetic effect is to produce an adult ill-adapted to the adult environment. We are rather aghast to learn that a perusal of the literature has left Whyte with the impression that such genes have been neglected. The existence of a very large class of embryonic lethals is very well known to evolutionists and geneticists in general. Elementary textbooks of population genetics always begin by a discussion of gene-frequency changes in populations, in which unconditional lethals are segregating, before going on to the discussion of more subtle forms of natural selection [see, for example, C. C. Li's textbook, *Introduction to Population Genetics*]. If general works on evolution fail to deal explicitly with such lethal genes, it is largely because they *are* so well known that it hardly seems worthwhile calling further attention to them. As a matter of fact, Lewontin and Dunn have recently published [*Genetics* **45**, 706 (1960)] a report on the evolutionary dynamics of a series of unconditional embryonic lethals in wild populations of *Mus musculus*.

There is, however, a more subtle and more important point here. Students of evolution have emphasized the interaction of environment with genotype because they have for some time realized that there is no real distinction between "developmental selection," as Whyte so aptly calls it, and postembryonic adaptation. There is no sharply defined boundary between unconditional embryonic lethals and those whose effect is intimately bound up with environment. There is, rather, a continuous spectrum of gene effects, from those genes whose action seems virtually independent of any environmental modification, to those whose sensitivity to environment encompasses every slight change of physical and biotic milieu.

We would venture so far as to say that no gene is totally independent of environment in its expression, and therefore the fitness of every genotype is in some measure a function of environment. It is, of course, true that early embryonic lethals whose effect is to completely disrupt the normal morphogenetic pattern to the point of death are less susceptible of modification. But they are not wholly insensitive to it. In general, the more protected