pected alterations, which are, of course, opposite in sign to those affecting extensors of the same joints.

These various studies on postural tonus are best observed by the subject himself, for he alone can be certain that they are involuntary phenomena. However, they have been tried on numerous persons who were not aware of the responses to be expected, and the results, so far, have been concordant.

The evidence for the effects of light on muscle tonus, which has been obtained in the course of these studies, indicates that man possesses latent positive heliotropism. As demonstrated by Garrey[2] for the robber fly, the alterations of tonus are directed in such a way as to assist in turning the body toward the light.

Observations on these reflexes have proved very useful for the teaching of neurophysiology. It is hoped that they may also be of assistance in evaluating the degree of excessive or diminished tone of muscles in neurological examinations or in tests of fitness or fatigue.

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CHOLINESTERASE

IN SCIENCE (November 19, 1943) an attempt has been made by de Laubenfels[1] to claim for Alles and Hawes the priority of our discovery that two distinct cholinesterases exist in the animal body: a specific or true cholinesterase and a non-specific or pseudocholinesterase.[2]

Alles and Hawes,[3, 4] to whose work we referred in our first communication,[2] consider the cholinesterase activity of whole blood as due to the activities of a serum and a cell enzyme. This classification is based on a misconception. Experiments reported by us[5] show that the cholinesterase activity of serum is due to the presence of two distinct enzymes, one of which is specific like the enzyme in red blood cells[2] and brain,[6] the other being a non-specific catalyst. Consequently, any statement regarding the properties of the so-called serum enzyme would always refer to the properties of a mixture of these two enzymes. De Laubenfels' assertion that Alles and Hawes, who moreover were unaware of the existence of a specific and a non-specific enzyme, have "thoroughly demonstrated" the existence of the true and pseudo-cholinesterase is therefore invalid.

Regarding de Laubenfels' suggestion that the authors select more suitable names for discriminating between the two enzymes, we feel that the prefix "pseudo" emphasizes the non-specificity of the enzyme to which the name cholinesterase, suggestive of substrate specificity, has hitherto been applied. As we mentioned in the article in SCIENCE, the term "pseudocholinesterase" has been provisionally chosen until such time as the physiological function of this enzyme has been determined.

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APPARENT TIME ACCELERATION WITH AGE

I HAVE read the letters to SCIENCE on the apparent time acceleration with age, and I should like to add a comment that is based on a study of numbers I made several years ago. My thought is that our sensations of elapsed time is strongly influenced by the number of remembered and half-remembmered things that have occurred. Thus at age ten a single day may bring to a boy a number of new events, sensations and thoughts, while at 50 a considerably greater time must elapse before an equal increase is accumulated. These things that fix themselves in our memories are our units of time, and if at 50 a week passes without a remembered event that week is telescoped toward the vanishing point.

An astonishingly large number of natural phenomena are arranged on a logarithmic scale. Thus we may say that an eleven-pound dog is slightly larger than a ten-pound dog, but an 801-pound horse is the same size as an 800-pound animal. Here we would require an 880-pound horse (+10 per cent. as in the case of the dog) before we would admit a perceptible difference. This mode of thought, which sets up a logarithmic scale of measurement, is inherent, I believe, and it has strongly influenced our factual literature, of which memory of past events is a part.

Returning to our sense of elapsed time, I believe that we must add a fixed fraction to our accumulated sense of time before we admit the addition of a new unit, and this makes our elapsed time sense follow the same law that governs our sense of brightness, loudness, weight, etc.

FRANK BENFORD

I HAVE been interested in the discussion of the apparent acceleration of time with the age of the

[2] W. E. Garrey, *Jour. Gen. Physiol.*, 1: 101, 1918.
[1] M. W. de Laubenfels, SCIENCE, 98: 2551, 450, 1943.
[2] B. Mendel and H. Rudney, *Biochem. Jour.*, 37: 1, 59, 1943.
[3] G. A. Alles and R. C. Hawes, *Jour. Biol. Chem.*, 133: 2, 375, 1940.
[4] R. C. Hawes and G. A. Alles, *Jour. Lab. and Clin. Med.*, 26: 5, 845, 1941.
[5] B. Mendel, D. B. Mundell and H. Rudney, *Biochem. Jour.*, 37: 4, 473, 1943.
[6] B. Mendel and H. Rudney, SCIENCE, 98: 2539, 201, 1943.

individual, but I am wondering if we are not really thinking of our present recollection of the passage of time in youth and in later years rather than of the actual feeling of the passage of time that we experienced as it passed. Of course, no individual can compare his own time sense with that of any other individual, younger or older, because such sense is purely subjective and there is no basis of comparison; and it would seem practically impossible, too, for any of us to remember just how fast the days and months seemed to go by at any particular period in his past, so that here again we have no good basis for comparison. We can, however, compare the elapsed time between the remembered events of our past as they now lie in our memories. In my own case the elapsed time between my tenth and my twentieth years, for instance, seems much greater as I look back upon it than that between my fortieth and fiftieth. The reason for this I believe to be that in later years things that happened after we reached maturity seem much nearer *in proportion* than the events of childhood and youth, and this because we feel that they might have happened only yesterday, whereas the youthful happenings belong to another age.

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I HAVE read with much interest the papers recently published in SCIENCE as a sequel to a first discussion inaugurated by Frank Wilen some time ago on the "Apparent Time Acceleration with Age." However, I was surprised at the purely psychological treatment of the question and at the fact that all your contributors seemed to think that they were dealing with a new subject. May I remind them that the problem has been thoroughly investigated since 1916, when the first paper on the influence of age on the process of cicatrization was published in the *Journal of Experimental Medicine* (xxiv, 461), then in the *Proceedings* of the American Philosophical Society (1917) and later in the C. R. Ac. Sc., etc. The notion of "physiological time," different from physical time and its method of measurement, were introduced by the writer and discussed by many authors in this country, Professor Hoagland among others. It was fully developed (mathematically and psychologically) in a book published six years ago in New York.[1] Last year, at the April meeting of the American Philosophical Society, the writer presented a paper in which the different aspects of the question were expounded at length. An interesting discussion followed. I feel certain that Messrs. Carlson, Abbott and Harriss will be interested by the odd ten papers and the two or three books dealing exhaustively with this problem in a strictly scientific way, published up till 1936 in the United States, England, France and Germany. The most important references are to be found in the book mentioned below.

P. LECOMTE DU NOÜY

[1] Lecomte du Noüy, "Biological Time," Macmillan, N. Y., 1937.

THE SCIENCE MOBILIZATION BILL

THE letter from Dr. Leland H. Taylor on the Science Mobilization Bill (SCIENCE, November 26) seems to miss the point. He bases his argument on generalities, which are no answer to the specific objections to the specific provisions of the Kilgore bill which its opponents have adduced.

For instance, Dr. Taylor formulates two "pertinent" questions. The first reads, "Does our present organization of science promote the fullest advancement of scientific knowledge?" Since no human institution is perfect, the answer is obviously "No," but how helpful is it in determining whether the specific provisions of the Kilgore bill will accelerate or retard that advancement? Precisely the same comments apply to Dr. Taylor's second question.

Dr. Taylor seems to take at face value certain sweeping charges against industry which have been made in Washington. Does an accusation amount to proof which a scientist should accept? Has Dr. Taylor read the detailed refutation of many of those charges? He complains that only "a few liberal journals of small circulation" (does he so characterize *The New York Times* and *New York Herald Tribune,* which gave full space to those charges?) printed the accusations. The fact is that the charges were given much more space than the subsequent refutations, which may explain why Dr. Taylor missed the latter.

But even if the charges are accepted as proof, the case reads about as follows: Industrial research has resulted in inventions; inventions have been patented; and patents have in a few cases been unfairly used to extend monopoly beyond the bounds of the legitimate restricted monopoly which every patent confers. Therefore research must be reorganized and put under different control. An analogous case would be—research has produced a new and better alloy; that alloy has been used to make better knives; a few individuals have used those knives to commit murder. Therefore we must reorganize metallurgical research and put it under different control. Would it not be more logical to enforce, and strengthen, if need be, the laws against unfair restraint of trade and homicide?

Finally Dr. Taylor makes much of "selfish interest" as the mainspring of present industrial research, and condemns it. Others call it "enlightened self-interest," and praise it. What other motive force would Dr.