The results in this first experiment were so encouraging that further tests of sulfa drugs, especially sulfamerazine, for the treatment of furunculosis are to be made shortly.

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Sir Oliver Lodge, Lord Kelvin, and Hertzian Waves

Apropos the recently announced Soviet claim that Russian scientists had anticipated Marconi, a story worth recalling was told by John Allen Harker, the British physicist, at a meeting held in Browning Hall, Walworth, London, on 26 November 1914:

"I remember the British Association meeting in Liverpool—in 1896, I think it was. At the end of the meeting on the last morning Sir William Preece, who was then chief electrician to the Post Office, had been describing, in the course of a debate on transmission of wireless signals, the fact that a young Italian had come a few months previously to his laboratory at the Post Office, and had succeeded in showing what was then an extremely novel thing: that wireless signals could be transmitted over a distance of about a mile. That young man was Mr. Marconi, whose name is now so familiar to us all. After the morning's work was done I was clearing away my apparatus in the preparation room attached to the Physical Lecture Theatre, where the meeting was held, and was having a word with Sir Oliver Lodge. As we were talking, Lord Kelvin came in—came up to Sir Oliver just like a schoolboy let out of school. For the program was over and he felt, like the rest of us that we had done our work. He said: 'Let's see, Lodge, weren't you on with something of that sort—with Hertzian waves?' 'Yes,' said Sir Oliver, 'and under the circumstances I'm sorry I didn't show this experiment myself. I have been so busy as General Secretary of the Association that I haven't had time to do what I planned to do during this meeting. I have been telegraphing by wireless signals between my house and this laboratory, and I intended to have had the installation fixed up to demonstrate to the members of this section.' Kelvin asked with enthusiasm: 'How far is it to your house? How far have you succeeded in getting good signals?' 'Oh, about two miles,' said Sir Oliver. I shall never forget the reply of Lord Kelvin. He said: 'That's right, Lodge. If Mr. *Macaroni* can go a mile, surely *you* can go two miles!' "

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Inactivation of Hypertensin

In an article by O. M. Helmer and K. G. Kohlstaedt (*Science*, 1945, **102**, 422) the action of horse-radish peroxidase on angiotonin, pepsitensin, and epinephrine was discussed. The authors also mention that, whereas small concentrations of iodine have only a limited inactivating effect on these substances, the peroxidase reaction is greatly enhanced by the addition of small amounts of potassium iodide.

We have previously shown (New York Academy of Sciences Conference on Experimental Hypertension, 9–10 February 1945) that the inactivation of hypertensin by iodine is related to the tyrosine present in the molecule—as established by the use of the Gerngross, Voss, and Herfeld reaction. The parallelism between decrease of hypertensin activity and intensity of iodation is illustrated in Table 1.

TABLE 1

INACTIVATION OF HYPERTENSIN BY IODINE

Hypertensin (cc.)	Iodine added (micrograms)	Pressure effect in the cat (mm./Hg)
0.1	52	22
0.1	104	12
0.1	156	7
0.1	208	4
0.1	—	24

We have also shown that the tyrosine titrated by the Gerngross reaction is transformed into di-iodotyrosine or iodine tyrosine, substances with little or no activity. It is shown in Table 2 that the hypertensive activity of hypertensin (angiotonin) is suppressed when tyrosine is blocked with iodine.

TABLE 2

	Control	1 drop	2 drops	3 drops	4 drops
Iodine added	0	52	104	156	208
Titrated tyrosine ..	180γ	142γ	115γ	85γ	40γ
Pressor activity in cat (in mm.) ...	25	21	15	9	3

Since we have established that an inactivation of the same order occurs with postpituitary hormone (pituitrin), we believe that tyrosine also plays a role in the hypertensive activity of these substances.

It appears that tyrosine combines with iodine only when it is present as free iodine or as iodine with a positive charge; this is obtained when peroxide and potassium iodide or solutions of metallic iodine or iodine chloride are used. The results of Helmer and Kohlstaedt show that potassium iodide used in conjunction with an oxidizing reaction destroys hypertensive activity and thus confirm our previous statements.

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Request for Reprints on Virus Diseases

Since the end of the war, letters and articles have appeared in numerous scientific periodicals requesting that reprints be sent to scientific workers in countries that have been out of contact with progress in America since 1940. One needs no more than a brief visit to become deeply impressed with the high regard these men have for the contributions and advances originating in American laboratories. There is a great need and an intense desire for reprints of published work in all fields.

I would like to extend to investigators in the field of virus diseases the request of two European colleagues—

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