Pierre Lépine, director of the Virus Division of the Pasteur Institute in Paris, and Valentine D. Soloviev, director of the Experimental Department of the Institute of Epidemiology and Microbiology in Moscow. Dr. Soloviev is in London at the present time and is gathering material for a book on virus diseases. He is anxious that it be as current and complete as possible.

I shall be glad to forward any material to them, or reprints may be sent directly to Dr. Lépine in Paris and to Dr. Soloviev at 65, Inverness Terrace, London, W 2, England.

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On the Use of "Fission"

In both the technical literature and popular accounts dealing with nuclear fission one notices inconsistent or incorrect uses of the various forms of the word "fission." Curiosity on the part of the writer prompted him to look into an unabridged dictionary for help, with the following result. The work consulted was *Webster's new international unabridged dictionary.* (2nd ed.) Springfield, Mass.: G. & C. Merriam, 1946.

"Fission" is, of course, the noun, as well as both the transitive and intransitive verb forms. The correct adjective describing nuclei capable of undergoing the process is "fissile," pronounced either fĭs'ĭl or -īl. As with similar words ending in -ile, the former pronunciation is preferred in this country, while the latter is more common in Britain. The universally encountered "fissionable" is not, as far as I can find, given by any of the accepted authorities.

It is regrettable that a word having such awkward parts of speech as "fission" came to be used to describe what is probably one of the most important phenomena in nature. However, from the point of view of meaning, "fission" is probably the most appropriate of all the terms that might have been settled upon, since it conveys the idea of spontaneity of occurrence, which such words as "split," "cleave," and the like fail to do.

Since the biologists have been using the word for a long time, it would be interesting to hear if they have encountered difficulties in the application of this term.

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A Note on the Meaning of Normal

This note is prompted by (1) the recent publication of a paper by the present writer, entitled "The meaning of normal" (*Yale J. Biol. Med.*, 1945, **17**, 493) and (2) the even more recent publication of a report by the Grant Study of the Grant Foundation, Inc. (Clark W. Heath and collaborators. *What people are.* Cambridge, Mass.: Harvard Univ. Press, 1945). The Grant Study claims to be investigating normal young men; further, it claims that its concept of normal is in essential agreement with that proposed by the present writer. The latter feels obligated to challenge the Grant Study's statement, since it applies specifically to him and since it seems to him that their use of the term normal is a clear example of the very looseness against which he protests; indeed, it has the further appearance of involving self-contradiction.

By the Grant workers "'normal' is defined as the *balanced,* harmonious blending of functions which produces good integration" (p. 3). With that the present writer would agree, but the authors then proceed to state that none of their subjects "nor indeed any man is quite free from abnormalities. . . . To be otherwise would in itself be 'abnormal'" (p. 3). And here we can see only discongruity, a shuffling back and forth of meaning which has no place in scientific discussion. The first use of abnormality is obviously in the sense of malfunction; the second, just as obviously in the hackneyed and incorrect sense of average. These two uses are mutually incompatible and serve only to obscure whatever concept is being advanced. Moreover, *if* no man is free from abnormality (a general negative which the authors somewhat boldly assert), then what excuse can be offered for labeling their subjects normal when their subjects are men?

Finally, they add: "The term 'normal' is automatically defined by the nature of the method of selection of the young men" (p. 4). It is here that they claim agreement with the present writer, but very certainly it is here that he totally disagrees. For this last definition is merely an operational one, not strictly a rational one at all. The meaning of normal is now made to depend upon the method of the subjects' selection; and there is a difference amounting to contradiction between selecting the subjects in accordance with the defined meaning of normal and defining the meaning of normal in accordance with the selection of the subjects. This is the precise fallacy to which the writer referred when he wrote: "There are those who despair of discovering fundamental human design through a direct consideration of the apposite data and hope to come upon it more easily by observing a sufficiently numerous and otherwise 'fair' sample of specimens. Can we not see that any such plan is doomed to failure by its inherent contradictions?" (*Yale J. Biol. Med.*, p. 496). To define normal in accordance with the exigencies of the selection of laboratory subjects is to take leave of any possibility of investigating the true nature of the normal, since now normality is already defined in terms of those who (for all we know) may as well lack it as not.

All this is a far cry from the definition of normal as "that which functions in accordance with its inherent design" (*Yale J. Biol. Med.*, p. 500). The whole purpose of investigating the problem of normality is to discover its real nature—that is, the basic human design from which every specimen *may* be shown to depart in this or that fashion; it is not the attempt to cram any particularly selected group into a merely terminological pigeonhole.

We should, of course, inquire how the Grant Study in fact did select its subjects. The report states: "Mainly

four factors have shared in the selection of students: (1) chance; (2) the opinion of the Deans who were particularly acquainted with the young men; (3) the health records . . . ; (4) a certain minimal standard of excellence in pre-college academic ratings'' (p. 110). In one case, ''nine names not approved by the Deans were eliminated''; in another, ''twenty-one additional names were selected by chance'' (p. 112).

Now all this was plainly a practical and intelligent way in which to proceed with the necessary selections, but the point is that it bears no reference whatsoever to the normal. It seems plain that such a method must choose, and did choose, a typical cross-section of the usual Harvard undergraduate population (the study was conducted at Harvard), neither the extraordinary geniuses nor the unusually incapable, neither the most remarkable physical specimens nor the most ailing—in short, the average. Then why not call them so?

There was a single basic criterion of selection which seems to have underlain the four specific criteria: ''On the whole it was preferred to select participants from the point of view of 'successful living' (rather) than from arbitrary, rigid standards which would limit the study to a particular group of individuals'' (p. 110). Thus, the basic criterion was clearly of the adaptation-to-environment kind; and again, where is there any reference here to the normal? In an abnormal environment those who adapt successfully must naturally do so through the operation of their congruent abnormalities, whereas the question of the normality or abnormality of any given environment *vis-à-vis* human beings can be decided only *after* the problem of human normality itself has been settled. Such procedures as those here mentioned can, and do, accomplish no more than to beg the essential question.

In this writer's view the nature of the human paradic is as it is, irrespective of the divergences from that functioning design of any particular selection of subjects at Harvard or elsewhere; nor can the matter of their divergence or their approximation in respect of the normal be determined by an *ad hoc* investigation of the subjects themselves. The question of the normal is plainly one of the efficient functioning inherent in the design, and ''efficient functioning'' must itself be defined by reference to the *design,* not by reference to its possibly compulsory distortion—''successful living'' —at the behest of any undetermined environment. Until this distinction is clearly grasped there can be no possibility of investigating the somewhat important question of the human norm.

It is to be hoped that nothing here said will imply the writer's disapproval of the Grant Study. The importance of the results already attained is apparent in its published report; and the value of a project concerned with the careful investigation of the average or usual member of our society rather than of the exaggeratedly abnormal member is undeniable in this writer's opinion. His only criticism is directed against the gratuitous intrusion of the term, normal, and his own alleged agreement with the actual misuse of that term—a misuse that seems especially unwarranted since the project needs no justification beyond its asserted purpose as presented excellently in the title of its report, *What people are.* That is quite important enough an undertaking to stand by itself, without any unrelated prejudgment as to whether people, as they are, are normal or not.

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Book Reviews

Biological actions of sex hormones. **Harold Burrows. Cambridge, Engl.: at the Univ. Press, 1945. Pp. x + 514. $8.50.**

This book is an excellent compendium of the literature on the sex hormones up to and including 1941. It is arranged in the conventional manner, listing the characteristics of each hormone, its effects on the endocrine glands, and finally its effect on all organs and tissues upon which observations have been made. The book is divided into six parts: four chapters on gonadotrophins, one general chapter on gonadal hormones, five on androgens, ten on estrogens, two on progestins, and finally a single chapter on the sex hormones of the adrenal cortex. The subject of estrogens is probably the most completely covered and includes a chapter on factors in the causation of cancer.

The book is primarily a descriptive account of numerous experiments, with many of the results listed in tabular form. There seem to be relatively few errors in handling the data in the references, and none that the reviewer detected would invalidate any conclusions. There is a minimum of selection, interpretation, and correlation. However, it is an excellently organized book and reads easily and smoothly. One feels that it fulfills the author's wish that it be a tribute to John Hunter (1728–1793), who, he says, is ''the first and greatest'' of ''the pioneers of sex hormone physiology.''

In spite of the author's statement that ''much work done in foreign lands has been given inadequate consideration,'' the literature reviewed (up to 1941) is surprisingly complete. There are approximately 2,000 titles in the list of references. The book has an appendix